

Lacan Contra Althusser: Dialectical Materialism vs Nominalism

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Althusser's Marxism

In this paper, I will explore the consequences of rethinking the alliance and relation between Marxism and psychoanalysis, and more concretely between Althusser and Lacan. It is part of an ongoing investigation and study on the contemporary relevance of Louis Althusser's project. This study is driven by the following question: is Althusser's work *at all* repeatable (in the Žižekian understanding of the term)? And if the answer is yes, then what is it in his project that remains thinkable in our conjuncture? This question becomes even more pressing given the revival of the interest in his work in recent years. Perhaps it would be safe to argue that the revival of the interest in Althusser is predominantly conditioned by the revival of the scholarship of one of his main philosophical influences, Spinoza. As a consequence, it is not surprising that the greatest Althusserian scholars are specialists in Spinoza. It suffices to recall his students Étienne Balibar and Pierre Macherey, whose works on Spinoza are perhaps one of the greatest outcomes of what can be called the Althusserian field. In this instance, the return to Althusser implies its conditioning to the oeuvre of Spinoza. But, there is another level, another practice, in and through which Althusser's thought can gain a new dimension – pairing it with Lacanian psychoanalysis.

But, let us focus for a moment on what makes the return to Althusser philosophically and politically worthy in the present conjuncture. Many of his critics have argued that Althusser's Marx is an unknown Marx, or even an imaginary one. The Marx of which Althusser spoke, so they tell us, is an invented one, a Marx which cannot be found in his own writings. Perhaps the best-known example is that of his former student, Jacques Rancère, who in his *Althusser's Lesson* sets out to comment on Althusser's Marxism, which according to him, was misleading, a Marxism of closure, as it were.¹ It was a violent act of separating the paths, where Rancère accuses his former teacher of elitism, and sets to work out his theory in which there is no place for a "master" to speak to the masses, but the masses themselves go through subjectivisation.

In his autobiography, Althusser repeats several times that his knowledge on Marx was fairly limited. However, following Althusser's own lesson, we should reject this claim of its importance² and strip him off from the position of authority of his own thought. It is also important to note that later in his life Althusser became very critical of his concept of the *epistemological break* and came to admit that Marx did not break away from Hegel (and Feuerbach). Decisive in this 'conversion' was Jacques Bidet's *Que faire du 'Capital'?*³

Regardless of whether Althusser has read Marx correctly, or read him at all, he was able to formulate some of the most profound and sophisticated epistemological, political and philosophical theses of the time. And this is what should be of our concern: not the true Marx, but the best Marx, as Robert Pfaller brilliantly argues.⁴ To supplement, or rather to advance further Pfaller's thesis, I would argue that whatever Marx we get in Althusser's writings, it is not a Marx without Marxism, a depoliticized and culturalized Marx. To write about Althusser is to write about a Communist militant who also did philosophy. And this is the crucial element as well as the true difficulty in re-reading Althusser's work, regardless of the seasonal trend which is currently declaring him to be relevant. In our "neurotic obsessive" predicament, the *true* philosophical gesture is to avoid both a thoughtless acceptance of a master's thinking or the metonymical displacement from master to master. From the standpoint of historical materialism, far more interesting than critiquing these trends and to a certain degree pointing out on their 'falsity', is to focus on reading and interpreting them as indicators par excellence of the work of ideology in (our) situation. Again, it is easy to demonstrate the limits of Althusser – and many have done so – but it is much more productive to engage with precisely those limits and work *through them*. There is only one way in which we can understand Althusser's work (Marx's too, for that matter). Marx writes that "in so far as political economy is bourgeois, i.e., in so far as it views the capitalist order as the absolute and ultimate form of social production, instead of as

a historically transient stage of development, it can only remain a science while the class struggle remains latent or manifests itself only in isolated and sporadic phenomena.”⁵ Althusser cannot be read from a neutral position, or out of “objective knowledge”, but one has to be engaged, which in his own terms would be that to read Althusser means to occupy a position in philosophy *and* politics. Only from a partial position within the conjuncture of our time, one can read Althusser (and Marx). In other words, it is only from the proletarian position in philosophy (and politics) that one can read and understand Marxism in general, Althusser included. In older times, this position had a name: it was called a partisan position.

Althusser’s work stood for a double breakthrough: on the one hand, he was able to break away with orthodox Marxism, and fight against the ‘spontaneous ideology’ of post-68 capitalism, postmodernism on the one - and, on the other hand, it was able to continue thinking through the ambitions that characterized the previous sequences of Marxist theory. My thesis thus runs as follows: the return to Althusser in the contemporary philosophical-political conjuncture means the confrontation with that kind of current on the Left which has abandoned the difficult task of rethinking the difficult Marxist categories (i.e. exploitation, class formation and class struggle, et cetera). In this regard, it is our task to proceed from where he left off. Most of his critics (and the same holds for the majority of critiques towards Slavoj Žižek) presume that the road to socialism is clear and we need to stick to the old understanding of class, socialism, class struggle, etc. This is the point in which we should insist, more than ever, in the crucial importance of pure thinking, that is to say, to paraphrase Hegel, in philosophy without further determinations. Or, to quote Althusser, “Marxist theory can fall behind history, and even behind itself, if ever it believes that it has arrived.”⁶

We can speak of the limits of Althusser, and we should do so, but we should always bear in mind that in a given instance, the limits of Althusser’s project are simultaneously the limits of the 20th century socialist experiments. In this regard, the critique of Althusser’s project should be done *simultaneously* with the critique of the previous century socialism.

Marxism and Psychoanalysis

Psychoanalysis (and especially the Freudian-Lacanian one) and Marxism have a very complicated relation. There are many attempts which try to couple Marxism and psychoanalysis. Ernesto Laclau, for example, in his short text *Psychoanalysis and Marxism* suggests that the only way to think the relation between psychoanalysis and Marxism is through what, following Heidegger,

he calls “de-struction” of the history of Marxism, which means going beyond classical concepts such as “class”, “capital”, etc. In other words, for Laclau, it is post-Marxism as the political field which can co-exist, or more precisely intersect with psychoanalysis.⁷ For Laclau, the dialogue between the two disciplines is possible only under that condition.

The next point to be made here, already criticized by Žižek, concerns the attempt to find points of intersection and supplements between psychoanalysis and Marxism. One of the most common ways to understand this relation is to understand psychoanalysis as a supplement to Marxism. An almost classical example would be to take a situation in which the “objective conditions” for the revolution exist, but the revolution doesn’t take place. Usually, it is expected that psychoanalysis will provide the coherent explanation for the failure. Following this logic would imply that both psychoanalysis and Marxism are untenable in themselves, that both disciplines are in structural crisis, unable to answer to the new developments in our societies, and that help from another discipline is needed. Rejecting the two above-mentioned cases, this paper starts from the following premise: there is no *a priori* compatibility between Freud and Marx, and Lacan and Althusser. One cannot read Marx’s and Freud’s “fetish” as the same or complementary concepts; or surplus-value with surplus-enjoyment. Nor simply trying to find sentences in Lacan and Marx, which could serve as a support of one’s argument. As a philosopher once said everything resembles everything else in one way or another... but this means simply nothing. A much more refined dialectics should be put to work in order to reconcile both Freud/Lacan with Marx and Marx with Hegel, which goes well beyond the focus and the aims of this paper. This also means that there is no such thing as the “Marxist side” of Lacan as seen in his *Seminar XVII*, which is *the* Seminar in which the trio of Freud, Hegel and Marx are overly present.

The structure of psychoanalysis and that of Marxism are different. It is unimaginable for analysts to revolt on the couch, as do the workers in the factories. It is equally unconceivable for the analysts to get organised in a union or a Party, like the proletariat does. There is no natural affinity between the two disciplines. Simply put: the object of psychoanalysis is the unconscious, whereas for Marxism, it is the class struggle. In this sense, it seems to be rather difficult to imagine a class struggle in the field of the unconscious. This is the error of Wilhelm Reich, who attempted to locate the effects of the unconscious (Freud) with the effects of class struggle (Marx). No wonder that for him, the sexual liberation is associated also with the proletarian revolution, and the post-Bolshevik revolution was its realisation. For Lenin as well as for Althusser (although one can trace this back to Marx), the class struggle exists in three domains: economic, political and theoretical. Todd McGowan

provides an excellent account for the differences between Freud and Marx: “What distinguishes both Marx and Freud as thinkers is their understanding of social antagonisms. Where Freud sees antagonism manifesting itself in the excessive suffering of the individual subject, Marx sees it playing out in class struggle.”⁸ It is in this sense that McGowan aims to formulate the political theory of psychoanalysis, which is based on the Freudian concept of the death-drive. The inability to think and incorporate the death-drive in its project, represents the fundamental limitation of Marxism, McGowan argues: “The politics of psychoanalysis after Marxism is an emancipatory project based on the self-sacrificing enjoyment located in the death drive. Marxism is able to theorize sacrifice as necessary for future pleasure, but it is unable to conceive sacrifice as an end in itself, as a source of enjoyment.”⁹

Marxism is concerned with the class struggle and the working class taking over the state power. In other words, Marxism aims to grasp the effects of the class struggle. On the other hand, psychoanalysis begins and is concerned with individuals (analysands) and their sufferings.

But, are Marxism and psychoanalysis founded in such antagonistic positions as many are inclined to think?¹⁰ In 1976 Althusser wrote a short essay entitled *On Freud and Marx*. This is one of his most important essays, but as it often happens, it remains largely forgotten, if not repressed both by Lacanians and Althusserians. Althusser argues that, like Marx, Freud offered us an example of thought in dialectical materialism. For Althusser, Freud is truly a materialist because he rejects the primacy of consciousness, whereas the use of the categories of displacement, overdetermination, condensation and so on, belong to the field of the dialectic. But, there is another dimension to this paper, which is far more important for determining the *field* in which Marxism and psychoanalysis can intersect and co-exist. According to Althusser, the two other elements that Marxism and (Freudian) psychoanalysis have in common are a) they are both conflictual sciences, and b) their ultimate enemy is not an external attack, but revisionism:

It is a fact of experience that Freudian theory is a conflictual theory. From the time of its birth, and the phenomenon has not ceased to reproduce itself, it has provoked not only strong resistance, not only attacks and criticisms but, what is more interesting, attempts at *annexation* and revision. I say that the attempts at annexation and revision are more interesting than simple attacks and criticisms, for they signify that Freudian theory contains, by the admission of its adversaries, something *true* and dangerous. Where there is nothing true, there is no reason to annex or revise. There is therefore

something true in Freud that must be appropriated but in order that its meaning may be revised, for this truth *is* dangerous: it must be revised in order to be neutralized. There is a relentless dialectic in this cycle. For what is remarkable in the dialectic of resistance-criticism-revision is that the phenomenon that begins outside of Freudian theory (with its adversaries) always ends up within Freudian theory. It is internally that Freudian theory is obliged to defend itself against attempts at annexation and revision: the adversary always ends up by penetrating it and producing a revisionism that provokes internal counterattacks and, finally, splits (*scissions*). A conflictual science, Freudian theory is also a scissional science and its history is marked by incessantly recurring splits.¹¹

The same holds for Marxism, too. For Althusser, both Marxism and Freudian theory, have to defend themselves from themselves, as it were, from their inner deviations. He held that a rupture was inherent in psychoanalytic theory, as well as in Marxism; they are both situations in the very field in which they recognise as conflictual. It is for this reason that Althusser maintains that some practices need their concepts of such practices, in order to defend themselves against revisionism, opportunisms, et cetera.

A useful reference to Badiou can be done here. In his *Theory of the Subject*, Badiou makes a reference to the “black sheep of materialism”, where he says that Marxists should move beyond the linguistic idealism that has set in after the “discursive materialism” of Lacan, Foucault, Althusser, etc. For Badiou, only a materialist theory of the subject will divide the *idealinguisterie* into its ideal and material aspects, opening up to a renewal of Marxism again.¹²

From this, we should proceed to the equally complicated duo, the relation between Althusser and Lacan is controversial and certainly not clarified in the terms of *philosophical* overlappings and influences, as well as the formation of the thinkers based on the writings of the other. This means that my aim is not to reconstruct the Althusser-Lacan relation, the influences of one onto another and vice versa. It will not be concerned with the concept of *interpellation*, or with the debts to Lacan of Althusser’s conceptualisation of ideology, or with his readings of Freud and Lacan. To date, the most productive debate between Lacanians and Althusserians is reflected in the debate on the concept of interpellation between Mladen Dolar and Slavoj Žižek on the one hand, and Robert Pfaller on the other.¹³

In *Seminar XX*, Lacan makes a very interesting point, drawing parallels between Marx and Lenin, Freud and himself: “Marx and Lenin, Freud and Lacan are not coupled in being. It is via the letter they found in the Other that, as

beings of knowledge, they proceed two by two, in a supposed Other.”¹⁴ We should read this is from the perspective of the dialectical distinction between the founding figure and the formalization figure, introduced by Slavoj Žižek.¹⁵ In Marxism, it was Lenin who formalized Marx with the party-form organisation and intervention in the historical situation; in psychoanalysis, it was Lacan who formalized Freud; and in Christianity, Christ was formalised by St.Paul. In this sense, we can introduce a new level, which connects both practices. With Althusser’s Marxism and Lacan’s psychoanalytic theory, we are dealing with a relation that is unable to surpass its founding figures (Marx, Freud). Every reformulation, ‘correction’, and advancement goes not through a refusal, but to a ‘return’ (to Marx with Althusser, to Freud with Lacan). In epistemological terms, the knowledge of both theories is constitutively antagonistic, that is to say, errors are always-already part of the ‘real’ knowledge, which to repeat the previous claim of Althusser, revisionism is in a certain sense a constitutive part of the discipline. Marxism and psychoanalysis are ‘unique’ in the sense that they both determine the limitations of themselves, and they work through them to open them up.

In an apparent level, there is no such thing as a philosophical foundation of Marxism and psychoanalysis; they exist on another theoretical level and practice. In Lacanian practice, the end of psychoanalysis, or the dissolution of transference happens when the analysand comes to experience how the big Other (analyst) doesn’t have the truth about his/her desire. That is to say, how the desire of the analysand has neither guarantees nor grounds, it exists only as authorised by him/herself. The desire of the analysands has no support in the Other and he’s the instance of its authorisation. In this sense, we have a shift from the epistemological level to the ontological one. Or in psychoanalytic terms, the end of psychoanalysis is the shift from the desire to the drive. Marxism aims at transforming the object which “constitutes” it, thus at the same time, it gives rise to the revolutionary agent.

The contemporary Left dreams of a society in which social pathologies, neurosis, psychoses are eradicated. That the happy socialist paradise of equality also implies the well-being and happiness of all. But, let us cite Freud: “You will be able to convince yourself that much will be gained if we succeed in transforming your hysterical misery into common unhappiness. With a mental life that has been restored to health you will be better armed against that unhappiness.” If we replace three words from Freud’s passage about the purposes of analysis with the purposes of communism, we get the following result: You will be able to convince yourself that much will be gained if we succeed in transforming expropriation of labour into common unhappiness. With a social life that has been restored to justice you will be better armed against

that unhappiness.¹⁶ That is, one of the most important aspects of communism is to include the discontent into society. Discontent as such will never disappear, but we can utilize it for more creative and progressive purposes. This brings us to a crucial dimension of Marxism as well as psychoanalysis, that of the practice.

But, before we proceed with it, let us pause for a moment and point out a curious detail which points to a rather interesting difference between Althusserianism and Lacanianism. As we said earlier, Marxism, as the theory of Communism, is concerned with masses and classes, whereas two people constitute the psychoanalytic practice. Now comes the paradox of Althusser: unlike Lacan, Althusser didn't think of establishing his school or unifying his philosophy in a formal system. This is why François Matheron can claim, "the field of Althusserian studies has still not been constituted."¹⁷ Against developing a philosophical system, Althusser chose another path: that of philosophically intervening in particular political, ideological and philosophical conjunctures. In other words, a renewed Marxism would be of interest to all, but no systematization that would allow for this was made, while analysis is of interest to a few, but it was formalized to be available to all. Given the "interventionist" aspect of Althusserian philosophy and Marxism, I would argue that the "Althusserian" field is never fully constituted, but it exists only insofar as it is *in constitution*. In this sense relies on of their crucial differences.

On the Dialectical Materialism

One of the most important aspects of Althusser's work is the major switch from dialectical materialism to materialism of the encounter. Faced with the crisis of Marxism in 1970, he chose to abandon his philosophical apparatus of dialectical materialism and pursue another path; that of aleatory materialism, while at the same time remaining a Communist. In his text *Limits of Marx*, he announced that "at last the crisis of Marxism has exploded." As a result, he sought to rethink the potential of Marx's thought and Marxism in general. Althusser was seeking for, to paraphrase Badiou, new forms of political and philosophical subjectivity, without and free from the confines of the "theoretical monstrosity" called dialectical materialism.¹⁸

Let us make a short detour into the main core of Althusser's understanding of dialectical materialism.¹⁹ In an essay called *Materialist Dialectics*, Louis Althusser defines practice from the standpoint of a certain notion of rule (all practices, theoretical and ideological included, transform a raw material into a determinate product). Departing from this, we could criticize the theory of transformation as being the notion of concrete labour in capitalism. Isn't it the capitalist mode of production that has created the theory

of abstract labour, which is this general pattern all activities are supposed to carry as their infra-structure (the pattern of transforming an indeterminate x into a determinate y)? What if militant work requires a different theory of transformation in order to break away from the capitalist mode of production? Let us proceed with the beginning of Althusser's essay, which, in line with the "spirit of previous century Marxism", he gives either an outline or an attempt towards something. He begins this important essay with a very complicated proposal, which in fact condenses the whole problematic of dialectical materialism:

This article proposes the term *Theory* (with a capital T to designate Marxist 'philosophy' (dialectical materialism) - and reserves the term *philosophy* for ideological philosophies.²⁰

But, what does a practice means? Let us quote a longer passage, which can shed light to this definition as well as to his remarks on Lacan:

"I shall call Theory (with a capital T), general theory, that is, the Theory of practice in general, itself elaborated on the basis of the Theory of existing theoretical practices (of the sciences), which transforms into 'knowledges' (scientific truths) the ideological product of existing 'empirical' practices (the concrete activity of men). This Theory is the materialist *dialectic* which is none other than dialectical materialism. These definitions are necessary for us to be able to give an answer to this question: what is the use of a theoretical expression of a solution which already exists in the practical state? - an answer with a theoretical basis.²¹

Notice the strange equivalence of *dialectical materialism* and *materialist dialectic*.²² Should it be read as one and the same concept? Althusser is talking about a specific form of dialectics and equally a specific form of materialism. But, we should be precise on this point: for Althusser, "*Marxism-Leninism has always subordinated the dialectical Theses to the materialist Theses.*" This leads us to another crucial aspect, that of the relation between the thought and practice:

The exact theoretical expression of the dialectic is relevant first of all to those practices in which the Marxist dialectic is active; for these practices (Marxist 'theory' and politics) need the concept of their practice (of the dialectic) in their development, if they are not to find

themselves defenseless in the face of qualitatively new forms of this development (new situations, new 'problems') – or to lapse, or relapse, into the various forms of opportunism, theoretical or practical. These 'surprises' and deviations, attributable in the last resort to 'ideological errors', that is, to a theoretical deficiency, are always costly, and may be very costly.²³

Theory (with a capital T) is not the opposite of practice, the determinant field/aspect, but it is the ground upon which the practice itself is constituted, that is to say, where the production and 'manifestation' of knowledge is always-already part of the practice as such. As he himself puts it, "theory is important to practice in a double sense: for 'theory' is important to its own practice, directly.

But the *relation* of a 'theory' to its practice, in so far as it is at issue, on condition that it is reflected and expressed, is also relevant to the general Theory (the dialectic) in which is theoretically expressed the essence of theoretical practice in general, through it the essence of practice in general, and through it the essence of the transformations, of the 'development' of things in general."²⁴

This should be further understood against the common sense understanding of both theory (i.e. 'critical theory', which Althusser would qualify as a 'spontaneous ideology of theorists') and practice (the conviction that the Left has to be done away with theory and engage in the real and actual transformation of the world.)

Let us proceed in a schematic fashion, dear to Althusser himself, in order to recapitulate this problematic.

Philosophy declares positions, whereas theory produces problems. This is a very rigid and mechanical distinction but it might well provide us with the background. Althusser coined a new concept: his materialism is now called Theory, with a capital T. His materialism is Marxist philosophy, as he argues in the opening of this essay. Althusser is concerned with resolving problems through Marxist practice:

By *practice* in general I shall mean any process of *transformation* of determinate given raw material into a determinate *product*, a transformation effected by a determinate human labour, using determinate means (of 'production'). In any practice thus conceived, the *determinant* moment (or element) is neither the raw material nor the product, but the practice in the narrow sense: the moment of the *labour of transformation* itself, which sets to work, in a specific structure, men, means and a technical method of utilizing the means.

This general definition of practice covers the possibility of particularity: there are different practices which are really distinct, even though they belong organically to the same complex totality. Thus, 'social practice', the complex unity of the practices existing in a determinate society, contains a large number of distinct practices. This complex unity of 'social practice' is structured, we shall soon see how, in such a way that in the last resort the determinant practice in it is the practice of transformation of a given nature (raw material) into useful *products* by the activity of living men working through the *methodically organized* employment of determinate *means of production* within the framework of determinate relations of production.²⁵

Then he goes on arguing that

As well as production social practice includes other essential levels: political practice – which in Marxist parties is no longer spontaneous but organized on the basis of the scientific theory of historical materialism, and which transforms its raw materials: social relations, into a determinate product (new social relations); ideological practice (ideology, whether religious, political, moral, legal or artistic, also transforms its object: men's 'consciousness'); and finally, *theoretical practice*. Ideology is not always taken seriously as an existing practice: but to recognize this is the indispensable prior condition for any theory of ideology. The existence of a *theoretical practice* is taken seriously even more rarely: but this prior condition is indispensable to an understanding of what theory itself, and its relation to 'social practice' are for Marxism.²⁶

Following this, the work of Althusser, and especially his *For Marx and Reading Capital* should be understood as a critique of both hitherto existing conceptualisation of dialectical materialism and historical materialism, as well as the development of philosophical theses which guarantee and further develop the scientificity of Marx's historical materialism and its philosophical "effect", dialectical materialism. In order to break away with the philosophical and political obstacles in which it was caught, and freeing it from the various forms of deviations, Althusser announced a philosophical, that is, a historical and epistemological reading of Marx. The relation between science and philosophy is clear: philosophy goes through a radical transformation after every scientific breakthrough and science is *the* condition of philosophy. In other words, philosophy exists only under the conditions of science and politics.

The distinction between science and philosophy compels Althusser to call Marx a scientist and not a philosopher. For him, historical materialism is a science, the science of history, which was inaugurated by Marx in 1845, placing Marx in the same category as Thales, Galileo, and so on. That said, he sets the primacy of the science of history, with philosophy which accompanies it, i.e. dialectical materialism. The latter is always underdeveloped in relation to the former. In this sense, dialectical materialism is always behind. As he puts it elsewhere apropos the relation between science and philosophy, this is a determinate situation for philosophy: “*Outside of its relationship to the sciences, philosophy would not exist.*” Or, as he puts it in *Reading Capital*, “the theoretical future of historical materialism depends today on deepening dialectical materialism, which itself depends on a rigorous critical study of *Capital*. History imposes this immense task on us. Insofar as our modest means will allow, we should like to make our contribution.”²⁷

The crucial question runs as follows: why did Althusser have to abandon dialectical materialism in favour of the materialism of the encounter? It seems to have to do with nominalisms’ absolute homogeneity and immanence (all is in the same plane) while the “Theory of practice” which tries to put all practices at the same level (theory becomes one form of practice), but still keeps a certain difference between them, might not have been “immanentist” enough for his materialism, because it only writes the determinate moments (practice and theoretical practice) and not the indeterminate (or aleatory) ones (which he thinks nominalism can write or think more directly.)

Many of his commentators pointed out the *continuity* in Althusser’s thought, that is to say, they find elements of the materialism of the encounter from the 1960s. For G.M.Goshgarian, a translator and commentator of Althusser’s work, insists that from 1970 Althusser transformed his philosophy and thus reformulated his dialectical materialism into materialism of the encounter. In his understanding, only by reading Althusser’s late texts are we able to understand Althusser’s earlier philosophical periods.²⁸ The question of continuity in Althusser’s work in general, and particularly with regard to the presence of aleatory materialism throughout his work is a very difficult one. One of the ways to properly understand the continuity is if we emphasise the question of *materialism* and the *practice* it requires in order to be the philosophy of our time. In my understanding, this radical shift in his work remains one of the most important aspects in the whole of the Althusserian project. Due to the scope of this essay, I will limit myself to a few propositions. The first one concerns his understanding of practice. If we explore the consequences of rethinking its notion based on the Lacanian and Žižekian psychoanalytical and philosophical thought - especially considering the theory

of the drives, where means and ends can be inverted, and the theory of the subject, where the result of a transformation is not always determined, but is sometimes negative and elusive - we come to understand his limits.

Another aspect is, to understand it along the lines of the crisis of Marxism, in which Althusser, while completely recognising it, remained unquestionably loyal to Communism and the proletarian position in philosophy. Thus, aleatory materialism should be understood as his (last) attempt to rethink the communist project.

What is the materialism that Althusser defends in his late writings? In his autobiography, writing on Spinoza, Althusser says that:

he was also a nominalist, and Marx taught me that nominalism was the royal road to materialism. In fact, it leads only to itself, but I can think of hardly any more profound *form* of materialism than nominalism. Without offering any explanation of the origins of its meaning, Spinoza declared: 'We have a true idea', a 'norm of truth' provided by mathematics - yet another fact offered without any explanation of its transcendental origins. What is more, he was a man who believed in the *facticity* of facts, which was astonishing in a supposedly dogmatic person who deduced the existence of the world from God and his attributes! Nothing could be more materialist than this thought without origin or end.²⁹

The basic thesis of nominalism is "there are only cases", which Althusser borrows from Wittgenstein's "the world is everything that is a case". For him, this is a "superb sentence *says everything*."³⁰ When asked about his understanding and conceptualisation of nominalism and Marx's thesis that nominalism is the antechamber of materialism, Althusser gives the following elaboration:

Precisely; and I would go still further. I would say that it is not merely the antechamber of materialism, but materialism itself. Certain ethnologists have made a striking observation: that in the most primitive of observable societies, those of the Australian Aborigines or African Pygmies, nominalist philosophy seems to hold sway in person - not only at the level of thought, that is, of language, but also in practice, in reality. Conclusive recent studies have shown that, for these societies, there exist only singular entities, and each singularity, each particularity, is designated by a word that is equally singular. Thus the world consists exclusively of singular, unique objects, each with its own specific name and singular properties. 'Here and now',

which, ultimately, cannot be named, but only pointed to, because words themselves are abstractions - we would have to be able to speak without words, that is, to show. This indicates the primacy of the gesture over the word, of the material trace over the sign.³¹

Our position towards this thesis should be: Althusser became the Althusser only with *For Marx and Reading Capital*. His aleatory materialism should be subjected to the same critique to which he subjected his earlier period and especially to dialectical materialism. And an unexpected ally emerges here: Jacques Lacan. In (an unpublished) *Seminar XVIII* from 1971, Lacan critiques Althusser from the standpoint of (none other than) dialectical materialism:

If there is something I am, it is clear that I am not a nominalist. What I want to say is that my starting point is not that the name is something like a nameplate which attaches itself, just like that, onto the real. And one has to choose. If one is a nominalist, one has to renounce completely dialectical materialism, so that, all in all, I evidently reject the nominalist tradition which is effectively the only danger of idealism which can arise in a discourse like mine. The point is not to be a realist in the sense in which one was a realist in Medieval times, in the sense of the realism of the universals; the point is to emphasize that our discourse, our scientific discourse, can only find the real insofar as it depends on the function of the semblant. The articulation, and I mean the algebraic articulation, of the semblant-and because of this we are only dealing with letters-and its effects, this is the only apparatus which enables us to designate what is real. What is real is what opens up a hole in this semblant, in this articulated semblant which is the scientific discourse. The scientific discourse progresses without even worrying if it is a discourse of semblance or not. All that matters is that its network, its texture, its lattice, as one is used to say, makes the right holes appear at the right place. The only reference reached by its deductions is the impossible. This impossible is the real. In physics, we aim at something which is real with the help of the discursive apparatus which, in its crispness, encounters the limits of its consistency.³²

What is Lacan really saying with this? Lacan touches on one of the most important aspects of the philosophical 'debate', precisely because he takes a position which is anti-Althusser and anti-Foucault, to mention just the two. The sense in which Lacan is not a nominalist, is

not to be a realist in the medieval sense, but in the sense that our (scientific) discourse “can only find the real insofar as it depends on the function of the semblant”: reality is a semblant, but not in the simple sense that it is a deceptive appearance hiding true Being—there is nothing, no true substantial real, behind the veil of phenomenal reality. Reality is a semblant in the sense that its structure already materializes a certain fantasy which obfuscates the Real of a social antagonism. This is why we “can only find the real insofar as it depends on the function of the semblant”: by way of identifying the impossibilities, cracks, antagonisms which underlie and generate the inconsistent multiplicity of semblants.³³

For Lacan, there are not only particulars and the reality itself, but there are social antagonisms, which do not exist “as a case”, but that one has to refer to. To formulate it differently, there are many particularities which try to resolve the same antagonism, and Žižek’s example here is that of modernity. There are different ways which try to deal with capitalist modernity and its antagonisms: one is liberal democracy which argues that liberal freedoms will ‘tame’ class struggle, the other way is that of fascism (capitalism without class struggle/antagonisms), and so forth. All these are various attempts to handle the same central antagonism. In Lacan’s terms, this is the universality, which for him, is always the Real.

Now, to go back to our previous argument on the theory of the drives, which could be the crucial path which could solve this problem. Let us propose a thesis, by which we will end this paper: the Lacanian ontology of drives, given its immanent and transcendent aspect at the same time, is the ontology that solves the problem of nominalism in Althusserian philosophy. But, the question that remains unanswered is that about the relation between the death drive and practice. In what follows, I will propose a few ‘working thesis.’

Freud’s pleasure principle and its beyond, that is the death drive, has been an object of various interpretations. Gilles Deleuze argues that everything cannot be neither accounted for, nor governed by the pleasure principle. But, the Freud’s position is that in order to account for the pleasure principle, a more radical dimension has to be posited: that of the death drive and the compulsion to repeat, which makes it possible for the pleasure principle to act.

But what is the drive, and more precisely, the death drive? The death drive appears with the subjectivity, just as the subject enters or is alienated into the symbolic order. The constitution of the subject into the symbolic order is traumatic, it presents a loss of something which one doesn’t have, an

originary loss, and this is the point at which death drives aims at. This is why the repetition, which is a part of the death drive, is not the repetition of the same, but the repetition of the originary loss, which is the 'source' of enjoyment.

The drive is that something which persists, goes on even after the psychoanalytic session is over, that is, after the 'traversing the fantasy'. *In Seminar XI* asks "What, then, does he who has passed through the experience of this opaque relation to the origin, to the drive, become? How can a subject who has traversed the radical phantasy experience the drive? This is the beyond of analysis, and has never been approached."³⁴ Lacan's wager is: at the end of psychoanalysis, after traversing the fantasy, the desire is transformed into drive.

Neurotics always take the desire for demand. He mistakenly looks for a desire, where the field of that of the drive. But the desire always begins with a misrecognition, it is always-part of the nature of the desire. The neurotic does so because s/he believes in the loss of an object, but they fail to see that the object become such only through the loss. In this sense, it tries to do away with the drive by reducing desire to the desire of and for something, and thereby s/he works with the ideals alone, i.e. the ideal of the lost object. Desire always looks for a new object, object which would satisfy its needs. But, if this object were to be 'found', the desire would cease being such. For this reason, desire doesn't attempt satisfaction, but it attempts to maintain itself as a desire. It is always an imaginary anticipation of that which would realise a given want, or an imaginary sense of fulfilment. Any practice that is not based on this, would and shall be a practice that is not based on ideals. And perhaps, this is what Althusser wanted.

¹ Jacques Rancière, *Althusser's Lesson* (London/New York: Continuum, 2011). It is interesting to note here that all of his former students, at one point of their careers, distanced themselves from Althusser's philosophy and project. It seems that the only way for his students to pursue their own philosophical trajectories was to break away from Althusser's project.

² Judging from his own work, it is clear that one cannot trust him on this point. A better assessment on this would be to take into account that he was very self-deprecating, which incidentally is *the* leitmotif of his autobiography. Instead, we should rather assume that he thinks he didn't read it due to his insecurity, which is the best way of protecting himself from judgments and critique of his work.

³ Jacques Bidet, *Que faire du 'Capital'?* (Paris: PUF, 1985), in English translated

as Jacques Bidet, *Exploring Marx's Capital: Philosophical, Economic and Political Dimensions* (Leiden/Boston: Brill, 2006).

⁴ For an excellent defence of the contemporary relevance of Althusser, see Robert Pfaller, *Althusser's Best Tricks*, *Crisis and Critique*, 2:2, 2015, pp.25-45.

⁵ Karl Marx, *Capital Vol. 1* (New York, Penguin, 1976) p.96.

⁶ Louis Althusser, "Is it Simple to be a Marxist in Philosophy," in *Philosophy and the Spontaneous Philosophy of the Scientists and other essays* (London: Verso, 1977), p.230.

⁷ Ernesto Laclau, *Psychoanalysis and Marxism*, *Critical Inquiry*, Vol. 13, No.2, Winer 1987, pp.330-333.

⁸ Todd McGowan, *Enjoying What We Don't Have: The Political Project of Psychoanalysis* (Nebraska: The University of Nebraska Press, 2013), p.1.

⁹ Ibid.p.2.

¹⁰ It is interesting to repeat that in the socialist countries as well as most of the Western Communist Parties, psychoanalysis was considered a bourgeois discipline, an enemy of Marxism and the proletariat.

¹¹ Louis Althusser, *On Freud and Marx, Rethinking Marxism: A Journal of Economics, Culture & Society*, 4:1, Spring 1991, p.19.

¹² Alain Badiou, *Theory of the Subject* (New York: Continuum: 2009), pp.185-189.

¹³ Mladen Dolar, *Beyond Interpelation*, *Qui parle*, vol. 6, no. 2, Berkeley, CA; Slavoj Žižek, *The Sublime Object of Ideology* (London: Verso, 1989), Slavoj Žižek, "Class Struggle or Postmodernism? Yes, please", in Žižek, Slavoj, Laclau, Ernesto, & Butler, Judith, *Contingency, Hegemony, Universality: Contemporary Dialogues on the Left*, London: Verso; Slavoj Žižek, *Absolute Recoil: Towards a New Foundation of Dialectical Materialism*, London, Verso; Robert Pfaller, "Negation and its Reliabilities: An Empty Subject for Ideology", in Slavoj Žižek (ed) *Cogito and the Unconscious* (Durham: Duke University Press, 1998).

¹⁴ Jacques Lacan, *The Seminar of Jacques Lacan. Book XX. Encore 1972- 1973* (New York/London: Norton, 1999,) p.97.

¹⁵ Slavoj Žižek, *Repeating Lenin* (Arkzin; Zagreb, 2002), p.32.

¹⁶ Joseph Breuer and Sigmund Freud, “Studies on Hysteria”, in *The Standard Edition of the Complete Psychological Works of Sigmund Freud*, vol. 2, (London: Hogarth Press, 1955), p.305.

¹⁷ François Matheron, “Louis Althusser, or the Impurity of the Concept”, in Jacques Bidet and Stathis Kouvelakis, eds., *Critical Companion to Contemporary Marxism* (Leiden: Brill, 2008) p. 503.

¹⁸ In an interview conducted by Fernanda Navarro, Althusser explains that “it would be any exaggeration to say that Stalin’s political strategy and the whole tragedy of Stalinism were, *in part*, based on ‘dialectical materialism’, a philosophical monstrosity designed to legitimize the regime and serve as its theoretical guarantee - with power imposing itself on intelligence”, Louis Althusser, “Philosophy and Marxism: Interviews with Fernanda Navarro, 1984-87,” in *Philosophy of the Encounter: Later Writings, 1978-87* (London: Verso, 2006) p.244. For (also) an analysis of Althusser’s critique of Stalinism, see: Agon Hamza & Gabriel Tupinambá, *On the Organisation of Defeats, Crisis and Critique*, 3:1, 2016, pp.427-441.

¹⁹ For a more detailed analysis on Althusser’s dialectical materialism, see Agon Hamza, “Going to One’s Ground: Žižek’s Dialectical Materialism”, in *Slavoj Žižek and Dialectical Materialism*, A.Hamza & F.Ruda (eds) (Basingstoke: Palgrave Macmillan, 2015), pp.163-176.

²⁰ Louis Althusser, *For Marx*, (London: Verso, 2005) p.162.

²¹ *Ibid.*, p.168.

²² For a detailed elaboration on this, see Gabriel Tupinambá, “Splitting Althusser at the Point of Religion”, in *Althusser and Theology: Religion, Politics and Philosophy*, Agon Hamza, ed. (Leiden: Brill, forthcoming).

²³ Louis Althusser, *For Marx*, (London: Verso, 2005) p.169.

²⁴ *Ibid.*

²⁵ *Ibid.*, p.167.

²⁶ Ibid., p.166.

²⁷ Louis Althusser & Étienne Balibar, *Reading Capital* (London: Verso, 2009), p.84.

²⁸ G.M.Goshgarian, “Translator’s Introduction”, in Louis Althusser, *Philosophy of the Encounter: Later Writings, 1978-87* (London: Verso, 2006), pp.xiii-xlix.

²⁹ Louis Althusser, *The Future Lasts Forever* (New York: The New Press, 1993) p.217.

³⁰ Louis Althusser, “Philosophy and Marxism: Interviews with Fernanda Navarro, 1984-87,” in *Philosophy of the Encounter: Later Writings, 1978-87* (London: Verso, 2006) pp.265.

³¹ Ibid.

³² Quoted from Slavoj Žižek, *Less Than Nothing: Hegel and the Shadow of Dialectical Materialism* (London: Verso, 2012), p. 780.

³³ Ibid., p.782.

³⁴ Jacques Lacan, *The Seminar of Jacques Lacan Book XI, The Four Fundamentals of Psychoanalysis* (New York/London: W.W. Norton & Company, 1998), p.273.



BY

New directions for nominalist philosophers of mathematics

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Abstract The present paper will argue that, for too long, many nominalists have concentrated their researches on the question of whether one could make sense of *applications of mathematics* (especially in science) without presupposing the existence of mathematical objects. This was, no doubt, due to the enormous influence of Quine’s “Indispensability Argument”, which challenged the nominalist to come up with an explanation of how science could be done without referring to, or quantifying over, mathematical objects. I shall admonish nominalists to enlarge the target of their investigations to include the many uses mathematicians make of concepts such as structures and models to advance *pure mathematics*. I shall illustrate my reasons for admonishing nominalists to strike out in these new directions by using Hartry Field’s nominalistic view of mathematics as a model of a philosophy of mathematics that was developed in just the sort of way I argue one should guard against. I shall support my reasons by providing grounds for rejecting both Field’s fictionalism and also his deflationist account of mathematical knowledge—doctrines that were formed largely in response to the Indispensability Argument. I shall then give a refutation of Mark Balaguer’s argument for his thesis that fictionalism is “the best version of anti-realistic anti-platonism”.

Keywords Nonstandard analysis · Indispensability Argument · Nominalism · Fictionalism · Group theory

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1 Introduction

Philosophers of mathematics classified as ‘nominalists’ reject the Platonic idea that the mathematician is exploring, and providing the world with information about, a realm of “nonphysical objects”: the numbers, sets, functions and the like that mathematical theorems are supposedly about.¹ Nominalists believe that attempting to understand the nature of mathematics guided by such a conception is hopeless. Instead, they seek to achieve their goals by studying the way mathematics is used and developed in science and engineering and also by investigating both the history of mathematics and current mathematical practices—without the assumption that such things as mathematical objects truly exist.²

The present paper will argue that, for too long, many nominalists have concentrated their researches on the question of whether one could make sense of *applications of mathematics* (especially in science) without presupposing the existence of mathematical objects. This was, no doubt, due to the enormous influence of Quine’s “Indispensability Argument”,³ which challenged the nominalist to come up with an explanation of how science could be done without referring to, or quantifying over, mathematical objects. But how mathematics is applied in science and engineering is only one aspect of mathematical practice that requires investigating. It is true that I try to characterize the nature of mathematics in a way that is consistent with an overall account of science that is both widely accepted by experts and also consistent with our knowledge of the world.⁴ But, from my perspective, the goal of philosophy of mathematics is to provide an accurate “Big Picture” account of the essential nature of mathematics, as it is actually practiced.⁵ So I see no reason why philosophers should focus all their attention to just applications of mathematics in science.

I shall admonish nominalists to enlarge the target of their investigations to include the many uses mathematicians make of concepts such as structures and models to advance *pure mathematics*. I shall illustrate my reasons for admonishing nominalists to strike out in these new directions by using Hartry Field’s nominalistic view of mathematics as a model of a philosophy of mathematics that was developed in just the sort of way I argue one should guard against. I shall support my reasons by providing grounds

¹ The mathematician G. H. Hardy expressed a Platonic view of mathematics when he wrote: “I believe that mathematical reality lies outside us, and that our function is to discover or *observe* it, and that the theorems which we prove, and which we describe grandiloquently as our ‘creations’ are simply our notes of our observations” (Hardy 1941, pp. 63–64).

² For a sympathetic account of nominalism as I view it, see (Chihara, 2004, Chap. 6), especially Sect. 2.

³ One finds, in the literature, references to the Quine–Putnam Indispensability Argument because Hilary Putnam has put forward a version of Quine’s argument that has been widely discussed (see Chihara 2004, pp. 123–127). Other philosophers have developed versions of Quine’s argument (see Chihara 2004, pp. 126–127, for another example).

⁴ The idea is that philosophy is the search for a *coherent* account of the world and our place in it. Thus, philosophers of mathematics should seek an understanding of mathematics that is consistent with the other views we accept about the universe and about us. For a clearer and fuller explanation of my view of the nature of philosophy of mathematics, see the introduction to my (Chihara 2004), where a more detailed explanation of the view is given in the context of presenting my view of what philosophy is.

⁵ What I mean by a “Big Picture” account is explained in detail (Chihara 2004, p. 1).

for rejecting both Field’s fictionalism and also his deflationist account of mathematical knowledge—doctrines that were formed largely in response to the Indispensability Argument. I shall then give a refutation of Mark Balaguer’s argument for his thesis that fictionalism is “the best version of anti-realistic anti-platonism”.

2 Field’s responses to Quine’s argument

One of the most widely discussed arguments for a Platonic view of mathematics is what is called “Quine’s indispensability argument”. I call the original form of the argument “Quine’s challenge to the nominalist” because Quine can be regarded as challenging the nominalist to specify a system of mathematics that would be both adequate for the needs of the scientist and also nominalistic, in so far as it does not require the user to presuppose the existence of mathematical objects. If the challenge could not be met, then Quine could plausibly maintain that we have good reasons for believing that mathematical objects exist. Subsequently, many closely related forms of the “indispensability argument” have been proposed and defended.⁶ Much recent work in the philosophy of mathematics has focused on various forms of this argument and the published responses to them. Field’s account of mathematics is one such response.

Carl Hempel’s once defended the thesis that mathematics is “empty of factual content”—a thesis that gave rise to the need to explain how a theory that is “empty of factual content” (as Hempel characterizes mathematics) can be so useful—indeed essential—for the empirical sciences. Hempel responded to this need by espousing the view that mathematics functions in science as a kind of “theoretical juice extractor” by aiding scientists to infer the factual implications of their empirical theories—implications that are, in theory, deducible directly from the empirical laws and statements of the theories. Field came up with a more precise and mathematically attractive version of Hempel’s theoretical juice extractor view, the core idea of which is a principle, according to which any nominalistic sentence that can be deduced from a nominalistic theory with the aid of mathematics can also be deduced from the nominalistic theory alone.⁷ Using this principle, it is argued (against Quine) that the nominalist can legitimately use Platonic versions of mathematics in science without incurring a commitment to the existence of mathematical objects. The above principle allows the nominalist to use standard versions of set theory to draw nominalistic conclusions from nominalistic versions of science without presupposing the existence of mathematical objects, because the nominalist need not assume (or presuppose) that the theorems of the mathematical theory being used in this way are true. Field can thus adopt a fictionalist attitude toward mathematics: the theorems of mathematics that apparently refer to all sorts of objects that do not exist in the physical world can be regarded as sentences in works of fiction and not as truths (Field 1980, p. 2).

⁶ See (Chihara, 2004, Chap. 5, Sects. 2 and 3), for a detailed account of various versions of the Indispensability Argument, as well as a discussion of Quine’s challenge to the nominalist.

⁷ See (Chihara, 2004, pp. 109–111), for more details regarding Hempel’s idea and how Field produced a more rigorous version of Hempel’s idea.

3 Field's account: some details

Although the above rough sketch of Field's overall response to the indispensability argument should be sufficient for most readers to follow the essentials of the discussion to follow, some additional details of this response will be noted here, if only to facilitate making certain points more clearly and more precisely.

In this section, it should be understood that the languages and theories to be discussed are to be taken to be the languages and theories of first-order logic.⁸ For the most part, Field takes mathematics to be Zermelo–Fraenkel set theory, since it is widely accepted that any mathematical theory needed in the empirical sciences could be formalized in that set theory. Since he wants to allow the chosen mathematical theory to “speak” of things that the scientific theory discusses, he chooses Zermelo–Fraenkel set theory with urelements (henceforth ‘ZFU’). Now as a first approximation to what he wants his principle (henceforth ‘Conservation Principle’)⁹ to be, consider:

If N is a nominalistic theory, then $ZFU + N$ is a *conservative extension* of N (or as I shall sometimes put it: “ZFU is conservative over N ”)

where:

- (1) $ZFU + N$ is the theory obtained by conjoining the two theories ZFU and N , taking the vocabulary of the new theory to be the union of the vocabularies of the two theories and taking the assertions of the new theory to be the union of the assertions¹⁰ of the two theories;
and where:
- (2) T^* is a conservative extension of T if, every theorem of T^* that is a sentence of T is also a theorem of T .

Thus, the first approximation formulated above tells us that any sentence of the nominalistic theory N that is derivable in the joint theory $ZFU + N$ is derivable in N .

One problem with the above formulation of the Conservation Principle is that N might in some way contradict ZFU or, in effect, attribute all sorts of strange properties to sets when N is conjoined with ZFU . For example, N might say “Everything satisfies Newton's Laws”, thus implying that sets satisfy Newton's Laws. Of course, N is not really talking about sets when it asserts that everything satisfies Newton's Laws. So Field suggests that the quantifiers of N should be “relativized” so as to be explicitly about nonmathematical objects. This can be done as follows: introduce into the joint language, a monadic predicate M meaning ‘is a mathematical object’ and relativize the quantifiers of N to the nonmathematical objects in the following way: ‘ $(x)(Fx \rightarrow Gx)$ ’ becomes ‘ $(x)(-Mx \rightarrow (Fx \rightarrow Gx))$ ’ and ‘ $(\exists x)Fx$ ’ becomes ‘ $(\exists x)(-Mx \& Fx)$ ’. As for ZFU , it already talks about nonsets, so we need only add another monadic predicate to the joint vocabulary meaning ‘is a set’ and then add to its axioms a sentence that says “Every set is a mathematical object”, i.e. $(x)(Sx \rightarrow Mx)$.

The above details are generally omitted in discussions of Field's Conservation Principle because it is seen as a rather minor point and because Field decides in his book

⁸ This is not meant to suggest that Field always restricts his Conservation Principle to first-order logic.

⁹ This is the principle that Field calls “Principle C” in Chap. 1 of Field (1980).

¹⁰ The “assertions of a theory” is a set that is closed under the consequence relation.

not to introduce a special notation for the modified versions of $ZFU + N$ (he just assumes that $ZFU + N$ is written that way from the start (p. 12)). I provide these details because I wish to highlight a feature of Field's account that is frequently overlooked in discussions of his view: as Field formulates ZFU , set theory appears to be a kind of metaphysical theory. The metaphysical nature of its assertions is made explicit by the fact that, among its theorems, there are assertions of the form $(\exists x)Mx$, thus implying that the theory asserts the existence of mathematical objects.¹¹ Because Field believes that mathematical objects do not exist, he is convinced that ZFU cannot be a true theory.

It should be noted that treating mathematical theorems as metaphysical in nature is not new with Field. The two most eminent of the Platonist philosophers of mathematics, namely Kurt Gödel and Willard Quine, believed that the assertions of mathematics are metaphysical in nature. Of course, not all philosophers of mathematics adopt such a position.¹²

The version of mathematics presented above is of little interest to one concerned with applications of mathematics, since the vocabularies of the two conjoined theories have essentially nothing in common: there is practically no way that the mathematical theory can "interact" with the nominalistic part, so it is hard to see how such a theory can have much use in science. Thus, Field expands the vocabulary of the mathematical part to include the vocabulary of N so that the nominalistic vocabulary can appear in such axioms as *Separation* and *Replacement*. The expanded mathematical system, ZFU^* , can then map nominalistic objects to both pure and "mixed" mathematical objects (See Field 1980, pp. 9–10).

The Conservation Principle can now be stated as follows:

[CP] If N is a nominalistic theory, then $ZFU^* + N$ is a conservative extension of N .

What is one supposed to conclude from Field's reasoning about the Indispensability Argument? Field uses the Conservation Principle to justify one of the more controversial of his theses about mathematics: mathematics, according to Field, is not a "body of truths" and "no part of mathematics is true" (p. viii). Field's justification for adopting this remarkable thesis is tied to his attitude toward the Indispensability Argument. In the beginning of the preface to *Science Without Numbers*, he asks: "[W]hat good argument is there for regarding standard mathematics as a body of truths?" His answer: "The only non-question-begging arguments I have ever heard for the view that mathematics is a body of truths all rest ultimately on the applicability of mathematics to the physical world . . ." (Field 1980, p. viii). It is clear that the arguments Field has in mind here are the various versions of the Indispensability Arguments that his conservation strategy is supposed to undermine.¹³ Convinced that his strategy refutes

¹¹ The reason I suggest that ZFU is regarded as a sort of *metaphysical* theory is not because it has existence assertions, but rather because it seems to assert the existence of *mathematical* objects—something that metaphysicians typically assert, deny, or argue about.

¹² See, for example, my own position on this question in Chihara (2004).

¹³ Cf. "I believe it becomes clear that there is one and only one serious argument for the existence of mathematical entities, and that is the Quinean argument that we need to postulate such entities in order to

those “non-question-begging arguments”, Field concludes that “there is no reason to regard *any part of mathematics* as true”,¹⁴ and this leads Field to adopt the view that the sentences of mathematics are like those in works of fiction.

In concluding this section, I would like to emphasize two features of Field’s account of mathematics that contribute considerably to its attractiveness.

3.1 Field’s “no reinterpretation” account of mathematics

Field’s account of mathematics does not require that mathematical sentences be given any special interpretation in order to be applied in science; nor does it require that a special kind of logic be used in such applications. The nominalist can even employ, for example, ZFU—a first-order theory as standardly understood—in applying mathematics to science. Thus, he writes:

The way that has proved most popular among nominalistically inclined philosophers is to try to *reinterpret* mathematics—reinterpret it so that its terms and quantifiers don’t make reference to abstract entities . . . My approach is different: I do not propose to reinterpret any part of classical mathematics . . . (Field 1980, p. 2).

This feature of Field’s account has been thought to lead to the conclusion that the account fits actual mathematical practice perfectly. Since the nominalist can use the standard classical systems of mathematics to draw nominalistic conclusion from nominalistic theories, there does not appear to be any place for Field’s account to conflict with any mathematical practice.

3.2 Field’s fictionalism

Fictionalism has a substantial history. In the 1970s, I sketched a view about mathematics in which set theory was likened to a work of fiction. The view was a form of Platonism in so far as its adherents felt no intuitionist qualms about using the law of excluded middle, saw nothing wrong with the notion of the set of all real numbers or with impredicative specifications of sets, and did not think that mathematics was concerned with reasoning about mental constructions or strings of symbols. But these theorists did not believe in the existence of mathematical objects. They were not “ontological platonists”, but rather were “mythological platonists”. They could agree with those theorists who hold that the continuum hypothesis is neither true nor false by regarding the hypothesis as being analogous to the statement that ‘Hamlet’s nose was 4 1/2 inches long’—an assertion most thinkers would regard as not properly evaluated in terms of truth and falsity. When set theory is regarded in this fictionalist way, one

Footnote 13 continued

carry out ordinary inferences about the physical world and in order to do science” (p. 5). Also: “The hardest part of showing that the application of mathematics doesn’t require that the mathematics that is applied is true is to show that mathematical entities are dispensable in a way that theoretical entities in science are not . . .” (p. viii).

¹⁴ (Field, 1980, p. viii), italics mine.

could give explanations of certain sorts of facts that Gödel had used to support various Platonic conclusions.¹⁵

Interestingly, Field expresses sympathy for this early form of fictionalism in his book.¹⁶ And a very similar kind of fictionalism is espoused there by Field. Since he believed that he could refute the only non-question begging reason for thinking that mathematics is a body of truths, he concluded that there is no good reason for thinking that any part of mathematics is true, commenting:

This is not of course to say that there is something wrong with mathematics; it's simply to say that mathematics isn't the sort of thing that can be appropriately evaluated in terms of truth and falsehood. (Field 1980, p. viii)

4 The claimed superiority of fictionalism

The fictionalist view described above has been thought to be superior to all other nominalistic views that have been developed. Specifically, Mark Balaguer writes that “fictionalism is the best version of anti-realistic anti-platonism” (Balaguer 1998, p. 102). The grounds for this remarkable assessment of the relative superiority of fictionalism over all other anti-realistic anti-platonist theories is said to be based upon the fact that all competing nominalistic accounts of mathematics differ from fictionalism not in any ontological way, but “only in the interpretations that they provide for mathematical theory and practice” (p. 102). But then, it is argued, fictionalism can be seen to be superior to its competitors because of two admirable features claimed by Balaguer to be possessed by fictionalism. First, it is asserted that “there is nothing in mathematical practice that runs counter to fictionalism” (Balaguer 1998, p. 103). The idea is that the fictionalist account of mathematics fits perfectly the actual practice of mathematics. This first assertion supports the second: “fictionalism interprets our mathematical theories in a very standard, straightforward, face-value way, [whereas] other versions of anti-realism . . . advocate controversial, non-standard, non-face-value interpretations that seem to fly in the face of actual mathematical practice” (Balaguer 1998, p. 102).

5 Field's deflationist view of mathematical knowledge

Field's doctrine that the sentences of mathematics are like those in works of fiction is an important feature of his overall view of mathematics. It is this doctrine that motivates his “deflationist” position about mathematical knowledge—a position that attempts to account for “mathematical knowledge” without requiring the possessor of mathematical knowledge to know that any specific mathematical theorem is true. Since these theorems are held to be neither true nor false, it is thought that they could not be known to be true. Thus, in his article “Is Mathematical Knowledge Just Logical Knowledge?”, Field claims that “what separates someone who knows lots of math-

¹⁵ See (Chihara, 1973, Chap. 2, Sect. 2). Leslie Tharp developed and extended some of the ideas of mythological platonism in an attempt to arrive at a general account of the nature of mathematics in Tharp (1989). See, in this connection, Chihara (1989).

¹⁶ See (Field, 1980, fn. 4).

ematics from one who knows only a little mathematics is not that the former knows many and the latter knows few of such claims as those that mathematicians commonly provide proofs of . . .” (p. 81). What separates them, according to Field, is “empirical knowledge (e.g., about what other mathematicians accept and what they use as axioms)” and, more importantly, knowledge of a purely logical sort (p. 82). Thus, it is claimed that what chiefly distinguishes someone with lots of mathematical knowledge from someone with little is that the former, but not the latter, has lots of knowledge of truths of the form:

- (i) It is logically necessary that if A , then B
and
- (ii) It is logically possible that A .¹⁷

But what does it mean to say of a proposition that ‘it is logically necessary or logically possible’? Field tells us that “the modal knowledge which deflationism allows is knowledge of purely logical possibility—deflationism does not allow knowledge of mathematical possibility in an interesting sense” (1989, p. 85, n. 7). As Field uses the modal operator,

It is logically possible that ‘ $(\exists x)(x$ is a bachelor & x is married)’

is true. In Field’s modal logic, there are no “meaning postulates” that specify “logical” relations among the predicates. Thus, it is logically possible that there are married bachelors (in Field’s sense of the operator). In another work, he emphasizes the restricted sense he gives to his modal operator by limiting the logical truths to sentences “true by logical form alone” (Field 1992, pp. 114–115), noting that the logical truths he has in mind are “purely logical”.

Let us, then, distinguish two quite different theses being promulgated by Field. There is first the thesis about mathematical truth:

[NT] No part of mathematics is true.

Then there is the deflationist thesis about mathematical knowledge that is in essence:

[MK] What the mathematician knows, that the non-mathematician does not, are modal facts of the form (i) and (ii).

Of course, the two views are related. It is hard to conceive of a philosopher espousing [MK] if she did not already believe [NT]. A philosopher who accepted [NT] could not believe that the proofs mathematicians produce, in the course of doing what are called “proving a theorem”, are proofs of the truth of the theorem proved. Thus, [NT] pushes one to espouse a view according to which the theorems of set theory are not true but only logical consequences of the axioms of the set theory being assumed; and the knowledge obtained as a result of the proof is not knowledge of the truth of the theorem proved but only knowledge of something like:

It is logically necessary that (if A , then T)

¹⁷ (Field, 1984, p. 85).

where A is some finite set of axioms and T is the theorem proved. Consequently, [NT] pushes one toward some such proposition as [MK] and also provides [MK] with what plausibility it has. Since Field came to his belief in [NT] largely as a result of his juice extractor response to the Indispensability Argument, it can be seen that the deflationist view of mathematical knowledge was itself formed largely in response to the Indispensability Argument.

6 Doubts to be raised

There have been many criticisms of Field's view of mathematics—too many to discuss here.¹⁸ Still, some nominalists, who are primarily concerned with responding to the various indispensability arguments, may find Field's view of mathematics both promising and plausible. What I shall argue is that this view of mathematics is not so attractive when one is targeting for investigation the many uses of structures in *pure mathematics*, where reference to, and reasoning in terms of, structures have been found to be strikingly fruitful, even though the value of such uses cannot be *adequately understood or explained* in terms of the Conservation Principle.¹⁹ Furthermore, I shall give grounds for rejecting both his fictionalism and his deflationist account of mathematical knowledge, as well as Balaguer's argument for the superiority of fictionalism. However, to explore in a preliminary way reasons for questioning the cogency of Field's views on mathematics, consider the following developments in the history of mathematics.

7 A new approach to algebra

Two historians of mathematics have noted the following developments:

[U]ntil the 19th century, algebra was largely the science of (determinate and indeterminate) equations,²⁰ whereas in the 19th century there appeared in it completely new concepts and objects, such as groups, rings, fields, ideals, . . . [which] brought about a changed view of the subject matter of algebra. Specifically, the task of algebra was now seen to be the study of systems of arbitrary nature for which there are defined operations with properties more or less similar to those of addition and multiplication of numbers.²¹

In 1930, Bartel van der Vaerden published an influential textbook entitled “Modern Algebra”, which articulated the developing view of algebra that was coming into

¹⁸ See (Chihara, 2004, Chap. 11).

¹⁹ This is not to say that the fictionalist cannot give some sort of explanation of its value in terms of conservatism. I only claim here that such an explanation would not be adequate.

²⁰ It is noted that:

A system of polynomial equations is said to be indeterminate if it has fewer equations than variables, and to be determinate if the number of equations equals or exceeds the number of variables . . . The term ‘indeterminate’ is most commonly applied to systems with fewer equations than variables for which integer or rational solutions are sought. Such systems, also called ‘Diophantine’, have been particularly influential in the development of algebra . . . (Bashmakova and Smirnova 2000, pp. xv–xvi).

²¹ (Bashmakova and Smirnova, 2000, p. xiii).

vogue. The historian of mathematics Leo Corry describes this new view of algebra as *the structural approach to algebra*:

The essence of the structural approach to algebra lies in the recognition that it is mathematically enlightening to conceive a handful of concepts (groups, rings, fields, etc.) as individual “varieties” of the same mathematical “species”. . . , namely, the species of algebraic structures. With the adoption of this approach, the study of algebraic structures gave algebraic research a new focus, subsuming under it the traditional tasks of the discipline, namely, the study of polynomial forms and polynomial equations, and the problem of the solvability of polynomial equations. Moreover, under the new approach to algebra, this discipline came to cover under its unified scope, the study of other related, but theretofore separated domains of research, particularly algebraic number theory. ²²

The developments being described above had far-reaching and long-lasting consequences in the field. ²³ The subsequent widespread adoption of the structural approach both in algebraic research and in the teaching of the subject yielded many benefits: many open problems were solved in an economic and elegant way, previously solved problems were presented in a new and interesting light, and new mathematically intriguing problems were formulated (Corry 1996, p. 9). In a word, the new approach was mathematically *fruitful*.

Without attempting any sort of deep analysis of these developments in the subject, one can see one obvious reason why the structural approach has appeared attractive to algebraists: the efficiency with which the subject matter of algebra can be studied and developed in terms of algebraic structures is a real advantage. It is the sort of efficiency that aids mathematicians in learning and teaching the material. This efficiency can be observed in the following way of presenting the fundamentals of algebra.

Imagine a graduate course in abstract algebra that begins with the theory of groups. Such a start would be fitting since the concept of group is both simply expressed and easily grasped, even though it is “one of the most important concepts of modern

²² (Corry, 1996, p. 9). One should not infer from the above that van der Waerden was solely or even principally responsible for advancing the structural approach to algebra. His textbook “assembled the important results that had been obtained during the last decades of research in its domain of concern and exposed them in a systematic, and didactically clear, fashion” (Corry 1996, p. 8). It should also be noted that the quotation from Bashmakova and Smirnova (2000) emphasizes that, until the 19 th century, algebra was largely the science of *both* determinate and indeterminate equations, whereas the quotation from Corry (1996) gives no indication of the importance of indeterminate equations for the early algebraists. In the Introduction to their work, Bashmakova and Smirnova write:

Modern history of mathematics seems to be dominated by the view that up to the 1830s the mainspring of the development of algebra was the investigation and solution of determinate algebraic equations, and especially their solution by radicals. We will show that this viewpoint is one-sided and gives a distorted representation of its evolution. In short, we claim that the role of indeterminate equations in the development of algebra was no less important than that of determinate equations. (Bashmakova and Smirnova 2000, p. xv.)

²³ van der Waerden’s text was still being referred to in algebra courses, even when I was a graduate student in mathematics. Also, a referee informed me that van der Waerden’s book was used as the textbook for an abstract algebra course he attended in the nineties.

mathematics”.²⁴ One can proceed by first giving the definitional axioms of a group as follows:

A group is a system consisting of set and an operation on the set such that: (a) the set is closed under this operation; (b) the operation is associative; (c) the set has a right identity element; and (d) every element of the set has a right inverse.

An example of a group usually given is the integers under addition. Then, after proving from the axioms the basic features of all groups, the term ‘ring’ can be defined as follows:

A ring is a system consisting of a set S and two binary operations on S , *plus* (symbolized ‘+’) and *times* (symbolized ‘×’), such that:

- (1) the system consisting of S with the operation *plus* is a commutative group;
- (2) S is closed under the operation *times*, and *times* is associative;
- (3) for every a, b , and c in S ,

$$a \times (b + c) = (a \times b) + (a \times c)$$

$$(b + c) \times a = (b \times a) + (c \times a)$$

An example of a ring that is frequently given is the integers under addition and multiplication. One can then apply to that part of a ring consisting of S with plus all that was learned earlier about abstract groups. For example, it can be immediately inferred that, in a ring, there is only one additive identity; that any element’s right inverse is unique and is identical to the element’s left inverse; and that the inverse-of-a-sum law holds—that is, the inverse of a sum of elements is identical to sum of the inverses of the elements, taken in reverse order. Clearly, much redundancy of exposition and learning can, in this way, be eliminated. The effectiveness of this sort of efficiency is illustrated by the following example.

8 Nonstandard analysis

Nonstandard analysis was developed by Abraham Robinson in the 1960s to provide a “framework for the development of the Differential and Integral Calculus by means of infinitely small and infinitely large numbers”²⁵ (Robinson 1996, p. xiii). Infinitely small numbers (or infinitesimals), as well as infinitely large numbers, can be introduced by first defining the real numbers as equivalence classes of Cauchy sequences

²⁴ (Bashmakova and Smirnova, 2000, pp. 125–126). These authors go on to write:

Group theory plays a crucial role in quantum physics. Wolfgang Pauli, one of the creators of quantum physics, wrote that the ideas of group theory belong to “the most powerful instruments of modern physics”, and that, in his view, their fruitfulness is “far from having been exhausted” (W. Pauli, *Collected Scientific Papers*, ed. R. Kronig and W. F. Weisskopf, 2 vols., Interscience, 1964). The subsequent evolution of physics brought with it a splendid confirmation of Pauli’s words. (pp. 126–127)

²⁵ This framework was used to develop more advanced branches of analysis, such as Functional Analysis and the theory of functions of complex variables.

of rational numbers and then introducing an equivalence relation over *arbitrary* sequences (not necessarily Cauchy) of real numbers to specify a new totality of numbers (the “hyperreal numbers”) which are defined to be certain equivalence classes of these sequences of real numbers.²⁶ An ordering relation can then be defined over the hyperreals, and then proved to be a linear ordering. Addition and multiplication of the hyperreals can be so defined as to satisfy the usual algebraic laws.²⁷

Among these hyperreal numbers, there are numbers that, by the above mentioned linear ordering, are positive (greater than zero) but less than every positive real number. Such numbers are called ‘positive infinitesimals’. In addition to the positive infinitesimals, there are negative infinitesimals. A hyperreal number is an infinitesimal if it is a positive infinitesimal, zero, or a negative infinitesimal.

To see how the introduction of infinitely small numbers can mesh with clear intuitions about fundamental concepts in the Differential and Integral Calculus, Robinson gave the following example:

[J]ust as the (mean) velocity moving along a straight line between times t_1 and t_2 is given by

$$(s_2 - s_1) / (t_2 - t_1)$$

where s_2 and s_1 are the corresponding displacements of the particle relative to a fixed point, so one is inclined intuitively to define the instantaneous velocity of the particle at time t_1 by the same ratio, where $t_2 - t_1$ and hence $s_2 - s_1$ are now supposed to be infinitely small.²⁸

Noting that Leibniz adopted just such an approach to his calculus, Robinson concluded with the comment that “a method which had been given up as untenable has at last turned out to be workable” (Robinson 1973, p. 16).

Now the algebraic structure described above can be proved to be a *non-Archimedean* ordered field.²⁹ One can then apply to the structure of the hyperreal numbers everything proved in abstract algebra about fields, thus shortening the needed proofs and theoretical exposition of this novel structure. In particular, since a field is a special kind of ring,³⁰ one can infer immediately that the inverse-of-a-sum law holds for addition of the hyperreal numbers.

Essentially everything I said above about algebraic structures is acceptable to classical mathematicians—but not to the fictionalist. The ring of integers under addition and multiplication is a set of numbers, and hence is, for Field and his followers, a mere fiction that has no more reality than unicorns and Chimeras, and the mathematical theorems about them are taken to be like the sentences in a work of fiction: not appropriately evaluated for truth or falsehood. What, then, could be the rationale for developing an algebraic theory consisting of a great many non-factual sentences about fictional objects that are not appropriately called true or false? One cannot easily

²⁶ The equivalence relation is called by H. J. Keisler “an ultraproduct equivalence relation”, asserting that such a relation can be proved to exist (Keisler 1976, p. 879).

²⁷ Sums and products are defined in the usual way of defining operations with Cauchy sequences.

²⁸ (Robinson, 1973, p. 16).

²⁹ See (Davis, 2005, pp. 45–56), and (Keisler, 1971, p. 822).

³⁰ A ring is a field if it satisfies the following conditions: (1) the operation \times (multiplication) is commutative; (2) it has at least two elements; (3) it contains a multiplicative identity; (4) it has no proper divisors of zero (i.e. the additive identity); (5) every element that differs from zero has a multiplicative inverse.

argue that the structural view has proved to be fruitful in mathematics because that sort of algebra turns out to be conservative over nominalistic theories. Since the structural approach to algebra was justified by its fruitfulness in *pure mathematics*, where the distinction between nominalistic and platonic languages needed in the proof of the Conservation Principle is simply not made, *such a strategy does not adequately account for the universal acceptance of the modern algebraic approach by pure mathematicians, most of whom were not especially concerned with applying algebra to science.*³¹

9 Groups of transformations

Let S be a set and consider the set of 1–1 transformations of S onto itself. This set of transformations of S forms the basis for a well-known group. We can define an operation (to be called ‘addition’) involving these transformations as follows: Let O be any ordering of S . If P_1 and P_2 are such transformations, then let $P_1 + P_2$ be the ordering that results from first applying transformation P_1 on O and then applying transformations P_2 on the ordering that results from the first transformation. It can be seen that $P_1 + P_2$ will result in an ordering identical to what results from performing some one of the possible $n!$ transformations, say P_k , of the original ordering O . Thus, we can take $P_1 + P_2$ to be identical to P_k . In short, the collection of these transformations can be regarded as closed under this sort of addition. It is a trivial matter to go on to show that this operation of addition is associative. Also, the trivial transformation that leaves the position of everything unchanged can be specified to be the *identity* transformations (or the identity element of the addition operation). Since for every transformation P , there is a transformation that undoes the changes that P makes, one can conclude that every transformation has a right inverse, and hence that this system is a group.³²

We can immediately infer that all the usual properties of groups hold of this system. We can infer, for example, that the identity element is unique; that any element’s right inverse is also unique and is identical to the element’s left inverse. We can also conclude that the inverse of $(P_1 + P_2)$ is identical to (the inverse of $P_2 +$ the inverse of P_1).

Groups of transformations are particularly important in algebra because of the following fundamental theorem (due to Arthur Cayley):

Every group is isomorphic to some transformation group.³³

³¹ This is not to suggest that the fictionalist cannot come up with some sort of explanation of the development of modern algebra in terms of conservatism. However, any such explanation, I believe, will be neither adequate nor very plausible.

³² Lest one think that this example is esoteric and artificial, it should be noted that transformation groups play an important role in geometry—furthermore, the study of transformation groups “provided the original impetus to the development of the theory of groups” (Jacobson 1951, p. 15).

³³ See (Jacobson, 1951, pp. 28–29), for a proof of this theorem.

10 A quasi-model of group theory

Imagine now that eight wooden blocks have been lined up to produce a linear ordering. Each of the $8!$ (or $40,320$) transformations of the set of blocks can be regarded as yielding a permutation of the original ordering. Thus, if $t(x)$ is such a 1–1 transformation, then just think of replacing each x in the original ordering with $t(x)$: the ordering of the set that results from these replacements yields a reordering that is a permutation of the original ordering—so long as there is some block y which is not the same as $t(y)$. For the case of the identity transformation, we do not obtain a true reordering, but for sake of simplicity of discussion, we regard the identity transformation as yielding a permutation. Thus, we will say that there are $8!$ (or $40,320$) permutations of this ordering.

We shall define addition of these permutations as we did earlier for the permutation groups, except now we say that $P_1 + P_2$ is the ordering that results when first the permutation P_1 is actually performed by some competent agent armed with a suitably programmed computer³⁴ and then P_2 is actually performed by that same agent on the ordering that results from the first permutation. We now have a quasi-model of group theory.

Why do I call it a “quasi-model” (and not a model)? Well, defining the addition operation in terms of *what results* from an actual agent performing various permutations makes the operation dependent upon the powers of the agent and the computer used. If the agent were god-like, then perhaps we would have a model of group theory, but since we are concerned here with an actual (human) agent, we need to take account of the possibility that the agent may commit errors or become completely exhausted at some time. Thus, we are not in a position to assert that all the axioms of group theory are true under the interpretation, and we do not have a genuine model but only a quasi-model. The situation is not unlike the case of an actual computer modeling a Turing machine: since any actual computer is liable to make errors or break down and are limited in the number of operations it can perform, they cannot be true Turing machines, but they can mimic a Turing machine in various ways. Similarly, the above quasi-model can be used to model permutation groups in certain ways.

Consider now the theorem of group theory: “Every element has only one right inverse”. Using the above quasi-model, we can test the truth of the theorem by examining any number of specific cases of right inverses of permutation sums and seeing if what the law states does indeed hold. (Actually, most mathematicians will be able to see from a single case that the theorem will hold for every case). So does not this result suggest that the theorem of group theory has a content that can be tested and verified?

This time, consider the inverse-of-a-sum law of groups. We could take any two permutations P_1 and P_2 of the ordered eight wooden blocks and then perform the sum of permutations $P_1 + P_2$. We could then determine the inverse permutation, say P_j , of the sum. We could also determine which permutation, say P_k , is the sum of the inverse of

³⁴ It can be stipulated that the computer is programmed to: (a) output the inverse of a permutation x of the blocks, when x is inputted; and (b) output the sum of x and y , when the two permutations x and y are inputted.

P_2 + the inverse of P_1 . As in the previous case, finding that $P_j = P_k$ would constitute a verification of the truth of the inverse-of-a-sum law. Of course, we could verify the truth of the theorem additionally by taking another pair of permutations and repeating the above verification for that pair. Indeed, we could, in theory, test the theorem for all possible pairs of these permutations and thus get a very convincing verification of the inverse-of-a-sum law. Clearly, the inverse of a sum law can be repeatedly tested and found to be true, very much like a statement of an empirical science. Of course, verifications of this sort have in fact been carried out by countless students throughout the world, using essentially the above type of quasi-model. Such verifications cast considerable doubt on Field's thesis [NT] that no part of mathematics is true.³⁵ Since Field has produced no good reason to deny that the theorem is true, the preponderance of evidence is surely on the side of the mathematicians who believe that the theorem is true.³⁶ We could devise similar verifications of many of the theorems of group theory to undermine the plausibility of Field's [NT].

What about Field's deflationist thesis [MK]? Field's only reason for espousing the view that, apart from some empirical knowledge of what mathematicians may assert, what distinguishes the mathematician who knows a lot of mathematics from someone who knows little or no mathematics is modal knowledge of the two forms listed earlier. But does the algebraist's knowledge of the inverse of a sum law of group theory consist merely in the knowledge of what other mathematicians may assert? Let (*) be statement:

In group theory, the inverse of a sum is equal to the sum of the inverses of the summands taken in reverse order.

Surely it would be a mischaracterization to describe the algebraist's knowledge of the truth of (*) as knowledge of what other mathematicians will respond to various questions about inverses. For, even if she believed that all other mathematicians had somehow had their knowledge of the inverse of the sum law completely erased by an evil demon, she could still know that (*) is true. Then should we maintain that the algebraist's knowledge of the truth of (*) *must be* knowledge of logical necessity of one of the two types Field listed? Most specifically, must the mathematician's knowledge be knowledge that *it is logically necessary that*, in group theory, the inverse of a sum is equal to the sum of the inverses of the summands taken in reverse order? Here, I must recall to your attention that, as Field articulates his concept of logical necessity, (*) is not logically necessary. (Recall that, as Field defines 'logical necessity', it is not logically necessary that all bachelors are unmarried).

Besides, even if (*) were logically necessary, still (*) expresses a proposition distinct from:

³⁵ Those who have studied my book (Chihara 2004) may wonder how I can allow that the theorem about inverses of sums is true. Such readers should note that the theorem expresses a feature of the group structures that is essentially identical to what the sentence expressing the structural content of the theorem does.

³⁶ Before concluding, from a rejection of the Indispensability Argument, that there is no reason to believe that any part of mathematics is true, one should surely investigate why a great many, if not practically all, mathematicians believe that much of mathematics is, in some way, true. Such a common and widespread belief cannot be based upon the Indispensability Argument.

(**) It is logically necessary that in group theory, the inverse of a sum is equal to the sum of the inverses of the summands taken in reverse order.

(*) and (**) express different propositions, and some mathematicians could know (*) without knowing (**). Thus, we can see that difference between the set of propositions known by mathematicians and the set of propositions known by non-mathematicians will include more than propositions of the two types Field specifies.

The above discussion shows the dubiousness of Field's two theses [NT] and [MK]. It also underlines the importance of *the specific content possessed by an individual theorem*—something that tends to be overlooked by those accepting the theoretical juice extractor view.³⁷ After all, what the algebraist infers from the inverse-of-sum law is quite different from what she infers from the uniqueness-of-the-right-inverse theorem, and she would verify the former theorem in a way that is very different from the way she would verify the latter theorem. Do we not, then, have compelling reasons to think that the inverse-of-a-sum theorem *has a specific content* which tells us something quite definite about inverses of sums of permutations of arbitrary sequences of these blocks? The omission, in Field's account, of this striking feature of individual theorems is undoubtedly due to the fact that his fictionalist understanding of mathematics was developed almost solely in response to the indispensability argument—a response which required no role at all to be played by the contents of individual theorems. We can see now how a philosopher of mathematics, by focusing so intently and single-mindedly on just one problem or contentious issue in the philosophy of mathematics, can be led to assert and to maintain a doctrine as implausible as Field's that “there is no reason to regard *any part of mathematics* as true”.

11 How Field might attempt to revise his account

David Etlin suggested at my talk that Field might respond to my objections by revising his position to allow *some* theorems of mathematics to be true: according to the revised position, only those theorems that explicitly assert the existence of mathematical objects would be counted as false. Thus, the theorem asserting that every group has a unique identity could be classified as true, since it does not explicitly assert the existence of any mathematical objects.

Now let us look more carefully at the suggested revision. The idea is to classify as false only those theorems that explicitly assert the existence of mathematical objects. However, it can be easily established that this revised fictionalism will be saddled with serious problems arising from the fact that sets of mathematical theorems, all the members of which would be classified as true, could still imply the existence of mathematical objects and thereby entail what would be classified as a false statement. For example, the following theorems of set theory would be classified as true, since neither explicitly asserts the existence of any mathematical objects:

If no unit set is empty, then there is an empty set.

³⁷ Cf. the idea of *the structural content of a mathematical theorem* discussed in great detail in my (Chihara 2004).

No unit set is empty.

But the conjunction of the two implies that there is an empty set, which must be classified as not true. This shows that the revised-fictionalist must come up with a better way of distinguishing the theorems that are to be classified as true from those that are to be classified as false, if she is to avoid the sort of problems I have indicated above.

A reviewer for this journal has suggested the following way of revising Field's account of mathematics:

The obvious modification is to claim that some non-existence entailing mathematical claims are true [T]he most natural suggestion is to treat mathematical predicates as having empty extensions and mathematical names as empty names. On this view, some non-theorems of (for instance) Peano Arithmetic are true. For instance, the claim "Every number is prime" is true, since there are no numbers There is, of course, a serious question about how to handle empty names in logic, but this does not pose a special problem for the case of mathematics, since it arises in non-mathematical contexts, too. Whatever free logic we adopt for non-mathematical contexts can be extended to mathematics.

I would now like to respond to this suggestion first by making my position clear. I certainly never claimed, nor did I ever suggest, that there is *no way* for Field to revise his account of mathematics so as to obviate my objections above, while preserving his basic position on the conservatism of classical mathematics. No doubt, there are many different ways of devising such a revision. But one of the main points of my paper was to show that developing one's view of mathematics by focusing almost solely on applications of mathematics in science can lead to a distorted understanding of the nature of mathematics. Also, I wished to show that, had Field explored more thoroughly features of pure mathematics, he probably would have adopted a significantly different brand of nominalism. These points, I shall argue, are not undermined by any such revision.

If Field were to accept the reviewer's suggestion, he could not continue to hold that his account of mathematics does not require any "reinterpretation" of mathematics. Interpreting mathematics in the way being suggested certainly does require a significant reinterpretation of mathematics. It is clear that practicing mathematicians do not understand mathematical sentences in the way being suggested.

Consider the reviewer's suggestion regarding the form of free logic to be employed in mathematical contexts. Evidently, following the suggested revision would involve changes in our practices that would make the mathematician's theorizing even more complicated and more difficult than it is. Certainly, these are not changes in our mathematical practices that one should advocated lightly.

Field's account of mathematics, if revised in the way suggested, would not be in perfect agreement with our present mathematical practices. Contemporary mathematicians do not assert that such sentences as 'All natural numbers are prime' and 'Every set has a member' are true. Nor do they maintain that 'There are infinitely many prime numbers' is a false statement. The revised position simply would not fit the actual practices of contemporary mathematicians.

Also, these revisions would require Field to abandon his "fictionalism", according to which it is not appropriate to assess mathematical theories in terms of truth and falsehood: since, under the suggested revision, many, if not all, the statements of

mathematics are either true or false, it would indeed be appropriate to evaluate such theories in terms of truth and falsehood.³⁸

The abandonment of his fictionalism would also engender trouble for Field's deflationist account of mathematical knowledge. This is because, according to the suggested revision, there would be many mathematical truths—truths expressed by, for example, theorems of group theory—that are known to be true by mathematicians but not known to be true by non-mathematicians. For example, (*) would express a truth, but (***) would not; and, as was shown earlier, (*) would *not* be of either of the two modal forms specified by Field's deflationist account. In short, accepting the reviewer's suggested revision would require the abandonment, or significant revision, of Field's deflationist account.

12 A reexamination of the claim that fictionalism is superior to all its rivals

I would like now to return to Balaguer's argument for the superiority of fictionalism over *all* its nominalistic rivals. Recall that this reasoning was based, in part, on his assertion that there is nothing in mathematical practice that runs counter to fictionalism. To test this assertion, I should now like to reconsider the example of nonstandard analysis.

One of the central principles of nonstandard analysis is given by the *Transfer Principle*—a theorem, really, that tells us very roughly that there is an extension of the real numbers that includes the infinitesimals of nonstandard analysis and that has the same properties as the standard real numbers in so far as those properties can be expressed in a certain formal language.³⁹ To give an account that is a bit less rough, and focusing on a simple form of the principle,⁴⁰ let us suppose that a formal quantificational language L is specified for referring to the sets of Zermelo–Fraenkel set theory—a language for which the semantics is recursively given in a standard way, such that, for any (closed) sentence s of the language, and any structure M of the appropriate sort, s must be either true in M or false in M . Then, if s is a sentence of L in which the only constants are individual constants (i.e. constants that denote only “individuals”—i.e. urelements or non-sets of the structures M_1 and M_2), and if M_1 is a structure appropriate for L with a universe whose real numbers are the standard ones, and M_2 is a structure appropriate for L with a universe whose “real” numbers are the nonstandard hyperreal numbers, then

$$s \text{ is true in } M_1 \text{ iff } s \text{ is true in } M_2$$

Davis points out that the Transfer Principle “is typically used by first proving a desired result in the nonstandard universe, and then, noting that the result is express-

³⁸ It should be noted that the revised version would also not fit Balaguer's notion of “fictionalism”, according to which the sentences of mathematics (and hence the theorems of mathematics) are like the sentences of fictional works in which references are made to non-existent objects and so are regarded as not true (Balaguer 1998, p. 12). Thus, Balaguer's fictionalist would classify all theorems containing reference to groups as not true.

³⁹ See (Davis, 2005, p. 2).

⁴⁰ For a precise statement of the theorem, see (Davis, 2005, pp. 22–28).

ible in the language [L in the above discussion of the Transfer Principle], concluding that it holds in the standard universe as well” (Davis 2005, p. 3). Now is this mathematical practice compatible with the fictionalist’s view? Well, for the fictionalist, the Transfer Principle is not true. So the fictionalist cannot appeal to the theorem to justify drawing the type of inference Davis described above, and it would seem that the fictionalist is in no position to accept the reasonableness of the above mathematical practice. Evidently, contrary to what Balaguer has claimed, *there are some mathematical practices that do run counter to the fictionalist’s position.*

Both Greg Ray (at the conference) and a reviewer have suggested a response to the above objection. Cannot the Transfer Principle be proved within ZFU? If so, couldn’t Field argue, by appealing to the conservatism of ZFU, that he would be justified in using the principle to draw nominalistic conclusions from nominalistic theories? Well, strictly speaking, the Transfer Principle cannot be proved *within* ZFU alone, since ZFU can only refer to sets and not to all the things that the principle talks about. Of course, one could prove in a suitable metatheory that certain number-theoretic relations, such as the *proof relation*, can be represented in ZFU. Then, using the device of Gödel numbering, one can, in a sense, “express” such statements as “Sentence S is provable in ZFU”. So there might be some hope that, by using such metamathematical devices, even the Transfer Principle could be expressed and proved in ZFU.

Let (A) be the statement:

The Transfer Principle is provable in ZFU.

Now Field holds that no part of mathematics is true. So even if (A) were proved in some metamathematical theory, Field would not be able to conclude that (A) is true. Besides, Field would not be in a position to know that all the recursively decidable number-theoretic relations and predicates that are needed to express the Transfer Principle in ZFU are indeed representable in ZFU. This is because he would not be in a position to assert the truth of the theorems of the metamathematical system that are needed to prove the Transfer Principle in ZFU in the way being envisaged. Specifically, various number-theoretic theorems, such as the Chinese Remainder theorem, that are used in proving the representation theorem cannot be claimed by Field to be true. Thus, it is hard to see how he could claim to prove (A).

Perhaps it will be maintained that Field does not have to know that (A) is true or even that the Transfer Principle is true. He needs only to construct a first-order derivation from the axioms of ZFU of the “Transfer Principle” (that is, the set theoretical statement that, supposedly, can be shown to express via coding the Transfer Principle), and then he could use the Conservatism of ZFU to justify using the Transfer Principle to derive nominalistic conclusions from nominalistic theories.

Well, this strategy won’t work for a variety of reasons. First of all, nobody is going to construct such a first-order derivation of the “Transfer Principle”: it is simply not practically possible. Besides, even if, by some miracle, some one did actually construct such a derivation, no one could realize that the sentence derived did express the Transfer Principle, without relying on many theorems of proof theory—in effect relying on the truth of those theorems. Furthermore, it is not obvious such a derivation is even possible. Even to construct a sentence of ZFU that expresses the Transfer Principle would require some rather powerful concepts, such as the relations *x is true in*

structure y and structure z satisfies the sentence w —relations that are not effectively decidable and hence not representable in Peano Arithmetic.

Another strategy would be to attempt to formulate a metamathematical theory, say M , that includes ZFU as a subtheory, and that has a vocabulary (with the appropriate rules of inference involving its terms) which is sufficiently strong to permit one both to express the Transfer Principle directly (without having to use proof theoretical devices) and also to derive the Principle from the axioms of M . In short, one could attempt to formalize, in first-order logic, the usual model theoretic proof of the Transfer Principle. Then, if one could prove that M is conservative over nominalistic theories, the applied mathematician could use this Transfer Principle to derive in M nominalistic conclusions from nominalistic theories formalizable in M , without having to maintain that the Principle is true.⁴¹

A serious problem with this suggestion is the hurdle of proving the conservatism of M . No one has proved such a Conservation Principle for any theory as complicated as M would have to be. And given the powerful model-theoretic semantical notions that would have to be formalized in M , it is not clear that such a theorem could be proved. Certainly, the kind of proof Field gave of the conservation of ZFU over nominalistic theories could not be simply carried over for this case. Field would have to deal with a much more complicated theory using M as the target theory than he did using ZFU.

But even if one could prove the conservatism of M , another problem would have to be faced. Supposing that the Transfer Principle were provable in M , the Conservation Principle, however, would not: it would be a sentence of a metatheory of M . Any applied mathematician or scientist willing to use the Transfer Principle in the way being suggested would have to have some assurance that the Principle could be reliably used in that way. But could the fictionalist offer such an assurance based on his proof of the Conservation Principle? How could he? First of all, the axioms of any metatheory used to prove conservation would undoubtedly be nominalistically unacceptable. Besides, according to Field, the very statement of the Conservation Principle is not supposed to be factual. For the fictionalist, the statement of the principle is like a sentence in a work of fiction: neither true nor false.

More generally, one can see that Fictionalism runs into problems whenever model theory is employed to justify using a metalogical theorem to draw inferences. Not surprisingly, Field has been forced by this problem to resort to some unusual logical maneuvers. Consider, for example, his use of model theory to prove the conservatism of ZFU. How was he able to square such a proof with his fictionalistic view of mathematical theorems? He claimed that the model theory was used only as an assumption in a kind of *reductio ad absurdum* argument against his platonic opponents. Thus, he wrote: “if I am successful in proving *platonistically* that abstract entities are not needed for ordinary inferences about the physical world or for science, then anyone who wants to *argue* for Platonism will be unable to rely on the Quinean argument that the existence of abstract entities is an indispensable assumption” (Field 1980, p. 6). It

⁴¹ This is basically the strategy suggested by a referee, who wrote: “I don’t see why Field cannot just make use of standard mathematics and metamathematics, all understood to be conservative over nominalistic theories.” The problem, of course, is the idea that Field can just take all that metamathematics to be conservative over nominalistic theories.

is clear that Field thus saw himself as not relying upon his model theory to arrive at the *knowledge of a mathematical truth*.⁴²

But did he not make use of such metalogical theorems as the completeness and soundness theorems of first-order logic in his theorizing about the nature of space? For example, he wrote:

[R]ecall that conservativeness as I defined it initially is a *semantic* notion, . . . but in referring to some of the arguments as proof-theoretic, and in the way I wrote the proof in note 15, I showed that it was the syntactic notion I was dealing with. The justification for the shift from semantic to syntactic notions is of course the Gödel completeness theorem for first-order logic. (Field 1980, p. 115, n. 30)

The above reasoning illustrates how Field frequently justified drawing conclusions by appealing to metalogical theorems—conclusions that could only be legitimately drawn if the truth of the theorem cited were justified. In other words, he frequently treated metalogical theorems, not as mere sentences in a work of fiction, but rather as expressing proved propositions. But to do so simply does not fit his avowed fictionalist stance. It was, perhaps, in an attempt to justify at least some such cases of appealing to metalogical theorems that Field developed a kind of *ersatz* semantic theory. Thus, he published an article attempting to show that a nominalist can legitimately use certain metalogical theorems of model theory, such as the completeness and soundness theorems of first-order logic (Field 1989). Field's basic strategy in developing his *ersatz* semantic theory is to utilize certain modal principles which, he argues, enable him to justify his use of his modal versions of the completeness and soundness theorems.

That Field's *ersatz* justification of his use of metalogical theorems is cogent and reasonable can, I believe, be doubted,⁴³ but in any case, it is easy to see that there is nothing in it to justify an acceptance of the kind of complex set theoretical models required to generate nonstandard analysis, let alone the high-powered models used in much of contemporary model theory.⁴⁴ Furthermore, the fact that Field has to resort to *ersatz* versions of soundness and completeness in the above way exposes the implausibility of Balaguer's claim that there are no mathematical practices that run counter to the fictionalist's view of mathematics. For we have seen that whenever standard model theory is used in classical mathematics to draw some conclusions about what is provable or what must hold in some structure, Field is forced to rely upon his own version of semantic theory to try to capture what was concluded by the classical

⁴² For more on this topic, see (Chihara, 1990, pp. 320–321).

⁴³ See my (Chihara 2004, Chap. 11), especially Sect. 3, for my doubts about his justification.

⁴⁴ This defect in Field's account of mathematics is strikingly similar to a defect in Graeme Forbes's anti-realist account of modal logic. An odd feature of Forbes's account is the absence of any sort of significant role for the mathematical structures of standard possible worlds semantics to play. See, for example, my (Chihara 1998), where I wrote:

[A]lmost all of Forbes's account is focused on the task of explaining how possible worlds sentences can be used to assess the validity and invalidity of modal arguments. But possible worlds semantics is used for much more than assessing validity and invalidity, just as the model theory of first-order logic is concerned with much more than just the evaluation of the validity of arguments. In the case of first-order logic, for example, the Homomorphism Theorem is applied to provide information about definability within structures, elementarily equivalent structures, and decidability. (p. 167)

mathematician, thus relying upon just the sort of “controversial, non-standard, non-face-value interpretations that seem to fly in the face of actual mathematical practice” that Balaguer attributes to the rivals of fictionalism.

Well, might not Field try to employ the reductio technique that he used in (Field 1980) to answer my present objections to Balaguer’s thesis? More specifically, might he not attempt to use the strategy of reductio ad absurdum to justify his acceptance of the mathematical practice described by Davis? How would that strategy work? Perhaps we can start by assuming, as part of the reductio strategy, that the platonic model theory M is true. Then, from this assumption, one can prove in M the truth of the Transfer Principle. One can then infer, based upon the assumption, the soundness of the practice described by Davis. But what follows from that? What is the absurdity? How do we arrive at what is wanted?

Perhaps we should attempt a more direct reductio argument for the validity of the mathematical practice described by Davis. In other words, assume that that mathematical practice is not valid, and try to deduce a logical absurdity of some sort. Where does that get us? Nowhere so far as I can see.

The lack of a genuine model theory in Field’s account of mathematics adequate for even nonstandard analysis points to a serious defect, since (as I indicated earlier) the heavy appeal to structures and models is one of the most common and important features of contemporary mathematics.⁴⁵ My own *structural account of mathematics* was fashioned to provide the nominalist with the material to understand such uses of structure in mathematics.⁴⁶

13 Conclusion

My aim in this paper was to show how developing one’s view of mathematics by focusing almost solely on applications of mathematic in science can lead to a very distorted understanding of the nature of mathematics. This is because, although applications of mathematics can teach us much about the nature of mathematics, there is also much to be learned about mathematics from considerations of pure mathematics, where matters of applications carry no weight at all.⁴⁷ There clearly needs to be, by

⁴⁵ An indication of the importance of structure for contemporary mathematics is to be found in the enormous body of work published under the name ‘Nicolas Bourbaki’—“the collective pseudonym of a changing and secret group of mathematicians, most of them French, who have collaborated since the 1930s with the intention of achieving a complete and definitive compilation of mathematical knowledge” (Borowski and Borwein 1991, p. 60). This group has emphasized the importance of structure in mathematics, publishing more than 36 volumes of mathematics, in which they classify the various areas of mathematics in terms of structure.

⁴⁶ See Chihara (2004), especially Chaps. 7 and 8.

⁴⁷ Hardy was an illustrious mathematician who worked almost exclusively in pure mathematic: primarily analysis and number theory (see Newman’s commentary on Hardy in (Newman 1956, p. 2024)). He assessed his own mathematical contributions to human knowledge with the words:

I have never done anything ‘useful.’ No discovery of mine has made, or is likely to make, directly or indirectly, for good or ill, the least difference to the amenity of the world. (Newman 1956, p. 2026)

philosophers of mathematics, a careful investigation of pure mathematics, if anything like an adequate account of mathematics is to be produced.

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Footnote 47 continued

Interestingly, in 1908, he wrote a letter to *Science* on a problem involving the transmission of dominant and recessive Mendelian characters in a mixed population. In the letter, he showed that the proportion of dominant and recessive genes in a randomly mating population remains constant unless there are outside influences. As this law of population genetics was independently discovered by Wilhelm Weinberg, it became known as the Hardy-Weinberg law. Ironically, this law has become one of the central principles of population genetics!

Logical Consequence for Nominalists

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ABSTRACT: It has repeatedly been argued that nominalistic programmes in the philosophy of mathematics fail, since they will at some point or other involve the notion of logical consequence which is unavailable to the nominalist. In this paper we will argue that this is not the case. Using an idea of Nelson Goodman and W.V. Quine's which they developed in Goodman and Quine (1947) and supplementing it with means that should be nominalistically acceptable, we present a way to explicate logical consequence in a nominalistically acceptable way.

Keywords: Philosophy of mathematics, nominalism, logical consequence, inferentialism, Nelson Goodman, W.V. Quine.

1. *The Argument from Logical Consequence*

We do not have any strong convictions concerning the question of the existence or non-existence of abstract objects. We do, however, believe that ontological fastidiousness is *prima facie* a good attitude to adopt. More precisely, ontological parsimony provides a *pro tanto* reason for theory choice. Nelson Goodman added to this fairly obvious principle the methodological observation that a more parsimonious theory can always easily be turned into a theory that is ontologically more lavish; and so a nominalistic theory can be turned into a platonistic theory without any problem. There is, however, no guarantee that the other way around is generally available Goodman (1977, p. L). So, hedging your bets, it is advisable to resist an extravagant ontology for as long as possible. The ontic extravaganza that Goodman zoomed in on was that of the calculus of classes, or set theory as it is commonly known today. Nominalism for Goodman meant the renunciation of classes, platonism their acceptance.

Goodman was certainly one of the most vocal defenders of nominalism in the last century. Nowadays, the attempts he had made together with W.V. Quine to achieve a nominalistic foundation of mathematics are often regarded as a complete failure. In a very influential synopsis of various nominalistic strategies, John P. Burgess and Gideon Rosen write:

Goodman and Quine made it their priority to reconstrue the kind of science in which mathematics is applied, and especially the kind of mathematics applied in science. [...] After some modest initial progress, the project of Goodman and Quine reached an impasse. (Burgess and Rosen 1997, p. 5)
[...] Quine, after the failure of his joint project with Goodman, soon came to reconsider, and eventually came to recant, his nominalism. (Burgess and Rosen 1997, p. 32)

It seems to us that this underestimates Goodman and Quine's achievements.¹ As we hope to show in this paper, the strategy that Goodman developed together with Quine

¹ Moreover, Burgess and Rosen's claim in the second part of the quotation does not strike us as historically correct regarding Quine's surrender to platonism.



in their paper “Steps Toward a Constructive Nominalism” Goodman and Quine (1947) can aid closing one of the most problematic open gaps in contemporary nominalism.

This open gap concerns the notion of logical consequence as employed by nominalists. It has been argued, on various grounds, that a nominalistically acceptable and at the same time adequate explication of logical consequence is unobtainable. This objection is usually raised against specific nominalistic programmes that aim to prove the dispensability of any ontological commitment to abstract objects for science.² Many nominalists have to rely in their projects on logical consequence at some point. In the next section we will briefly sketch two cases.

Prima facie, logical consequence should be unproblematic for the nominalist. Logic, of all things, should not carry any ontological commitments, let alone ontological commitments to any dubious entities such as abstract objects. In fact, many philosophers of logic believe that this is a mark of logicity. However, anti-nominalists have produced the following *Argument from Logical Consequence*:

- (P1) Without using the notion of *logical consequence* the nominalist cannot explain mathematical practice and its contribution to science.
- (P2) *Logical consequence* can only be satisfactorily explicated with appeal to abstract objects.
- (P3) But *logical consequence* is a notion that stands in need of explication.
- (C) The nominalist cannot explain mathematical practice and its contribution to science.³

As we said, the reasoning behind premise (P1) will be explained in more detail in the next section. (P2) and (P3) could be argued for as follows: One might begin by arguing that the most influential explication of logical consequence is the model-theoretic approach which goes back to the work of Alfred Tarski. This approach, however, makes use of set theory and is thus committed to the existence of abstract objects. Alternative, proof-theoretic explications also fail to be nominalistically acceptable, in that they employ sets or sequences of sentences, which are abstract objects too see, e.g., Hale and Wright (1992, p. 112). Indeed, sentences themselves are usually taken to be abstract objects: types, say. This much should support (P2).

(P3) blocks the move just to assume the semantic consequence relation to be primitive and therefore not in need of any explication. This move — which is in fact chosen by some contemporary nominalists — is in conflict with the epistemological motivation that many nominalists had in the first place. At least those nominalists who eschew abstract objects because of their “epistemology” should not accept a mysterious and inexplicable consequence relation either.

In this paper we aim to rebut this version of the Argument from Logical Consequence. In section 3., we argue that one of the main assumptions which this argument

² See, for instance, Hale and Wright (1992, 1994), Parsons (1990), Resnik (1983, 1985), Shapiro (1993, 1997).

³ A similar argument, called “the argument from the philosophy of logic” is made in Wilholt (2006), the argument here is modeled after Wilholt’s.

rests on is mistaken. We argue for an explication of logical consequence along inferentialist lines. We show how a consequence relation, so understood, can be explicated in a nominalistically acceptable way, using the techniques developed by Goodman and Quine in Goodman and Quine (1947). In the final section we discuss the prospects for using this explication of logical consequence in the two nominalistic programmes described in the following section.

2. *Two Nominalist Programmes*

Nominalists, as we understand them, renounce classes, and *a fortiori*, objects which are often considered to be constructed from them, such as numbers. Consequently, there are three positions that nominalists can adopt with respect to statements of mathematics: they can be regarded as true but not about numbers, sets or other mathematical objects; or as “strings of marks without meaning” Goodman and Quine (1947, p. 111); or as literally false Field (1980, p. 2 and *passim*). Nominalistic projects are usually concerned with the reconstruction of the success of mathematical *practice*. As Goodman and Quine expressed it, the challenge the nominalist has to face is to “account for the fact that mathematics can proceed with such remarkable agreement as to methods and results” Goodman and Quine (1947, p. 111). Moreover, mathematics apparently makes an important contribution to science, and the nominalist is challenged to account for this fact too. In what follows, we will sketch two ways in which contemporary nominalists have tried to give an account of mathematics, *eliminative structuralism* and *fictionalism*. We will see that both accounts make use of the notion of logical consequence at crucial places.

2.1. *Logical Consequence and Eliminative Structuralism*

Structuralism in the philosophy of mathematics holds that (pure) mathematics is the deductive study of structures as such. A nominalistic version of this view holds that mathematics does not, in fact, make statements about any abstract objects that constitute such structures (e.g., the natural numbers), but about whatever occupies the positions in a natural number system, a system of objects that happens to instantiate the relations that the realist assumes to be obtaining between the natural numbers. Accordingly, a mathematical statement like

$$(M) \quad 3 + 9 = 12$$

would be interpreted as

(M*) In any natural number system S , the object in the 3-place-of- S S -added to the object in the 9-place-of- S results in the object in the 12-place-of- S . Shapiro (1997, p. 85)

This way, the ontologically problematic statements of mathematics also come out nominalistically acceptable: ‘numbers exist’ comes to ‘every natural number system has objects in its places’:

The programme of rephrasing mathematical statements as generalizations is a manifestation of structuralism, but it is one that does not countenance structures, or mathematical objects for that

matter, as bona fide objects. Talk of numbers is convenient shorthand for talk about all systems that exemplify the structure. Talk of structures generally is convenient shorthand for talk about systems. (Shapiro 1997, p. 85)

Such an “eliminative structuralism” faces the *problem of vacuity*: in order to make sense of large parts of mathematics, one seems forced to accept a rich background ontology, and the nominalist just lacks the resources to provide one.⁴ To see how the problem arises, consider φ to be a sentence in the language of arithmetic.⁴ Eliminative structuralism understands φ as something of the form

(φ^*) For any system S , if S exemplifies the natural number structure, then $\varphi[S]$.

$\varphi[S]$, here, is obtained from φ by interpreting the arithmetic terminology and the variables in terms of the objects and relations of S . Let us assume that there are only finitely many concrete objects. In this case, φ^* comes out vacuously true, no matter what φ is, since nothing exemplifies the natural number structure. Accordingly, an eliminative structuralist account of arithmetic will need to assume infinitely many objects in its background ontology, an eliminative structuralist account of Euclidean geometry a background ontology of the cardinality of the continuum, etc.

There seem to be two ways for the nominalist to meet this challenge. One way would be to assume that there are actual, concrete structures that can play the role of the background ontology. At least for all cases of applied mathematics the nominalist will need to hold that there is such an actual system that instantiates the mathematical structures applied. The problem here seems to be that mathematical truth and falsity will depend on contingent matters about the actual world, namely on whether there are actual systems of objects that instantiate the mathematical structures. But does the falsity of ‘All even numbers are prime’ depend on whether or not there exist infinite totalities of concrete entities that constitute ω -sequences?⁵

It is not entirely clear how strong this counter-argument is. After all, the nominalist typically feels forced to give an account for mathematics in the first place, because of the apparent usefulness of mathematics for science. But *that* mathematics is useful for science could be considered a contingent matter, accordingly, the nominalist need only explain why mathematics works, when it does. In a world with only a few objects, it might be possible to use mathematical structures that only require a comparatively small background ontology; in a finite universe, for instance, a mathematics of finite structures might be all that is required for science.

A similar reply, that would bring us closer to the second way for the nominalist to answer the problem of vacuity, could point out that also the platonist alternative will assume that there are *actually* enough abstract objects to play the role of the background ontology. Doesn’t that make mathematical truth and falsity equally counterfactually dependent on the existence of abstract objects? The difference is that platonists are prepared to assume that their abstract objects exist *necessarily*, while the actual systems of concrete entities of the nominalist exist only contingently.

⁴ The example is taken from Shapiro (1997, p. 85).

⁵ For a related worry see Wilholt (2006).

But instead of assuming sufficiently rich actual systems that might or might not exist, depending on the way the world is, in order to instantiate the mathematical structures, one could rephrase eliminative structuralism as a claim about possible systems, the possibility of which depends on logic alone. Accordingly, our sentence of arithmetic φ would be interpreted as saying

(φ^{**}) For any *logically possible* system S , if S exemplifies the natural number structure, then $\varphi[S]$.

or as

(φ^{***}) Necessarily, for any system S , if S exemplifies the natural number structure, then $\varphi[S]$.

Geoffrey Hellman (1989) carries out such a programme of *modal eliminative structuralism*. Instead of assuming that there are actual systems that instantiate mathematical structures, the modal eliminative structuralist would only assume that there are possible systems that exemplify these structures — what could be ontologically less problematic? Merely assuming that some systems are possible should not commit one to anything, or so one might hope.

However, there is a problem: in order for his account to work, the modal eliminative structuralist either needs an account of logical possibility or must assume that the notion of logical possibility is primitive and not in need of further explication. The platonist can challenge the latter assumption and claim that our grasp of the modal terminology when applied to mathematics is too sophisticated to be considered primitive. It is rather mysterious how we manage to use these notions the way we do if it was not for the fact that they are themselves mathematically mediated:

When beginning students are first told about logical possibility, logical consequence, etc., most of them seem to have *some* idea of what is meant, but consider how much their initial “intuitions” differ from our “refined” ones. The anti-realist owes us some account of how we plausibly could come to understand the notions in question (as applied here) *as we in fact do*, independent of our mathematics. Without this it is empty to use a word like “primitive” [...]. (Shapiro 1993, p. 475)

These worries concerning the involvement of modality might be silenced by pointing out that logical possibility is unproblematic since it can be defined with recourse to logical consequence or logical truth: φ is logically possible if and only if $\neg\varphi$ is not a logical truth. (We assume that logical truth is defined as a degenerate case of logical consequence — see section 3. for our concrete proposal.)

In this case, however, an account of logical consequence is needed that does not rely on abstract objects, in the way that the model-theoretic explication does.

2.2. *Logical Consequence and Fictionalism*

In order to see that this problem not only arises for the modal eliminative structuralist, we will briefly sketch the problem as it arises for Hartry Field’s fictionalism.

Field tries to defend nominalism against the objection that mathematical objects must exist to the best of our knowledge, by trying to undercut the platonist’s *Indispensability*

Argument. In particular, he tries to undercut the platonist's claim that mathematics is indispensable for science, instead he attempts to show how science can be done "without numbers" Field (1980, 1989).

The idea is the following: Field formulates part of physics — namely, Newtonian mechanics — in a way that does not involve reference to any abstract objects. He formulates this nominalistic theory N using a second-order mereology (at least in one version of the programme). He then proves metatheoretically that a platonistic extension of N , $N + S$, is *conservative* with respect to N : there are no nominalistically storable consequences of $N + S$ that are not consequences of N alone. In other words: in deriving nominalistically storable conclusions, mathematics basically does the same as logic. Logic might do it in a more long-winded fashion than mathematics, so the latter has a practical use in making derivations shorter and more elegant, but any consequence is also available without mathematics.

There are two places in which logical consequence seems to matter. First it seems to matter when spelling out the second-order logic the nominalist wants to use to accompany nominalized physics. Some have articulated the worry that the logic used here might already undermine the nominalistic enterprise (e.g. Resnik (1985, p. 163)). However, the more urgent problem seems to come with the fact that the conservativeness claim itself is formulated in terms of logical consequence:

[T]he fictionalist thesis of conservativeness is stated in terms of logical consequence, and the two best historical explications of this are unavailable to the fictionalist. (Shapiro 1993, p. 461)

Again, Field assumes like Hellman that the notion of logical possibility can be taken as a primitive Field (1991). Of course he must then face the same epistemological challenges as the modal eliminative structuralist.

3. *Logical Consequence without Models*

3.1. *Inferentialism*

The most generally accepted explication of logical consequence is, no doubt, the model-theoretic construal which goes back to the work of Alfred Tarski. Logical consequence is, in the modern formulation, defined using set theory: a sentence φ is a logical consequence of a set of sentences Γ if and only if φ is satisfied by all models that satisfy all members of Γ . The models mentioned here are sets, and satisfaction is defined in set-theoretic terms.

That this notion is not available to the nominalist has been remarked above, and this point was raised by many authors, in particular in connection to Field's programme.⁶ There is another approach to logical consequence, however, that at least *prima facie* does not involve nominalistically unpalatable notions, which goes back to the work of Gerhard Gentzen (1935) and puts the logical inference rules in the centre of attention. In recent times such approaches, which variably have been subsumed under the labels 'inferentialism' or 'proof-theoretic semantics', have received considerable attention; prominent

⁶ In addition to Field himself, e.g. Field (1989, pp. 30–31), see for instance Resnik (1983, 1985, §2), Hale and Wright (1992, 1994), Shapiro (1983, 1993, §2), MacBride (1999, §4).

proponents include Michael Dummett, Ian Hacking and Dag Prawitz.⁷ Inferentialism insists that the meaning of the logical constants is determined by their introduction- and elimination-rules, and that these rules (so far as they are the correct ones) are self-justifying. No further appeal to model-theoretic semantics, truth-tables or the like is needed in order to argue for the validity of the rules.

As is well known, conditional, negation, and universal quantification suffice to characterise classical logic,⁸ so we will restrict our attention to these connectives. According to the moderate inferentialism recommended here, logical consequence is characterised by the following rules which can be added to any formal language that contains ‘ \supset ’, ‘ \neg ’, and ‘ \forall ’, and has appropriate syntactical categories for these rules to operate on:⁹

$$\begin{array}{ccc}
 \frac{\begin{array}{c} [\varphi] \\ \vdots \\ \psi \end{array}}{\varphi \supset \psi} \supset I & \frac{\begin{array}{c} [\varphi] \quad [\varphi] \\ \vdots \quad \vdots \\ \psi \quad \neg\psi \end{array}}{\neg\varphi} \neg I & \frac{\Phi(t)}{\forall x\Phi(x)} \forall I \\
 \\
 \frac{\varphi \quad \varphi \supset \psi}{\psi} \supset E & \frac{\neg\neg\varphi}{\varphi} \neg E & \frac{\forall x\Phi(x)}{\Phi(t)} \forall E .
 \end{array}$$

The $\forall I$ -rule has the standard proviso that ‘ t ’ does not occur free in Φ or in any of the relevant assumptions. The square parentheses in $\supset I$ and $\neg I$ indicate that φ is an assumption that is discharged by the application of these rules (thus, strictly speaking, we have also the rule of assumption).¹⁰

Logical consequence is then explicated using these rules:

A sentence φ is a *logical consequence* of some premises Γ if and only if there is a derivation of φ from Γ whose single lines are either sentences of Γ , result by applications of the above rules from previous lines, or are assumptions that are discharged by applications of $\neg I$ or $\supset I$.

φ is a *logical truth* if and only if it is thus derived using no undischarged premises.

The trouble with this definition is that it mentions notions that do not appear to be readily available to the nominalist: sentence, line and derivation, which are usually taken to be abstract types. If we want to spell out everything in detail, we will also have to mention variables, terms, logical symbols, and more. Also, ‘ Γ ’ looks suspiciously like the

⁷ See, e.g., Dummett (1973), Hacking (1979), Prawitz (1965, 1971, 1991), but also Milne (1994), Read (2000), Wright (2007).

⁸ There is, of course, an issue concerning whether the logic should indeed be classical. This is, however, assumed by Goodman and most contemporary nominalists and will not be discussed here. Needless to say, inferentialism is not committed to classical rules of inference.

⁹ For simplicities sake, we will assume that no other logical axioms or rules are present for the language in question prior to the introduction of these rules.

¹⁰ For more details see, for example, Prawitz (1965).

name of a set. The definition has, hence, as of yet some significant gaps that are still to be filled in — in a nominalistically acceptable way. This problem will be addressed in section 4.

3.2. *Second-Order Logic*

With this conception of logical consequence at hand (*modulo* the gaps that will be filled in §4.), logical means more powerful than those provided by first-order logic become available. We can supplement our rules by a pair of rules for second-order quantifiers. Second-order logic allows generalisation into predicate-position in much the same way that first-order logic allows generalisation into name-position: with quantifiers binding variables that take the place of these expressions.¹¹ As we will see in the final section, the adoption of second-order logic is crucial to several nominalistic projects. We will see in §4., however, that the second-order quantifier also comes in handy in our explication of logical consequence, albeit without being strictly required.

The rule for the second-order quantifiers we add are:

$$\frac{\Phi(T)}{\forall X^n \Phi(X^n)} \forall^2 \text{I} \qquad \frac{\forall X^n \Phi(X^n)}{\Phi(\Xi)} \forall^2 \text{E} .$$

In $\forall^2 \text{I}$, T is a n -place predicate letter or free variable that must not occur free in Φ or any of the relevant assumptions. In $\forall^2 \text{E}$, Ξ is an open sentence with n argument places;¹² no variables in Ξ are to be bound in $\Phi(\Xi)$ that are not already bound in Ξ .

For sure, second-order logic has attracted a profusion of criticism. Most prominent amongst the complaints are incompleteness allegations, and Quine's famous claim that second-order logic is nothing but "set theory in sheep's clothing" Quine (1970, pp. 66–68). The former complaint is usually framed thus: the non-axiomatisable consequence relation of second-order logic on the standard model-theoretic conception is intractable and does, hence, not qualify as *logical* consequence. We will content ourselves here with the observation that the second-order consequence relation we are after is proof-theoretic, and that therefore this intractability objection does not arise. This paper is not the place for a more detailed discussion.¹³

¹¹ For more details compare the current "bible" of second-order logic: Shapiro (1991). Shapiro favours the model-theoretic conception of logic. For a proof-theoretic characterisation see, e.g., Prawitz (1965).

¹² We allow instantiation with open sentences in this rule, rather than just predicate letters, in order to gain the proof-theoretic strength of what, in axiomatic systems of second-order logic, is known as the *comprehension schema*: $\exists X^n \forall \langle x \rangle_n (X^n \langle x \rangle_n \equiv \Phi \langle x \rangle_n)$ (where ' $\langle x \rangle_n$ ' abbreviates a string of variables, ' $x_1 x_2 \dots x_n$ '). We bracket a discussion of comprehension here, and also dodge the issue of quantification over functions which is usually included in a formulation of second-order logic. For details see Shapiro (1991, §3.2). Although it is irrelevant here, the reader might be interested in noting that n -place functions can be simulated by using $(n + 1)$ -place predicates: the clause $\lceil \forall \langle x \rangle_n \exists! y F^{n+1} \langle x \rangle_n y \rceil$ indicates that $\lceil F^{n+1} \rceil$ is in effect an n -place function (where ' $\exists!$ ' stands for the first-order definable 'there is exactly one').

¹³ Such a discussion can, however, be found in Rossberg (2004). The for the inferentialist more pressing problem of a proof-theoretic notion of incompleteness is discussed in Rossberg (2006) and Wright (2007).

Quine's complaint about the set-theoretic commitment that second-order quantification allegedly brings about has been contested in various places, on various grounds. To name but a few: George Boolos famously provided a plural interpretation of the monadic second-order quantifiers that dispenses with any commitment to sets and which at least David Lewis found nominalistically acceptable.¹⁴ Crispin Wright argued that second-order quantification cannot bring about new ontological commitment: if predicates, as Quine contends for instance in Quine (1948), do not themselves carry any ontological commitment to sets (or properties), then this commitment cannot suddenly arise when one generalises into predicate position; much like first-order quantification does not suddenly bring about commitment to new objects when applied in a language that contains non-referential terms.¹⁵ Even assuming that Quine is correct about the ontological commitment of a theory being exhibited by the first-order quantifiers, there does not appear to be any way of arguing from there that the second-order quantifiers bring about a commitment to sets.

As mentioned above, the involvement of sets is obvious if a model-theoretic approach to logical consequence is chosen. This, however, is nothing peculiar to second-order logic on this conception, but is the case for ordinary first-order logic as well. But since we here attempt to manage without model theory altogether, this problem does not arise either. We thus leave the discussion at this stage in order to return to the question of the significant gaps that still remain in our explication of logical consequence.

4. *Proofs and Tokens*

4.1. *Concatenation Theory*

The problem of providing a nominalistically acceptable theory of syntax and proof-theory for a formal language was tackled by Goodman and Quine in their joint paper "Steps Toward a Constructive Nominalism" using the Calculus of Individuals, developed by Henry S. Leonard together with Goodman, and a theory of token-concatenation.¹⁶ Their effort has been found wanting due to a couple of limitations, which we aim to overcome here.

¹⁴ Boolos (1984, 1985), Lewis (1991). The plural interpretation has subsequently been further developed, see e.g. Rayo (2002), Rayo and Uzquiano (1999). Agustín Rayo and Stephen Yablo Rayo and Yablo (2001) attempt to provide an interpretation of polyadic second-order quantification, roughly along Boolos's lines and inspired by Arthur Prior's work Prior (1971, chapter 2). Peter Simons also draws conclusions with respect to the debate about higher-order logic from Prior; see Simons (1993, 1997). The plural interpretation itself has been found wanting in various respects — see, e.g., Linnebo (2003), Resnik (1988) or Shapiro (1991, §9.1.1) — and is not further pursued for the purpose of the present paper.

¹⁵ Wright (1983, pp. 132–133), more recently again and in more detail in Wright (2007). Rayo and Yablo (2001) suggest a similar principle.

¹⁶ Goodman and Quine (1947); see also Martin (1958) for a detailed study of token-concatenation theories in this context. The Calculus of Individuals was published in Leonard and Goodman (1940); for a study of its development see Rossberg (2009). An investigation into second-order versions of calculi of individuals can be found in Niebergall (2009).

To build a syntax for a formal language, we have to be able to say what a well-formed formula — or (open) sentence — of this language is. To do this in a nominalistically acceptable way, not only mention of sets has to be avoided, the sentences themselves also have to turn out not to be abstract entities. Goodman and Quine suggest to make sense of what a sentence is by identifying it with its concrete inscriptions. Marks on paper, in instance, can be said to fall under a predicate ‘Fmla’ (for well-formed formula). In order to give a definition of this predicate, they start out with primitive predicates that are true of concrete inscriptions if these have the familiar shape of the logical symbols, variables, etc., and then build up the language in a way analogous to the common recursive definition of a language that the platonist uses. To do so, they use a primitive three-place predicate ‘C’ which applies to token inscriptions. ‘C(x, y, z)’ expresses that x is a token inscription that is the concatenation of y and z . For convenience, we can define a four-place predicate ‘C(x, y, z, w)’ as ‘ $\exists t(C(x, y, t) \wedge C(t, z, w))$ ’, and analogously for five- and six-place predicates for concatenation (the number of terms following the ‘C’ will disambiguate which predicate it is).

But first things first. The primitive unary predicates we will be using are ‘Vee’, ‘UVee’, ‘Ac’, ‘LPar’, ‘RPar’, ‘Neg’, ‘Cond’, ‘UpsA’, which are true of physical objects if they have the shape of a lower case ‘ v ’, and upper case ‘ V ’ (for use as first- and second-order variables, respectively), an accent ‘ \prime ’, a left parenthesis ‘(’, a right parenthesis, ‘)’, a negation sign, ‘ \neg ’, a conditional sign, ‘ \supset ’, and an upside-down A, ‘ \forall ’, respectively.¹⁷

Let a character, ‘Char’, be any of the things that the predicates above are true of Goodman and Quine (1947, p. 113):

$$\text{Char}(x) =_{df} \text{Vee}(x) \vee \text{UVee}(x) \vee \text{Ac}(x) \vee \text{LPar}(x) \vee \text{RPar}(x) \vee \\ \text{Neg}(x) \vee \text{Cond}(x) \vee \text{UpsA}(x).$$

And let an inscription, ‘Insc’, be either a character, or a concatenation (note that concatenation was introduced as applying only to inscriptions — fully explicit, a recursive definition would be in order):

$$\text{Insc}(x) =_{df} \text{Char}(x) \vee \exists y \exists z C(x, y, z).$$

The first thing we obviously need for the syntax is a sufficient supply of distinct variables. These can be formed out of lower- and upper-case vees, concatenated with strings of accents. For this, we can define a string of accents as

$$\text{AcString}(x) =_{df} \text{Insc}(x) \wedge \forall y ((\text{Part}(x, y) \wedge \text{Char}(y)) \supset \text{Ac}(y)).$$

¹⁷ Goodman and Quine are more economic in their choice: they replace ‘ \neg ’ and ‘ \supset ’ by the Sheffer stroke, ‘|’, for alternative denial, in terms of which the former two are definable; they also form the universal quantifier using parenthesis, ‘(v)’, in lieu of ‘ $\forall v$ ’, and do not have upper case variables. Since they aim to provide a syntax for first-order set theory, they have the additional ‘ ϵ ’ for membership. There might be concerns about left and right parentheses having the same shape, albeit rotated by 180°. To dissolve such worries, one could either appeal to the orientation of the inscription, or use a different shape for one of the parenthesis, say, ‘]’ instead of ‘)’. See Goodman and Quine (1947, p. 112).

(Again, a recursive definition would have been possible that takes one accent as the base case, and defines in the recursion step an accent-string to be any concatenation of accent strings.) Note that ‘Part’ is the two-place predicate that is introduced and axiomatised in the class-free subsystem of the Calculus of Individuals Leonard and Goodman (1940).

First- and second-order variable, ‘FVbl’ and ‘SVbl’, respectively, can thus be defined:

$$\text{FVbl}(x) =_{df} \text{Vee}(x) \vee \exists y \exists z (\text{Vee}(y) \wedge \text{AcString}(z) \wedge C(x, y, z))$$

$$\text{SVbl}(x) =_{df} \text{UVee}(x) \vee \exists y \exists z (\text{UVee}(y) \wedge \text{AcString}(z) \wedge C(x, y, z)).$$

That is, any lower-case vee, possibly followed by a string of accents, is a first-order variable, and any upper-case vee, possibly followed by a string of accents, is a second-order variable.

Goodman and Quine go on to develop the syntax like this in a painstaking detail which we will not go into here. They define quantifiers, (in our case an upside-down A followed by a variable, orders distinguished by the order of the variable), atomic formulae, and formulae. They then inscriptionally set up some logical axioms, which we, of course, dispense with here. It follows the definition of a substitution, immediate consequence (a formula that can be arrived at by one application of a rule), that of a line (of a proof), and lastly a proof itself (as a list of lines all of which are immediate consequences of previous lines or axioms). Since we allow for assumptions, our construction first defines a derivation as a list of lines all of which are immediate consequences of previous lines or assumptions; a proof (of φ) will then be a derivation whose last line (φ) does not depend on any undischarged assumption. A theorem, finally, is the last line of a proof.

These sketchy remarks on the construction must here suffice as a hint on the actual construction. Goodman and Quine give their construction in full detail,¹⁸ and this is easily amended to suit our proposal here if our hints above are followed. Note that we have not included any constants in the language, neither names nor predicate constants. Thus, all our formulae so far contain only variables and logical constants (‘ \forall ’, ‘ \neg ’, ‘ \supset ’). Identity is standardly defined in second-order logic (by Leibniz’ Law), and other constants can easily be introduced into the construction of the language.¹⁹

4.2. *The Proof is Out There*

If the notion of proof thus defined only encompassed discernible marks on paper, the consequence relation defined with its help would be very restrictive. Goodman and Quine, indeed, suggest that instead we take inscriptions to be any appropriately formed portion of matter, whether it is against a contrasting background or not.

¹⁸ See Goodman and Quine (1947). Note that Goodman and Quine’s definition D10 is defective, but easily mended, as noted by Henkin in Henkin (1962, p. 192, fn. 3). See also Martin (1958).

¹⁹ Goodman and Quine mention in various passages of Goodman and Quine (1947) the problem they have in defining the ancestral. Leon Henkin (1962) provides a solution; Goodman later states Goodman (1972, p. 153) that the technique he developed himself in Goodman (1977, chapters IX and X) will serve the purpose. Since we assume that the version of second-order logic presented here is nominalistically acceptable, we can simply rely on Frege’s original definition of the ancestral in Frege (1879, §26).

Then the only syntactical description that will fail to have inscriptions answering to them will be those that describe inscriptions too long to fit into the whole spatio-temporally extended universe. This limitation is hardly likely to prove embarrassing. (If we ever should be handicapped by gaps in the proof of an inscription wanted as a theorem, however, we can strengthen our rules of inference to bridge such gaps; for, the number of steps required in a proof depends on the rules, and the rules we have adopted can be altered or supplemented considerably without violation of nominalistic standards.) (Goodman and Quine 1947, p. 121)

We suggest the amendment to allow to count as inscriptions any appropriately formed space-time region, whether it is occupied by matter or not. Let us also note that even employing more and more abbreviations, other useful definitions and rules other our primitive ones, in a finite universe, we will *eventually* run out of actual concrete inscriptions, no matter how generously construed. We thus suggest to side with Field Field (1980) and take it for granted that our universe is infinite, and, in fact, contains a continuum of space-time points (i.e., 2^{\aleph_0} -many); and we do not consider this as a violation of nominalism²⁰ — until further notice. We discuss the infinity of the universe in this and other respects in the next section.

This generous conception of an inscription might seem objectionable to some, at first glance, since it means that all proofs are already out there — and quite literally so. We literally discover proofs, that is, the space-time regions that are proofs, e.g. by outlining the proof-shaped regions with a pencil (note, however, that this is not the only way to learn that a proof exists). The initial feeling of offence will in most cases subside when it is pointed out that the situation is exactly analogous for the platonist who takes proofs to be abstract objects: types, for instance. These are also commonly assumed to exist independently of anyone finding them (e.g. by tokening them). The only difference is that the nominalist's proofs are concrete objects. In principle, it should thus also be possible to use nominalistic analogues of any way of demonstrating the existence of a proof that the platonist uses. Next to transcribing it, there is, for example, proving that the inscription must exist. (This proof will be an inscription again, but it need not be the proof whose existence is thus demonstrated.)

Rejecting the generous conception of an inscription, and thus not only denying an actual infinity of proofs, but also the existence of proofs that have not been written down, would mean to adopt a position even more radical than Goodman's nominalism. It seems that Stanisław Leśniewski embraced this very restrictive conception of proof see Simons (2002), which not only entails that proofs come into existence when they are first written down, but also that they cease to exist when the last inscription (narrowly construed) is destroyed. Irrespective of how appealing this position is, it does not appear that a criticism along these lines could coherently be put forward by the platonist.

In our infinite universe there are thus infinitely many concrete proof-inscriptions (understood in the generous way specified above). Moreover, there are enough in the sense that there are *all* the proofs that a platonistic version of inferentialism accounts for. Our explication of logical consequence is thus co-extensive with the platonistic inferentialist account of logical consequence.

²⁰ This, to be sure, has been contested; see, e.g., MacBride (1999).

5. *Is this Enough? (Or is it too much?)*

With the help of concatenation theory, an inferentialist conception of logical consequence, and speculations about the size of the actual universe, we arrived at an explication of logical consequence that might seem nominalistically acceptable. Whether it indeed *is* nominalistically acceptable is a question that does not allow for a straightforward answer. First of all, nominalists might disagree with one another about what resources in fact count as nominalistically acceptable.

Further, they might disagree on what an explication is supposed to do, and, in particular, disagree about the relation in which explicatum and explicandum must stand to each other, in order for an explication to be adequate.

Finally, they might disagree about the use they want to make of the explicatum in their theories. We mentioned two nominalist programmes above and said what role the notion of logical consequence plays in their projects. Below, we will address the question to what extent the explication of logical consequence suggested here can be used in these projects. Space constraints will not allow us to pursue all these questions in sufficient detail.

5.1. *A Ballet Dancing Brick Layer*

Let us first turn to the assumptions we made about the size of the universe. To be on the safe side, we assumed that our universe is large enough to contain the inscriptions of all proofs the platonist assumes to exist. But does the assumed size of the universe not sin against the nominalist's standards and put everything in jeopardy that we have achieved so far? Some anti-nominalists have questioned our assumption that space-time points could be considered nominalistically acceptable, since they believe that the presumed mark of the concrete (having causal powers) is merely metaphorically instantiated by space-time points e.g. Resnik (1985), MacBride (1999). We will not go into this discussion here, and simply assume that *if* a nominalist (like Hartry Field) finds space-times points ultimately acceptable, then so be it. But we want to claim that our explication of logical consequence would also be acceptable for Goodman. Would he have accepted an assumption of uncountably many points of physical space-time?

Remembering Goodman's early paper with Quine, one might think that he would not have accepted such an assumption. The project by Goodman and Quine was not only nominalist, it was also finitist:

We decline to assume that there are infinitely many objects. Not only is our own experience finite, but there is no general agreement among physicists that there are more than finitely many objects in all space-time. If in fact the concrete world is finite, acceptance of any theory that presupposes infinity would require us to assume that in addition to the concrete objects, finite in number, there are also abstract entities. (Goodman and Quine 1947, p. 106)

Goodman and Quine's project descended from their joint efforts with Alfred Tarski and Rudolf Carnap in the early 1940s to develop the foundations of arithmetic in a way that respects finitism (there is only a finite number of individuals), physicalism/reism (there are only physical things), and nominalism (there are only variables for individuals,

not for universals).²¹ The motivations for this project were already at that time rather heterogeneous. Tarski claimed not to understand languages that do not satisfy these conditions, thus citing an epistemic reason for these constraints. However, he apparently also thought that finitism is just a consequence of there being only finitely many objects in the world.

Carnap, on the other hand, shared Tarski's insistence on finitism only to a certain degree. Insofar as he was motivated to require finitism for the foundations of arithmetic, his motivation was empiricist: since each confirmation is based on finite observations, our knowledge is limited to the finite (independent of how many objects the world contains). But Carnap also claimed to understand infinite conceptions of arithmetic, making sense of them in terms of what we characterized above as modal eliminative structuralism:

It seems to me that I actually understand, in a certain degree, infinite arithmetic [...]. To the question of Tarski and Quine, how I interpret this, when the number of things is perhaps finite: I do not know exactly but perhaps through mere positions instead of things [...]. A position is an ordering possibility for a thing. I do not have the intuitive rejection of the concept of possibility as Tarski and Quine do. (Carnap's notes on the discussions with Tarski and Quine, RC-090-16-25, Carnap Archives Pittsburgh, as cited in (Mancosu 2005, p. 344))

As Goodman and Quine report in the beginning of their 1947 paper, the discussion in the early 40s did not lead to a final solution. Goodman and Quine had a new idea though how to address the problem. Instead of trying to formulate arithmetic with finitely many objects, platonist mathematics was simply treated as a meaningless language that did not require any interpretation in terms of acceptable objects. Instead, they went meta-mathematical: they devised a nominalistically acceptable way to speak about the way that platonist mathematics, considered as a mere "apparatus", can work. Since this way of doing meta-mathematics did not involve notions of arithmetic, the problem of interpreting the numbers as concrete objects, which had bothered Carnap and Tarski, did not reoccur for their proposal.

Finitism is however not fully unproblematic for an inscriptionist account. Above we assumed — to be on the safe side — that the universe should comprise an uncountable number of space-time points in order to allow for inscriptions of proofs of arbitrary length. Goodman and Quine believed that such an assumption is not needed, as quoted above, since stronger rules and auxiliary definitions could be introduced.

As we said above, it is not clear that this move can help in all cases. Many authors have pointed out, however, that the nominalist is free to chose other ressources. Michael Resnik straightforwardly suggests that in order to develop a nominalistically acceptable meta-mathematics that is workable, the nominalist has to assume an infinite universe. A Fieldian conception of space-time would help providing one:

If we followed Field we would find it much easier to develop a nominalistic syntax than did Quine and Goodman, because we find an infinitude of inscriptions in his already posited continuous space-time. (Resnik 1983, p. 518)

²¹ Cf. Mancosu (2005).

Also Henkin (1962) admits that Goodman and Quine need not make any assumptions regarding finitism for their *nominalistic* project. Goodman, in particular, was prepared to divorce nominalism from finitism. In “A World of Individuals” (Goodman (1956), reprinted in Goodman (1972)) Goodman pointed out that nominalism is not logically connected to finitism:

The nominalist is unlikely to be a nonfinitist only in much the same way a bricklayer is unlikely to be a ballet dancer. The two things are at most incongruous, not incompatible. Obviously by the stated criterion for nominalism [essentially, the rejection of classes], some systems with infinite ontologies are nominalistic, and some systems with finite ontologies are platonistic. (Goodman 1972, p. 166)

And later, in *Problems and Projects* Goodman (1972) he clearly seems to be ready to give up finitism for the sake of nominalism, in response to Alonzo Church’s challenges to Goodman and Quine’s finitistic syntax:

In the first place, I should point out that this letter [by Alonzo Church, in which Church lists tasks that he thinks the finitist still has to accomplish] predated “A World of Individuals”, where nominalism is carefully distinguished from finitism. Our position in “Steps” was indeed finitistic as well as nominalistic; but finitism, although a friendly companion of nominalism, is neither identical with nor necessary to it. (Goodman 1972, p. 154)

We thus content ourselves with the fact that Goodman would have found the assumption of an infinite universe nominalistically acceptable.²²

However, there remains a problem: the assumption made about the size of the universe was introduced as an empirical hypothesis about the actual world. We usually assume such hypotheses to be *contingently* true, if true at all. This, however, appears to conflict with the very nature of logic. Logical consequence is usually assumed to be a matter of necessity. How can a contingent assumption serve as its foundation?

5.2. *The Size of the Universe and Logical Consequence*

Three problems need to be distinguished here. The first concerns a vague feeling, the second and third can be put forward in a precise way.

There might be a vague and uncomfortable feeling arising, given our explication of logical consequence, that the size of the universe, or the existence of some peculiarly shaped space-time regions, just should have nothing to do, generally speaking, with what follows *logically* from what. Vague worries are difficult to address, but here are some remarks which might help, at least, to get into the spirit.

First, a semi-technical point: the explication of logical consequence only indirectly depends on the size of the universe. The consequence relation is pinned down by the inferentialist proposal: what follows from what is determined by the inference rules. The trouble only arises in the *metatheory* when an explicit definition of logical consequence is asked for. There, logical consequence is defined as a certain relation. A relation requires relata, and for the nominalist nothing but concrete things can serve as such. The explication of a sentence, or, more generally, the provision of a nominalistically acceptable *syntax*, primarily involves the worrisome sentence tokens. But if infinitely

²² Compare also Goodman’s brief remark on Field in Goodman (1984, p. 53).

many sentences are indeed needed, and sentences are concrete objects, then there will have to be infinitely many concrete things.

Further, the nominalist might ask back what the opponent expected a nominalistically acceptable definition to look like. Obviously, it will only mention concrete entities, what else could it do? These are the only entities that nominalists allow themselves, after all. Note, however, that no cunning coding tricks are used in our proposal here which are employed elsewhere, in order to achieve arguably nominalistic reconstructions of mathematics. Let us emphasise that, rather than assuming a large enough ontology to allow for the interpretation of some mathematical notion,²³ the proposal here is in exact correspondence with the actual practice of proof (or, rather, the idealised version of it that is commonly assumed in the discussion of logical consequence). The ontology here assumed, concrete inscriptions of proofs, is precisely the gold standard of proof in logical and mathematical practice: the provision of a written down version of a proof to demonstrate that the inference holds. The explication of logical consequence presented here thus comes with an epistemology already attached. This was one of the motivations for the project in the first place.

The first precisely formulated problem concerns the counterfactual dependence of the extension of logical consequence given our proposal on the size of the universe. Let us here take for granted that the actual universe is infinite and thus big enough for the nominalist definition of logical consequence to be extensionally equivalent to the platonist inferentialist conception of logical consequence. The dependence on actual inscriptions makes logical consequence nevertheless counterfactually dependent on the size of the universe: suppose there is a possible finite universe and that there is some proof inscription that “uses up” all space-time in that universe. Say the last line of this proof, the consequence of the argument, is ψ , and one of the premises it rests on is φ , then it seems that the nominalist would be forced to say, that $\ulcorner \varphi \supset \psi \urcorner$ is *not* a logical consequence of the rest of the premises that ψ was originally derived from. The rule for \supset -introduction, also known as *conditional proof*, would of course licence the inference, but since we ran out of space-time, this inference cannot be drawn.²⁴ We would be forced to say that in *this* universe, $\ulcorner \varphi \supset \psi \urcorner$ is *not* a logical consequence of the premises, while in our (infinite) universe it is. But, surely, the size of the universe should not matter for the question, what follows from what.²⁵

Goodman, we think, would not have considered this objection to be seriously damaging. His meta-philosophical conception of explications would have counted the explication we arrived at as adequate, since extensional equivalence is sufficient for this purpose. Indeed, Goodman is famous for insisting that even co-extensionality is too strong a requirement for adequacy of explications Goodman (1977, pp. 3–22). Goodman’s weak requirements for adequate explications are certainly met by the account

²³ As, e.g., in Lewis (1991) or Niebergall (2005).

²⁴ This, in effect, amounts to Alonzo Church’s demand, that the inscriptional account suggested by Goodman and Quine would have to be able, *inter alia*, to sustain the deduction theorem. The letter in which Church raises this criticism is published in Goodman (1972, pp. 153–154), alongside with Goodman’s dismissive response.

²⁵ See Wilholt (2006, pp. 122–123) for a related worry.

proposed here. Thus we have at least an account of logical consequence before us that would have satisfied Goodman.

But there is the second problem which questions whether the the actual universe is, in fact, infinite and thus big enough for the nominalist definition of logical consequence to be extensionally equivalent to the platonist inferentialist conception of logical consequence.²⁶ Suppose the above derivation described again. If the universe turns out to be finite so that there is not enough space-time for an actual concrete derivation of $\lceil \varphi \supset \psi \rceil$. In this case, $\lceil \varphi \supset \psi \rceil$ is actually not a logical consequence of the relevant premises, despite the fact that the rule of conditional proof would licence the inference if only the universe was bigger.

Besides biting the bullet and admitting that $\lceil \varphi \supset \psi \rceil$ does, in this case, not actually follow from the relevant premises, we can see three strategies that nominalists of different temperaments might adopt to avoid this problem.

If we are happy to allow ourselves modal notions, we could amend the proposal to let logical consequence be a relation between *possible* concrete sentence tokens. The nominalist could point out that the modality required for this treatment does not fall foul of the common obscurity objections with which modal notions are traditionally attributed. No vague speculation or guesswork is required to work out how the relation will extend beyond the actual universe. Our schematic rules determine this extension precisely. A slightly bigger universe would contain no surprises as to what follows from what. We merely have more inscriptions there that look exactly the same, only that some are longer, and behave in exactly the same way as the actual inscriptions do.

The case would, indeed, be much like the following. Imagine a mathematician who runs out of paper while scribbling down a certain proof just before she can write down the conclusion of the proof (which is an application of conditional proof). It would be madness to claim that, owing to the lack of paper and the resulting lack of the last line, there is no telling how the concrete inscription of the proof would go on, or that it is obscure to say that it is *possible* to extend the proof. It is perfectly obvious how the proof would continue if she had another sheet of paper. It does not matter for this purpose whether she *actually* bothers to get another sheet. It would also make no difference if there happened to be no more paper in the universe at all, or, indeed, anything else to write on. Obviously, the conclusion follows, whether our mathematician finds another sheet of paper or not, whether there exists any more paper in the universe or nor, or whether there is enough space-time for the last line or not. But this unproblematic notion of possibility is all that is needed in the “modal” version of the explication of logical consequence.

Further, this strategy has a variant: instead of quantifying over possible sentence tokens in the definition of logical consequence, one could employ a *constructibility* operator akin to that which Charles Chihara introduced in his nominalistic programme Chihara (1990). Logical consequence would not be a relation between possible sentence tokens, but between *constructible* sentence tokens, where ‘constructible’ is obviously not to be

²⁶ Note that it would be sufficient if *any* aspect of the (concrete) physical universe were infinite for a similar construction to go through. Thus, the case described here involves that *nothing* physical is infinite or infinitely divisible: not just space, but also time, electromagnetic force, wave-length, gravity, etc.

analysed as ‘possible to construct’ — otherwise this variant would be no more than a complication of the modal strategy above. What is constructible, again, would have to take us beyond what actually can be constructed given the finite cardinality of the universe that we here assume.

Lastly, a *counterfactual* approach could be taken. Counterfactuals should again best not be taken as to be reduced to possibility in order to not merely complicate the modal proposal, and there are reasons to refrain from such attempts in any case.²⁷ If, for instance, a Stalnaker-Lewis-style analysis of counterfactuals was given, one would end up not only with a yet to be explained notion of possibility (or possible world), but also in want of an explication of the similarity (or closeness) relations which analyses of this ilk require in addition. The definition of logical consequence on this account would thus still be in terms of a relation between concrete sentence tokens; it would, however, state something like: a sentence φ is a *logical consequence* of some premises Γ if and only if, if the universe were large enough to contain the required inscriptions, there would be a derivation of φ from Γ whose single lines are either sentences of Γ , etc.

These three proposals contain notions — possibility, constructibility, counterfactual — which themselves are in want of explanation. If any of the three is adopted, one would have to let go of the hope to define it in terms of logical consequence, on pain of vicious circularity. This might seem a high price to pay. On the other hand, perhaps the techniques suggested here could be utilised for such explications. Attempting this is beyond the scope of this paper, but the beginning of an explication of possibility along the lines suggested here might be made using the inferentialist account of modality.²⁸

In what follows, we discuss these proposals in relation to the above mentioned nominalistic projects.

5.3. *Two Nominalist Programmes Revisited*

It seems that, although the notion of logical consequence is available to the nominalist, there will still remain open problems in the nominalistic programmes that we have introduced above. Let us first discuss the problem as it arises for fictionalism, and then turn to the problem that seems to remain for modal eliminative structuralism.

As we have said, Field wants to prove the conservativeness of mathematics, in order to show that mathematics is — despite being a useful tool for shortening derivations — dispensable for the derivation of physical consequences from physical theory. It seems obvious that Field cannot establish the relevant conservativeness result using the explication of logical consequence provided here. As Stewart Shapiro Shapiro (1983) has shown, one can formulate a Gödel sentence, G , in the nominalistic language, such that G is not provable in the nominalistic theory N , but provable (via Field’s bridge principles) in $N + S$, where S is, e.g., Zermelo-Fraenkel set theory. According to our analysis, mathematics is thus not conservative over nominalistic physics: there are logical consequences of $N + S$, formulated in the nominalistic language, that are not logical consequences of N , in the explicated sense of logical consequence.

²⁷ In fact, Timothy Williamson has recently suggest to do it the other way around: to analyse modal notions in terms of counterfactuals; see Williamson (2005).

²⁸ See for instance Prawitz (1965) and Read (2008).

For Hellman's project, things do not seem to be much better. First of all, explicating the notion of possibility that modal eliminative structuralism assumes in terms of logical consequence won't help if modal notions are essential for the explication of logical consequence (cf. the previous section). By this variant of the explication we would be led back in a full circle.

Taking either the counterfactual option or the proposal involving the notion of constructibility, the circularity worry does not arise (at least not initially), but what we have achieved is to patch up an account that was found wanting because it used an unexplained notion of logical possibility by using a further unexplained notion. This is little progress, if progress at all.

As we indicated above, if the nominalist accepts Goodmanian standards for the adequacy of explications, this problem might not arise, since in that case no notion of possibility needs to be assumed for the explication of logical consequence. However, the rich actual system of space-time could directly be put to use as an instantiation of all structures that a structuralist should²⁹ be interested in. If there is no reason to be a modal eliminativist in the case of logical consequence, there surely does not seem to be such a reason in the case of mathematics. Thus, while logical consequence on this variant of the explication might help the structuralist nominalist, it at the same time seems wholly superfluous.

A platonist might also sense a problem if — as in the case of Hellman's original proposal — the logic is assumed to be second-order. In this case, there will be a sentence, G , formulated exclusively in (second-order) logical vocabulary, such that G is not a logical truth of second-order logic, but a logical truth of third-order logic.³⁰ 'Logical truth' is here used in the inferentialist sense, such that something is a logical truth if it is a logical consequence of the empty set of premises, i.e. (in nominalistic terms) if there is a derivation-inscription of an inscription of that sentence that accords to the rules and contains no undischarged assumptions. We can call this the *non-conservativeness* of third-over second-order logic.³¹

The objection might be formulated thus: although G is not a logical truth (of second-order logic), it is nevertheless not the case that $\Diamond\neg G$, since $\neg G$ is ruled out by third-order logic. G is a theorem of third-order logic, and thus $\Box G$, i.e. $\neg\Diamond\neg G$.

It seems, however, that this problem might be surmountable with an amendment in the notion of possibility. There are two options that come to mind. First, one could be a relativist about logical possibility, second, one could be a minimalist about this notion.

According to relativism, $\neg G$ is not logically impossible *simpliciter*, but logically possible relative to second-order logic and logically impossible relative to third-order logic,

²⁹ If we assume that the structuralist is interested in making sense of mathematics as it is put to use in the sciences.

³⁰ Third-order logic is the next step up in an infinite hierarchy of n -th order logics, that is in many ways similar to the hierarchy of simple type theory. Second-level predicates are introduced to apply to the "ordinary" predicates of first- and second-order logic which are now called first-level predicates. Third-order variables stand in the place of second-level predicates in the same way that second-order variables stand in the place of first-level, i.e. ordinary predicates. Third-order quantifiers bind these.

³¹ For a proof and discussion of the non-conservativeness of third-over second-order logic see Rossberg (2006); the proof is based on a result of Leivant (1994, §3.7).

because G is a theorem of third-order logic, but no theorem of second-order logic. Since G contains only second-order vocabulary, this move seems only promising if we assume that logical consequence and logical truth (and *a fortiori* the “meaning” of a sentence containing logical vocabulary), also in case of sentences that use only a restricted selection of logical vocabulary, is always determined “holistically” by all introduction and elimination rules of a given logic. Since G is a sentence of two different logics (second-order and third-order logic), it thus can be a logical truth in one, while failing to be a logical truth in the other.

One might also attempt to surmount the problem by being a minimalist about logical possibility, such that some sentence S counts as a logical possibility *simpliciter* only if there is no logic in the hierarchy of (n -th)-order logics (or maybe an even wider range of logics), such that $\neg S$ would be a logical truth in that logic (and, accordingly, some sentence T is a logical truth *simpliciter* if it is a logical truth of some logic). This paper is not the place to discuss these options in any detail, but note that Crispin Wright suggests, for related reasons, introducing the quantifiers of all orders up to ω (and possibly beyond) at once.³²

That we have no better news for nominalistic programmes hardly is the fault of the notion of logical consequence. If logical consequence in a nominalistically acceptable conception is less “powerful” than the platonist notion, then this merely means that there is work yet to be done for the nominalist, but not that they should resign themselves to relying on platonistic smoke-screens.

If logical consequence turns out to be insufficient to explicate some notion that a particular nominalist project requires, then the task will have to be done with the assistance of other nominalistically acceptable means. Using, on the other hand, a notion of logical possibility as an inexplicable primitive seems to have the same advantages as theft over honest toil.³³

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³² Wright (2007), but see also Rossberg (2006) for a problem with this proposal.

³³ This paper was presented at the workshop “Abstract Objects in Semantics and the Philosophy of Mathematics” in Paris, February 28 – March 1, 2008. We would like to thank the audience for the helpful discussion. We are also indebted to Catarina Dutilh Novaes and Michael Gabbay for comments and criticisms of an earlier version of this paper.

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Logical Consequence for Nominalists

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ABSTRACT: It has repeatedly been argued that nominalistic programmes in the philosophy of mathematics fail, since they will at some point or other involve the notion of logical consequence which is unavailable to the nominalist. In this paper we will argue that this is not the case. Using an idea of Nelson Goodman and W.V. Quine's which they developed in Goodman and Quine (1947) and supplementing it with means that should be nominalistically acceptable, we present a way to explicate logical consequence in a nominalistically acceptable way.

Keywords: Philosophy of mathematics, nominalism, logical consequence, inferentialism, Nelson Goodman, W.V. Quine.

1. *The Argument from Logical Consequence*

We do not have any strong convictions concerning the question of the existence or non-existence of abstract objects. We do, however, believe that ontological fastidiousness is *prima facie* a good attitude to adopt. More precisely, ontological parsimony provides a *pro tanto* reason for theory choice. Nelson Goodman added to this fairly obvious principle the methodological observation that a more parsimonious theory can always easily be turned into a theory that is ontologically more lavish; and so a nominalistic theory can be turned into a platonistic theory without any problem. There is, however, no guarantee that the other way around is generally available Goodman (1977, p. L). So, hedging your bets, it is advisable to resist an extravagant ontology for as long as possible. The ontic extravaganza that Goodman zoomed in on was that of the calculus of classes, or set theory as it is commonly known today. Nominalism for Goodman meant the renunciation of classes, platonism their acceptance.

Goodman was certainly one of the most vocal defenders of nominalism in the last century. Nowadays, the attempts he had made together with W.V. Quine to achieve a nominalistic foundation of mathematics are often regarded as a complete failure. In a very influential synopsis of various nominalistic strategies, John P. Burgess and Gideon Rosen write:

Goodman and Quine made it their priority to reconstrue the kind of science in which mathematics is applied, and especially the kind of mathematics applied in science. [...] After some modest initial progress, the project of Goodman and Quine reached an impasse. (Burgess and Rosen 1997, p. 5)
[...] Quine, after the failure of his joint project with Goodman, soon came to reconsider, and eventually came to recant, his nominalism. (Burgess and Rosen 1997, p. 32)

It seems to us that this underestimates Goodman and Quine's achievements.¹ As we hope to show in this paper, the strategy that Goodman developed together with Quine

¹ Moreover, Burgess and Rosen's claim in the second part of the quotation does not strike us as historically correct regarding Quine's surrender to platonism.



in their paper “Steps Toward a Constructive Nominalism” Goodman and Quine (1947) can aid closing one of the most problematic open gaps in contemporary nominalism.

This open gap concerns the notion of logical consequence as employed by nominalists. It has been argued, on various grounds, that a nominalistically acceptable and at the same time adequate explication of logical consequence is unobtainable. This objection is usually raised against specific nominalistic programmes that aim to prove the dispensability of any ontological commitment to abstract objects for science.² Many nominalists have to rely in their projects on logical consequence at some point. In the next section we will briefly sketch two cases.

Prima facie, logical consequence should be unproblematic for the nominalist. Logic, of all things, should not carry any ontological commitments, let alone ontological commitments to any dubious entities such as abstract objects. In fact, many philosophers of logic believe that this is a mark of logicity. However, anti-nominalists have produced the following *Argument from Logical Consequence*:

- (P1) Without using the notion of *logical consequence* the nominalist cannot explain mathematical practice and its contribution to science.
- (P2) *Logical consequence* can only be satisfactorily explicated with appeal to abstract objects.
- (P3) But *logical consequence* is a notion that stands in need of explication.
- (C) The nominalist cannot explain mathematical practice and its contribution to science.³

As we said, the reasoning behind premise (P1) will be explained in more detail in the next section. (P2) and (P3) could be argued for as follows: One might begin by arguing that the most influential explication of logical consequence is the model-theoretic approach which goes back to the work of Alfred Tarski. This approach, however, makes use of set theory and is thus committed to the existence of abstract objects. Alternative, proof-theoretic explications also fail to be nominalistically acceptable, in that they employ sets or sequences of sentences, which are abstract objects too see, e.g., Hale and Wright (1992, p. 112). Indeed, sentences themselves are usually taken to be abstract objects: types, say. This much should support (P2).

(P3) blocks the move just to assume the semantic consequence relation to be primitive and therefore not in need of any explication. This move — which is in fact chosen by some contemporary nominalists — is in conflict with the epistemological motivation that many nominalists had in the first place. At least those nominalists who eschew abstract objects because of their “epistemology” should not accept a mysterious and inexplicable consequence relation either.

In this paper we aim to rebut this version of the Argument from Logical Consequence. In section 3., we argue that one of the main assumptions which this argument

² See, for instance, Hale and Wright (1992, 1994), Parsons (1990), Resnik (1983, 1985), Shapiro (1993, 1997).

³ A similar argument, called “the argument from the philosophy of logic” is made in Wilholt (2006), the argument here is modeled after Wilholt’s.

rests on is mistaken. We argue for an explication of logical consequence along inferentialist lines. We show how a consequence relation, so understood, can be explicated in a nominalistically acceptable way, using the techniques developed by Goodman and Quine in Goodman and Quine (1947). In the final section we discuss the prospects for using this explication of logical consequence in the two nominalistic programmes described in the following section.

2. *Two Nominalist Programmes*

Nominalists, as we understand them, renounce classes, and *a fortiori*, objects which are often considered to be constructed from them, such as numbers. Consequently, there are three positions that nominalists can adopt with respect to statements of mathematics: they can be regarded as true but not about numbers, sets or other mathematical objects; or as “strings of marks without meaning” Goodman and Quine (1947, p. 111); or as literally false Field (1980, p. 2 and *passim*). Nominalistic projects are usually concerned with the reconstruction of the success of mathematical *practice*. As Goodman and Quine expressed it, the challenge the nominalist has to face is to “account for the fact that mathematics can proceed with such remarkable agreement as to methods and results” Goodman and Quine (1947, p. 111). Moreover, mathematics apparently makes an important contribution to science, and the nominalist is challenged to account for this fact too. In what follows, we will sketch two ways in which contemporary nominalists have tried to give an account of mathematics, *eliminative structuralism* and *fictionalism*. We will see that both accounts make use of the notion of logical consequence at crucial places.

2.1. *Logical Consequence and Eliminative Structuralism*

Structuralism in the philosophy of mathematics holds that (pure) mathematics is the deductive study of structures as such. A nominalistic version of this view holds that mathematics does not, in fact, make statements about any abstract objects that constitute such structures (e.g., the natural numbers), but about whatever occupies the positions in a natural number system, a system of objects that happens to instantiate the relations that the realist assumes to be obtaining between the natural numbers. Accordingly, a mathematical statement like

$$(M) \quad 3 + 9 = 12$$

would be interpreted as

(M*) In any natural number system S , the object in the 3-place-of- S S -added to the object in the 9-place-of- S results in the object in the 12-place-of- S . Shapiro (1997, p. 85)

This way, the ontologically problematic statements of mathematics also come out nominalistically acceptable: ‘numbers exist’ comes to ‘every natural number system has objects in its places’:

The programme of rephrasing mathematical statements as generalizations is a manifestation of structuralism, but it is one that does not countenance structures, or mathematical objects for that

matter, as bona fide objects. Talk of numbers is convenient shorthand for talk about all systems that exemplify the structure. Talk of structures generally is convenient shorthand for talk about systems. (Shapiro 1997, p. 85)

Such an “eliminative structuralism” faces the *problem of vacuity*: in order to make sense of large parts of mathematics, one seems forced to accept a rich background ontology, and the nominalist just lacks the resources to provide one.⁴ To see how the problem arises, consider φ to be a sentence in the language of arithmetic.⁴ Eliminative structuralism understands φ as something of the form

(φ^*) For any system S , if S exemplifies the natural number structure, then $\varphi[S]$.

$\varphi[S]$, here, is obtained from φ by interpreting the arithmetic terminology and the variables in terms of the objects and relations of S . Let us assume that there are only finitely many concrete objects. In this case, φ^* comes out vacuously true, no matter what φ is, since nothing exemplifies the natural number structure. Accordingly, an eliminative structuralist account of arithmetic will need to assume infinitely many objects in its background ontology, an eliminative structuralist account of Euclidean geometry a background ontology of the cardinality of the continuum, etc.

There seem to be two ways for the nominalist to meet this challenge. One way would be to assume that there are actual, concrete structures that can play the role of the background ontology. At least for all cases of applied mathematics the nominalist will need to hold that there is such an actual system that instantiates the mathematical structures applied. The problem here seems to be that mathematical truth and falsity will depend on contingent matters about the actual world, namely on whether there are actual systems of objects that instantiate the mathematical structures. But does the falsity of ‘All even numbers are prime’ depend on whether or not there exist infinite totalities of concrete entities that constitute ω -sequences?⁵

It is not entirely clear how strong this counter-argument is. After all, the nominalist typically feels forced to give an account for mathematics in the first place, because of the apparent usefulness of mathematics for science. But *that* mathematics is useful for science could be considered a contingent matter, accordingly, the nominalist need only explain why mathematics works, when it does. In a world with only a few objects, it might be possible to use mathematical structures that only require a comparatively small background ontology; in a finite universe, for instance, a mathematics of finite structures might be all that is required for science.

A similar reply, that would bring us closer to the second way for the nominalist to answer the problem of vacuity, could point out that also the platonist alternative will assume that there are *actually* enough abstract objects to play the role of the background ontology. Doesn’t that make mathematical truth and falsity equally counterfactually dependent on the existence of abstract objects? The difference is that platonists are prepared to assume that their abstract objects exist *necessarily*, while the actual systems of concrete entities of the nominalist exist only contingently.

⁴ The example is taken from Shapiro (1997, p. 85).

⁵ For a related worry see Wilholt (2006).

But instead of assuming sufficiently rich actual systems that might or might not exist, depending on the way the world is, in order to instantiate the mathematical structures, one could rephrase eliminative structuralism as a claim about possible systems, the possibility of which depends on logic alone. Accordingly, our sentence of arithmetic φ would be interpreted as saying

(φ^{**}) For any *logically possible* system S , if S exemplifies the natural number structure, then $\varphi[S]$.

or as

(φ^{***}) Necessarily, for any system S , if S exemplifies the natural number structure, then $\varphi[S]$.

Geoffrey Hellman (1989) carries out such a programme of *modal eliminative structuralism*. Instead of assuming that there are actual systems that instantiate mathematical structures, the modal eliminative structuralist would only assume that there are possible systems that exemplify these structures — what could be ontologically less problematic? Merely assuming that some systems are possible should not commit one to anything, or so one might hope.

However, there is a problem: in order for his account to work, the modal eliminative structuralist either needs an account of logical possibility or must assume that the notion of logical possibility is primitive and not in need of further explication. The platonist can challenge the latter assumption and claim that our grasp of the modal terminology when applied to mathematics is too sophisticated to be considered primitive. It is rather mysterious how we manage to use these notions the way we do if it was not for the fact that they are themselves mathematically mediated:

When beginning students are first told about logical possibility, logical consequence, etc., most of them seem to have *some* idea of what is meant, but consider how much their initial “intuitions” differ from our “refined” ones. The anti-realist owes us some account of how we plausibly could come to understand the notions in question (as applied here) *as we in fact do*, independent of our mathematics. Without this it is empty to use a word like “primitive” [...]. (Shapiro 1993, p. 475)

These worries concerning the involvement of modality might be silenced by pointing out that logical possibility is unproblematic since it can be defined with recourse to logical consequence or logical truth: φ is logically possible if and only if $\neg\varphi$ is not a logical truth. (We assume that logical truth is defined as a degenerate case of logical consequence — see section 3. for our concrete proposal.)

In this case, however, an account of logical consequence is needed that does not rely on abstract objects, in the way that the model-theoretic explication does.

2.2. *Logical Consequence and Fictionalism*

In order to see that this problem not only arises for the modal eliminative structuralist, we will briefly sketch the problem as it arises for Hartry Field’s fictionalism.

Field tries to defend nominalism against the objection that mathematical objects must exist to the best of our knowledge, by trying to undercut the platonist’s *Indispensability*

Argument. In particular, he tries to undercut the platonist's claim that mathematics is indispensable for science, instead he attempts to show how science can be done "without numbers" Field (1980, 1989).

The idea is the following: Field formulates part of physics — namely, Newtonian mechanics — in a way that does not involve reference to any abstract objects. He formulates this nominalistic theory N using a second-order mereology (at least in one version of the programme). He then proves metatheoretically that a platonistic extension of N , $N + S$, is *conservative* with respect to N : there are no nominalistically storable consequences of $N + S$ that are not consequences of N alone. In other words: in deriving nominalistically storable conclusions, mathematics basically does the same as logic. Logic might do it in a more long-winded fashion than mathematics, so the latter has a practical use in making derivations shorter and more elegant, but any consequence is also available without mathematics.

There are two places in which logical consequence seems to matter. First it seems to matter when spelling out the second-order logic the nominalist wants to use to accompany nominalized physics. Some have articulated the worry that the logic used here might already undermine the nominalistic enterprise (e.g. Resnik (1985, p. 163)). However, the more urgent problem seems to come with the fact that the conservativeness claim itself is formulated in terms of logical consequence:

[T]he fictionalist thesis of conservativeness is stated in terms of logical consequence, and the two best historical explications of this are unavailable to the fictionalist. (Shapiro 1993, p. 461)

Again, Field assumes like Hellman that the notion of logical possibility can be taken as a primitive Field (1991). Of course he must then face the same epistemological challenges as the modal eliminative structuralist.

3. *Logical Consequence without Models*

3.1. *Inferentialism*

The most generally accepted explication of logical consequence is, no doubt, the model-theoretic construal which goes back to the work of Alfred Tarski. Logical consequence is, in the modern formulation, defined using set theory: a sentence φ is a logical consequence of a set of sentences Γ if and only if φ is satisfied by all models that satisfy all members of Γ . The models mentioned here are sets, and satisfaction is defined in set-theoretic terms.

That this notion is not available to the nominalist has been remarked above, and this point was raised by many authors, in particular in connection to Field's programme.⁶ There is another approach to logical consequence, however, that at least *prima facie* does not involve nominalistically unpalatable notions, which goes back to the work of Gerhard Gentzen (1935) and puts the logical inference rules in the centre of attention. In recent times such approaches, which variably have been subsumed under the labels 'inferentialism' or 'proof-theoretic semantics', have received considerable attention; prominent

⁶ In addition to Field himself, e.g. Field (1989, pp. 30–31), see for instance Resnik (1983, 1985, §2), Hale and Wright (1992, 1994), Shapiro (1983, 1993, §2), MacBride (1999, §4).

proponents include Michael Dummett, Ian Hacking and Dag Prawitz.⁷ Inferentialism insists that the meaning of the logical constants is determined by their introduction- and elimination-rules, and that these rules (so far as they are the correct ones) are self-justifying. No further appeal to model-theoretic semantics, truth-tables or the like is needed in order to argue for the validity of the rules.

As is well known, conditional, negation, and universal quantification suffice to characterise classical logic,⁸ so we will restrict our attention to these connectives. According to the moderate inferentialism recommended here, logical consequence is characterised by the following rules which can be added to any formal language that contains ‘ \supset ’, ‘ \neg ’, and ‘ \forall ’, and has appropriate syntactical categories for these rules to operate on:⁹

$$\begin{array}{ccc}
 \frac{\begin{array}{c} [\varphi] \\ \vdots \\ \psi \end{array}}{\varphi \supset \psi} \supset I & \frac{\begin{array}{c} [\varphi] \quad [\varphi] \\ \vdots \quad \vdots \\ \psi \quad \neg\psi \end{array}}{\neg\varphi} \neg I & \frac{\Phi(t)}{\forall x\Phi(x)} \forall I \\
 \\
 \frac{\varphi \quad \varphi \supset \psi}{\psi} \supset E & \frac{\neg\neg\varphi}{\varphi} \neg E & \frac{\forall x\Phi(x)}{\Phi(t)} \forall E .
 \end{array}$$

The $\forall I$ -rule has the standard proviso that ‘ t ’ does not occur free in Φ or in any of the relevant assumptions. The square parentheses in $\supset I$ and $\neg I$ indicate that φ is an assumption that is discharged by the application of these rules (thus, strictly speaking, we have also the rule of assumption).¹⁰

Logical consequence is then explicated using these rules:

A sentence φ is a *logical consequence* of some premises Γ if and only if there is a derivation of φ from Γ whose single lines are either sentences of Γ , result by applications of the above rules from previous lines, or are assumptions that are discharged by applications of $\neg I$ or $\supset I$.

φ is a *logical truth* if and only if it is thus derived using no undischarged premises.

The trouble with this definition is that it mentions notions that do not appear to be readily available to the nominalist: sentence, line and derivation, which are usually taken to be abstract types. If we want to spell out everything in detail, we will also have to mention variables, terms, logical symbols, and more. Also, ‘ Γ ’ looks suspiciously like the

⁷ See, e.g., Dummett (1973), Hacking (1979), Prawitz (1965, 1971, 1991), but also Milne (1994), Read (2000), Wright (2007).

⁸ There is, of course, an issue concerning whether the logic should indeed be classical. This is, however, assumed by Goodman and most contemporary nominalists and will not be discussed here. Needless to say, inferentialism is not committed to classical rules of inference.

⁹ For simplicities sake, we will assume that no other logical axioms or rules are present for the language in question prior to the introduction of these rules.

¹⁰ For more details see, for example, Prawitz (1965).

name of a set. The definition has, hence, as of yet some significant gaps that are still to be filled in — in a nominalistically acceptable way. This problem will be addressed in section 4.

3.2. *Second-Order Logic*

With this conception of logical consequence at hand (*modulo* the gaps that will be filled in §4.), logical means more powerful than those provided by first-order logic become available. We can supplement our rules by a pair of rules for second-order quantifiers. Second-order logic allows generalisation into predicate-position in much the same way that first-order logic allows generalisation into name-position: with quantifiers binding variables that take the place of these expressions.¹¹ As we will see in the final section, the adoption of second-order logic is crucial to several nominalistic projects. We will see in §4., however, that the second-order quantifier also comes in handy in our explication of logical consequence, albeit without being strictly required.

The rule for the second-order quantifiers we add are:

$$\frac{\Phi(T)}{\forall X^n \Phi(X^n)} \forall^2 \text{I} \qquad \frac{\forall X^n \Phi(X^n)}{\Phi(\Xi)} \forall^2 \text{E} .$$

In $\forall^2 \text{I}$, T is a n -place predicate letter or free variable that must not occur free in Φ or any of the relevant assumptions. In $\forall^2 \text{E}$, Ξ is an open sentence with n argument places;¹² no variables in Ξ are to be bound in $\Phi(\Xi)$ that are not already bound in Ξ .

For sure, second-order logic has attracted a profusion of criticism. Most prominent amongst the complaints are incompleteness allegations, and Quine's famous claim that second-order logic is nothing but "set theory in sheep's clothing" Quine (1970, pp. 66–68). The former complaint is usually framed thus: the non-axiomatisable consequence relation of second-order logic on the standard model-theoretic conception is intractable and does, hence, not qualify as *logical* consequence. We will content ourselves here with the observation that the second-order consequence relation we are after is proof-theoretic, and that therefore this intractability objection does not arise. This paper is not the place for a more detailed discussion.¹³

¹¹ For more details compare the current "bible" of second-order logic: Shapiro (1991). Shapiro favours the model-theoretic conception of logic. For a proof-theoretic characterisation see, e.g., Prawitz (1965).

¹² We allow instantiation with open sentences in this rule, rather than just predicate letters, in order to gain the proof-theoretic strength of what, in axiomatic systems of second-order logic, is known as the *comprehension schema*: $\exists X^n \forall \langle x \rangle_n (X^n \langle x \rangle_n \equiv \Phi \langle x \rangle_n)$ (where ' $\langle x \rangle_n$ ' abbreviates a string of variables, ' $x_1 x_2 \dots x_n$ '). We bracket a discussion of comprehension here, and also dodge the issue of quantification over functions which is usually included in a formulation of second-order logic. For details see Shapiro (1991, §3.2). Although it is irrelevant here, the reader might be interested in noting that n -place functions can be simulated by using $(n + 1)$ -place predicates: the clause $\lceil \forall \langle x \rangle_n \exists! y F^{n+1} \langle x \rangle_n y \rceil$ indicates that $\lceil F^{n+1} \rceil$ is in effect an n -place function (where ' $\exists!$ ' stands for the first-order definable 'there is exactly one').

¹³ Such a discussion can, however, be found in Rossberg (2004). The for the inferentialist more pressing problem of a proof-theoretic notion of incompleteness is discussed in Rossberg (2006) and Wright (2007).

Quine's complaint about the set-theoretic commitment that second-order quantification allegedly brings about has been contested in various places, on various grounds. To name but a few: George Boolos famously provided a plural interpretation of the monadic second-order quantifiers that dispenses with any commitment to sets and which at least David Lewis found nominalistically acceptable.¹⁴ Crispin Wright argued that second-order quantification cannot bring about new ontological commitment: if predicates, as Quine contends for instance in Quine (1948), do not themselves carry any ontological commitment to sets (or properties), then this commitment cannot suddenly arise when one generalises into predicate position; much like first-order quantification does not suddenly bring about commitment to new objects when applied in a language that contains non-referential terms.¹⁵ Even assuming that Quine is correct about the ontological commitment of a theory being exhibited by the first-order quantifiers, there does not appear to be any way of arguing from there that the second-order quantifiers bring about a commitment to sets.

As mentioned above, the involvement of sets is obvious if a model-theoretic approach to logical consequence is chosen. This, however, is nothing peculiar to second-order logic on this conception, but is the case for ordinary first-order logic as well. But since we here attempt to manage without model theory altogether, this problem does not arise either. We thus leave the discussion at this stage in order to return to the question of the significant gaps that still remain in our explication of logical consequence.

4. *Proofs and Tokens*

4.1. *Concatenation Theory*

The problem of providing a nominalistically acceptable theory of syntax and proof-theory for a formal language was tackled by Goodman and Quine in their joint paper "Steps Toward a Constructive Nominalism" using the Calculus of Individuals, developed by Henry S. Leonard together with Goodman, and a theory of token-concatenation.¹⁶ Their effort has been found wanting due to a couple of limitations, which we aim to overcome here.

¹⁴ Boolos (1984, 1985), Lewis (1991). The plural interpretation has subsequently been further developed, see e.g. Rayo (2002), Rayo and Uzquiano (1999). Agustín Rayo and Stephen Yablo Rayo and Yablo (2001) attempt to provide an interpretation of polyadic second-order quantification, roughly along Boolos's lines and inspired by Arthur Prior's work Prior (1971, chapter 2). Peter Simons also draws conclusions with respect to the debate about higher-order logic from Prior; see Simons (1993, 1997). The plural interpretation itself has been found wanting in various respects — see, e.g., Linnebo (2003), Resnik (1988) or Shapiro (1991, §9.1.1) — and is not further pursued for the purpose of the present paper.

¹⁵ Wright (1983, pp. 132–133), more recently again and in more detail in Wright (2007). Rayo and Yablo (2001) suggest a similar principle.

¹⁶ Goodman and Quine (1947); see also Martin (1958) for a detailed study of token-concatenation theories in this context. The Calculus of Individuals was published in Leonard and Goodman (1940); for a study of its development see Rossberg (2009). An investigation into second-order versions of calculi of individuals can be found in Niebergall (2009).

To build a syntax for a formal language, we have to be able to say what a well-formed formula — or (open) sentence — of this language is. To do this in a nominalistically acceptable way, not only mention of sets has to be avoided, the sentences themselves also have to turn out not to be abstract entities. Goodman and Quine suggest to make sense of what a sentence is by identifying it with its concrete inscriptions. Marks on paper, in instance, can be said to fall under a predicate ‘Fmla’ (for well-formed formula). In order to give a definition of this predicate, they start out with primitive predicates that are true of concrete inscriptions if these have the familiar shape of the logical symbols, variables, etc., and then build up the language in a way analogous to the common recursive definition of a language that the platonist uses. To do so, they use a primitive three-place predicate ‘C’ which applies to token inscriptions. ‘C(x, y, z)’ expresses that x is a token inscription that is the concatenation of y and z . For convenience, we can define a four-place predicate ‘C(x, y, z, w)’ as ‘ $\exists t(C(x, y, t) \wedge C(t, z, w))$ ’, and analogously for five- and six-place predicates for concatenation (the number of terms following the ‘C’ will disambiguate which predicate it is).

But first things first. The primitive unary predicates we will be using are ‘Vee’, ‘UVee’, ‘Ac’, ‘LPar’, ‘RPar’, ‘Neg’, ‘Cond’, ‘UpsA’, which are true of physical objects if they have the shape of a lower case ‘ v ’, and upper case ‘ V ’ (for use as first- and second-order variables, respectively), an accent ‘ \prime ’, a left parenthesis ‘(’, a right parenthesis, ‘)’, a negation sign, ‘ \neg ’, a conditional sign, ‘ \supset ’, and an upside-down A, ‘ ∇ ’, respectively.¹⁷

Let a character, ‘Char’, be any of the things that the predicates above are true of Goodman and Quine (1947, p. 113):

$$\text{Char}(x) =_{df} \text{Vee}(x) \vee \text{UVee}(x) \vee \text{Ac}(x) \vee \text{LPar}(x) \vee \text{RPar}(x) \vee \\ \text{Neg}(x) \vee \text{Cond}(x) \vee \text{UpsA}(x).$$

And let an inscription, ‘Insc’, be either a character, or a concatenation (note that concatenation was introduced as applying only to inscriptions — fully explicit, a recursive definition would be in order):

$$\text{Insc}(x) =_{df} \text{Char}(x) \vee \exists y \exists z C(x, y, z).$$

The first thing we obviously need for the syntax is a sufficient supply of distinct variables. These can be formed out of lower- and upper-case vees, concatenated with strings of accents. For this, we can define a string of accents as

$$\text{AcString}(x) =_{df} \text{Insc}(x) \wedge \forall y ((\text{Part}(x, y) \wedge \text{Char}(y)) \supset \text{Ac}(y)).$$

¹⁷ Goodman and Quine are more economic in their choice: they replace ‘ \neg ’ and ‘ \supset ’ by the Sheffer stroke, ‘|’, for alternative denial, in terms of which the former two are definable; they also form the universal quantifier using parenthesis, ‘(v)’, in lieu of ‘ $\forall v$ ’, and do not have upper case variables. Since they aim to provide a syntax for first-order set theory, they have the additional ‘ ϵ ’ for membership. There might be concerns about left and right parentheses having the same shape, albeit rotated by 180°. To dissolve such worries, one could either appeal to the orientation of the inscription, or use a different shape for one of the parenthesis, say, ‘]’ instead of ‘)’. See Goodman and Quine (1947, p. 112).

(Again, a recursive definition would have been possible that takes one accent as the base case, and defines in the recursion step an accent-string to be any concatenation of accent strings.) Note that ‘Part’ is the the two-place predicate that is introduced and axiomatised in the class-free subsystem of the Calculus of Individuals Leonard and Goodman (1940).

First- and second-order variable, ‘FVbl’ and ‘SVbl’, respectively, can thus be defined:

$$\text{FVbl}(x) =_{df} \text{Vee}(x) \vee \exists y \exists z (\text{Vee}(y) \wedge \text{AcString}(z) \wedge C(x, y, z))$$

$$\text{SVbl}(x) =_{df} \text{UVee}(x) \vee \exists y \exists z (\text{UVee}(y) \wedge \text{AcString}(z) \wedge C(x, y, z)).$$

That is, any lower-case vee, possibly followed by a string of accents, is a first-order variable, and any upper-case vee, possibly followed by a string of accents, is a second-order variable.

Goodman and Quine go on to develop the syntax like this in a painstaking detail which we will not go into here. They define quantifiers, (in our case an upside-down A followed by a variable, orders distinguished by the order of the variable), atomic formulae, and formulae. They then inscriptionally set up some logical axioms, which we, of course, dispense with here. It follows the definition of a substitution, immediate consequence (a formula that can be arrive at by one application of a rule), that of a line (of a proof), and lastly a proof itself (as a list of lines all of which are immediate consequences of previous lines or axioms). Since we allow for assumptions, our construction first defines a derivation as a list of lines all of which are immediate consequences of previous lines or assumptions; a proof (of φ) will then be a derivation whose last line (φ) does not depend on any undischarged assumption. A theorem, finally, is the last line of a proof.

These sketchy remarks on the construction must here suffice as a hint on the actual construction. Goodman and Quine give their construction in full detail,¹⁸ and this is easily amended to suit our proposal here if our hints above are followed. Note that we have not included any constants in the language, neither names nor predicate constants. Thus, all our formulae so far contain only variables and logical constants (‘ \forall ’, ‘ \neg ’, ‘ \supset ’). Identity is standardly defined in second-order logic (by Leibniz’ Law), and other constants can easily be introduced into the construction of the language.¹⁹

4.2. *The Proof is Out There*

If the notion of proof thus defined only encompassed discernible marks on paper, the consequence relation defined with its help would be very restrictive. Goodman and Quine, indeed, suggest that instead we take inscriptions to be any appropriately formed portion of matter, whether it is against a contrasting background or not.

¹⁸ See Goodman and Quine (1947). Note that Goodman and Quine’s definition D10 is defective, but easily mended, as noted by Henkin in Henkin (1962, p. 192, fn. 3). See also Martin (1958).

¹⁹ Goodman and Quine mention in various passages of Goodman and Quine (1947) the problem they have in defining the ancestral. Leon Henkin Henkin (1962) provides a solution; Goodman later states Goodman (1972, p. 153) that the technique he developed himself in Goodman (1977, chapters IX and X) will serve the purpose. Since we assume that the version of second-order logic presented here is nominalistically acceptable, we can simply rely on Frege’s original definition of the ancestral in Frege (1879, §26).

Then the only syntactical description that will fail to have inscriptions answering to them will be those that describe inscriptions too long to fit into the whole spatio-temporally extended universe. This limitation is hardly likely to prove embarrassing. (If we ever should be handicapped by gaps in the proof of an inscription wanted as a theorem, however, we can strengthen our rules of inference to bridge such gaps; for, the number of steps required in a proof depends on the rules, and the rules we have adopted can be altered or supplemented considerably without violation of nominalistic standards.) (Goodman and Quine 1947, p. 121)

We suggest the amendment to allow to count as inscriptions any appropriately formed space-time region, whether it is occupied by matter or not. Let us also note that even employing more and more abbreviations, other useful definitions and rules other our primitive ones, in a finite universe, we will *eventually* run out of actual concrete inscriptions, no matter how generously construed. We thus suggest to side with Field Field (1980) and take it for granted that our universe is infinite, and, in fact, contains a continuum of space-time points (i.e., 2^{\aleph_0} -many); and we do not consider this as a violation of nominalism²⁰ — until further notice. We discuss the infinity of the universe in this and other respects in the next section.

This generous conception of an inscription might seem objectionable to some, at first glance, since it means that all proofs are already out there — and quite literally so. We literally discover proofs, that is, the space-time regions that are proofs, e.g. by outlining the proof-shaped regions with a pencil (note, however, that this is not the only way to learn that a proof exists). The initial feeling of offence will in most cases subside when it is pointed out that the situation is exactly analogous for the platonist who takes proofs to be abstract objects: types, for instance. These are also commonly assumed to exist independently of anyone finding them (e.g. by tokening them). The only difference is that the nominalist's proofs are concrete objects. In principle, it should thus also be possible to use nominalistic analogues of any way of demonstrating the existence of a proof that the platonist uses. Next to transcribing it, there is, for example, proving that the inscription must exist. (This proof will be an inscription again, but it need not be the proof whose existence is thus demonstrated.)

Rejecting the generous conception of an inscription, and thus not only denying an actual infinity of proofs, but also the existence of proofs that have not been written down, would mean to adopt a position even more radical than Goodman's nominalism. It seems that Stanisław Leśniewski embraced this very restrictive conception of proof see Simons (2002), which not only entails that proofs come into existence when they are first written down, but also that they cease to exist when the last inscription (narrowly construed) is destroyed. Irrespective of how appealing this position is, it does not appear that a criticism along these lines could coherently be put forward by the platonist.

In our infinite universe there are thus infinitely many concrete proof-inscriptions (understood in the generous way specified above). Moreover, there are enough in the sense that there are *all* the proofs that a platonistic version of inferentialism accounts for. Our explication of logical consequence is thus co-extensive with the platonistic inferentialist account of logical consequence.

²⁰ This, to be sure, has been contested; see, e.g., MacBride (1999).

5. *Is this Enough? (Or is it too much?)*

With the help of concatenation theory, an inferentialist conception of logical consequence, and speculations about the size of the actual universe, we arrived at an explication of logical consequence that might seem nominalistically acceptable. Whether it indeed *is* nominalistically acceptable is a question that does not allow for a straightforward answer. First of all, nominalists might disagree with one another about what resources in fact count as nominalistically acceptable.

Further, they might disagree on what an explication is supposed to do, and, in particular, disagree about the relation in which explicatum and explicandum must stand to each other, in order for an explication to be adequate.

Finally, they might disagree about the use they want to make of the explicatum in their theories. We mentioned two nominalist programmes above and said what role the notion of logical consequence plays in their projects. Below, we will address the question to what extent the explication of logical consequence suggested here can be used in these projects. Space constraints will not allow us to pursue all these questions in sufficient detail.

5.1. *A Ballet Dancing Brick Layer*

Let us first turn to the assumptions we made about the size of the universe. To be on the safe side, we assumed that our universe is large enough to contain the inscriptions of all proofs the platonist assumes to exist. But does the assumed size of the universe not sin against the nominalist's standards and put everything in jeopardy that we have achieved so far? Some anti-nominalists have questioned our assumption that space-time points could be considered nominalistically acceptable, since they believe that the presumed mark of the concrete (having causal powers) is merely metaphorically instantiated by space-time points e.g. Resnik (1985), MacBride (1999). We will not go into this discussion here, and simply assume that *if* a nominalist (like Hartry Field) finds space-times points ultimately acceptable, then so be it. But we want to claim that our explication of logical consequence would also be acceptable for Goodman. Would he have accepted an assumption of uncountably many points of physical space-time?

Remembering Goodman's early paper with Quine, one might think that he would not have accepted such an assumption. The project by Goodman and Quine was not only nominalist, it was also finitist:

We decline to assume that there are infinitely many objects. Not only is our own experience finite, but there is no general agreement among physicists that there are more than finitely many objects in all space-time. If in fact the concrete world is finite, acceptance of any theory that presupposes infinity would require us to assume that in addition to the concrete objects, finite in number, there are also abstract entities. (Goodman and Quine 1947, p. 106)

Goodman and Quine's project descended from their joint efforts with Alfred Tarski and Rudolf Carnap in the early 1940s to develop the foundations of arithmetic in a way that respects finitism (there is only a finite number of individuals), physicalism/reism (there are only physical things), and nominalism (there are only variables for individuals,

not for universals).²¹ The motivations for this project were already at that time rather heterogeneous. Tarski claimed not to understand languages that do not satisfy these conditions, thus citing an epistemic reason for these constraints. However, he apparently also thought that finitism is just a consequence of there being only finitely many objects in the world.

Carnap, on the other hand, shared Tarski's insistence on finitism only to a certain degree. Insofar as he was motivated to require finitism for the foundations of arithmetic, his motivation was empiricist: since each confirmation is based on finite observations, our knowledge is limited to the finite (independent of how many objects the world contains). But Carnap also claimed to understand infinite conceptions of arithmetic, making sense of them in terms of what we characterized above as modal eliminative structuralism:

It seems to me that I actually understand, in a certain degree, infinite arithmetic [...]. To the question of Tarski and Quine, how I interpret this, when the number of things is perhaps finite: I do not know exactly but perhaps through mere positions instead of things [...]. A position is an ordering possibility for a thing. I do not have the intuitive rejection of the concept of possibility as Tarski and Quine do. (Carnap's notes on the discussions with Tarski and Quine, RC-090-16-25, Carnap Archives Pittsburgh, as cited in (Mancosu 2005, p. 344))

As Goodman and Quine report in the beginning of their 1947 paper, the discussion in the early 40s did not lead to a final solution. Goodman and Quine had a new idea though how to address the problem. Instead of trying to formulate arithmetic with finitely many objects, platonist mathematics was simply treated as a meaningless language that did not require any interpretation in terms of acceptable objects. Instead, they went meta-mathematical: they devised a nominalistically acceptable way to speak about the way that platonist mathematics, considered as a mere "apparatus", can work. Since this way of doing meta-mathematics did not involve notions of arithmetic, the problem of interpreting the numbers as concrete objects, which had bothered Carnap and Tarski, did not reoccur for their proposal.

Finitism is however not fully unproblematic for an inscriptionist account. Above we assumed — to be on the safe side — that the universe should comprise an uncountable number of space-time points in order to allow for inscriptions of proofs of arbitrary length. Goodman and Quine believed that such an assumption is not needed, as quoted above, since stronger rules and auxiliary definitions could be introduced.

As we said above, it is not clear that this move can help in all cases. Many authors have pointed out, however, that the nominalist is free to chose other resources. Michael Resnik straightforwardly suggests that in order to develop a nominalistically acceptable meta-mathematics that is workable, the nominalist has to assume an infinite universe. A Fieldian conception of space-time would help providing one:

If we followed Field we would find it much easier to develop a nominalistic syntax than did Quine and Goodman, because we find an infinitude of inscriptions in his already posited continuous space-time. (Resnik 1983, p. 518)

²¹ Cf. Mancosu (2005).

Also Henkin (1962) admits that Goodman and Quine need not make any assumptions regarding finitism for their *nominalistic* project. Goodman, in particular, was prepared to divorce nominalism from finitism. In “A World of Individuals” (Goodman (1956), reprinted in Goodman (1972)) Goodman pointed out that nominalism is not logically connected to finitism:

The nominalist is unlikely to be a nonfinitist only in much the same way a bricklayer is unlikely to be a ballet dancer. The two things are at most incongruous, not incompatible. Obviously by the stated criterion for nominalism [essentially, the rejection of classes], some systems with infinite ontologies are nominalistic, and some systems with finite ontologies are platonistic. (Goodman 1972, p. 166)

And later, in *Problems and Projects* Goodman (1972) he clearly seems to be ready to give up finitism for the sake of nominalism, in response to Alonzo Church’s challenges to Goodman and Quine’s finitistic syntax:

In the first place, I should point out that this letter [by Alonzo Church, in which Church lists tasks that he thinks the finitist still has to accomplish] predated “A World of Individuals”, where nominalism is carefully distinguished from finitism. Our position in “Steps” was indeed finitistic as well as nominalistic; but finitism, although a friendly companion of nominalism, is neither identical with nor necessary to it. (Goodman 1972, p. 154)

We thus content ourselves with the fact that Goodman would have found the assumption of an infinite universe nominalistically acceptable.²²

However, there remains a problem: the assumption made about the size of the universe was introduced as an empirical hypothesis about the actual world. We usually assume such hypotheses to be *contingently* true, if true at all. This, however, appears to conflict with the very nature of logic. Logical consequence is usually assumed to be a matter of necessity. How can a contingent assumption serve as its foundation?

5.2. *The Size of the Universe and Logical Consequence*

Three problems need to be distinguished here. The first concerns a vague feeling, the second and third can be put forward in a precise way.

There might be a vague and uncomfortable feeling arising, given our explication of logical consequence, that the size of the universe, or the existence of some peculiarly shaped space-time regions, just should have nothing to do, generally speaking, with what follows *logically* from what. Vague worries are difficult to address, but here are some remarks which might help, at least, to get into the spirit.

First, a semi-technical point: the explication of logical consequence only indirectly depends on the size of the universe. The consequence relation is pinned down by the inferentialist proposal: what follows from what is determined by the inference rules. The trouble only arises in the *metatheory* when an explicit definition of logical consequence is asked for. There, logical consequence is defined as a certain relation. A relation requires relata, and for the nominalist nothing but concrete things can serve as such. The explication of a sentence, or, more generally, the provision of a nominalistically acceptable *syntax*, primarily involves the worrisome sentence tokens. But if infinitely

²² Compare also Goodman’s brief remark on Field in Goodman (1984, p. 53).

many sentences are indeed needed, and sentences are concrete objects, then there will have to be infinitely many concrete things.

Further, the nominalist might ask back what the opponent expected a nominalistically acceptable definition to look like. Obviously, it will only mention concrete entities, what else could it do? These are the only entities that nominalists allow themselves, after all. Note, however, that no cunning coding tricks are used in our proposal here which are employed elsewhere, in order to achieve arguably nominalistic reconstructions of mathematics. Let us emphasise that, rather than assuming a large enough ontology to allow for the interpretation of some mathematical notion,²³ the proposal here is in exact correspondence with the actual practice of proof (or, rather, the idealised version of it that is commonly assumed in the discussion of logical consequence). The ontology here assumed, concrete inscriptions of proofs, is precisely the gold standard of proof in logical and mathematical practice: the provision of a written down version of a proof to demonstrate that the inference holds. The explication of logical consequence presented here thus comes with an epistemology already attached. This was one of the motivations for the project in the first place.

The first precisely formulated problem concerns the counterfactual dependence of the extension of logical consequence given our proposal on the size of the universe. Let us here take for granted that the actual universe is infinite and thus big enough for the nominalist definition of logical consequence to be extensionally equivalent to the platonist inferentialist conception of logical consequence. The dependence on actual inscriptions makes logical consequence nevertheless counterfactually dependent on the size of the universe: suppose there is a possible finite universe and that there is some proof inscription that “uses up” all space-time in that universe. Say the last line of this proof, the consequence of the argument, is ψ , and one of the premises it rests on is φ , then it seems that the nominalist would be forced to say, that $\ulcorner \varphi \supset \psi \urcorner$ is *not* a logical consequence of the rest of the premises that ψ was originally derived from. The rule for \supset -introduction, also known as *conditional proof*, would of course licence the inference, but since we ran out of space-time, this inference cannot be drawn.²⁴ We would be forced to say that in *this* universe, $\ulcorner \varphi \supset \psi \urcorner$ is *not* a logical consequence of the premises, while in our (infinite) universe it is. But, surely, the size of the universe should not matter for the question, what follows from what.²⁵

Goodman, we think, would not have considered this objection to be seriously damaging. His meta-philosophical conception of explications would have counted the explication we arrived at as adequate, since extensional equivalence is sufficient for this purpose. Indeed, Goodman is famous for insisting that even co-extensionality is too strong a requirement for adequacy of explications Goodman (1977, pp. 3–22). Goodman’s weak requirements for adequate explications are certainly met by the account

²³ As, e.g., in Lewis (1991) or Niebergall (2005).

²⁴ This, in effect, amounts to Alonzo Church’s demand, that the inscriptional account suggested by Goodman and Quine would have to be able, *inter alia*, to sustain the deduction theorem. The letter in which Church raises this criticism is published in Goodman (1972, pp. 153–154), alongside with Goodman’s dismissive response.

²⁵ See Wilholt (2006, pp. 122–123) for a related worry.

proposed here. Thus we have at least an account of logical consequence before us that would have satisfied Goodman.

But there is the second problem which questions whether the the actual universe is, in fact, infinite and thus big enough for the nominalist definition of logical consequence to be extensionally equivalent to the platonist inferentialist conception of logical consequence.²⁶ Suppose the above derivation described again. If the universe turns out to be finite so that there is not enough space-time for an actual concrete derivation of $\lceil \varphi \supset \psi \rceil$. In this case, $\lceil \varphi \supset \psi \rceil$ is actually not a logical consequence of the relevant premises, despite the fact that the rule of conditional proof would licence the inference if only the universe was bigger.

Besides biting the bullet and admitting that $\lceil \varphi \supset \psi \rceil$ does, in this case, not actually follow from the relevant premises, we can see three strategies that nominalists of different temperaments might adopt to avoid this problem.

If we are happy to allow ourselves modal notions, we could amend the proposal to let logical consequence be a relation between *possible* concrete sentence tokens. The nominalist could point out that the modality required for this treatment does not fall foul of the common obscurity objections with which modal notions are traditionally attributed. No vague speculation or guesswork is required to work out how the relation will extend beyond the actual universe. Our schematic rules determine this extension precisely. A slightly bigger universe would contain no surprises as to what follows from what. We merely have more inscriptions there that look exactly the same, only that some are longer, and behave in exactly the same way as the actual inscriptions do.

The case would, indeed, be much like the following. Imagine a mathematician who runs out of paper while scribbling down a certain proof just before she can write down the conclusion of the proof (which is an application of conditional proof). It would be madness to claim that, owing to the lack of paper and the resulting lack of the last line, there is no telling how the concrete inscription of the proof would go on, or that it is obscure to say that it is *possible* to extend the proof. It is perfectly obvious how the proof would continue if she had another sheet of paper. It does not matter for this purpose whether she *actually* bothers to get another sheet. It would also make no difference if there happened to be no more paper in the universe at all, or, indeed, anything else to write on. Obviously, the conclusion follows, whether our mathematician finds another sheet of paper or not, whether there exists any more paper in the universe or nor, or whether there is enough space-time for the last line or not. But this unproblematic notion of possibility is all that is needed in the “modal” version of the explication of logical consequence.

Further, this strategy has a variant: instead of quantifying over possible sentence tokens in the definition of logical consequence, one could employ a *constructibility* operator akin to that which Charles Chihara introduced in his nominalistic programme Chihara (1990). Logical consequence would not be a relation between possible sentence tokens, but between *constructible* sentence tokens, where ‘constructible’ is obviously not to be

²⁶ Note that it would be sufficient if *any* aspect of the (concrete) physical universe were infinite for a similar construction to go through. Thus, the case described here involves that *nothing* physical is infinite or infinitely divisible: not just space, but also time, electromagnetic force, wave-length, gravity, etc.

analysed as ‘possible to construct’ — otherwise this variant would be no more than a complication of the modal strategy above. What is constructible, again, would have to take us beyond what actually can be constructed given the finite cardinality of the universe that we here assume.

Lastly, a *counterfactual* approach could be taken. Counterfactuals should again best not be taken as to be reduced to possibility in order to not merely complicate the modal proposal, and there are reasons to refrain from such attempts in any case.²⁷ If, for instance, a Stalnaker-Lewis-style analysis of counterfactuals was given, one would end up not only with a yet to be explained notion of possibility (or possible world), but also in want of an explication of the similarity (or closeness) relations which analyses of this ilk require in addition. The definition of logical consequence on this account would thus still be in terms of a relation between concrete sentence tokens; it would, however, state something like: a sentence φ is a *logical consequence* of some premises Γ if and only if, if the universe were large enough to contain the required inscriptions, there would be a derivation of φ from Γ whose single lines are either sentences of Γ , etc.

These three proposals contain notions — possibility, constructibility, counterfactual — which themselves are in want of explanation. If any of the three is adopted, one would have to let go of the hope to define it in terms of logical consequence, on pain of vicious circularity. This might seem a high price to pay. On the other hand, perhaps the techniques suggested here could be utilised for such explications. Attempting this is beyond the scope of this paper, but the beginning of an explication of possibility along the lines suggested here might be made using the inferentialist account of modality.²⁸

In what follows, we discuss these proposals in relation to the above mentioned nominalistic projects.

5.3. *Two Nominalist Programmes Revisited*

It seems that, although the notion of logical consequence is available to the nominalist, there will still remain open problems in the nominalistic programmes that we have introduced above. Let us first discuss the problem as it arises for fictionalism, and then turn to the problem that seems to remain for modal eliminative structuralism.

As we have said, Field wants to prove the conservativeness of mathematics, in order to show that mathematics is — despite being a useful tool for shortening derivations — dispensable for the derivation of physical consequences from physical theory. It seems obvious that Field cannot establish the relevant conservativeness result using the explication of logical consequence provided here. As Stewart Shapiro Shapiro (1983) has shown, one can formulate a Gödel sentence, G , in the nominalistic language, such that G is not provable in the nominalistic theory N , but provable (via Field’s bridge principles) in $N + S$, where S is, e.g., Zermelo-Fraenkel set theory. According to our analysis, mathematics is thus not conservative over nominalistic physics: there are logical consequences of $N + S$, formulated in the nominalistic language, that are not logical consequences of N , in the explicated sense of logical consequence.

²⁷ In fact, Timothy Williamson has recently suggest to do it the other way around: to analyse modal notions in terms of counterfactuals; see Williamson (2005).

²⁸ See for instance Prawitz (1965) and Read (2008).

For Hellman's project, things do not seem to be much better. First of all, explicating the notion of possibility that modal eliminative structuralism assumes in terms of logical consequence won't help if modal notions are essential for the explication of logical consequence (cf. the previous section). By this variant of the explication we would be led back in a full circle.

Taking either the counterfactual option or the proposal involving the notion of constructibility, the circularity worry does not arise (at least not initially), but what we have achieved is to patch up an account that was found wanting because it used an unexplained notion of logical possibility by using a further unexplained notion. This is little progress, if progress at all.

As we indicated above, if the nominalist accepts Goodmanian standards for the adequacy of explications, this problem might not arise, since in that case no notion of possibility needs to be assumed for the explication of logical consequence. However, the rich actual system of space-time could directly be put to use as an instantiation of all structures that a structuralist should²⁹ be interested in. If there is no reason to be a modal eliminativist in the case of logical consequence, there surely does not seem to be such a reason in the case of mathematics. Thus, while logical consequence on this variant of the explication might help the structuralist nominalist, it at the same time seems wholly superfluous.

A platonist might also sense a problem if — as in the case of Hellman's original proposal — the logic is assumed to be second-order. In this case, there will be a sentence, G , formulated exclusively in (second-order) logical vocabulary, such that G is not a logical truth of second-order logic, but a logical truth of third-order logic.³⁰ 'Logical truth' is here used in the inferentialist sense, such that something is a logical truth if it is a logical consequence of the empty set of premises, i.e. (in nominalistic terms) if there is a derivation-inscription of an inscription of that sentence that accords to the rules and contains no undischarged assumptions. We can call this the *non-conservativeness* of third-over second-order logic.³¹

The objection might be formulated thus: although G is not a logical truth (of second-order logic), it is nevertheless not the case that $\Diamond\neg G$, since $\neg G$ is ruled out by third-order logic. G is a theorem of third-order logic, and thus $\Box G$, i.e. $\neg\Diamond\neg G$.

It seems, however, that this problem might be surmountable with an amendment in the notion of possibility. There are two options that come to mind. First, one could be a relativist about logical possibility, second, one could be a minimalist about this notion.

According to relativism, $\neg G$ is not logically impossible *simpliciter*, but logically possible relative to second-order logic and logically impossible relative to third-order logic,

²⁹ If we assume that the structuralist is interested in making sense of mathematics as it is put to use in the sciences.

³⁰ Third-order logic is the next step up in an infinite hierarchy of n -th order logics, that is in many ways similar to the hierarchy of simple type theory. Second-level predicates are introduced to apply to the "ordinary" predicates of first- and second-order logic which are now called first-level predicates. Third-order variables stand in the place of second-level predicates in the same way that second-order variables stand in the place of first-level, i.e. ordinary predicates. Third-order quantifiers bind these.

³¹ For a proof and discussion of the non-conservativeness of third-over second-order logic see Rossberg (2006); the proof is based on a result of Leivant (1994, §3.7).

because G is a theorem of third-order logic, but no theorem of second-order logic. Since G contains only second-order vocabulary, this move seems only promising if we assume that logical consequence and logical truth (and *a fortiori* the “meaning” of a sentence containing logical vocabulary), also in case of sentences that use only a restricted selection of logical vocabulary, is always determined “holistically” by all introduction and elimination rules of a given logic. Since G is a sentence of two different logics (second-order and third-order logic), it thus can be a logical truth in one, while failing to be a logical truth in the other.

One might also attempt to surmount the problem by being a minimalist about logical possibility, such that some sentence S counts as a logical possibility *simpliciter* only if there is no logic in the hierarchy of (n -th)-order logics (or maybe an even wider range of logics), such that $\neg S$ would be a logical truth in that logic (and, accordingly, some sentence T is a logical truth *simpliciter* if it is a logical truth of some logic). This paper is not the place to discuss these options in any detail, but note that Crispin Wright suggests, for related reasons, introducing the quantifiers of all orders up to ω (and possibly beyond) at once.³²

That we have no better news for nominalistic programmes hardly is the fault of the notion of logical consequence. If logical consequence in a nominalistically acceptable conception is less “powerful” than the platonist notion, then this merely means that there is work yet to be done for the nominalist, but not that they should resign themselves to relying on platonistic smoke-screens.

If logical consequence turns out to be insufficient to explicate some notion that a particular nominalist project requires, then the task will have to be done with the assistance of other nominalistically acceptable means. Using, on the other hand, a notion of logical possibility as an inexplicable primitive seems to have the same advantages as theft over honest toil.³³

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³² Wright (2007), but see also Rossberg (2006) for a problem with this proposal.

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Nominalism and History

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The paper focuses on Nominalism in history, its application, and its historiographical implications. By engaging with recent scholarship as well as classic works, a survey of Nominalism's role in the discipline of history is made; such examination is timely, since it has been done but scantily in a purely historical context. In the light of recent theoretical works, which often display aporias over the nature and method of historical enquiry, the paper offers new considerations on historical theory, which in the author's view may solve some of the contradictions that have surfaced in recent times. The Nominalistic stance is argued against by disputing theorists such as Paul Veyne, who has made a case for Nominalism in history. A brief philosophical section introduces Nominalism in its metaphysical dimension and the discussion is speedily brought to its significance for history. The paper also proposes a solution to the misconstrued yet too often vague application of scientism in history, and offers theoretical grounds that might solve some of the 'stormy grounds' historiography finds itself today. Articles by Marcel Gauchet and *History and Theory's* Anton Froeyman and Bert Leuridan are engaged with, as well as Murray Murphy's books on the philosophy of history. Works by Georg Gadamer, Marc Bloch, Benedetto Croce, Hyppolite Taine, and Anthony Grafton crucially inform the discussion and brace the consequential conclusion.

Keywords: Nominalism; Particulars & Universals in History; Nomothetic History; Empiricism; Hermeneutics; History as Humanity vs Science; Historical Narrative

Introduction

The role of Nominalism in history has been seldom discussed, seldom considered, and even more seldom analyzed. Surely if asked, any historian would tender an opinion on the Nominalist/Realist antithesis, and favor one persuasion over the other as it relates their convictions about what is sound historical methodology. But most, I venture to say, would recognize that they have not given deep thought about how this crucial philosophical argument applies to their historical thinking. The dispute between Realists and Nominalists has raged in metaphysics for over two millennia—and is still the source of animated debates—but in historical circles the argument, save a few instances, has not been confronted directly on its own, diametrical terms. The aim of this essay is to present the reader with an assessment of the role of Nominalism in history, which, as far as I know, has not yet been wholly laid out for an outright historical readership. Although I shall present the various pertinent sources and their theories in order for readers to form an opinion of their own, I should like to point out that this essay's objective is to discredit the idea that strict Nominalism alone be an apposite stance in conceiving history. Still, I believe particulars to be the cornerstone for historical understanding; and yet, I am also convinced that historians who ignore universals and exclusively scrutinize particulars will find their work wanting of characteristics, which if overlooked, shall fatally compromise their historical apprehension. In other words, I wish to show that though particulars have a vital place in history—and we shall see why—Nominalism is epistemologically deficient,

especially in history, for universals are an inalienable aspect of human understanding, and thus are essential for a thorough conception of history and a comprehensive historical grasp: history's singularly extensive compass requires a broad vision that accepts both universals and particulars.

Robert Hume appositely stated that, "[t]he philosophy of history has long been a stormy ground, and it will probably remain so." (Hume, 1999: p. 13) In fact, philosophers of history are continuously examining and arguing over the ontology of history; its fractious, hybrid nature is ultimately an endless source of speculation and fervid discussion. The main point of contention—which is also fundamental for our analysis—seems to me the question of whether history is a science or a humanistic pursuit, and, if it is the latter, how and in what way does it differ from other humanistic disciplines, due to its para-scientific slant. While it is not the task of this essay—provisionally—to enter this acerbic dispute, our study of Nominalism and history must necessarily include a discussion of history as science and history as a humanity, because those who favor the Nominalistic stance tend toward the persuasion of history as a form of empirical knowledge, and thus view it through scientific lenses; on the other hand, Realists envision history as a discipline under the aegis of the humanities, whose epistemic tools are hermeneutics rather than Method.¹

I will show that despite the quagmire of opinions, currents, and theories, whether one regards history as a science or as a

¹As has been shown by Gadamer in *Truth and Method*. Gadamer, however, did not write about Nominalism at all.

humanistic pursuit strict Nominalism is fatally reductive to either conviction: it is in serious philosophical tension with the former and irreconcilable with the latter.

Since Nominalism and Realism's connection to history can best be judged with a clear understanding of their more abstract, philosophical perspective, I will first present the Nominalist/Realist antithesis in its purely philosophical dimension; this should provide the necessary understanding when the argument is applied to history. Next, I will cover the few, meaningful sources that pertain to our discussion; these shall be from different philosophies of history, which examine the question on a purely historiographical basis. I have already stated that the sources dealing directly with Nominalism and history are scanty: philosophers of history who labor either for the scientific or the humanistic view often do not confront Nominalism and Realism directly, and thus some degree of inference shall be required to locate their stance from their arguments.² I will then cover the most heated and productive dispute—that between Paul Veyne and Marcel Gauchet, over the legitimacy of historical Nominalism—which is the only modern debate directly centered on the philosophical and historiographical positions we are examining. Lastly, I will both attempt to present what seems to me the most sensible solution to the tendentious, Nominalist/Realist polemic and provide a sound argument against these unnecessarily polarized tenets and their role in history. I hope to provide at the very least, a certain degree of mental order—of food for thought—rarely furnished to the practicing historian, on the relation of Nominalism and history.

A Brief Philosophical Preparation

It is a well-known fact that the first to posit the theory of universals was Plato. This epistemological theory is of cardinal importance: it has engaged metaphysical speculation since its inception; its claims have been as fecund as any great question in philosophy. In fact, whether we know the world through its intelligible manifestations, through direct perception, or, whether we only really know the world through Forms, the unintelligible, is really the marrow of epistemology. Throughout his dialogues, Plato continuously alludes to what he refers to as "Forms". The *Republic* is the classic, most quoted example: "shall we proceed as usual and begin by assuming the existence of a single nature or Form for every set of things which we call by the same name?" (Plato, 1961: p. 820) These words, pronounced by Socrates in the dialogue, are as ambiguous as they are famous. David Armstrong, who has devoted the most comprehensive modern survey of Nominalism and Realism in his two-volume work, *Universals & Scientific Realism*, points out the ambiguity of this passage: "But is Plato here arguing that the Form is required for the name to be meaningful? That is the way in which he is often interpreted. However, it is at least as plausible to suggest that the underlying argument is that sameness of name requires sameness of nature in the things named." (Armstrong, 1978: Vol. 1, p. 98) Despite the ambiguity of the act of naming things³ in the statement quoted from *Republic*—whether sameness of name requires sameness of nature in

the things named or whether Forms are a prerequisite for the ability to name things—particulars are understood by Plato to be a subset of Forms ("the existence of a *single* nature or Form for every *set* of things which we call by the same name"). In *Parmenides*, Plato is more specific in detailing the discrete existence of Forms: "Do you believe that there is such a thing as likeness itself apart from the likeness that we possess? Certainly I do, said Socrates." (Plato, 1961: p. 924) Here Plato is positing that universals and particulars exist as separate entities.

The Platonic theory of Forms is very complex due to the allusive, almost epigrammatic way which Plato scatters his dialogues with his references to them; Plato's idea of universals is open to a number of interpretations, which may lead to rather different conclusions. It is not our concern here to examine the theory of universals in all its metaphysical ramifications, but to lay the basic philosophical principles which will be engaged when discussing Nominalism and Realism and their role in historical perception; the reader must merely be made aware of the choices offered by this vital epistemological dilemma: does our knowledge stem exclusively through our direct perception of particulars or as an emanation of Forms? Do we accept or reject the existence of universals? This is the question which has fomented endless discussion—and in some cases derision: Wittgenstein famously claimed this problem to be a non-issue.

But let us now define with as much clarity as possible what Nominalists and Realists believe. I shall call upon David Armstrong's definition for both terms:

There are those philosophers who hold that when we say truly that two tokens are of the same type, then sameness is to be understood in terms of strict identity. The two different tokens have something strictly identical. [...] If, for instance, two different things have the same color, then this must be taken strictly. One and the same thing, the color, is a constituent of the two things. Historically, these philosophers are called **Realists** and are said to believe in the reality of universals.

On the other side there are philosophers who think that when we say that a number of tokens are all of the same type, then all that we are saying is that the different tokens are non-overlapping parts of some larger whole or unity (the tokens are all member of one class, or they all resemble each other in a certain way, or some other such formula). The sameness of the tokens is only loose and popular.

These philosophers hold with John Locke, that "all things that exist are only particulars". There are no strict identities reaching across different tokens; there are no universals. Philosophers who hold such a view are traditionally called **Nominalists**. (Armstrong, 1989: p. 5)

In other words, Nominalism is the rejection of universals, while Realism is the belief in their existence. There are various forms of Nominalism—Concept Nominalism, Class Nominalism, Resemblance Nominalism, which all fall under the so-called heading of Predicate Nominalism—as well as various kinds of Realism: Immanent Realism and Scientific Realism. These distinctions are immaterial to our discussion and I shall relegate them to broader, strictly metaphysical discussions. It is now time to speak of the concepts outlined above in relation to history and see how they affect our conceiving history.

²Such inferences shall not be unmotivated and neither discretionary nor arbitrary.

³Plato dedicated his *Cratylus* to discussing the implications of naming things (especially at the end of the dialogue from sections 438 to 440), man as a name-giving creature, and language's relationship to truth.

Particulars and History

The first to proclaim that historians dealt above all with particulars, or “singulars”, as he referred to them, was Aristotle. In *Poetics* Aristotle distinguished the historian from the poet as follows:

The distinction between historian and poet is not in the one writing prose and the other verse—you might put the work of Herodotus into verse, and it would still be a species of history; it consists really in this, that the one describes the thing that has been, and the other a kind of thing that might be. Hence poetry is something more philosophic and of graver import than history, since its statements are of the nature rather of universals; the statements of history are singulars. (Aristotle, 1946: p. 1451b)

Aristotle thus inaugurated the Nominalist viewpoint in the discipline of history. This persuasion had enough thrust to persevere until today and has been the source of many fruitful debates in historiography, though most debates are not always conscious of the Nominalism/Realism antithesis at their root nor its implications when applied to the discipline of history; furthermore, the friction between universals and particulars is aggravated, when, as historians, we apply the indefeasible element of our discipline—temporality—in our consideration. For example, when we say that, “a certain person whom we saw today is the very same person that we saw yesterday [...] does that mean that the person today and the person yesterday are actually identical?” (Armstrong, 1989: p. 3) Thus we can see how temporality complicates particulars and their perception, for we can say with some confidence that the person yesterday is the same as the person today. But only loosely. Strictly speaking, “they are different temporal parts of a single four-dimensional entity, the person.” (Armstrong, 1989: p. 4) This ties itself to a principle—controversial in philosophy—called the Identity of Indiscernibles, which holds that “if a and b have all their properties in common, then a is identical with b. In other words, sameness of properties gives sameness of thing.” (Armstrong, 1989: p. 3) Universals, too, are affected by this principle, for in order to recognize them—just as we do particulars—we must apply the principle of Identity of Indiscernibles. In history, this involves the recognition of sameness through time for universals—if we believe in them, of course. But since history is the study of the *change* of human practices, particulars clearly bear greater significance for the historian, for it is by analyzing particulars that we can readily recognize change: temporality makes particulars dependent on their temporary instant, and therefore significant inasmuch as they reveal the historical moment we are examining. On the other hand, in the animated debate between Paul Veyne and Marcel Gauchet, we shall see that universals are not to be discounted. The dispute was centered on particulars—how, why, and to what extent they are meaningful in historical inquiry. In examining their claims, I think what should transpire is that particulars alone and the refutation of universals—Nominalism—severely limits the historian’s gaze.

Paul Veyne and the Chimera of the Nominalist Historian

Paul Veyne is the modern historian who wrote explicitly about Nominalism and history, and who argued for a Nominal-

ist outlook. He wrote about it in his theoretical writings on history. His first book, *Writing History*, is full of driving ideas about historical methodology as well as fruitful considerations about the ways a historian crafts his work. Though throughout the book Veyne offers a number of stimulating insights, he often stumbles in contradictions that mar the coherence of his thought: Veyne’s view of history is staunchly Nominalist, yet his statements are often incongruous with the implications of Nominalism. Very early he states that, “nothing is more reasonable than a Nominalist conception of history” (Veyne, 1984: p. 43) and explains the legitimacy of this position by stating that “we know historical types do not exist in themselves, that events are not reproduced with the constancy of living species, that the typical in history is a choice [...] in short, the types are infinite in number, since they exist only through us. Once again, we have to come to the conclusion of historical Nominalism.” (Veyne, 1984: p. 121) Veyne’s argument is essentially that the historian ought to look to particulars and reject universals—which here he calls “historical types”—since only the former can reveal the historical moment in its uniqueness, thus implying that the belief in universals hampers a historian’s understanding. In his most well known essay, *Foucault Revolutionizes History*, Veyne further expanded this idea:

In short, in any given era the set of practices gives rise, on a given material point, to a unique historical countenance in which we think we recognize what is called, in vague terms, historical science or religion; but what takes shape at that same point in another era will have its own unique and very different countenance and, conversely, a countenance vaguely similar to the earlier one will take shape at a some other point. This is what denying the existence of natural objects means: across the ages we do not encounter the evolution or modification of a single object that always appears in the same place. (Veyne, 1997: p. 171)

Again, Veyne makes a powerful and convincing case for the uniqueness of any historical moment, but he is less persuasive when he argues that across the ages we never encounter the same things. A few pages later, Veyne drives this point further: “there is no concrete trans-historical truth.” (Veyne, 1997: p. 174)

Marcel Gauchet fulminated Veyne for his extreme Nominalist position. In an article called *Le nominalisme historien. A propos de “Foucault révolutionne l’histoire” de Paul Veyne*, Gauchet faulted Veyne for the stringent Nominalism he displayed in his essay on Foucault, and claimed Veyne’s ideas to be the result of a naïve skepticism “scepticisme naïf” (Gauchet, 1984: p. 409), reminiscent of a second-degree scientism that could never allow authentic historicity. For Gauchet, the Nominalist epistemology is a “nullifying” philosophy: “[with his] strange epistemology, no time is seriously considered by Veyne to historical conditions of formation of this knowledge of historical fact according to standards of accuracy. This generalized genealogy excludes but one genealogy: *its own*. In other words, everything is historical, except history.” (Gauchet, 1986: p. 407)⁴ Furthermore, according to Gauchet, Veyne is ignorant of the foundations of historical methodology. He refers

⁴Or, étrange épistémologie, à aucun moment il n’est sérieusement réfléchi chez Veyne aux conditions historiques de constitution de cette connaissance du fait historique selon des normes d’exactitude. Cette vision généalogique généralisée n’exclut qu’une généalogie: la sienne propre. Tout est hiostorique, en somme, sauf l’histoire.

to Veyne's "authentic misunderstanding" citing Veyne's claim that historians of antiquity and the Middle Ages were a-critical, because they built their histories upon their predecessors'. According to Gauchet, this view is summary and erroneous, since Gauchet points out that historians before historicism were indeed critical⁵ but were so with completely different criteria from our own. For Gauchet, Veyne's ignorance creates a fatal blind spot in Veyne's historical epistemology; it is that spot, which accounts for the Nominalist's fortune.

The main task which an authentic epistemology of today must provide: dissolve the sophisms that naturally result in this renewed version of universal mobility. The fact that everything is historical does not mean that everything is relative, that history is made of nothing but radical heterogeneities and woven by singular, incomparable emergences. But it is precisely this challenge, that all is historical, that must be met. What does it mean, exactly? That death, tears, childhood, dreams, sexuality, folly are historical in their essence and not like natural objects always identical with themselves; but what does that truly mean? Since there is uncertainty about this point, skepticism and historical Nominalism arise and thrive. (Gauchet, 1986: p. 406)⁶

However Gauchet did explain particulars' own, rightful place in conceiving history, and wrote about it exemplarily: "History [is] the *emergence, the advent* of forms than cannot be *explained*, except by missing what matters in them, that is, what these forms have that is *incomparable*. From this comes the necessity of the historian's Nominalism, the only position that can adequately open him to the *inexplicable singularities* of a process of permanent innovation." (Gauchet, 1986: p. 403)⁷

Paying close attention to particulars to make the historian "open to the singularities" of a period is a process which we shall examine in its metaphysical dimension; but after Gauchet's sensible statement we have established particulars' exigency and that universals alone are insufficient for conceiving history. For example, if we consider the idea of "the State" we surely cannot find much in common among the Roman State in the first century AD, the State under Louis XIV, and, say, the bureaucratic Napoleonic State.⁸ Clearly, if we were to look exclusively through the lens of forms we'd make historically insignificant statements. (However, further on I shall present a more sophisticated concept of universals that discounts Nominalism in every epistemological maneuver.)

⁵He is quite right in saying so: see Nadel, 1964.

⁶[...] la tâche prioritaire que doit se proposer une authentique épistémologie historique aujourd'hui: dissoudre les sophismes qui paraissent naturellement découler de cette version renouvelée de l'universelle mobilité. Le fait que tout est historique ne signifie aucunement que tout est relatif, que l'histoire n'est fait que d'hétérogénéités radicales et tissé que des surgissements incomparables. Mais c'est très précisément à ce défi du *tout est historique* qu'il s'agit de répondre. Qu'est-ce au juste que cela veut dire? Que la mort, les larmes, l'enfance, la rêve, la sexualité, la folie soient d'essence historique et non pas autant d'objets naturels toujours identiques à eux-mêmes, qu'est-ce que cela véritablement signifie? Car c'est de l'incertitude sur ce point que naissent et prospèrent le scepticisme et le nominalisme historiques.

⁷L'histoire qui est le *surgissement, l'avènement* de formes qui ne saurient s'*expliquer*, sauf à manquer ce qui compte en elles, à savoir ce que elles comportent d'incomparable. D'où le nécessaire nominalisme de l'historien, seul à même de l'ouvrir adéquatement aux *singularités inexplicables* d'un processus d'innovation permanente.

⁸The example is Gauchet's.

Gauchet thus expounded brilliantly on the need to concentrate on particulars in historical enquiry. But the benefits of a composed Nominalism in conceiving history surely are not new: "Herder set a universal historical worldview against the Enlightenment's teleological view of history [...] to acknowledge that each period has its own right to exist, in its own perfection." (Gadamer, 2004: p. 198) I think it should now be evident that particulars do have a fundamental role in our understanding of a historical moment.

And yet, as Fustel de Coulanges said, "History is not the accumulation of facts and events of every sort that have been produced in the past: it is the science of human societies" (Bloch, 2005: p. 71)⁹ and as such, one must be aware that, as we saw above, contrary to Veyne's stating the contrary, "trans-historical truths" exist, because there *are* constants in human nature—vanity, rapacity, the wish for a better position in society, love, lust, etc. And the historian who disregards timeless human traits inevitably shall not set them against the period he is studying—which is of course exemplary and unrepeatable—thus finding his compass of vision considerably diminished by such heedlessness. Furthermore, Veyne's negation of trans-historical truths presents another, perhaps greater problem, especially for historians: a generation inherits certain beliefs and practices from a previous one; as that transference occurs, these beliefs and practices gradually change. By denying these constants the effects of temporality on man are ignored. That is nonsensical for an historian, whose charge is to be a most sensitive needle on the scale of change over time.

Let us remember Dilthey's precept that "we can explain things but we understand men". This important distinction shoulders us to what is probably the most insightful quote from Marc Bloch's *The Historian's Craft*, that undisputed masterpiece of twentieth-century historical methodology: "If men, who are the object of our study, fail to understand us, how can we feel that we have accomplished more than half our mission?" (Bloch, 1953: p. 86-87) Therefore, if we be understanding of men, how could we discount universals, which inexorably constitute his nature? And furthermore, the rightful insistence that these two great historians placed on *understanding over explanation* is really a charge that explanation in history has scientific inclinations, either by virtue of an unobtainable law-covering model, or by impossible empiricism for the causal explanations of events. And so, a crucial aporia rises before us here: a choice must be made between hermeneutic and scientific knowledge, for a hermeneut *cannot* be an empiricist. This was demonstrated most powerfully by Gadamer in *Truth and Method*: Gadamer revealed hermeneutics to be an ulterior form of knowledge bearing truth *outside* Method—the indefeasible foundation of natural science.

With this in mind, Veyne's contradictions begin to emerge, and they reveal to be problematic. Two contradictions in Veyne's *Writing History*, which diminish the efficacy of his theory considerably, are most pertinent to our discussion. The first is his statement that "[t]he historical explanation is not nomologic, it is causal; as causal, it contains something general". Is Veyne flirting here with Realism ("it contains something general") and contradicting his purported Nominalism

⁹La storia non è l'accumulazione degli avvenimenti d'ogni tipo che si sono prodotti nel passato: essa è la scienza delle società umane.

Marc Bloch cited de Coulanges's affirmation in his last, scattered papers on history, written just before being shot in 1944 by the Gestapo.

with the irruption of universals in historical explanation? Secondly, Veyne, argues that “history is not a science.” (Veyne, 1884: p. 144) But Veyne, in his essay on Foucault, praised him for his empiricism—for statements such as, “history becomes an empirical science of events and that radical mode of being that prescribes their destiny to all empirical beings, to those particular beings that we are” (Foucault, 1994: p. 219)—which according to Veyne made Foucault a better kind of historian. We have already seen the ontological tension that arises from these two positions standing side by side, for it is impossible for them to be bridged in any way.

Even in his most defensible apology for Nominalism Veyne finds himself hampered by this single-handed theoretical concern: “[...] historical Nominalism, the vague character of sub-ordinary causality, makes it that no order of causes constantly imposes itself as more decisive than the others.” (Veyne, 1984: p. 280) I think Veyne’s insistence on Nominalism, is the manifestation of a natural and widespread fear among historians—the fear of being faulted for not being sufficiently analytical. But analytical acumen is but one ingredient that makes a great historian. And so, Gauchet’s charge of Veyne’s relative ignorance of historical methodology seems correct to me: Veyne’s statement above, which upon its first reading seems sound, is really only valid for attempts at causal explanations of historical events, but it is utterly useless for broader notions of history, which as I suggested above, must also understand man, and gauge with great accuracy the change in human practices. It is worthwhile here to remember Vico’s celebrated dictum that history “discloses the realm of culture, not nature”: and so, the historian who handles culture must take into account human temperament, and the latter—it should be stressed again—has universals and forms that ought not to be dismissed. In addition, Vico’s statement is an excellent refutation—on its own—for using natural science’s practices (contra Veyne and his empirical stance).

Despite my quibbles with Veyne’s theoretical writing, I should like to point out that when he actually writes history, Veyne is a great historian, and practices history in the most integral fashion. But in his theoretical writings there is an underlying philosophical uneasiness, which stands in the way of his speculations. However, this should not diminish Veyne’s accomplishments in our eyes, since often people do something very well even if the theory they use to explain what they do is flawed.

Nominalism’s Attempts at a View of History Conforming to Scientific Knowledge

An interesting yet defective attempt for a philosophy of history analogous to science and that brims with sharp Nominalistic positions is offered by Murray Murphy. In his book, *Philosophical Foundations of Historical Knowledge*, Murphy seeks rather hopelessly to reconcile the theories of historical causal explanation put forth by Carl Hempel and Paul Oppenheim in their Deductive-Nomological model for scientific explanation—with which they sought to explain any given historical event with a series of “logically deductive premises” (Murphy, 1994: p. 98)—and the concept of “culture”, which according to Murphy must also be considered as an explanation for human actions. Murphy casts eight propositions about historical knowledge, which he believes to be verifiable; the last is also the thorniest, for it states that “human actions are causally explain-

able.” (Murphy, 1994: p. x)

For human actions to be causally explainable, Murphy upholds to the so-called “covering law”:

This [...] model of explanation [is] from the metaphorical idea that the general law “covers” the particular case. It involves certain presuppositions that should be noted. One is that all laws are general, that is, the law cannot contain any reference to a particular. This was seen as necessary to rule out “general” statements such as “All chairs in this room are made of wood”. For the same reason, Hempel and Oppenheim stipulated that laws could contain only purely qualitative properties, so that properties referring to particulars (e.g., “earthly”) are proscribed. (Murphy, 1994: p. 98)

At first this may seem to be a Realist position, since laws evince some form of ‘generality’ and are meant to be universal. But Nominalists hold that only physical particulars in space and time exist, and that universals, which do not, are at best subsequent to particular things; therefore general laws are brought into being by particulars, and, in the case of history, laws would provide predictability, which of course is unfeasible. Robert Hume, who, in his excellent book, *Reconstructing Contexts: The Aims and Principles of Archaeo-Historicism* labels himself an empiricist, attacks Murphy’s view by saying, “I think Murphy is overstating his case. To say that human action has causes is one thing; to say that we can identify them is something else” (Hume, 1999: p. 15). The last clause is apodictic: it is disarmingly obvious that every historian who has attempted to predict the future by using causal explanations for past events to forge laws for events, such as, say, revolutions, has always failed. Obviously, the covering law refutes universals categorically and places Murphy firmly in the Nominalist camp; Murphy accepts the covering law as an explanation for causality of events as well as action in history: “I believe there is no real doubt that the covering law model provides an explanation for an action [...]” (Murphy, 1994: p. 155). Hume expounds on the reasons for the mania of giving history a scientific footing to give it epistemological certainty as well as the deriving distortions of such attempts brilliantly.

Historians have long suffered from a dangerous hankering to be as precise and rigorous as physicists, and more than half a century ago history took a terrible turn when Hempel published “The Function of General Laws in History”. Historians spent the next thirty years trying to get out from under the demands that follow from Hempel’s attempt to impose on history the logical structure of explanation he found in physics. The gist of the “covering law model” is simplicity itself: explanation can be achieved “by subsuming what is to be explained under a general law”.

In the cold aftermath of repentance at leisure, this is manifestly a lunatic idea. If history has general “laws”, they are not of the sort to be found in classical physics. Physical science attempts to deal with something more or less available in the present; history attempts to explain the past now unrecapturable except via extrapolation from traces. The degree to which billiard balls can be used to explain human behaviour is evidently limited. More than a century ago Dilthey rightly distinguished between physical science (concerned with causal explanation of present

phenomena) and history (concerned with comprehension of a vanished past). (Hume, 1999: pp. 15-16)

That a self-proclaimed empiricist like Robert Hume states unequivocally the fatal pitfalls of scientism applied to history is significant; and lunacy is indeed what incites a statement by Michael Scriven, which Murphy quotes in defense of his eighth proposition, which as we saw above states that “human actions are causally explainable”, and which Scriven drives to its paroxistic locus: “causality is the most important explanatory function in history.” (Murphy, 1994: p. 102) Now I should like to know which historian, in Scriven’s or Murphy’s view, has fully *explained* the cause(s) of, say, the French Revolution, which is about the most written event in history. Can an arsenal of “scientists” redact a nomological system for the Revolution? Compared to these desiccated attempts, Hyppolite Taine’s overemphasized and infinitely figurative concept of “l’esprit classique” is indeed a tonic—for the *comprehension* of the spirit which animated the Revolution:

[Taine’s] thesis is that the philosophy of the eighteenth century was the product of the “classic spirit”, which was invented by Descartes and the essence of which was to pursue the absolute, and worship uniformity. When the French mind turned to politics it proceeded to prescribe according to the dictates of pure reason. This neglect of the individual, the concrete, the real, was the mark alike in literature, of the Philosophes and of the Revolution, and its predominance was the main cause of the tragedies of modern France. (Gooch, 1952: p. 228)¹⁰

Taine, like most historians of the nineteenth century, believed that history had both scientific and literary claims, which lent his history to a number of critical approaches. In his great work, *Les Origines de la France Contemporaine*, Taine had mastered the lessons of the august German school of history of the nineteenth century—that great efflorescence, which had produced unparalleled works written by men who engaged in the rigorous praxis of basing their histories on primary sources, as well as the necessity of understanding the reasons behind the actions of men. Accordingly, Taine employed a monumental archival knowledge with an almost unique, insightful psychological understanding; his great work is thus at the same time political philosophy, psychological history, social ethics, and, owing to its unique literary focus, literary criticism as well. For Taine, history was both an art and a science; his concept of the “esprit classique” sought—as Dilthey urged—also to *comprehend*, not just to *explain*. Bloch’s reiteration of this is thus noteworthy: “This faculty of understanding the living is, in very truth, the master quality of the historian.” (Bloch, 1953: p. 43)

The rich idea of “l’esprit classique” opposes Veyne’s much vaunted idea that Foucault’s merit—and supposed superiority—was that he was both an empiricist and a profoundly skeptical thinker “who believed only in the truth of facts [...] never in the truth of ideas” (Veyne, 2010: p. 1)¹¹ and that thanks to this supposedly sharpened, empiricist gaze, he managed to “peel away the banalities and notice that there is more to ex-

plain” (Veyne, 1997: p. 156) than what was previously understood about a period. But in arguing that Foucault revolutionized history Veyne forced the issue.¹² Again, the example of the French Revolution stands before us: if we are to explain it through facts, such as, among others, the failure of France to reflect “the change of the distribution of property and wealth [that] ceased to be the prerogative of a few” (Acton, 2000: p. 1), or the 1788 drought, we shall find that facts are not at all enough: I can think of a number of droughts and food shortages in numerous principalities in the eighteenth century, non of which resulted in a revolution, as neither did the iniquitous and anachronistic socio-economic conditions of the Kingdom of the Two Sicilies in the nineteenth century. Taine’s argument of the *thought* that permeated France has much import and cannot be discounted: unquestionably, ideas do exist and possess a truth just as facts do, much to Veyne’s discomfiture.

The imposing—and slanted—theoretical structure that Murphy is elevating, is irretrievably weakened by a fatal contradiction, which is his latest work, *Truth and History*, is most flagrantly evident:

History, as all historians agree [sic], is a form of empirical knowledge. Accordingly, the logic of history is similar to that of other forms of empirical knowledge. The basis of historical work is evidence, which as every philosopher of history, from Collingwood on, has agreed, consists of observations made on artifacts from the past. [...] It follows that the historian’s basic task is the finding and the interpretation of such artifacts. (Murphy, 2009: p. 177)

Interpretation? Is Murphy stretching his hand to hermeneutics? Again, as with Veyne, the empirically-leaning historian is faced with the irreconcilable, logical disjunction of being both an empiricist and a hermeneutist—a hopeless desire. Murphy also manifests a serious epistemological inconsistency when at first he states, “as an empiricist, I do not believe there is any way of knowing reality except through the theory that best explains our data, and I see nothing to be gained by the belief in an unknowable metaphysical entity.” (Murphy, 2009: p. 12) But only a page later, Murphy takes umbrage with Bas van Fraassen, whom he considers too severe an empiricist, due to his intransigence,¹³ which he considers it to be “an extreme form of empiricism that denies reality to anything not directly observable by us with our unaided senses.” (Murphy, 2009: p. 14) But empiricism *is* severe in that it must obey rigorous rules and it does not allow unobservable data to be admitted for theoretical purposes. It need be so: if we were to betray its framework—*Method*—our entire scientific knowledge would collapse.

Let me address Murphy’s statement that “History, as all historians agree, is a form of empirical knowledge”. Can empiricism, explanation, causality, and all these conceptual ingredients of the scientifically minded, yield statements of such profundity as the following by Johann Huizinga?

The great divide in the perception of the beauty of life comes much more between the Renaissance and the modern period than between the Middle Ages and the Renaissance. The turnabout occurs at the point where art and life

¹⁰For Taine’s own, extensive presentation of “l’esprit classique”, see Chapter 2 of Book 3 in: Taine, 1986.

¹¹Here Veyne is projecting his thought onto Foucault’s: in *The Order of Things*, Foucault evinces a clear regard for ideas and their veracity (See next footnote).

¹²Veyne misrepresents his interpretation of Foucault in a number of ways. For a closer look at Veyne’s flawed interpretation of Foucault, see Franchetti, 2011.

¹³See Fraassen, 1980.

begin to diverge. It is the point where art begins to be no longer in the midst of life, as a noble part of the joy of life itself, but outside of life as something to be highly venerated, as something to turn to in moments of edification or rest. The old dualism separating God and the world has thus returned in another form, that of the separation of art and life. Now a line has been drawn right through the enjoyments offered by life. Henceforth they are separated into two halves—one lower, one higher. For medieval man they were all sinful without exception; now they are all considered permissible, but their ethical evaluation differs according to their greater or lesser degree of spirituality.

[...] For the medieval man enjoyment per se is sinful. The Renaissance had managed to free itself from the rejection of all the joy of life as something sinful, but had not yet found a new way of separating the higher and lower enjoyments of life; the Renaissance wanted an unencumbered enjoyment of all of life. The new distinction is the compromise between the Renaissance and Puritanism that is at the base of modern spiritual attitudes. It amounted to a mutual capitulation in which one side insisted on saving beauty while the other insisted on the condemnation of sin. [...] Only after the Puritan worldview lost its intensity did the Renaissance receptiveness to all the joys of life gain ground again; perhaps even more ground than before, because beginning the eighteenth century there is a tendency to regard the natural per se as an element of the ethically good. Anyone attempting to draw the dividing line between the higher and lower enjoyments of life according to the dictates of ethical consciousness would no longer separate art from sensuous enjoyment, the enjoyment of nature from the cult of the body, the elevated from the natural, but would only separate egotism, lies, and vanity from purity. (Huizinga, 1996: pp. 40-41)

Can logical induction yield insights into human nature such as this by Fernand Braudel?

Pius V was indeed one of these “upstarts”, not a “princely” pope, not a man familiar with the ways of the world and prepared to make those compromises without which ‘the world’ would not go round. He had the passion, rigour and intransigence of the poor. (Braudel, 1995: p. 1027)

These excerpts whose breadth reveal a singular comprehension of the past and of the human spirit is not based on mere particulars: it is an understanding that springs from the profound knowledge of a period’s facts inspired with the impulse of universality and a deep understanding of a specific culture, which undoubtedly includes particulars, but goes beyond them. (Reading the work of such historians reveals Collingwood’s notion that history is really the history of thought applied to history as bracing.) These remarkable excerpts come from historical masterworks of the twentieth century; both works are still source of admiration—and discussion. Is it possible that a Nominalist outlook by itself power such statements? Could the rejection of universals ever produce such singularly penetrating insights? I do not think so. A view that sees no strict identities reaching across different tokens—particulars—as the sole source of knowledge could never achieve what a Huizinga or a Braudel has. Surely, I am obviously not concerned with resolving the dispute—it never shall be—over whether history is a

form of knowledge that is attained through *Method*, or whether its knowledge is *hermeneutic*, different and autonomous from science, and thus belonging to the field of the humanities, but, I am concerned about arguing against Nominalism’s inadequacy, whatever persuasion a historian may hold about the nature of the knowledge of history.

Murphy is a potent thinker, but he is trapped by an epistemology “that is an empirical discipline located within science, rather than an a priori discipline prior to science” (Murphy, 1994, p. xii)¹⁴ and as such, he has an ax to grind, the ax of empiricism—a most encumbering ax—and after reading his lucid but overwrought ruminations one parts from his books feeling that the ideologue has exerted himself far too much to wield this ax, which may just be too grueling for history.

The reason for a number of philosophers of history’s case for Nominalism—and there are a number of them—is, I think, the fear of the specter of arbitrariness. And so, to avoid being labeled relativistic, the insecure historian legitimizes his methodology behind a gray scientism. I hope this essay be a warning to lesser historians who are not a Veyne, and do not possess his capability of immersing himself in a period’s specificity—regardless of the way he says one must go about it—and take refuge in a clerical empiricism which shears all beauty, effect, and meaning to their writing: the rejection of anything universal is the death of anything really historical, because it circumvents the human element—through time, of course—the paramount object of the historian’s gaze.

Let us examine more closely the claim that predictive laws and the generalizations they allow, be they causal or non-causal, are functional or indeed even possible in history. I wish to look at this more closely, because so much literature has been devoted to devise some kind of lawfulness in history. In a recent article in *History and Theory*, Bert Leuridan and Anton Froeyman argue for the use of general laws in historiography without the more extreme leanings of a Hempel or a Murphy: in no place do they claim history to be an empirical science. However, they insist that “laws in history can be made [...] clear and fruitful” (Leuridan; Froeyman, 2012: p. 182) by applying “pragmatic laws”, a “milder” form of lawfulness developed by Sandra Mitchell, which essentially holds that “evaluation is context-dependent” (Leuridan; Froeyman, 2012: p. 177) and thus “scientific generalizations [...] will seldom be completely universal [...] the important question is *how* and *to what extent* they are contingent. This means that if we want to use a generalization, we need to assess the stability of these conditions. [...] Stability is a very important parameter for the evaluation of a generalization’s usefulness.” (Leuridan; Froeyman, 2012: p. 177) Through this context-dependent view Leuridan and Froeyman believe a scientific generalization resembling a law is believed to be possible in history.

This argument is astute but it suffers from an ontological fallacy—as we’ve seen repeatedly, that of using scientific practices to define, delineate, and *delimit* historical apprehension. These stilted and somewhat artificial efforts are again the result of the fear of relativism, as if without some scientifically postured grounding history would cease to be a sound form of knowledge, like the natural sciences. These strained efforts seem to me unnecessary. The sibylline pretensions of causality in history are the result of a confusion about the epistemology of history, which in my view is hermeneutics and *not* scientific

¹⁴Murphy follows Quine’s view of epistemology and plainly states so.

method. If historiographers and philosophers of history did not have misconceptions about historical knowledge, or if they did not harbor any uncertainty of any other form of truth that is not scientific, it seems to me that they would not keep spearheading historical theory down a spurious path; and a lot of intellectual energy would not be dissipated.

It is perhaps for this reason, too, that footnotes became such an essential part of historical writing in modern times; as Anthony Grafton admirably put it, “footnotes are the outward and visible signs of this kind of history’s inward grace—the grace infused into history when it was transformed from an eloquent narrative into a critical discipline.” (Grafton, 1997: p. 24) In other words, footnotes are the underpinning of a discipline that its practitioners and theorists are often fearful could drift into mendacious waters. Grafton explains that the origin of history’s uncertainty was, in fact, “the vogue for Cartesian philosophy and experimental science. That, in turn, explains why Bayle felt it necessary to argue, at length, against the fashionable view that mathematics had an advantage over historical knowledge, in that ‘it leads to truths not susceptible to doubt’.” (Grafton, 1997: p. 208) We ought to keep in mind that a number of thinkers, of no less caliber than a Pascal, a Leibnitz, a Spinoza, a Bayle, a Vico all reacted against this constricting view of knowledge and argued “that those pure mathematicians and physicists, who are ignorant of and despise all other forms of knowledge, are wrong” (Grafton, 1997: p. 210)¹⁵ and that “certitudes of history, though different from those of mathematics, were far more concrete, more applicable to human life, and even more certain in a metaphysical sense than the profound abstractions of mathematics.” (Grafton, 1997: p. 208)¹⁶

That it was an illustrious scientist, who in modern times peremptorily contrasted between the two forms of knowledge we have been discussing is ironic: in a lecture given at the commencement of the academic year at the University of Heidelberg in 1862, Herman von Helmholtz made the historic distinction between the natural sciences and the human sciences, declaring the latter to be of superior and humane significance.

It is not easy for a scientific man to convey to the scholar or a jurist a clear idea of a complicated process of nature; he must demand of them a great power of abstraction from the phenomena, as well as a certain skill in the use of geometrical and mechanical conceptions, in which it is difficult for them to follow him. On the other hand an artist will perhaps find the natural philosopher too much inclined to mechanical and material explanations, which seem to him commonplace, and chilling as his feeling and enthusiasm. Nor will the scholar or the historian, who have some common ground with the theologian or the jurist, fare better with the natural philosopher. They will find him shockingly indifferent to literary treasures, perhaps even more indifferent than he ought to be to the history of his own science. In short, there is no denying that, while the moral sciences deal directly with the nearest and dearest interests of the human mind, and with the institutions it has brought into being, the natural sciences are concerned with dead, indifferent matter, obviously indispensable for the sake of its practical utility, but apparently without any immediate bearing on the cultivation of the

intellect. (Helmholtz, 1873: p. 9)

Helmholtz also argued against employing natural sciences’ epistemological parameters—what he called “logical induction”—in the human sciences:

We might possibly, in opposition to logical induction which reduces a question to clearly-defined universal propositions, call the moral science’s kind of reasoning aesthetic induction, because it is most conspicuous in the higher class of works of art. It is an essential part of an artist’s talent to reproduce by words, by form, by colour, or by music, the external indications of a character or a state of mind, and by a kind of instinctive intuition, uncontrolled by any definable rule, to seize the necessary steps which we pass from one mood to another. If we do find that the artist has consciously worked after general rules and abstractions, we think his work poor and commonplace, and cease to admire. On the contrary, the works of great artists bring before us characters with such a lifelikeness, with such a wealth of individual traits and such an overwhelming conviction of truth, that they almost seem to be more real than the reality itself, because all disturbing influences are eliminated. (Helmholtz, 1873: p. 16)

Finally, Helmholtz made an unequivocal statement about “aesthetic induction”: “This latter kind of induction, which can never be perfectly assimilated to forms of logical reasoning, nor pressed so far as to establish universal laws, plays a most important part in human life.” (Helmholtz, 1873: p. 15) That a man who was first and foremost a scientist, a physicist of no less caliber than the teacher of Max Plank, the pioneer of Quantum Physics, wrote such resounding words emphasizing the humanities’ superior importance is a lesson to all of us, especially to those who doubt the *truth* that the humanities reveal. But Helmholtz was a man of immense breadth of vision and is a figure in a class of his own who transcended the boundaries of science and art.¹⁷

If we compare Helmholtz’s idea that “logical induction” is not applicable to human sciences with Veyne’s theory of “retro-diction”, Helmholtz’s superior footing from which he is looking at the humanities is evident: Veyne wrote that, “[h]istorical synthesis is nothing but this operation of filling in; we shall call it ‘retro-diction’, borrowing the word from the theory of incomplete knowledge that is the theory of probabilities. [...] So all ‘retro-diction’ calls into play a causal explanation and perhaps even a true law. To study historical synthesis, or ‘retro-diction’, is to study the part played in history by induction and in what ‘historical causality’ consists.” (Veyne, 1984: pp. 144-145) Once again, these statements are spurred from the view that “logical induction” yields a superior form of knowledge to that of “aesthetic induction”. But Helmholtz annulled this fallacy. If Veyne had been acquainted with the modern German school of

¹⁷In addition to the countless and fundamental contributions to science—the law of conservation of energy, the electromagnetic equation, the invention of the acoustics resonator, the invention of the ophthalmoscope, and much more—Helmholtz laid out ideas, which were later developed by Freud that were indispensable for his forming of the concept of the unconscious. Furthermore, this authentic polymath developed the “Helmholtz resonator”, which was able to identify the pitch and the frequency of any sound; this machine as well as the book he wrote called *On the Sensations of Tone as a Physiological Basis for the Theory of Music* influenced musicologists up until the twentieth century.

¹⁵The quote is from Spinoza.

¹⁶The quote is from Bayle.

hermeneutics, which was commenced by Schleiermacher in the early nineteenth century and culminated with Gadamer¹⁸ in the late twentieth, who knows what interesting flowering of ideas for historiography would have flourished from his pen!

A Broader Concept of Universals

I wish to show that universals are not an incongruity in history—and neither in science—if we look at them from a wider perspective.

I cannot understand these other ingenious theories of causation. If someone tells me that the reason why a given object is beautiful is that it has a gorgeous color or shape or any such attribute, I disregard all these explanations—I find them all confusing—and I cling simply and straightforwardly and no doubt foolishly to the explanation that the one thing that makes that object beautiful is the presence in it or association with it, in whatever way the relation comes about, of absolute beauty. I do not go so far as to insist upon the precise details—only upon the fact that it is by beauty that beautiful things are beautiful. (Plato, 1961: pp. 81-82)

In this excerpt from *Phaedo*, Plato postulates that universals are capable of acting upon particulars (“it is by beauty that beautiful things are beautiful”): “In *Phaedo*, Plato endowed his Forms with causal power. They act upon particulars, giving the latter their nature, to the extent that they have a nature.” (Armstrong, 1978: Vol. 1, p. 128) Armstrong’s just observation brings him to theorize that universals and particulars may be conciliated even in empirical sciences, “since universals match up with the fundamental particles that science tells us about.” (Armstrong, 1989: p. 88) That should settle the case for the importance of universals in history with those, like Veyne, who tirelessly advocate for Nominalism for conceiving history. Furthermore, Armstrong rightly notes that “particulars have properties that stand in relations” (Armstrong, 1978: Vol. 2, p. 133) thus echoing Plato’s *Parmenides*, “I see nothing strange in [...] a proof that all things are one by having a share in unity and at the same time many by sharing in plurality.” (Plato, 1961: p. 923) Throughout his two volumes, Armstrong’s thorough discussion of Nominalism and Realism has the aim of accounting for universals’ existence as well as their having a role compatible with empiricism; by having shown that universals themselves possess properties and relations, which constitute *laws of nature*,¹⁹ he revoked the incompatibility of universals with empirical knowledge.

That is a tonic against the most skeptical philosophical thinkers, and, for what concerns us, the skeptical historiographers whom we have been examining.

So if an ideal is responsible for a manifestation of a particular, how would that manifest itself in practice—in conceiving history? For example, the idea of a unified Christian Empire existed from Charlemagne to Charles V; but in 800 AD what was achieved was very different from what the empire came to be under emperor Frederick II; and that, too, was different during Charles V’s rule. Frances Yates, for example, pointed out that Charles’ abdication in 1555 was an implicit realization that

the figure of an emperor under whom a unified Christianity could exist was anachronistic: the *ideal* of a Christian Empire had vanished.²⁰ In fact, the only real case for the existence of a Christian empire may be made for the fourth century AD, as André Piganiol persuasively outlined in his *L’Empire Chrétien*.²¹ According to him, for seventy years, from 325—the year of the Council of Nicaea, the first ecumenical council of the Church (promoted by Constantine the Great), in which the Trinity, the relationship between God the Father and The Son, the drafting of the Credo Niceum, as well as other fundamentals of doctrinal orthodoxy were settled—to 395—when Theodosius, the last emperor to rule over both halves of the Christianized Roman Empire died and the empire splintered forever in East and West, with the latter soon disintegrating with the Goth invasions—according to Piganiol a unified, Christian empire did exist. The perspective of such works, so rich in historical comprehension, is certainly the consequence of a regard for universals *as well as* particulars. And that seems to me understanding of a vanished past of the utmost value, just like the magisterial examples of Huizinga and Braudel.

With this concept of “powered” universals, even Gauchet’s justification for Nominalism can be criticized. If history is the study of *change* of human practices through time, and if, as Gauchet suggests, we understand historical events or practice merely as particulars, and thus Nominalism is “the only position that can adequately open him to the *inexplicable singularities* of a process of permanent innovation”, how are we to compare these singularities with another, if we do not see particulars as standing in relations? Would it be possible to set, using our example above, the Council of Nicaea *in relation* to the idea of empire? How could we make historical conclusions, if not against the necessary, immovable fixity of the universal which the particular we are examining is a set of? And even in the differentiation of particulars themselves—a fundamental task of the historian, as we’ve seen in the examples of “the State” in different epochs—how can one do so without universals? “Lacking universals, a Nominalist cannot relate them! So he is nailed to the Humean or the Singularist cross.” (Armstrong, 1978: Vol. 2, p. 151)

Dilthey spoke most convincingly of the relationship, and consequently of the existence of universals when he wrote that, “The individual always experiences, thinks, and acts in a sphere of commonality, and only in such a sphere does he understand. Everything that has been understood carries, as it were, the mark of familiarity derived from such common features. We live in this atmosphere; it surrounds us constantly; [...] we ourselves are woven into this common sphere. This results in a reciprocal dependence the way we apprehend each particular of the human sciences within the communal, historical whole of which it is a part [...] In the progress of the human sciences, [...] we apprehend the human world around us [from] the reciprocal dependence of universal and singular knowledge.” (Dilthey, 2002: pp. 168, 174)

Conclusion

In closing this essay I wish to sum up briefly the conclusions we can draw about Nominalism in history, and, as a result of my discussion, offer a view of history that may settle the

¹⁸For a discussion of the progression of hermeneutics’ gradual appropriation of its foundational role in history, see Franchetti, 2013.

¹⁹See Armstrong, 1978: Vol. 2, p. 4

²⁰See Yates, 1975: pp. 20-28.

²¹See Piganiol, 1947.

“stormy grounds” of historiography.

Particulars, we have seen, are fundamental for the historian to decipher, discern, and distinguish the period or the event he is scrutinizing; but, the consciousness of universals, too, is essential for the apprehension of any human occurrence. Nominalism or Nominalistic stances are upheld in historiography due to the disquieting but false pretense that accepting universals will induce haziness to the historian’s gaze: it is time that historians lose this fear, for the historian recreates a tapestry of the past through many “a ‘track’, as it were—the mark, perceptible to the senses, which some phenomenon, in itself inaccessible, has left behind.” (Bloch, 1953: p. 55) And so, both particulars *and* universals are essential for us to make sense of the world—even more so for historians who have to make sense of a world that no longer exists except in traces of it. Armstrong stated it perfectly when he said that, “[t]he conclusion drawn is that particularity and universality, irreducible to each other, are both involved in all existence.” (Armstrong, 1978: Vol. 1, p. xiv) This statement relates just as well to history.

Therefore particulars are absolutely indispensable, but the rejection of universals, Nominalism, is absolutely dispensable. Especially if we accept Armstrong’s wider view of universals that I have presented above, which makes universals reconcilable with empiricism. And that buries Nominalism for a Veyne—who not only stresses empiricism in historical apprehension, but also openly reject universals—but also for a Foucault and a Murphy, whom we have seen fixed on a view of history as an empirical discipline.

As to the status of history as an empirical discipline, we have seen that even if we were to accept the foolish idea that history is nothing but empirical knowledge, universals still could not be discounted. I hope to have shown to some extent that Nominalism in history is a misconstrual of the historian’s methodology, since, though essential, the scrutiny of particulars cannot occur *without* the awareness of universals. The historian must therefore harmonize *both* an empirical stance—especially when sources from the past are faced—and hermeneutical understanding. It is for this reason that history is an enormously difficult discipline, which Macaulay justly acknowledged to have but few masters; and it is for this reason that we historians must constantly be able to shift our thought from a macro to a micro degree of understanding; historical knowledge is just that.

It is true that history is indeed a form of factual knowledge, but since it pertains to occurrences in the past that cannot be tested, it cannot be considered a purely empirical discipline; yet, of course, there is factual evidence that forbids statements such as, “Louis XIV waged war against China in the tenth century”. That satisfies one of history’s offices—that of making accurate, discernible statements about the past. But obviously history has—justly—much wider contentions. Where the latter lie, *interpretation* enters the historian’s arsenal. And it is here that we are faced with a crux, which has cast an endless conceptual problem of history’s nature: Murphy has justly stated that, “it is fair to say that the philosophy of history is currently something of a mess.” (Murphy, 1994: p. x) This “mess” is principally due to the difficulty for minds grounded in an age which accepts scientific facts as the only source of truth to bridge the gap between *Method* and *hermeneutics*.²² I think it is precisely here that the fulcrum of the problem—and the solution—of the philosophy of history lies.

²²Here lies Gadamer’s invaluable contribution to our field: the persevering and patient elucidation that hermeneutics is a practice and *not* a method.

Salient examples are the texts we have examined: Veyne and Murphy all proclaim in their introductions that “history is not a science”, but in their discussion of “historical truth” they cannot hold back from Nominalism or logical formulae with lots of P’s and Q’s. This rather clearly shows that these historiographers have not bridged the gap between Method and hermeneutics, our discipline’s most equivocal aspect, which Anthony Grafton so rightly defined as “that strange hybrid of science and art.” (Grafton, 1997: p. 235)

So why are so many theoreticians so attached to a Nominalist position? As I have already noted, I think it is because in their eyes the specter of arbitrariness is raised every time we make a statement that employs universals, for it seems to them that calling upon universals leads down the relativistic path.

“The generations just prior to our own, in the last decades of the nineteenth century and even in the first years of the twentieth, were as if mesmerized by the Comtian conception of physical science. This hypnotic schema, extending to every province of the intellect, seemed to them to prove that no authentic discipline could exist which did not lead, by immediate and irrefutable demonstrations, to the formulation of absolute certainties in the form of sovereign and universal laws.” (Bloch, 1953: p. 14)

I believe along with Bloch and many others, that there is no need to apply scientism to history for fear of not being taken seriously; I have read endless, dispiriting volumes²³ that not only blindly defend Nominalism but advance even more radical and misconceived ideas of empirical strictures and law-covering models for history. The Nominalist view restricts from a complete and exhaustive understanding of history, because no matter which view one has of history, Nominalism is deficient, since it is irreconcilable from a humanistic perspective, and, from a scientific standpoint, Armstrong has convincingly noted that “where there are laws there exist universals.” (Armstrong, 1978: Vol. 2, p. 151) But such attempts to apply natural sciences’ systems to history imply that history is incapable of unveiling or unveiling truths opposed to scientific understanding.

[...] the human sciences are a long way from regarding themselves as simply inferior to the natural sciences. Instead, possessed of the intellectual heritage of German classicism, they carried forward the proud awareness that they were the true representatives of humanism. The period of German classicism had not only brought about a renewal of literature and aesthetic criticism, which overcame the outmoded baroque ideal of taste and of Enlightenment rationalism; it had also given the idea of humanity, and the ideal of enlightened reason, a fundamentally new content. More than anyone, Herder transcended the perfectionism of the Enlightenment with this new ideal of “cultivating the human” and thus prepared the ground for the growth of the historical sciences in the nineteenth century. The concept of self-formation, education, or cultivation (*Bildung*), which became supremely important at the time, was perhaps the greatest idea of the eighteenth century [...] (Gadamer, 2004: p. 8).

I should like to offer at this point a most compelling example of the untenability of strictly empirical views of history, which

²³Mark Day’s *The Philosophy of History*; Georg Iggers’s *Historiography in the Twentieth Century; What is History Now?* Edited by David Carradine; to cite just a few.

touches ever so closely upon history's essence itself. Say, for example, that historian *a* has read exactly the same texts pertaining to the Renaissance as historian *b*. Their vision and understanding of that period shall inevitably differ considerably. How can a view of history as an empirical form of knowledge withstand, let alone explain any discrepancy at all? Here, in my view, lies the marrow of history's dilemma: that different accounts would spring from sources identical with each other is the ever fascinating aspect of history, for history is but the thought of different men. The very root itself of the word "history" points to this: its root "his" is the Indo-European "vid" which simply means "view", suggesting that the cardinal factor in history-making is indeed a historian's own, particular outlook.

The latter is achieved by integrating judiciously history's singular, three main elements, which are unique to it: philology, hermeneutics, narrative. These are, according to me, the critical, constitutive elements of history. The first element is the most scientific in nature. Peculiarly though, it did not have historical pretensions at first, or, as some would rightly claim, it did not propose—or expect—to inaugurate modern historiography: when Lorenzo Valla wrote his devastating *On the Donation of Constantine* in 1440, he did not expect that what was intended mainly as a linguistic feat aimed at proving the Vatican's forgery²⁴ of a document purporting the Church to be the inheritor of the Roman Empire by the hands of emperor Constantine the Great would lead to a fundamental branch of modern historiography. But it did and from a philological endeavor modern history sprung into being.²⁵ The second, hermeneutics, is, as I have been suggesting throughout this essay, the practice through which an historian *understands* history and is able to articulate it. Narrative is the third element of history, which a history inevitably calls for in writing it. Despite the Annales school of history displaced for a while a strictly narrative focus—which was the cornerstone of the vast, synthetic historical works until the nineteenth century—I agree with Paul Ricoeur that "the narrativist interpretation is correct in its clear perception that the specifically historical property of history is preserved only by the ties, which continue to connect historical explanation to our narrative understanding." (Ricoeur, 1984: Vol. 1, p. 228) And even Braudel's *The Mediterranean and the Mediterranean World in the Age of Philip II* which is perhaps the greatest work to come out of the Annales school, as well as the most argumentative work against narrative in history, in its entirety is experienced as "a grand narrative of the retreat of the Mediterranean from general history." (Ricoeur, 1984: Vol. 1, p. 217) Yet, the narrativist claim is quickly returned in favor and does not need much defense today: through Arthur Danto, Paul Ricoeur and the postmodernist stance of Hayden White, narrative in history has been restored, though, at times, with some gross exaggerations.

If we were to acknowledge that the historian's task is to coordinate this triangulation I have outlined, I think many misunderstandings and their resulting abuses, which disorient current historiography would be avoided.

Lastly, I should like to mention Benedetto Croce, who of late is overlooked in English-speaking historiography. Croce was an authentic philosopher as well as an historian, who did not exhibit aporias in his thought; he elicited Collingwood's admiration, who said of him that "it was the clean cut which he [Croce]

made in 1893 between the idea of history and the idea of science that enabled him to develop the conception of history so much farther than any philosopher of his generation." (Collingwood, 1946: p. 193)

Croce's argument toward an identity of history completely independent from empiricism was to unhook it from the idea of "universal history". Universal history was an inheritance of German idealism; it was an ideal shared by the German historical school of the nineteenth century, which believed there was a history that existed in itself that was an objective act of self-consciousness part of a wider, collective consciousness. This was a manifestation of a concept that had originated in the eighteenth century with Voltaire's *Essai sur les Moeurs*, but which sprouted fully in Germany with Hegel's concept of *Weltgeist*; its effects persisted in historians' thought throughout the nineteenth century. Ranke was a paradigmatic example, for he believed history to be composed of "spiritual beings" which in their totality would constitute "world history"; Droysen sought to understand the "inner essence" of things; Dilthey conceived of the historical world as a text to be deciphered. But Croce thought that "unless there is some way of knowing the real that is independent of our data, the postulation of such an independent reality leaves us with an unknowable *ding-an-sich*." (Croce, 1923: p. 14) Croce then spoke crucially on the "thing-in-itself" in history: "we know at every moment all the history that we need to know [...] that 'remaining' history is the eternal phantom of the 'ding-an-sich', which is neither the 'thing' nor 'in-itself', but only the imaginative projection of the infinity of our action and of our knowledge." (Croce, 1923: p. 55) This statement was Croce's way of dispelling the notion of universal history. But Croce made sure to add that "to negate universal history does not mean to negate the universal in history." (Croce, 1923: p. 59)

Nominalism—the rejection of universals—again just seems to be an unsuitable stance in any sound philosophy of history.

I do not believe that we live in a Fukuyama-like moment (at the "end of history") but I do believe that if we continue to abuse and stretch history's fabric with theoretical forcings, Marc Bloch's warning may come true:

It is not itself inconceivable that our civilization may, one day, turn away from history, and historians could do well to reflect upon this possibility. If they do not take care, there is danger that badly understood history could involve good history in its disrepute. But should we come to this, it would be at the cost of a serious rupture with our most unvarying intellectual traditions. (Bloch, 1954: p. 5)

It is our task to see that this not be so.

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ARTICLE

The ontology of words: Realism, nominalism, and eliminativism

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Abstract

What are words? What makes two token words tokens of the same word-type? Are words abstract entities, or are they (merely) collections of tokens? The ontology of words tries to provide answers to these, and related questions. This article provides an overview of some of the most prominent views proposed in the literature, with a particular focus on the debate between type-realist, nominalist, and eliminativist ontologies of words.

Consider the word “omnishambles.” Famously used by Malcolm Tucker on the political satire “The Thick of It,” the word means (from the OED): “A situation that has been comprehensively mismanaged, characterized by a string of blunders and miscalculations.” The writers of the show came up with this word. They wrote it down, an actor spoke it, and the word has passed into common usage. But, a curious ontologist might ask what kind of entity did the authors bring into existence through their activity (or even if they did at all)? That is, what is a word?

The literature on the ontology of words has mainly focused on words as kinds or types—as things that can have instances or tokens. I will follow that trend here, outlining what the competing views take such kinds to be, and how these ontologies subsequently affect the answers to two interrelated questions that have dominated much of the literature. First, how should we individuate word kinds (or types), and, second, when is it the case that two token (or particular) words are instances of the same word-kind or type (A note on terminology. I will use “kind” and “type” interchangeably here.)

1 | CRITERIA FOR AN ONTOLOGY OF WORDS

We have a general practice of discussing and conceiving of words as entities in the world, and those words having certain characteristics or properties. Words have spellings, meanings, pronunciations, etc. Words play various roles

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in our lives. Some insult, some inspire, and words are central to communication. The aim of an ontology of words is to determine what entities, if any, can play those roles and possess (or instantiate) these properties. To do this, especially for those new to the debate, it would be useful to have some criteria in mind when assessing an ontology. Here are some initial proposals, drawn from (what I think are) common intuitions about words, with two caveats.

First, I have tried to phrase the criteria in a relatively neutral way. However, complete neutrality is likely impossible, and supporters of some ontologies may endorse a variation of some or all of the criteria rather than accept them as stated here. Others may reject some or all of the criteria entirely. This is fine as I do not intend them to be sacrosanct, but rather as a place to begin the discussion. It is also the case that other considerations such as theoretical virtues may also be important to theory choice. I therefore leave it open that parsimony, simplicity, elegance, or any other theoretical virtue might push us away from any criterion stated here.

Second, some of these criteria could be applied to token words and word-types. I take it that, ideally, we would have an ontology that accounts for the nature of both token words *and* word-types, and this may influence how we interpret any of the criteria.

The first criterion is that of *creation*.

Creation: whatever words are, they should be entities that account for the phenomena of “invention” or “coining.”

Words are created by people, perhaps for certain purposes, and within certain historical contexts. Creation speaks to the intuition that words exist only through the action of an agent. Raindrops forming patterns on my window, ants moving through spilt sugar, swamp words, waves forming patterns in the sand, and other bizarre natural phenomena are intuitively not instances of a word, and hence not sufficient to create a word (either *qua* token word or word-kind).¹

Second, *expressibility*:

Expressibility: whatever words are, they should be expressible through some means of externalization (speaking, writing, signing etc.)

Words are in some important sense social entities that are shareable through various forms of externalization. This makes word-kinds, at least to some degree, social kinds.² Importantly, expressibility is neutral as to how words are expressed. The most common form of expression (or externalization) of words in humans is through spoken language, but it is also done through writing, signing, and potentially other forms of linguistic communication.³

As stated, this is a requirement only that words could be expressed, not that they are. But, we might debate the scope of “could.” Some (e.g., Hawthorne & Lepore, 2011) hold that derivational morphology suggests that there are words that are composed of so many suffixes that those words would not be expressible, at least by humans with finite lifespans.⁴ Whether such words exist is an example of the debate about uninstantiated words that will come up again later in this paper. However, while the full details of expressibility are up for debate, the commonly accepted idea is that even if *some* words are not expressible, it must be the case that words can (in one sense of can) be expressed as it seems that some clearly are.

Third, *evolution* (or perhaps, *change*):

Evolution: whatever words are they should account for the apparent change of words (e.g., being spread, forgotten, changing meaning, spelling, or pronunciation).

Words are not static. They change their meanings, or at least *can* change their meanings, spellings, and pronunciations. As will be discussed below, it is difficult to find a property that some token of a particular word-type has that is also had by all other tokens of that word-type. Words are also lost. Intuitively, words from certain extinct languages that have no written record no longer exist.

This is to say nothing about how much change a word can undergo while remaining the same word. Different ontologies will be able to accommodate differing amounts of change, just as different accounts of the metaphysics of ordinary objects can accommodate differing amounts of change in medium-sized dry goods.

One last consideration is the relationship between the ontology of words and linguistics. Though it has been argued otherwise (e.g., Balletta, 2019), I will assume here that the aim of our ontology of words is to provide a conception of “word” that satisfies all of the scientific, philosophical, and everyday demands on it. That is, a conception that can provide answers to philosophical (and in this context primarily metaphysical) puzzles, whilst being (minimally) coherent with the empirical evidence, and maintaining as far as possible our ordinary way of talking about words. Perhaps this is not possible, with each of these ways of talking about words requiring its own specific notion of “word,” suggesting some form of ontological pluralism about words. However, the ontology of words typically proceeds with a unified single notion of “word” as the aim, with each theory attempting to provide the best balance between philosophical rigor and alignment with the empirical data. It remains debated, naturally, as to how well the ontologies achieve this aim.⁵

2 | TYPE-REALIST VIEWS

There are many different ways we could classify views about the ontology of words. Here, I will do so through the lens of (probably) the most discussed distinction, between “realist” and “nominalist” ontologies. In brief, the distinction is between views that posit the existence of “kind” or “type” level entities, and those that deny the existence of such entities. For each theory, the two interconnected questions I raised at the beginning of this paper will arise, and how satisfying an answer the theory provides to these questions will greatly influence how plausible the ontology. As we will also see, though, there is much disagreement amongst adherents of views within these broad categories.

2.1 | Platonism

Platonism, broadly understood, holds that words are abstract eternal types, which have instances—standardly, either physical instances (e.g., written or spoken tokens) or mental instances.⁶ Thus, following the convention to use lower case for particular entities, and capitals for type-level entities, a particular word, “table,” is an instance of the abstract word-type, “TABLE.” These types are genuinely existing entities, distinct from their instances, and (typically) Platonists posit a relation of instantiation as holding between word tokens and these abstract word-types (Wetzel, 2009).

Naturally, there are Platonic type-realist views that vary from this attempt to characterize the view, and in particular reject this “instantiation model” of the type-token relation. Katz’s Platonism, for example, holds that tokens are composite objects, composed of the abstract type and the some physical or psychological particular. Katz argues that it is a “tokening” relation, rather than instantiation (2000: chapter 5) that holds between the types and tokens.

Platonists (and many non-Platonic realists; see Section 2.2) are more united in arguing that we need to posit types in order to understand the truth of various ordinary and scientific claims about words. For example, when I say that “Paris’ contains 5 letters”, this is not standardly read as being a claim about some particular word, but the word-type “PARIS”. Granting that the claim is true, it is a true claim about some type-level entity, not just some particular instance(s).⁷ Hence, word-types must exist. Indeed, some Platonists (notably Katz, 1981, 2000; Postal, 2003, 2009; see also fn. 16) go further to argue that this evidence supports the view that the proper subject matter of linguistics must be abstract entities.

A major benefit of Platonism is that it provides a simple answer to the question of when it is the case that two particular words are tokens of the same word: “color” and “colour” are tokens of the same word because they are both instances of the same abstract type. As with Platonic views in other domains, the view allows that tokens of the same type need not share the same properties. This ensures that despite the difference in

spelling (and, in other cases, differences in pronunciation and meaning), “color” and “colour” are tokens of the same abstract type.

However, while Platonism allows for our intuition that there can be differences between tokens of the same type, what ensures that tokens are tokens of some type? What are the necessary and sufficient conditions on some token being a token of a type? The problem for the Platonist is that it may be that no good answer is forthcoming.

Platonists are quick to reject spelling and pronunciation as providing the identity conditions of words. This is because of the need to accommodate variation of tokens of a type, as shown by countless examples of alternative spellings and pronunciation of intuitively the same word. Semantic properties also cannot seem to provide a criterion of word identity. There are many cases where (at least intuitively) instances of the same word have distinct semantic properties, beyond changes arising from the circumstances of use, or where words have significantly changed their meaning over time, and yet are ordinarily thought to be the same word. For these and other reasons, there has been no major defense of a form of Platonism that appeals to phonetic, orthographic, or semantic properties in order to say when two tokens are tokens of the same type.

This difficulty in finding some shared property had by all tokens of a type have led some to appeal to intentions as necessary for the tokening of some type (e.g., Katz, 2000:153; cf. Kaplan, 1990, 2011 and discussion in Section 2.2). Others argue that there is no property that all tokens of the same type share except that of being tokens of the same type (Wetzel, 2002; 2009:106–7). If this later view is correct, then tokens are tokens of a type if they stand in the appropriate instantiation relation to the type, with no resemblance requirement either between tokens or between the token and the type. Somewhat relatedly, Hawthorne and Lepore argue for what they call an “abstracta-articulations” model. On their view, though words are abstract entities, the model “breaks with the standard type-token model’s picture of the relevant abstracta as pattern-like” (2011:38). On how to provide a criterion of identity for words, they are not sure that a positive answer can be given, suggesting instead that we should be “sloppy realists” wherein “there either are facts we may never know or simply no facts at all about the myriad borderline cases left unresolved by our capacity to settle questions in the area” (2011:36).

How persuasive sloppy realism or the appeal to the instantiation relation are, I think, depends on our antecedent commitment to a realism about word-types. Those with more nominalist tendencies will likely not find an appeal to the property of “being a token of the same type” convincing. To many nominalists this claim looks brute, possibly even ad hoc, and providing little scope for us to discover when it is the case that tokens are of the same type given that instantiation relations cannot be studied empirically. If types are posited to explain the sameness of words, then positing a “being a token of the same type” property looks like it is simply positing into existence a primitive that solves the initial problem (see Miller 2019c).

Moving on to other issues, Platonism about words faces versions of various familiar problems raised against other forms of Platonism. For example, granting the standard assumptions that abstract types are noncausal, the Platonist about words needs to be able to explain how it is that we can come to know words qua abstract eternal types.⁸ However, rather than rehearse familiar debates here, I will focus on some problems that are more specific to the debate about words.

One strong intuition that we have is that words are created entities—that we can and do “coin” new words, and that Shakespeare invented a “multitude” of “new-fangled” “auspicious” words. The problem for a Platonic account is that if words are abstract, eternal Platonic types, then how can we account for the sense that we create words? Indeed, many Platonists instead hold that we *discover* words (Katz, 2000:134; 168; see also the discussion in Begley, 2019). This strikes many as deeply counter-intuitive, and goes against our normal way of speaking about words as created.

Relatedly, we might object that taking words to be eternal entities suggests that there exist untokened word-types. That is, that words that have yet to be tokened already exist, and are waiting in some abstract realm to be first instanced. This, as well as being counter-intuitive, could have further consequences for the view that words are social entities created by, and for the use of, communities of speakers.

The typical response is to refine creation to distinguish between the creation of new tokens and the discovery of some eternal type. New tokens are created, and so are the ways of expressing abstract word types, and it is this that we mean when we say that Shakespeare created new words. He created new ways to express already existing eternal types, which prior to that point may have been untokened. Hawthorne and Lepore (2011) also argue that the findings of derivational morphology makes untokened abstract types less implausible than they initially seem, and in fact required in order to fully account for ordinary speakers to be able to understand new tokens that a speaker has not previously encountered.

This shift to tokens also typically explains word change in these kinds of realist accounts. As words are abstract eternal types, the words themselves cannot change. Instead, what changes are the ways in which words are expressed, and the relations that hold between particular sounds or ink patterns, and word-types. This means that for the Platonist, words evolve in the sense that the same sound may, over time, come to stand in an instantiation relation with a different word-type than previously.

2.2 | Non-Platonic realist views

Platonism is committed to words as eternal, unchanging, abstract entities. It is possible, though, to be a type-realist, but reject Platonism, with such views most often motivated by a desire to include words within a (more) naturalistic account and, borrowing a phrase from Kaplan, hold that words “live in the world, not in Plato’s heaven” (1990:111).

One way to hold an alternative type-realist view is to keep parts of the Platonic picture intact, but deny certain problematic specifically *Platonic* features of word-types. That is, to hold that there are genuinely existing abstract word-types, but deny that such types have one (or more) of the features that cause problems for the Platonist.

We have seen one example of such a twist on Platonism already in Hawthorne and Lepore’s abstracta-articulations model. Another comes from Szabó (1999) who argues against positing an instantiation relation between types and token, suggesting instead a “representation” relation. Importantly, Szabó does this, in part, to avoid a Platonic conception of types as eternal and unchanging, but to maintain a realism about types that can account for the historical nature of types. Under his view, token words are “type-representations” arising from “our tendency to apply terms referring to abstract entities to their standard representations too” (1999:160). Thus, a type is represented by its tokens, allowing us to explain how empirical information about a token can inform us about the nature of the type. Szabó argues that this is not possible under the instantiation model as it relies on inductive generalizations to move from knowledge of the token to knowledge of the type.

Yet another can be found in Irmak (2019). Irmak posits words as genuinely existing abstract entities—hence he is in my terms a realist—but they are *created* abstract entities. If correct, this would allow us to resist the problems I raised above for Platonism concerning the claim that we “discover” words, and the concern about already existing uninstantiated words.⁹

A very different type-realist ontology comes from Kaplan. Kaplan (1990, 2011) begins by noting the same variation that I have touched upon above: that instances of the same word can vary in spelling and pronunciation, and argues that this is sufficient reason to reject the view that words have “some fixed and perfect Platonic form” (1990:100).¹⁰ Instead, Kaplan proposes that utterances and inscriptions are “stages” of words, with words themselves being “the continuants made up of these interpersonal stages along with some more mysterious intrapersonal stages” (Kaplan, 1990:98). Particular utterances or inscriptions are instances of the same word if they are nodes on “a single, continuous tree of utterances, inscriptions, and quiescent storage” (2011:510). Though Kaplan resists the analogy (2011:508), this has reminded many of four-dimensionalist views in the metaphysics of persistence.

The main strength of this view is that it accounts for how words change and evolve over time. This is because what makes a token a token of a certain type is not that it resembles that type. Instead, Kaplan suggests that words are more like families. Like families, word-types (or, more precisely, continuants) may happen to resemble in certain ways, but they need not. The stages (or tokens) of a continuant (or word-type) may vary hugely whilst still being

tokens of the same type, including varying over time as spelling, pronunciation, and meanings associated with words shift and change. Rejecting any appeal to resemblance, what connects tokens of a type are relations that are “historical in nature and not apparent to perception” (2011:509). That is, it is the shared historical connection to other tokens of that type (to stages of that continuant) that makes two tokens of the same type.¹¹

The appeal to historical relations additionally ensures that this ontology can maintain that words are created entities. Two utterances or inscriptions are of the same word in that they “descend from a common ancestor” (2011:509). This means that there had to have been that common ancestor—a first token or stage—for later tokens to have descended from. Given that that common ancestor will itself be an utterance or inscription, this makes word-types created entities and rules out the existence of noninstantiated words.¹²

However, Kaplan also argues that the historical connection is itself not sufficient to token a word. Invoking the notion of “repetition”, Kaplan argues that “a sincere subject, intending to repeat a word that has been uttered by an examiner, will, indeed, utter that word” (2011:518). This is important for Kaplan's account as without it, given that tokens of the same type need not resemble at all, it would be open that any token could, without the knowledge of the speaker, turn out to be a token of any type. Repetition and intention together explain the continuity between speakers and within communities, as speakers are able to say the same word by intending to repeat words that others have spoken—by intending to utter a new stage that bears the correct historical relations to other stages of that continuant.

This means that so long as certain minimal capabilities are present (i.e., that the person is able to speak and is not simply producing grunts or other mere noises; see Kaplan, 2011:519), if a speaker intends to express a word, then they will succeed in expressing a token of that word, irrespective of how much it resembles other tokens of that word, thereby making the historical connection between tokens significantly intentional.

This reliance on intentions has been a source of many objections to the view. Cappelen (1999), for example, has argued that intentions cannot be part of the individuation of words, arguing that the intention to utter a token of a word is neither sufficient nor necessary for being a word token. This is because, under intentionalist views like Kaplan's, for a listener to know if something is a token of a word, they would need to know the intentions of the utterer of that word, something that we are often not in the position to do (1999:97). Thus, according to Cappelen, the knowledge of certain nonintentional (but still conventional) semantic facts is a necessary condition for communication as we observe it.¹³

It is important to stress that a stage-continuant ontologist need not accept intentionalism, but there are other objections arise more directly from the proposed ontology that would remain. For example, Kaplan holds that continuants are made up of, or composed by, stages. This seems to imply that if any stages that compose a word were different, then the word would also be different in virtue of being composed of different stages. Hawthorne and Lepore (2011: 7-8) have argued that this makes stages necessary, contra our intuition that any token might have been uttered differently, or even not at all. A possible response might lean on counterfactualist responses to similar objections raised against four-dimensionalism elsewhere in metaphysics, but this has yet to be fully developed in this literature.

3 | NOMINALISM

Nominalists about words hold that “no explanatory work will be done by picking out some one abstract entity as the sign type. That's to say, it might be that reifying sign types would be explanatorily superfluous” (Cappelen, 1999:100). Type-talk need not be rejected, but what these views have in common is the rejection of *ontologically committing* type-talk. Mention of types is *mere* talk, and is only a way of talking about sets, collections, or classes of suitably resembling tokens. Thus, these views only countenance the existence of tokens, rejecting the existence of types. The tokens “table” and “table” are thus distinct, and are the “same word” only in the sense that they

are members of the same set, collection, or class of tokens. In the rest of this section, my use of the term “type” should thus be read in a suitably nominalistic fashion.

The ontological parsimony achieved by positing only tokens is often a main motivation for accepting nominalism. Naturally, though, parsimony is only a virtue if the ontology is (at least) equally as explanatory as less parsimonious ontologies, and hence much of the focus of nominalist accounts has been to show how genuinely existing types are not needed, or even that a lack of types better accounts for the phenomena we observe. For example, by restricting our ontology to only word-tokens, the nominalist may avoid problems concerning the created status of words and uninstantiated words. If word-types are merely collections of tokens then those collections do not exist without members, and come into existence only when the tokens that compose the collection do.

The nominalist, though, still faces one of the central questions from above: what makes tokens tokens of the same type? Or, in the nominalist's terms, what makes two tokens members of the same (nominalistic) set, collection, or class? The nominalist still needs an answer to this question, despite their insistence that type-talk is not ontologically committing, if only due to the need to be able to adequately explain all of the ways that we typically talk about words that do seem to invoke types.

Older forms of nominalism attempted to respond to this question by appealing only to the “shape” or “form” of token words. That is, at least as it is often portrayed in the literature, nominalism about words is the view that the only relevant property of tokens to assess whether tokens are members of the same type are the spelling or pronunciation properties of those tokens. This means that we can only say that tokens are the tokens of the same type if the tokens are (exactly) resembling in their spelling or pronunciation. Accordingly, this has become known as shape- (or form-) theoretic nominalism.

Shape-theoretic nominalism has normally been attributed to Quine and Goodman as part of their broader nominalism applied to linguistic entities,¹⁴ and Bloomfield (1933). A full account of the reasons behind the restriction to “shape” properties for each of these authors would require a longer historical analysis than can be provided here, but one reason is an independent commitment to (or at least sympathy towards) (reductive) physicalism and behaviorism. For example, Bloomfield held that language was nothing more than sounds and ink patterns, and that “meanings” were reducible to complex behavioral analysis. He writes that:

“Non-linguists (unless they happen to be physicalists) constantly forget that a speaker is making noise, and credit him, instead, with the possession of impalpable ‘ideas’. It remains for linguists to show, in detail, that the speaker has no ‘ideas’, and that the noise is sufficient” (Bloomfield 1936: 93)

Given these further commitments, we can see why only spelling and pronunciation are acceptable to a shape-theoretic account. The “noise is sufficient” because there simply are no other properties, or those other properties are themselves reducible to “physical” properties (spelling and pronunciation) or the causal effects of those properties cashed out as a pattern of behaviors.

Shape-theoretic nominalism faces a number immediate and well-known counterexamples. By restricting membership of a type to tokens that are exactly resembling in spelling or pronunciation, the view would seem to predict that the tokens “color” and “colour” are tokens of different types, not just different spellings of the same type. Analogously, the view *prima facie* struggles to account for different accents. The shape-theoretic nominalist seems to be committed to the absurd claim that two speakers, one from Liverpool and the other from Newcastle, utter tokens that are not members of the same class due to the fact that they have very different accents.

Wetzel has taken this line of argument even further, suggesting that shape-theoretic nominalism also cannot accommodate the common phenomena such as that of misspelling. If I were to write “Pareiss” then we would intuitively think I have misspelt “Paris.” But Wetzel argues that the shape-theoretic nominalist cannot say this as there is no sense in which the tokens “Paris” and “Pareiss” are members of the same type (Wetzel, 2000). As the tokens have different spellings (ignoring any phonetic properties for now), they simply are not members of the same type. Many take these consequences as enough to show that shape-theoretic nominalism is false.

There are, though, nominalist ontologies that reject this restriction to *only* spelling and pronunciation properties as a guide to type-membership. These nominalists hold that there can be also types whose membership is determined by the resemblance of members (i.e., the tokens) with respect to other properties, including, but not limited to, semantic, grammatical, and intentional properties possessed by the tokens.

For example, modelled after bundle theories of objects, Miller (2019c) has argued that words are nothing more than bundles of certain (linguistic) properties. Under this ontology, tokens are bundles of properties, and types are collections of tokens, determined by the resemblance of properties that partly compose those token words qua members of that type. This allows Miller to hold that there are types whose members resemble with respect to any properties that we might take token words to possess, avoiding the limits within shape-theoretic nominalism, and thereby accommodating a wide range of intuitions we have about the sameness of words.

Indeed, by positing only resemblance relations between tokens, and not type-identity relations, the nominalist can also accommodate cases of nonexact resemblance by holding that some types may even be such that the members of the type nonexactly resemble in virtue of one (or more) of the properties possessed by those tokens. This, Miller argues, is able to explain our frequent talk about word-types where the tokens of that type are *relevantly* similar, where relevance reflects the various aims and purposes to which we put type-talk in our ordinary and scientific language.

Another, still nominalist approach has been to appeal to what Bromberger calls “archetypes” or “models” (Bromberger, 1989; Bromberger, 2011). Like other nominalists, Bromberger argues for a view of words that is intended to be able to maintain type-talk, without positing the existence of abstract entities. However, whilst the nominalism sketched above holds that types are collections of tokens, for Bromberger, tokens are members of quasi-natural kinds, and types are archetypes (or models) of those kinds. Types are models which are “object[s] so designed that, by finding the answers to some questions about it, a competent user can figure out the answer to questions about something else” (Bromberger, 1989, 62). Thus, to talk of the word “table” is not to talk about some genuinely existing abstract type, but instead to talk of a model that can be used to understand various tokens that resemble that model.

Bromberger, like other nominalists, argues that this modelling is reflective of our interests rather than being some metaphysical absolute: “no pair of objects stands (or fails to stand) in the model/modelled relation absolutely, but only relative to specific sets of questions, pairings of questions, and algorithms” (Bromberger, 1989, 63). This, Bromberger states means that “speaker-writer mind-brains endowed with grammars and lexicons leave no need for abstracta” (2011:496), in line with the nominalist denial of such entities.

A major weakness, or for some people a major benefit, of nominalism, in all its forms, is that it allows for a greater number of collections or models, with no collection or model ontologically more significant than another. Put another way, nominalism makes word-types more conventionalist than many ordinary speakers typically assume. This results in there being far more collections than those we typically recognize and accept, and those we do accept being only significant in that they are the most important for our contingent communicative or explanatory aims.

It also means that whether tokens are tokens of the same type becomes a relative matter, not absolute. The answer to questions about the “sameness” of tokens will depend on which collection of tokens we are interested in. To see this, consider again two tokens, “color” and “colour”. Typically, we think of these as tokens of the same type. Under nominalist ontologies, whether the tokens are of the same type will depend on which collection of tokens we are considering, or which model. Using Miller's ontology to illustrate, if the relevant type is one whose members possess (exactly) resembling semantic properties, then the tokens will be tokens of the same type. But, if the relevant type is one whose members possess (exactly) resembling orthographic properties (i.e., spelling), then they are not the “same” word. “color” and “colour” would then be tokens of different types.

It is important, though, that whilst being more conventionalist, the nominalist need not accept that “anything goes” when it comes to types. Some types will still be more gerrymandered than others, tracking unimportant (or even nonreal) distinctions amongst tokens and their properties. Nominalists can accept that our empirical research into words aims to find those types that track genuine, objective similarities and differences amongst

tokens, or those that are most pertinent to our explanatory aims. How objectionable this all is will likely come down to how firmly we want to retain the sorts of strong intuitions that initially motivated Platonic accounts against the idea that tokens are tokens of the same type “merely” in virtue of resembling each other.

4 | ELIMINATIVISM

The debate between the realist and the nominalist focuses significantly on the ontological status of word-types. The views disagree about whether word-types exist, but agree on the existence of word tokens. The last view I will outline is one strictly only about *token* words, and is the view that token words, and indeed all “standard linguistic entities,” do not exist as *concrete* entities. Words as we typically think of them qua ink patterns or sound waves are only an illusion, or are “intentional inexistent.” This is therefore an eliminativism about words.¹⁵

Here is an argument for the view. First, words cannot exist in space-time; they cannot be physical entities. The reason for this is that when we look closely at ink patterns and sound waves, we simply do not find objects instantiating the properties that are essential to words. In particular, we do not see the complex syntactic properties that words are taken to have by our best linguistic theories. In fact, when we look at acoustic strings, we do not even see any breaks between what we perceive to be distinct words. The subject matter of linguistics, which intuitively would include words, cannot be “physical” entities as the physical entities in the world, including words, do not have the sorts of properties of features that linguists investigate.¹⁶

What then is happening when we speak if we are not producing token words? The proposed answer is that words are “intentional inexistent”—they are “‘things’ that we represent and think of as ‘out there’, but which do not exist” (Rey, 2008:177). Rey calls this the “folieist” view, “according to which it is a kind of ‘folie à deux’ in which speakers and hearers enjoy a stable and innocuous illusion of producing and hearing standard linguistic entities” (2008:177).

This means that words are like perceptual illusions. In perceptual illusions we seem to see certain figures or shapes, but there is nothing more than that illusion. Similarly, when we seem to be uttering a word, we simply do not produce anything with the same structure or properties that words are typically taken to have. Speakers do make sounds, but those sounds are not words. Rather there is a shared illusion of the existence of words, such that the speaker and hearer can both infer certain intentional contents from those illusions, and this recognition of intentional content is all that there is to communication.

The major argument for this view is that positing words (and other linguistics entities) as being anything more than an illusion is simply not theoretically useful. That “all that need be true for the noises a speaker makes to have their intended effect is that they be perceived to have the tree structure that the speaker intended; and [...] something can be perceived to have a structure without it actually possessing that structure” (2006:554). Taking such entities to be “real” is not needed to explain the behavior of such entities within linguistic theory. Instead, the sound waves cause us to enter into perceptual states that are stable across time and people such that we can happily proceed as if words were real.

Objections to this view often focus on these claims that the view cannot accommodate some aspect of communication. For example, Barber (2006) argues that eliminativism about words cannot explain that communication involves (at least sometimes) the transfer of knowledge. This is because it conflicts with a principle that Barber argues underlies knowledge through testimony: the “no-false-lemmas principle” that holds that “belief is knowledge only if it is not based on falsehoods” (2006:412). Devitt (2006: 187) also raises objections to eliminativism due to its consequences for communication. Devitt argues that the guesswork that it requires for successful communication to occur—the “happy accident” that Rey relies on for speakers to be able to correctly infer the intentional content of others—could not account for the complex forms of communication we observe.

Devitt also takes on the initial motivation for eliminativism more directly, arguing that the lack of easily *perceptible* syntactic properties does not mean that such properties are not instantiated by words (and other linguistic

entities). This is because many relational properties are hard to perceive, but standardly taken to exist nonetheless (2006:185). Linguistic utterances thus, according to Devitt, really do instantiate syntactic properties, and hence it really is the case that the entities we typically take to be words have the properties that are essential to them.

Rey (2008) provides responses to these criticisms (and others) that I do not have the space to outline here. However, even if those responses are persuasive, it is still certainly the case that eliminativism about words requires a radical shift from our intuitive understanding of the nature of words. The question then is about how far we should be motivated by such intuitions, particularly if those common-sense intuitions can be shown to be in tension with empirical theorizing.

5 | SITUATING WORDS IN THE METAPHYSICS OF LANGUAGE

There is much more that could be said about the ontology of words, and it is still a field in its relative infancy. Words have been studied by philosophers, across a range of topics, but devoted explicit work on words has been, to borrow a phrase from Alward, more of “a trickle than a torrent” (2005:172). Here, I have focused on certain core ontological issues that have been discussed in the literature to date, especially the debate between the realist and nominalist about word-types. There remains, though, huge scope to develop alternative ontologies that may prove fruitful to our conception of words. For example, some have begun to explore the idea of word tokens as being constituted by, but distinct from, the matter that composes them. These views, drawing inspiration from well-known theories in the metaphysics of ordinary objects and debates about the statue and the lump, might open up new ways for accounting for various linguistic phenomena, and could bring work on the ontology of words more into line with developments in the wider field of social ontology.¹⁷

There are also wider metaphysical questions about words. For example, questions about persistence and change of *token* words, and there is a need to connect work on word-kinds with the wider literature on the metaphysics of kinds. Can a token of one type *become* a token of another type? Some scenarios suggest they can.¹⁸ If we accept the existence of word-kinds, then how should we understand the nature of those kinds? Are they natural kinds? Are words, as has often been suggested, like species? If so, what this means for our ontology of words will depend on various additional commitments we have about the metaphysics of kinds.¹⁹ These are merely indicative questions, and certainly not exhaustive of the range of metaphysical issues concerning words. Words have been understudied in the philosophical literature, at least from a metaphysical approach, and there is a lot of room for new developments on a range of topics.

There are also important questions about what a commitment to a particular ontology of words might mean for our other commitments in other philosophical domains. For example, one aspect that I have not touched upon, but certainly deserves research focus in the future, is the relationship between these ontologies and existing topics and theories in the philosophy of language. It would be extremely strange if our ontology of words did not have consequences for our broader philosophy of language and traditional issues therein. These connections have so far been very underexplored.²⁰

Looking at a bigger picture, the ontology of words should, in my view, be just one part of a broader metaphysics of language, as the investigation into a wide variety of questions arising from the nature of linguistic entities, be they linguistic objects like words, sentences, phrases etc., but also linguistic properties (such as grammatical properties). There are metaphysical questions about phrases, sentences, morphemes, phonemes, grammatical relations, and languages. All of these require their own specific ontological treatment, and any putative ontology of words should be minimally coherent with ontologies of these other linguistic entities.²¹

As touched upon above in passing, there also remains the central issue of how far an ontology of words (and metaphysics of language) should align with our linguistic theorizing. I suggested in this paper that there should be minimal coherence with empirical theorizing, but this leaves upon the question of coherence with which theories? Linguists, naturally, disagree about many aspects of language relevant to the ontology of words, and hence any

ontologist working on a theory about the nature of words may (implicitly or explicitly) make assumptions that cohere with some theories but conflict with others. It is beyond the scope of this paper to talk extensively about the relationship between philosophical work on words and linguistic theories that concern words, but the need for work on this connection is clear. Those working on the ontology of words (and the metaphysics of language more broadly) need to have some grasp on developments in the relevant areas of linguistics, and, going the other way, work in the ontology of words may uncover implicit metaphysical assumptions within linguistic theorizing. Personally, I am sceptical about attempts to read our ontology off of our linguistic theory, and we need to recognize that the aims of linguists are (at least often) different from those of ontologists. But, the ontology of words as a topic calls out for interdisciplinary connections and research.

To conclude, the ontology of words gives rise to many questions that need be answered within the broader enterprise of the metaphysics of language, with connections to existing research in various philosophical domains including (at least) social and political philosophy, philosophy of mind, and philosophy of science. The interdisciplinary potential is also clear to see. Language is studied, in various ways and to various ends, in a wide range of sciences, and there are unanswered questions concerning both how that work intersects with philosophical work on words and other linguistic entities, and about possible philosophical assumptions that lie within those endeavors. The correct ontology of words will ultimately be just one piece of this wider metaphysics of language.

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ENDNOTES

¹It is also the case that not all intentionally created patterns are words. It is more disputed whether there can be non-intentionally created words; see Munroe (2016) for a discussion of this in the context of theories in psycholinguistics. Accepting creation as stated is intended to require no commitment about what intentional acts or agency are, and what creatures are capable of such actions or possess agency.

²See Mason (2016) for an overview of the debate about the metaphysics of social kinds.

³I leave aside the difference between linguistic and non-linguistic communication as, *prima facie*, while non-linguistic communication may be complex, it does not involve *words*.

⁴The suggestion is that we can iteratively add suffixes and/or prefixes, *and* that the addition of each new suffix or prefix involves the creation of a new word. At some point these newly created words would be inexpressible - some could be infinitely long. The new words may not be accepted by ordinary language speakers, in part due to the cognitive difficulty of parsing them. However, this inability seems likely to be an issue of the working memory of humans, and may not be a good guide to the *existence* of these words.

⁵Due to space restraints, the precise details about how each theory is meant to be coherent with the empirical data cannot be discussed in this paper. For the most direct discussion of this, see Wetzel (2009) on Platonism and linguistics, Miller (2019c) on nominalist views and linguistics, and Nefdt (2019a) on structuralist accounts of words and linguistics.

⁶A relatively underdiscussed point in the literature is what mental tokens of words are in the sense of what sort or type of mental state we should take such words to be, or whether they are a distinct sort of mental state from those discussed elsewhere in the philosophical literature. This is an open question for both realist and nominalist ontologies, as supporters of both views are generally happy to posit token words as being either physical or mental.

⁷See Wetzel (2009: chap. 1) for an extended discussion of a range of data that the Platonist argues warrants the positing of types.

⁸See Wetzel (2009: chap. 2) for a response to this concern that leans heavily on responses developed in the context of Platonism in the philosophy of mathematics

⁹Of course, this requires a major shift in how we have typically thought about abstract entities. For more on this, see Irmak (2020).

¹⁰Note that Kaplan argues for this view as an alternative to what he calls “type-token models,” which is a form of Platonism that appeals to spelling and/or pronunciation as the criterion of word identity. The terminology is also tricky here. Kaplan dislikes the using the term “type,” but only due to its prior association with other ontological views. In this exposition, I am using the term “type” more neutrally, and hence we can equate types with Kaplan’s “continuants,” and tokens with his “stages.”

¹¹Similar views on the importance of history or origin on the individuation of word-types can be found in Irmak (2019), Millikan (1984: 74–75), and Sainsbury & Tye (2012: 4). See Miller (2019a) for an argument against taking historical properties as the correct criterion of individuation for words.

¹²Though Kaplan does hold that there are untokened sentence-types (and other linguistic types), holding that those types are abstract entities (2011:511).

¹³Though see Alward (2005) for a response to Cappelen’s concerns, but also further problems for Kaplan’s ontology relating to the role of words in communication.

¹⁴See in particular, amongst their other work, Goodman and Quine (1947), and Quine (1960, 1987).

¹⁵Again, this is a claim about the ontological status of token words, not types. The view is neutral about the existence of types; see Rey (2006, 2008: 181).

¹⁶See Nefdt (2019b) and Stainton (2014) for in depth discussion of the subject matter of linguistics specifically; here I focus only on how it contributes to the motivation for certain views about the ontological status of words. It is, though, worth noting that many writing in the nominalist-realist debate more recently have normally tried to stay neutral with respect to a related debate about the subject matter of linguistics.

¹⁷For discussion of these views concerning words and other social entities, see Epstein (2009, 2015) and Evnine (2016).

¹⁸For example, imagine a sentence written on a blackboard, reading “A bank is a financial institution.” Now someone erases all of the tokens, except the token “bank,” and then inscribes new tokens such that the sentence on the board now reads “The bank was home to many small creatures.” Intuitively, the token “bank” in the second sentence is of a different type than the token “bank” in the first, and difference in type indicates a difference in identity. However, *prima facie* the only changes to the token in this scenario are extrinsic, and changes in extrinsic properties are standardly taken to be insufficient for genuine change.

¹⁹See Wetzel (2009) for a defense of the view that words are like species. See Miller (n.d.) for a wider discussion of the nature of word-kinds in the context of recent work on the metaphysics of kinds.

²⁰For an example of this kind of work, see Miller (2019b) on the ontology of words and theories of quotation.

²¹See Nefdt (2019a) and Jackendoff (2018) for work that comments on this connection to the ontology of other sorts of linguistic entities. See also Santana (2016) for a good overview and discussion of the ontology of language debate more broadly.

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A Quinean Critique of Ostrich Nominalism

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CERTAIN OBJECTS ARE NATURALLY GROUPED TOGETHER; they have something in common. Realists claim that this fact demands explanation. For illustration, imagine that the world is restricted to the contents of a box, of which one might offer the following ordinary description:

Box World: There is a blue sphere. There is a green cube.
There is an orange sphere. There is a blue cone.

This world is supposed to share crucial features with our own. There are a number of objects that fall into natural groupings. For instance, the blue sphere and the blue cone naturally go together. They *share something* that the others don't. Realists posit an entity that is *instantiated* by both the sphere and the cone in order to explain what they have in common. This entity is called *a universal*. It is a *single thing* that the *many objects* have in common. Following David Armstrong (1980), let us call the realist's demand for an explanation of this grouping *the one-over-many problem*.

Nominalists see no reason to posit universals. According to the nominalist, the world contains only particular things. In this paper, we are concerned with a specific brand of nominalism that makes no concessions in response to the realist's demand for explanation. A nominalist of this brand acknowledges that the sphere and cone have something in common: they are both blue. However, she offers nothing further to explain this commonality. Armstrong (1978: 16; 1980) calls a nominalist of this variety an *ostrich*, since she purportedly sticks her head in the sand in response to the one-over-many-problem.¹

The ostrich says that she does not posit or ontologically commit to universals. To support this claim, ostrich nominalists universally

1. Devitt (1980) rejects the label 'ostrich nominalist' and argues that, instead, philosophers who posit universals in response to the so-called one-over-many problem are *mirage realists*: they adopt realism as a response to a problem that isn't really there. Despite this criticism, we shall use the label 'ostrich nominalist' for those philosophers who (i) do not posit universals, and (ii) do not view the realist's explanatory demand (*i.e.* the one-over-many-problem) as legitimate. We use this label solely for ease of exposition; the use of it should not be taken to indicate that we view the realist's explanatory demand as legitimate.

invoke Quine's criterion of ontological commitment. The ostrich follows Quine in holding that an agent commits to the ontology of the theory she endorses, and that the agent should endorse the best theory available to her. The theory's ontology is given by what it says there is:

Quine's Criterion: A theory has an ontological commitment to *Fs* if and only if it includes or entails a sentence that says that there are *Fs*.²

According to the ostrich, the description of the box world given above is the best theory available. She argues that this theory does not entail that there is an element that the blue sphere and blue cone both instantiate. Thus, the ostrich's preferred theory of the world does not have an ontological commitment to universals. Moreover, the ostrich argues that there is no reason to supplement the description of the box world given above to produce a theory entailing that there are universals. The ordinary description, argues the ostrich, is not explanatorily worse off than a theory that also says there are universals: additional explanatory principles pressed by the realist (such as truth-maker principles) are either illegitimate or fail to motivate realism.

If the ostrich is correct that her theory meets any legitimate explanatory burden that the realist's theory does, then conventional wisdom says she is in a strong position. As Devitt (1980) says in his argument against positing universals in response to the one-over-many problem:

In ontology, the less the better. Therefore the realist makes us ontologically worse off without explanatory gain. [97–8]

2. "We can very easily involve ourselves in ontological commitments by saying, for example, that there is something (bound variable) which red houses and sunsets have in common; or that there is something which is a prime number larger than a million. But this is, essentially, the only way we can involve ourselves in ontological commitments: by our use of bound variables" (Quine 1953b: 12). "When I inquire into the ontological commitments of a given doctrine or body of theory, I am merely asking what, according to that theory, there is" (Quine 1951b: 203–4).

According to this received view, the ostrich's preferred theory has the advantage of a more parsimonious ontology than the realist's when both are assessed by Quine's criterion. As the ostrich presents the matter, the realist's theory says that there are more things: it posits universals in addition to the particulars.

In this paper, we'll grant that standard demands for explanation should fail to move the ostrich. However, we'll argue that this is beside the point. The ostrich's claim to parsimony is simply wrong, even according to Quine's criterion. We'll argue that properly counting the ostrich's commitments using Quine's criterion yields a less parsimonious ontology than that of her realist rivals. To make our position clear: we are not trying to argue that any possible nominalist theory is less parsimonious than any possible realist theory. Rather, we aim to show that nominalist theories that have a chance of describing the actual world as we believe it to be are less parsimonious than corresponding realist theories. To make this point, we will continue to use our "box world" as a model of the actual world.

We concede that our claim may be surprising given the history of the debate, which often frames the choice between the theories as a tradeoff between ideology and ontology. We maintain, however, that this novelty issues from severe misinterpretations of Quine's criterion. In the course of this paper, we set out what we take to be the correct application of Quine's criterion and argue that this is precisely how Quine himself views the matter.

In Section I, we locate the disagreement between the ostrich and the realist more precisely. In Section II, we offer our new argument purporting to show that the ostrich has a less parsimonious ontology than the realist. The argument rests on our interpretation of Quine's criterion. As we understand this criterion, a theory has an ontological commitment to things of a sort just in case it says that there are things of that sort. (We use 'sorts', 'categories', and 'kinds' interchangeably.) We mean by this that a theory has an ontological commitment to, say, dogs in virtue of including the sentence 'there are dogs'. Yet, this theory needn't have ontological commitments to any specific dogs, since it

need not entail sentences that say there are any specific dogs. It need not say, for instance, that Fido or Rover exists. Similarly, a theory may have an ontological commitment to each particular dog without having an ontological commitment to *dogs*. This would happen if the theory says or entails of each dog (Fido, Rover, and so on) that it exists, but fails to say or entail that they are dogs. Importantly, in speaking of the sentence ‘there are dogs’ as generating an ontological commitment to things of the sort *dogs* we don’t mean to reify sorts in a way that would prejudice the debate. We use this vocabulary solely to simplify discussion by marking the distinction between an ontological commitment to dogs in general and ontological commitments to some specific dogs. The remainder of the paper is devoted to defending the argument from Section II — and, in particular, our interpretation of Quine’s criterion.

I.

The ostrich and realist disagree about whether to endorse a theory that ontologically commits to universals. One’s theory can be divided into the set of sentences that one endorses and their logical consequences. This suggests that an ostrich and a realist may disagree at one of three places: (a) they may agree about which theory to endorse but disagree about how to assess the ontological commitments of this theory; (b) they may disagree about whether to endorse a given sentence or a set of sentences; or, (c) they may agree about whether to endorse a set of sentences but disagree about the logical consequences of these sentences. Disputes between nominalists and realists have, in fact, taken all three forms.

We will briefly survey these disputes as they pertain to the description of the box world. The ostrich and the realist agree that this description is fitting. Yet they disagree about whether this should lead them to posit universals. We will suggest that, from a Quinean perspective, the most important disagreements between the ostrich and the realist concerning the description of the box world are of type (c).

Type (a) disagreements

Ostrich nominalists propose to assess the ontological commitments of a theory using Quine’s criterion: a theory ontologically commits to what it says there is. None of the statements in the description of the box world above says that there are universals. Moreover, the ostrich denies that these statements entail a sentence saying that there are universals. Thus, according to the ostrich, Quine’s criterion dictates that these statements alone are not sufficient to commit a theory including them to universals.

Some realists, in arguing for the need to posit universals, have departed from Quine’s criterion by supposing that a theory ontologically commits to an entity (*i.e.*, a universal) for every predicate deployed in expressing it.³ Others, including (perhaps most prominently) Armstrong, have defended an alternative to Quine’s criterion known as truth-maker theory.⁴ According to a truth-maker criterion, a theory is ontologically committed to the entities required to make its sentences true.⁵ We will briefly address the status of this truth-maker alternative

3. See, *e.g.*, Bergmann (1952: 430) and Russell (1912: 93–4).
4. Schaffer (2008) and Devitt (2010) have both argued that a truth-maker criterion of ontological commitment isn’t really an alternative to Quine’s criterion. While we are sympathetic to the arguments of these two philosophers, we have reservations about fully committing to this view here. Our reservations issue in large part from the fact that there are truth-maker theorists who obviously view the truth-maker criterion (perhaps wrongly) as a genuine alternative to Quine’s. Thus, Armstrong: “To postulate certain truthmakers for certain truths is to admit those truthmakers into one’s ontology. The complete range of truthmakers admitted constitutes a metaphysics [...]. I think that proceeding by looking for truthmakers is an illuminating and useful regimentation of the metaphysical enterprise [...]. But this raises the question of Quine, and the signaling of ontological commitment by what we are prepared to ‘quantify over’. Why should we desert Quine’s procedure for some other method? The great advantage, as I see it, of the search for truthmakers is that it focuses us not merely on the metaphysical implications of the subject terms but also on their *predicates*,” (2004: 23).
5. Armstrong (1997: 113–16; 2004) further develops the notion of a truth-maker. See also Heil (2003), and Cameron (2008: 4) for defenses of truth-maker criteria of ontological commitment.

to Quine's criterion, since appeals to it have been such a prominent feature of arguments against ostrich nominalism.

According to Armstrong, a truth-maker criterion of ontological commitment delivers different results from the Quinean criterion. In particular, Armstrong says that for Quine "predicates do not have to be taken seriously in considering the ontological implications of the statements one takes to be true" (Armstrong 1989: 89). We take ontological implications to be ontological commitments, since Armstrong says that he is comparing truth-maker theory to Quine's criterion of ontological commitment.⁶ That is, we take the ontological implications of a statement to be the ontology that one endorses if one takes that statement to be true. To illustrate his statement, Armstrong says that truth-maker theory requires an ontological ground (what Armstrong calls "a difference in the word"), which accounts for the difference between, *e.g.*, the case in which 'red' applies to a surface and the case in which 'green' applies to it. We take this to mean that the truth-maker theorist supposes that if person A endorses the statement 'Surface S is red', and person B endorses the statement 'Surface S is green', then person A and person B suppose that there are different things in the world. In other words, their theories *posit* or *ontologically commit* to different things. Armstrong seems to think that the proponent of Quine's criterion doesn't agree with this: she thinks, according to Armstrong, that person A and person B do not suppose that there are different things in the world. And if the two do not suppose that there are different things in the world, then it seems that they have the same ontological commitments.

At this point we want to sound a note of protest. Quine's criterion assigns a great deal of importance to predicates in the assessment of ontological commitment. Returning to Armstrong's example, even the Quinean concedes that 'There are red surfaces' and 'There are green surfaces' require different things to be in the world in order for either to be true. The former requires red surfaces; the latter requires green surfaces. Similarly, one who endorses 'Surface S is red' ontologically

6. Armstrong (2004) is quite explicit about this in the quote cited in our footnote 4 above.

commits to red surfaces, since the sentence entails that there are red surfaces. On the other hand, one who endorses 'Surface S is green' ontologically commits to green surfaces. Thus, the two theories have different ontological commitments, or require different ontological grounds. One who endorses 'Surface S is red' posits something in the world that is different from that which is posited by one who endorses 'Surface S is green'.

In sum, the Quinean about ontological commitment specifies the ontological commitments of the statement using the very predicates that follow 'there are'. So, predicates are relevant to the ontological commitments of a statement. We will develop this point more fully when we compare the realist's and nominalist's theories with respect to their ontological parsimony.⁷

Type (b) disagreements

Many realists attempt to argue for the existence of universals by positing substantive (or even purportedly trivial) principles of explanation. For instance, some realists endorse principles, such as the following: if *a* is green, then *a* is *something* (namely, green).⁸ Other realists explicitly endorse truth-making principles that in conjunction with the ordinary description provided above entail that there are universals. Ostriches offer a variety of responses to these arguments. Some simply deny that the relevant principles are true.⁹ Others accept the principles but deny that

7. We have framed Quine's criterion in terms of what a theory says or entails there is. Sometimes Quine frames the criterion in terms of what must exist in order for the theory to be true. We maintain that this modal formulation of the criterion has the same consequences as the formulation in terms of 'says that'. As before, the truth of a theory may necessarily entail that there are *Fs* without necessarily entailing that there are any specific *Fs*. A theory that says that there are green things cannot be true unless there are green things. Moreover, in our view, Quine uses 'must' in this context, as he does in others, to mean logical entailment. So 'what must exist if ...' just means 'what the theory entails exists'. In particular, it cannot be interpreted to mean 'metaphysically necessary'. Burgess and Rosen (1999: 226) make a similar point.

8. See, *e.g.*, Schiffer (1996).

9. Devitt (1980) rejects principles of both sorts.

they have the entailments claimed by the realist.¹⁰ Rather than entering debates over these issues, we are going to present a novel objection to ostrich nominalism that doesn't rely on these substantive principles.

Type (c) disagreements

Some realists would claim that the ordinary description of the box world (properly spelled out) simply entails that there are universals. Ostriches deny this. When disagreements over entailments arise, Quinean methodology dictates that a theory stated in ordinary (or even scientific) language should be replaced by — that is, *regimented* into — a notation that makes entailments perspicuous.¹¹ The resulting sentences need not uncover the “hidden meaning” of the originals, but should constitute the best theory that captures what is scientifically respectable in the original notation.¹² Essentially, disagreements of type (c) (disagreements over which inferences are valid) should be converted into disagreements of type (b) (disagreements over which sentences to endorse). Given this methodological principle, disagreements of the sort that occur between the realist and the ostrich nominalist become disagreements over which regimented theory to adopt. The realist thinks that the best regimented theory arising from

10. Sellars (1962) and Van Cleve (1994) deny that from the claim that a is F and the principle that if a is F , then a is *something*, one can infer that there is something which a instantiates. Some philosophers accept truthmaker principles but deny that they, combined with the observed facts, entail that there are universals. For discussions, see Parsons (1999), Lewis (2003), Lewis and Rosen (2003), MacBride (2005), and Melia (2005).
11. In particular, theories are to be regimented into the language of first-order logic; the relevant notion of entailment is first-order entailment.
12. The goal of regimentation is to construct a *new* theory that fulfills the function of an old theory but clarifies it in certain respects. As Quine says, “we do not claim to make clear and explicit what the users of the unclear expression had unconsciously in mind all along. We do not expose hidden meanings, as the words ‘analysis’ and ‘explication’ would suggest; we supply lacks. We fix on the particular functions of the unclear expression that make it worth troubling about, and then devise a substitute, clear and couched in terms to our liking, that fills those functions” (1960: 258–9). Different proposals for regimenting a theory *just are* different theories.

the ordinary description entails that there are universals. The ostrich denies this. We now turn to developing the realist's and ostrich's proposed regimentations. In the next section we compare their relative parsimony in order to argue that the realist's proposed regimentation results in a better theory than the ostrich's proposed regimentation.

The ostrich denies that one can infer that there is a universal, *blue*, from the sentence ‘there is a blue sphere’. The expression ‘blue’ in this statement is not accessible to quantification. This suggests that the ostrich thinks that the best regimented theory arising out of the description of the box world can be axiomatized by combining some basic principles with the following sentences:

$$\exists x (\text{Blue}(x) \wedge \text{Sphere}(x)) \quad \exists x (\text{Green}(x) \wedge \text{Cube}(x))$$

$$\exists x (\text{Orange}(x) \wedge \text{Sphere}(x)) \quad \exists x (\text{Blue}(x) \wedge \text{Cone}(x))$$

In these sentences, expressions such as ‘Blue(...)’ occur as monadic predicates. They are inaccessible to quantification in standard first-order logic. This means that there is no sense in which the ostrich's theory entails that there is a universal, *blue*, that the sphere has.

A realist of the kind under consideration believes that it follows from the description of the box world that there is such a thing as the universal *blue*. Moreover, she believes that this entailment follows without additional substantive principles. Consequently, our realist believes that ‘blue’ is accessible to quantification in the sentences comprising the description. This suggests that, in her view, these sentences are best regimented as follows:

$$\exists x (\text{IS}(x, \text{blue}) \wedge \text{IS}(x, \text{spherical}))$$

$$\exists x (\text{IS}(x, \text{green}) \wedge \text{IS}(x, \text{cubical}))$$

$$\exists x (\text{IS}(x, \text{orange}) \wedge \text{IS}(x, \text{spherical}))$$

$$\exists x (\text{IS}(x, \text{blue}) \wedge \text{IS}(x, \text{conical}))$$

In this regimented theory, 'IS(...,...)' occurs as a dyadic predicate and means roughly "instantiates". This expression is inaccessible to quantification in the realist's theory. By way of contrast, expressions such as 'blue' and 'spherical' occur as names in the realist's theory and are thus accessible to quantification.¹³ This means that the realist's theory entails that there are universals such as *blue* that the sphere instantiates. In order to fully secure this entailment, the realist needs two supplementary principles linking being instantiated with being a universal, and being a particular with the failure to be instantiated:

Universal: $\forall x$ (Universal(x) iff $\exists y$ IS(y, x))

Particular: $\forall x$ (Particular(x) iff $\neg \exists y$ IS(y, x))

In the statement of these principles, the realist makes use of two additional monadic predicates, 'Universal(...)' and 'Particular(...)'. These expressions are inaccessible to quantification, just as expressions such as 'Blue(...)' are in the ostrich's theory. We note in passing that some philosophers might treat these as definitions rather than principles, but for the sake of argument we will treat 'Universal(...)' and 'Particular(...)' as primitives.¹⁴

II.

Now for the crucial question: which of these regimented theories is better? Above, we mentioned that Devitt argues that the ostrich nominalist's theory is to be preferred on the grounds that it is more parsimonious. The ostrich's claim to ontological parsimony derives from the

13. It is open to the realist to develop a theory on which these expressions are replaced by definite descriptions. These descriptions may be purely qualitative if we assume that there are no indiscernible universals. Or the realist may replace the names with individualized descriptions in the manner of Quine's 'pegasizes' (1953b: 8).

14. We are agnostic about whether particulars or universals are essentially so. Moreover, we take no stand about whether particulars are essentially uninstantiated. For discussion of the modal status of these theses, see MacBride (1999; 2005b).

claim that her theory has fewer ontological commitments. She claims that the realist's theory shares all of the ontological commitments of her theory but is, in addition, committed to universals. Thus, the realist's theory purportedly has a greater number of ontological commitments than the ostrich's. We believe that this assessment is wrong and that realism has a more parsimonious ontology than ostrich nominalism does.

According to Quine's criterion, ontological commitments arise from existential sentences. Each theory entails a large number of existential sentences. Indeed, certain natural supplements to either theory of the box world (such as that there are only four objects in the box) will result in infinitely many existential entailments that are neither logically nor necessarily equivalent.¹⁵ Presumably, one can compare the ontological parsimony of these two theories without taking into account all of their existential entailments. Therefore, we restrict our attention to a subset of existential entailments of each theory, which both the realist and ostrich will agree generate all of the ontological commitments relevant to comparing the ontological parsimony of their theories. We will offer a more theoretical motivation for this choice of existential sentences in Section VII.

The ostrich will agree that her theory has the following existential entailments and that these entailments generate all of the ontological commitments relevant to comparing the relative parsimony of her theory with that of the realist.¹⁶

$\exists x$ Blue(x) $\exists x$ Sphere(x) $\exists x$ Green(x)

$\exists x$ Cube(x) $\exists x$ Orange(x) $\exists x$ Cone(x)

What commitments do these sentences generate for the ostrich's theory? By Quine's criterion of ontological commitment, the ostrich's

15. See our footnote 27.

16. This is not to say that the ostrich's entire theory can be reconstructed from these existential sentences. All that we are claiming is that the ostrich will point to these as sufficient to determine the relative ontological parsimony of her theory. See, for example, the quote from Parsons (1999) in Section III.

theory has (1) an ontological commitment to *blue things*, (2) an ontological commitment to *spheres*, (3) an ontological commitment to *green things*, (4) an ontological commitment to *cubes*, (5) an ontological commitment to *orange things*, and (6) an ontological commitment to *cones*.

What about the realist's theory? The ostrich will agree that the realist's theory has the following existential entailments:

$$\begin{array}{ll} \exists x \exists y \text{ IS}(x, y) & \exists x \exists y \text{ IS}(y, x) \\ \\ \exists x \text{ Universal}(x) & \exists x \text{ Particular}(x) \end{array}$$

She will also agree that these entailments generate all of the ontological commitments relevant to comparing the relative parsimony of the realist's theory with that of her own. By Quine's criterion, the realist's theory has (1*) an ontological commitment to *instantiating things*, (2*) an ontological commitment to *instantiated things*, (3*) an ontological commitment to *universals*, and (4*) an ontological commitment to *particulars*.

Comparing the commitments of the ostrich's and realist's theories, we find ourselves perplexed by the ostrich's claim that her theory has a more parsimonious ontology than the realist's does. Counting ontological commitments naïvely, ostrich nominalism looks to have a *less* parsimonious ontology than does realism. By our count, the ostrich's theory has six ontological commitments. By way of contrast, we count the realist's theory as having four ontological commitments. Six is greater than four. Therefore, the ostrich's theory is less parsimonious than the realist's.

There are a number of places at which our naïve assessment of the relative parsimony of realism in comparison to ostrich nominalism will be challenged. These challenges can best be addressed by enumerating the principles by which are counting:

Counting Principle 1

The ontological commitments listed in (1)–(6) are all distinct from each other, as are the ontological commitments listed in (1*)–(4*).

Counting Principle 2

(1)–(6) and (1*)–(4*) are the only ontological commitments of the respective theories relevant to determining which is more parsimonious.

Counting Principle 3

One determines which of two theories is more ontologically parsimonious by counting their respective ontological commitments.

In what follows, we defend each of these counting principles.

In light of the current state of the debate, Counting Principle 1 will likely be viewed as the most controversial. The ostrich will protest that it turns out that all of the blue things are either spheres or cones. So her theory's ontological commitment to blue things, the ostrich argues, is not in fact distinct from its ontological commitment to spheres and its commitment to cones. We will argue that these commitments are distinct – that according to Quine's criterion, a theory's ontological commitment to *things of a given sort* (blue things) is distinct from its commitments to specific objects (the specific cone and the specific sphere) even if the objects happen to fall under that sort. We will spend a substantial portion of this paper defending this claim, and anyone who understands this defense will understand the gist of our argument.

Counting Principles 2 and 3 might be viewed as less controversial than Counting Principle 1. However, in the final sections of this paper, we'll discuss a difficulty with assessing parsimony by simply counting the theory's ontological commitments to things of a sort. This difficulty will lead us to argue that ontological commitments to things of *explanatorily basic* sorts weigh more heavily in the assessment of ontological parsimony than ontological commitments to things of other sorts. Thus, we will amend Counting Principles 2 and 3 as follows:

Counting Principle 2*

(1)–(6) and (1*)–(4*) are the only ontological

commitments to things of explanatorily basic sorts of the respective theories.

Counting Principle 3*

A theory with many ontological commitments to things of explanatorily basic sorts is less parsimonious than a theory with few ontological commitments to things of explanatorily basic sorts.¹⁷

III.

In this section, we defend Counting Principle 1. Specifically, we defend the claim that the ontological commitments listed as (1)–(6) are distinct, and that, consequently, the ostrich has at least *six* ontological commitments. The contrasting view, defended by the ostrich, is that she has only *four* ontological commitments. According to the ostrich, her ontology includes only the following: a cone, a cube, and two spheres. If the ostrich is right, not all of the commitments listed in (1)–(6) are distinct, and Counting Principle 1 is a form of double-counting.

It is important to understand the ostrich's position and to see why it is wrong. As the ostrich understands the Quinean conception of ontology, an ontological commitment to green things just is an ontological commitment to the specific things that turn out to be green. Therefore, since all of the green things in the box turned out to be cubes, a theory that says that there are green things carries no commitments beyond those of a theory that says only that there are cubes. So the ostrich charges us with double-counting her commitments because

17. We don't want to commit to the view that ontological commitments to things of explanatorily non-basic sorts carry no weight in the assessment of ontological parsimony. Rather, our view is that one assesses the relative parsimony of two theories by determining which has more ontological commitments to things of explanatorily basic sorts. If the two theories have the same number of ontological commitments to things of explanatorily basic sorts, one then compares the theories' respective ontological commitments to things of explanatorily non-basic sorts.

we suppose that (3) the ontological commitment to green things is distinct from (4) the ontological commitment to cubes.

The ostrich's way of thinking about ontological commitment validates the following two inferences, which we will argue are problematic:

(I-1) Theory T has an ontological commitment to *Fs*. Therefore, there are some *Fs* to which it is ontologically committed.

(I-2) All *Fs* are *Gs*. Therefore, in saying that there are *Fs*, a theory incurs no ontological commitments beyond those it incurs in saying that there are *Gs*.

That the ostrich reasons in accordance with these principles is reflected in Armstrong's claim above, which is echoed by Josh Parsons in defense of ostrich nominalism: "[A]ccording to Quine's criterion of ontological commitment, to say 'There is a red surface' commits us to no more things than 'There is a surface' commits us to" (Parsons 1999: 327). As Parsons understands Quine's criterion, a theory, T_1 , that says there are red surfaces does not have any ontological commitments beyond those of a theory, T_2 , that says there are surfaces. The idea is that when one ontologically commits to surfaces, one's theory thereby includes in its ontology all of the things that turn out to be surfaces. So the theory does not gain any additional ontology if it also includes the claim 'There are red surfaces'. This form of reasoning generalizes to (I-2).

By way of contrast, we endorse Counting Principle 1 because we believe (i) that a theory's ontological commitment to things of a given sort is distinct from its potential commitments to the specific things that happen to fall under that sort, and (ii) that if one can ontologically commit to *Fs* without ontologically committing to *Gs*, then an ontological commitment to *Fs* is distinct from an ontological commitment to *Gs*. From (i) it follows that a theory's commitments to red things, green things, cubes, and so on are different from its commitments to the specific things that happen to be red, green, cubes, and so on. As a result, one could have any of these commitments without having the others. Thus, from (ii), an ontological commitment to green things

differs from an ontological commitment to cubes, even if all and only the green things are cubes (as is the case in the box world). We believe that there are decisive reasons for anyone who endorses Quine's criterion to agree with us about (i) and (ii).

Our argument for (i) assumes that Quine's criterion is correct and demonstrates that (I-1) and (I-2) are false. The failure of (I-1) and (I-2) implies that the ostrich's way of thinking about ontological commitment is wrong, and that, consequently, we should think of a theory's ontology as including the things of various sorts that it says there are. The substance of our argument will substantially mimic Quine's in "On What There Is" as we understand it, though the argument is independent of exegetical considerations.

(I-1) is problematic because it entails that we can never criticize our opponents for having too many ontological commitments. If (I-1) is correct, a theory's ontological commitments include only things that exist. For example, the ostrich might complain that the realist's theory is profligate because it has an ontological commitment to universals. But (I-1) precludes her from doing so. Once the ostrich says that the realist theory commits to universals, (I-1) requires the ostrich to concede that there are universals. As Quine (1953: 1–3) says in discussing the tangle of problems he calls Plato's Beard, when the party saying that there are fewer things tries to "formulate [her] difference of opinion" she seems "to be in a predicament". She "cannot admit that there are some things which [her opponent] countenances but [she does] not".

Quine's criterion, as we understand it, is specifically designed to avoid this consequence. It says that a theory ontologically commits to *Fs* just in case it says that there are *Fs*. Starting with the premise that a theory can say that there are *Fs* even if there aren't any, it immediately follows that one can ontologically commit to *Fs* even if there are no *Fs*. There are no, say, unicorns, but some theories say that there are unicorns. It would be odd indeed (particularly from a Quinean perspective) to claim that such theories are not ontologically committed to unicorns. For this reason, we believe that any understanding of Quine's criterion that validates (I-1) is incorrect.

The failure of (I-2) is more relevant to our criticism of ostrich nominalism. Parsons seems to invoke (I-2) to argue that theory T_1 has no more ontological commitments than theory T_2 despite the fact that T_1 says that there are red surfaces and T_2 does not. We believe that this inference is invalid for reasons Quine explicitly cites in support of his criterion of ontological commitment.

The problem with Parsons's way of thinking can be shown using his own example. By Quine's criterion, T_1 is ontologically committed to *red* surfaces (because it says that there are some), whereas T_2 is not committed to *red* surfaces (because it doesn't say that there are any). Thus, T_1 has an ontological commitment that T_2 lacks, even if they agree on which specific surfaces there are. So, a theory's commitment to red surfaces in general cannot be identified with its commitments to specific red surfaces (or, for that matter, its commitments to some specific surfaces).

This point can be generalized. The ostrich thinks of a theory's ontological commitment to *Fs* as a commitment to all of the specific things that turn out to be *F*. Thus, if all objects fall under some predicate '*F*', then any two theories that say that there are *Fs* share their ontologies. Consider ostrich nominalism and realism. Both of these theories agree that there are *self-identical things*, so they ontologically commit to self-identical things. As a matter of fact, everything falls under the predicate 'is a self-identical thing'. So on the ostrich's understanding, any theory that commits to self-identical things commits to all of the specific things that happen to be self-identical. Thus, the realist and ostrich are committed to the same specific things. But since specific things exhaust each theory's ontology, the realist and ostrich have the exact same commitments! Certainly this line of reasoning is wrong. The realist and ostrich have different ontologies. So any understanding of Quine's criterion that says otherwise is mistaken.

Quine (1953b: 1) offers a similar reason for rejecting (I-2). He notes that everyone will accept 'Everything' as an answer to the question 'What is there?'. But surely this doesn't entail that every theory has the same ontology. The interesting ontological disagreements concern cases — namely, whether there are such things as gods, universals, minds,

or material objects. A theory's ontological commitments should reflect its stance on these matters. For example, a theory that says there are self-identical things need not be committed to green things, even if it turns out that green things are among the self-identical things. To accept this, however, is to reject (I-2).

Our argument for (ii) from above relies on considerations already raised. Observe that a theory's ontological commitment to *Fs* is generated by the fact that it entails the sentence 'There are *Fs*'. A theory's ontological commitment to *Gs* is not generated by this entailment, but rather is generated by the fact that it entails 'There are *Gs*'. One could have either of these entailments without having the other. Thus, a theory could have either of these ontological commitments without having the other. So, by Leibniz's Law, the commitments must be distinct.¹⁸ For example, a theorist who asserts the existence of *green things* has an ontological commitment to *green things*. However, unless she also asserts that there are *cubes*, she does not have an ontological commitment to *cubes*. Indeed, asserting the existence of green things is compatible with *denying* that there are any cubes (and vice versa). Since the commitments are not generated in the same way, and since one could have one commitment without the other, they are different commitments. Counting Principle 1, which counts ontological commitments to things of different sorts as distinct ontological commitments, follows immediately.

We note in passing that Quine explicitly agrees with us about (i), and seemingly about (ii) as well, when he says:

My remaining remark aims at clearing up a not unusual misunderstanding of my use of the term 'ontic commitment'. The trouble comes of viewing it as my key ontological term, and therefore identifying the ontology of

18. Suppose that theory T_1 ontologically commits to *Fs* but not *Gs*, and that theory T_2 ontologically commits to *Gs* but not *Fs*. It follows that any theory T_3 that commits to *Fs* and to *Gs* thereby has at least two ontological commitments. T_3 shares a commitment to *Fs* with T_1 , and it shares a commitment to *Gs* with T_2 . But T_1 's commitment to *Fs* is not shared by T_2 . So, T_1 's commitment to *Fs* is distinct from T_2 's commitment to *Gs*. Thus, T_3 's commitment to *Fs* is distinct from its commitment to *Gs*.

a theory with the class of all things to which the theory is ontically committed. This is not my intention. The ontology is the range of the variables. Each of the various interpretations of the range (while keeping the interpretations of predicates fixed) might be compatible with the theory. But the theory is ontically *committed* to an object only if that object is common to all those ranges. And the theory is ontically committed to 'objects of such and such kind', say dogs, just in case each of those ranges contains some dog or other. [Quine 1969b: 315]

This is also how many of Quine's early expositors understood him. Church — echoing a point made in several places by Quine himself¹⁹ — cites as an advantage of Quine's criterion that it does mark the distinction between a commitment to things of a sort and a commitment to specific things that fall under that sort: "[I]f an ontological issue concerns the existence, not of some particular entity, but of entities of a certain category, then the criterion of ontological commitment which has reference to the use of a variable is more direct, and may take precedence over the criterion which has reference to the use of a name" (1958: 1009). From the fact that Quine thinks one ontologically commits not merely to things, but to things of a given sort, it follows straightforwardly that, *on his own view*, a theory that says that there are green things has an ontological commitment to *green things* — and not merely to specific things that are green. On the other hand, a theory

19. A related point is made by Stevenson (1976), who points to (Quine 1953b: 13; 1953c: 103; 1969a: 96–7). It follows from the fact that an ontological commitment to green things is not a relation to specific green things that ontological commitment is an intensional relation. This intensionality was noticed by Church (1958: 1012–14 (footnote 3)). Church illustrates this claim by citing the fact that an ontological commitment to unicorns is not the same as an ontological commitment to purple cows. For a discussion of the function of bare plural expressions such as 'lions', see Carlson and Pelletier (1995). For reasons that we will discuss below, we reject as misunderstandings arguments that infer from Quine's general extensionalism that he takes ontological commitment to be an extensional notion (see discussions in Cartwright 1954, Chihara 1968, and Brogard 2008).

that asserts the existence of cubes does not thereby have this commitment, even if all and only the cubes turn out to be green.

IV.

Quine's (1951a) distinction between ontology and ideology might be offered as evidence against our interpretation. The distinction is rarely spelled out explicitly, but the following provides a rough idea. A theory's ontology is what it says or entails there is. A theory's ideology is to be assessed in terms of the meaningful expressions — often predicates — that are required to articulate it, though a precise measure is rarely given. Quine introduces the distinction as a response to arguments from philosophers such as Gustav Bergmann (1952: 430) who hold that the occurrence of a meaningful predicate in a theory automatically commits the theory to an ontology of properties or universals (see also Russell 1912: 93–4). These philosophers maintain that one is ontologically committed to the meanings of expressions contained in the sentences entailed by one's best theory. Quine rejects this view. According to Quine, theories expressed using more predicates and other expressions make use of more ideological resources than theories expressed using fewer.

The distinction between ontology and ideology would be problematic for us if it meant that different predicates play no role in generating ontological commitments. Some ontologists have interpreted Quine's distinction in this way. Oliver (1996) seems to be among them. According to Oliver, a theory's ontological parsimony is determined solely by the number of specific individuals it says there are:²⁰

The ontological economy of a theory is measured by the number of entities within its ontology. The ideological

20. Lewis (1992) offers a related distinction between *whether things are* and *how they are*. An ostrich might attempt to apply this distinction to our discussion as follows: what a theory says about *whether things are* is its ontology and what a theory says about *how they are* is its ideology. This suggestion, however, does not lend any support to the thesis that a theory's ontological commitments are given by the specific individuals it says there are. Whether there are (*e.g.*) *dogs* or *things that are red* is a matter of *whether things are*, and yet doesn't concern the existence of any specific individuals.

economy of a theory is measured by the number of primitive, undefined predicates within its ideology. [3]

On Oliver's understanding, if two theories have minimal models of the same cardinality, then they are ontologically equivalent, even if one theory asserts that there are individuals of more sorts than the other.

In our view, this rests on a misunderstanding of Quine's distinction. Though the distinction is meant to allow for the fact that the use of a predicate in expressing a theory does not ontologically commit that theory to the referent of that predicate, it in no way follows from this that the use of a predicate brings no ontological commitments — and thus no effect on ontological parsimony — in its wake. Once again, a theory that says there are red surfaces has an ontological commitment to red things, but a theory that says merely that there are surfaces lacks this ontological commitment. Simply put, ideological differences can give rise to ontological differences.

It may be useful to provide another example. A theory expressed in a language containing a predicate such as 'unicorn' has more ideological resources than one that lacks this predicate. However, a theory may make use of this ideological resource without thereby having an ontological commitment to things of the sort *unicorn*. (The theory may even include the sentence 'There are no unicorns'.) In order to acquire this ontological commitment, the theory must include the claim that there are unicorns. The ideological resource 'unicorn', though it does not automatically generate a new ontological commitment, makes it possible for the theory to acquire one.

This is not to say that ideological differences always entail differences in ontology.²¹ We recognize that theories can differ ideologically but agree ontologically. For example, ideological differences between theories may fail to entail ontological differences when the theories are intertranslatable. The idea is that when a theory can be translated into a more fundamental idiom, its ontology is thereby reduced to

21. Indeed, Quine says: "Two theories can have the same ontology and different ideologies" (1951a: 14).

the ontology of the theory expressed in the more fundamental idiom. However, the ostrich nominalist is not proposing that the disparate predicates used in the expression of her theory ('blue', 'sphere', 'green', 'cube', 'orange', and 'cone') are eliminable by translation into a more fundamental idiom. Indeed, she takes these predicates as irreducible primitives: "[...W]e have nothing to say about what makes *a F*, it just is *F*; that is a basic and inexplicable fact about the universe" (Devitt 1980: 97). Irreducible primitives, we assume, are not translatable into a more fundamental idiom.

Importantly, we are not making the often-repeated point that realism, though more ontologically profligate, is more ideologically parsimonious than ostrich nominalism.²² The ostrich's theory incurs an ontological (and not purely ideological) cost in virtue of saying that there are things of more sorts than the realist's theory does. The ostrich chooses a theory with things of more sorts in its ontology than are in the ontology of the realist's theory. Thus, the ostrich's theory is more ontologically profligate than the realist's.

V.

We've argued for Counting Principle 1: that the commitments listed in (1)–(6) are distinct, as are those listed in (1*)–(4*). As a consequence, the ostrich has more distinct ontological commitments (six) than does the realist (four). At this point, the ostrich might maintain that, though the realist's theory is more parsimonious insofar as it is ontologically committed to things of fewer sorts, her own theory is more parsimonious insofar as it is ontologically committed to fewer specific individuals (or that the cardinality of her universe is smaller).

We first note that the ostrich has lost her purported advantage. She has maintained all along (see, e.g., Devitt 1980: 97–8) that her theory is more ontologically parsimonious (*simpliciter*) than the realist's. She may now maintain, at best, that her theory is better along one axis by which

ontological parsimony is assessed: she has fewer specific individuals in her ontology than the realist does. Nonetheless, given that sorts matter in the assessment of ontological commitments, her theory is worse along another axis by which ontological parsimony is assessed: she has more sorts of things in her ontology than the realist does.

This brings us to our defense of Counting Principle 2. We concede that the realist's universe contains more specific entities than the ostrich's, since it must also contain universals. In other words, the minimal model of the realist's theory is larger than the minimal model of the ostrich's theory. But we believe that one should compare the ontological parsimony of two theories by comparing their ontological commitments to things of various sorts. The more parsimonious theory has fewer of these commitments. On our view, the number of specific entities required by a theory matters very little, if at all, in the assessment of ontological parsimony. If this is correct, then we should compare the ostrich's and the realist's theories by determining whether the ontological commitments listed as (1)–(6) are greater in number than those listed as (1*)–(4*). Thus, (1)–(6) and (1*)–(4*) are the only ontological commitments relevant to comparing the parsimony of the respective theories.

This is an even more concessive position than is offered by some more sympathetic to ostrich nominalism than we are. For instance, Quine, whose lineage is, obviously, claimed by the ostrich nominalist, holds that no acceptable theory carries any ontological commitments to specific individuals. All ontological commitments, on Quine's view, are to things of a sort. This point arises from his discussion of the problem of Plato's Beard (1939; 1953b: 1–3). As discussed previously, Quine thinks that a theorist should be able to intelligibly deny the claims that give rise to ontological commitments in any given theory. This includes claims involving terms like 'Pegasus': it should be open to a theorist to reject an ontological commitment to Pegasus by denying 'Pegasus exists'. Quine worries that construing 'Pegasus' as a singular term renders this position unintelligible, and so he ultimately holds that all singular terms ought to be eliminated in favor of either (i) descriptive

22. This is how Oliver (1996) sees the situation. Bennett (2009: 62–5) and Sider (2009: 416–20) discuss the distinction between ontological and ideological parsimony.

predicates such as ‘the flying horse’, or else (ii) predicates such as ‘pegasizes’.²³ So, strictly speaking, no theories that Quine would consider acceptable entail that any specific individuals exist. Rather, they entail the existence of some individual or other of a sort: pegasizers, red things, sunsets, magnetic fields, etc.

We concede that there may be some way of making sense of a theory’s ontological commitments to specific individuals. One may, for example, consider the minimal cardinality of any model of a theory. We will call the aim of minimizing this cardinality *quantitative parsimony*, following Lewis (1973). A theory’s quantitative parsimony is the measure of its commitments to specific individuals.²⁴ Its *qualitative parsimony* is the measure of its ontological commitments to things of various sorts. A standard view is that quantitative, as opposed to qualitative, parsimony is not a theoretical virtue. For example, Lewis says: “I subscribe to the general view that qualitative parsimony is good in a philosophical or empirical hypothesis; but I recognize no presumption whatever in favor of quantitative parsimony” (1973: 87). Here, Lewis assigns no weight to quantitative parsimony in the assessment of a theory’s overall parsimony.

Daniel Nolan (1997) argues against Lewis that quantitative parsimony is a theoretical virtue in addition to qualitative parsimony, but even he would concede that qualitative parsimony matters more than quantitative in the overall assessment of ontological parsimony. Indeed, Nolan’s notion of quantitative parsimony is itself relative to sorts: the quantitative parsimony of a theory can be assessed only relative to each sort it posits. This suggests that the postulation of things of a new sort weighs more heavily than the postulation of new instances

23. Quine later comes to view names themselves as predicates. This change makes no difference to the overall point we’re making. For a discussion of these issues, see Fara (forthcoming).

24. Richard (1998) hints at another way of making sense of a theory’s commitment to specific individuals in terms of hyperintensionality. The concerns we will raise provide reason to think that quantitative parsimony weighs less in the assessment of ontological commitment, even if one accepts Richard’s suggestion.

of that sort in the assessment of ontological economy. Theories that do not posit things of the same sorts will be strictly incomparable as regards quantitative parsimony. That is, one cannot compare the quantitative parsimony of a theory that posits seven protons to a theory that posits five electrons. Along these lines, a theory that postulates things of many sorts, as does ostrich nominalism, will be less parsimonious both qualitatively and quantitatively as regards each of these sorts than a theory that postulates things of fewer sorts, as does realism. It is less parsimonious qualitatively because it postulates things of more sorts. It is less parsimonious quantitatively relative to each of these sorts because it postulates more than zero instances of each sort.

Why would a philosopher think that ontological commitments to specific individuals count less in the overall assessment of ontological parsimony than ontological commitments to things of a sort, or, indeed, that they don’t count at all? We see three types of reason for supposing this:

- (R-1) In canonical philosophical disputes, qualitative parsimony is, in fact, preferred to quantitative parsimony.
- (R-2) In canonical scientific disputes, qualitative parsimony is, in fact, preferred to quantitative parsimony.
- (R-3) Comparisons of quantitative parsimony collapse for theories with only infinite models.

We will discuss these points in sequence.

(R-1): We believe it is standard in philosophical disputes to prefer qualitative parsimony to quantitative parsimony in the assessment of overall parsimony. To take an example that is directly relevant to our dispute with the ostrich nominalist, many philosophers are unwilling to posit universals at all, regardless of their number. We agree with Russell (1912: 112) when he says, “[...H]aving admitted one universal, we have no longer any reason to reject others.”²⁵ We believe that many

25. Russell makes a similar point in (1912: 95–7; 1918: 150). There is a long tradition of regarding inferences to things of new sorts as less secure than inferences to

nominalists would agree as well. If we ask a trope theorist if she prefers to posit one universal or twenty additional tropes, she will invariably choose the tropes. Similarly, if we ask an ostrich nominalist if she prefers to posit one universal or twenty additional blue things, we are certain that she'll choose the latter. These choices reflect a preference structure favoring qualitative over quantitative parsimony.

To take another example from a distant field, consider the difference between an atheistic theory of the world and a theistic theory of the world. The theistic theory needn't commit to any specific individuals beyond those of the atheistic theory, since many religions hold that their deity or deities are among the human beings or other individuals posited by the atheistic theory.²⁶ The claim is that such a being is both human and divine. Moreover, theistic theories, in many cases, are far more quantitatively parsimonious than atheistic theories. The reason for this is that they may use their additional resources (*i. e.*, deities) to explain phenomena where the atheist is forced to posit additional atheistically acceptable processes. Early atheistic theories had to posit additional phenomena to account for the weather, whereas theistic theories were able to invoke a deity or deities. Likewise, certain atheistic theories may posit that there is no beginning in time and, consequently, may be forced to posit an infinite chain of processes, whereas the theists may simply posit a first cause, thereby restricting their universe to a finite sequence of causes. We find it clear that such an atheistic theory is more parsimonious overall due to its qualitative parsimony. This is despite the fact that the atheistic theory explicitly posits an infinitude of specific objects while the theistic theory posits

things of sorts that have already been acknowledged. In our view, these inferences are more precarious because asserting the existence of things of new sorts in one's theory affects its ontological economy more severely than a new entity of the same sort. Of course, we concede that less economical theories are sometimes more justified than more economical theories.

26. For example, proponents of a simplified version of the Christian doctrine of the trinity might hold that God ("the father") and the holy spirit are *strictly identical* to the human being Jesus. If this position were adopted, then anyone who posits Jesus would thereby also posit God and the holy spirit.

only a finite number. This is not to say that theism is unjustified or untrue, just that its motivation cannot come from considerations of overall parsimony, as we think is conceded by many theists.

(R-2). We believe that the preference for qualitative over quantitative parsimony in philosophical disputes stems, as it should, from the same choice structure active in scientific disputes. Scientists are willing to posit more specific individuals if it simplifies their overall theory by reducing the things of various sorts that they posit. The pre-scientific view of the world posited things of disparate sorts. This theory was replaced by a theory that posits things of a relatively small list of chemical sorts. Things of these chemical sorts are, in turn, supposed to be reduced to things of an even smaller number of sorts, such as protons, electrons, and neutrons (or even the flavors of quarks). For instance, scientists in the past may have taken there to be a primitive distinction between living and non-living things, or one that appealed to *sui generis* vital forces. However, the distinction between living and non-living things is now explained in terms of the chemical processes occurring in them. This development required positing more specific individuals and processes than had been previously recognized. But these specific posits paid their way because they allowed the theory to posit things of fewer sorts.

(R-3). A final reason to favor qualitative over quantitative parsimony derives from the difficulty of comparing theories with only infinite models. The specific worry is that any first-order theory with only infinite models has a countable model. Thus, any two theories that posit an infinitude of things will be equally quantitatively parsimonious. It is this concern that leads Nolan (1997) to reject the view that quantitative parsimony *simpliciter* is a theoretical virtue. Rather, he endorses the more complicated "thesis that we should minimize the number of entities of each kind that we postulate" (1997: 341). Thus, a theory that postulates infinitely many sets and seven material objects is less parsimonious than a theory that postulates infinitely many sets and five material objects. But two theories that posit different kinds will be strictly incomparable with regard to quantitative

parsimony. As we argued above, Nolan's view has the consequence that qualitative parsimony takes precedence as a theoretical virtue over quantitative parsimony.

VI.

Until now, we've suppressed a complication in our discussion of ontological commitment: we've undercounted the commitments of the theories of both the realist and the ostrich. We will argue that this undercount exposes a general puzzle about ontologically committing to things of a sort. Solving this puzzle, we'll argue, requires privileging ontological commitments to things of certain sorts (namely, those that are explanatorily basic) in the assessment of overall ontological parsimony.

We have spoken of theories as ontologically committing to things of a sort: to *spheres*, *green things*, *universals*, *particulars*, *instantiators*, and *instantiated things*. Moreover, we've argued as if these are the only ontological commitments that matter in the assessment of the comparative ontological parsimony of realism and ostrich nominalism. However, the realist and nominalist theories have ontological commitments to things of more sorts than those listed in (1)–(6) and (1*)–(4*). In addition to saying that there are blue things, the nominalist theory says that there are *blue spheres*, *green cubes*, and so on. It therefore has an ontological commitment to *blue spheres*, an ontological commitment to *green cubes*, and so on. But, it also has even more complex ontological commitments: it entails the existence of *green things such that there is a sphere*: $\exists x (\text{Green}(x) \wedge \exists y \text{Sphere}(y))$. Thus, it has an ontological commitment to *green things such that there is a sphere*.

The realist theory has its own additional commitments. It is committed to the existence of *things that instantiate green such that there are things that instantiate sphericity*: $\exists x (\text{IS}(x, \text{green}) \wedge \exists y \text{IS}(y, \text{sphere}))$; it therefore has an ontological commitment to things of this sort. Moreover, since it entails ' $\exists x \text{IS}(x, \text{green})$ ', the theory is ontologically committed to *things that instantiate green*.

Simple combinatorial reasoning suggests that even very simple theories have ontological commitments to things of infinitely many

distinct sorts, for such theories will entail infinitely many existential sentences. Any two existential sentences ' $\exists x \Phi x$ ' and ' $\exists x \Psi x$ ' generate ontological commitments to Φ s and to Ψ s, respectively. These commitments are distinct, because one can have an ontological commitment to Φ s without thereby having an ontological commitment to Ψ s. Indeed, ' Φ ' and ' Ψ ' will not even be necessarily equivalent. For brevity's sake, we confine a fuller elaboration of this argument to a footnote.²⁷

This suggests that there is a general problem with assessing a theory's overall ontological parsimony by counting its commitments to *things of a given sort*: most theories entail the existence of things of infinitely many sorts. They thereby incur infinitely many ontological commitments. The natural solution to this problem is to privilege some sorts over others in the assessment of overall ontological parsimony. We will first consider and reject a proposal for privileging some sorts over others in terms of the number of instances of the sort. We will then offer our own proposal in Section VII.

Some philosophers might suppose that an ontological commitment to Φ s weighs more heavily in the assessment of ontological parsimony than an ontological commitment to Ψ s, if there are more Φ s than Ψ s. On this view, the greater the generality of a sort, the more it counts in the assessment of ontological parsimony. This suggestion might be reinforced using the distinction between general categories and

27. To see our point, imagine that a theory asserts that there are $n-1$ cones in the box. Consider the following sequence of claims that follow from this theory: (C₁) There is an x such that $x=x$ and there are fewer than n cones in the box; (C₂) There is an x such that $x=x$ and there are fewer than $n+1$ cones in the box; (C₃) There is an x such that $x=x$ and there are fewer than $n+2$ cones in the box; etc. These claims give rise to the following ontological commitments, respectively: (OC₁) Self-identical things such that there fewer than n cones in the box; (OC₂) Self-identical things such that there fewer than $n+1$ cones in the box; (OC₃) Self-identical things such that there fewer than $n+2$ cones in the box; etc. A theory can consistently commit to things such that there are fewer than $m+1$ cones in the box without thereby committing to things such that there are fewer than m cones in the box. Thus, these commitments are distinct, as follows from the fact that the sentences generating them are not even necessarily equivalent. A similar argument holds if the theory says that there are infinitely many cones in the box.

subclasses that Quine (1951b) attributes to Carnap.²⁸ According to this view, ontological commitments to things falling under general categories weigh more heavily than ontological commitments to things falling under subclasses of those general categories. An ostrich arguing along these lines might suggest that since the realist theory posits things of two very general categories, *i. e.*, universals and particulars, it is less parsimonious than the ostrich's own theory, which posits things of only one very general sort, *i. e.*, particulars.²⁹

Privileging general categories in the assessment of parsimony strikes us as artificial. It isn't clear why the ostrich counts particulars and universals as the realist's most general categories when the realist has a more general category, namely, *things*. There are more things than there are particulars and universals — indeed, all particulars and all universals are things — so the realist has only one most general sort of entity. That is, the realist may retort that she has an ontological commitment to things of only one general category, *things* or *self-identical things*. Universals and particulars are only subclasses of this general category. Of course, the ostrich could also claim that

28. Quine proposes that the distinction should be abandoned: "Whether the statement that there are physical objects and the statement that there are black swans should be put on the same side of the dichotomy, or on opposite sides, comes to depend on the rather trivial consideration of whether we use one style of variables or two for physical objects and classes" (1951b: 208).
29. This may be what Melia (2005) means when he concedes that "sensible" (read: ostrich) nominalism is less ontologically parsimonious than other forms of nominalism (and realism if our argument is correct), but suggests that ostrich nominalism is nonetheless more "metaphysically parsimonious" than realism: "For although the sensible nominalist has dispensed with these metaphysical entities (such as universals), the individuals that the sensible nominalist postulates are themselves many and varied. Insofar as he thinks that some things have mass, other things have charge, other things have spin, and there is no unifying or constitutive account of these truths in terms of something more fundamental, he has postulated many different kinds of individuals. Yes, the sensible nominalist avoids a complicated metaphysics but, because of the richness and variety of his individuals, his overall ontology may still be unparsimonious" (71–2). We are unclear about the distinction Melia is drawing between metaphysical and ontological parsimony. He appears to describe a theory that minimizes the number of the most general sorts of things (*e. g.*, individuals vs. universals) as metaphysically parsimonious.

things constitute her most general category. But this just means that any assessment of the difference in ontological economy between the realist's theory and the ostrich's would require some further way of screening which sorts matter to such an assessment. This reveals that generality of a sort is an inadequate tool in the assessment of ontological economy.

VII.

We have thus far assumed that one theory is more ontologically parsimonious than another insofar as the former ontologically commits to things of fewer sorts than the latter does. Yet we've argued that almost every reasonable theory is ontologically committed to things of an infinite number of distinct sorts. Again, this suggests that some sorts need to be privileged over others in the assessment of ontological parsimony. So now the question is: How is this privileging to be effected?

Our answer appeals to a distinction between explanatorily basic and non-basic sorts. We believe that there is a natural sense in which something's being a blue square can be *explained* by its being blue and square. More generally, in an extensional language,³⁰ the use of atomic, monadic predicates that are not subject to paraphrase commit a theory to things of explanatorily basic sorts. More explicitly,

Monadic Predicates

A theory incurs an ontological commitment to things of an explanatorily basic sort for each atomic monadic predicate, P , such that the theory entails ' $\exists x Px$ '.

30. The issues become more complicated for an intensional language, such as one employing modal vocabulary. The fact that something is square may explain the fact that it is a square or a circle. However, it cannot explain the fact that the thing is necessarily a square or possibly not a square. Attempts to reduce the number of explanatorily basic modal categories include Carnap (1947), Lewis (1986), Sider (2001), etc., who attempt to explain claims about what is necessary and what is possible in terms of claims about possible worlds; as well as Fine (1994), who attempts to derive what is necessary for an object from facts about its essence.

This principle seems natural to us since we think that the fact that a molecular predicate in an extensional language applies to an object is explained by the distribution of atomic predicates that apply to it. Similarly, that the sort picked out by a molecular predicate applies to an object is explained by the distribution of atomic sorts that the object falls under. We will argue that explanatorily basic sorts matter more in the assessment of ontological parsimony than explanatorily non-basic sorts.

By distinguishing explanatorily basic from non-basic sorts, we don't mean to appeal to any special metaphysical conception of explanation. We believe that reducing the number of explanatorily basic categories (in the ordinary sense of 'explanation') is a goal of good scientific theorizing. A theory that appeals to the categories *massed particle*, *electrically charged particle*, and *magnetized particle* has more explanatorily basic sorts than one that appeals only to the sorts *massed particle* and *electromagnetically charged particle*. The former is thus less parsimonious than the latter, even if the latter requires more claims about the distribution of mass and charge (and thus more explanatorily non-basic sorts) in order to account for the observed phenomena. The explanatorily non-basic sorts, on the other hand, seem to count little by comparison. Once they have agreed to the existence of massed particles, scientists don't fret about saying that there are *things that are either massed particles or electromagnetically charged particles*. This commitment to *things that are either massed particles or electromagnetically charged particles* does not cost a theory its parsimony, or at least does not cost much.

In saying that ontological parsimony primarily concerns minimizing the number of ontological commitments to things of explanatorily basic sorts, we are simply generalizing this goal of scientific theorizing to our most comprehensive theory of the world. Recently, some philosophers³¹ have proposed that ontological parsimony requires minimizing the number of ontological commitments to things of explanatorily basic sorts in a more metaphysical sense of 'explanation'.

31. See discussions in Fine (2001; 2009), Cameron (2008; 2010), Schaffer (2008; 2009), and Sider (2009).

This more metaphysical notion of explanation is often called *grounding* or *dependence*. As we've said, our argument requires only the ordinary notion of explanation. Nonetheless, it's likely that a proponent of this more metaphysical conception of explanation will agree with what we've said, since she will likely agree that an object's falling under the sort *green sphere* is explained by (is grounded in or depends on) the distribution of the basic sorts under which it falls.

To illustrate our point, consider the ostrich's theory of the box world. Her theory has six atomic predicates, which give rise to six distinct ontological commitments (*i.e.*, commitments to *blue things*, *spheres*, *green things*, *cubes*, *orange things*, and *cones*). Her theory entails the sentence 'There is an x such that x is blue or x is green' and is thereby committed to *things such that they are blue or green*. Thus, the ostrich's theory is ontologically committed to things of at least seven sorts of things. Compare her theory to that of an imagined opponent, who, for whatever reason, feels the need to invoke an additional *atomic* predicate applying to, say, things that are blue or green. The ostrich's opponent might say that her theory is just as parsimonious as the ostrich's since it has just as many ontological commitments: they both have infinitely many.

We disagree with the ostrich's opponent. In the ostrich's theory, the category *being blue or green* is not explanatorily basic. In particular, something's being blue or green is explained by the fact that it is blue, or it is green. The ostrich's opponent introduces an additional predicate — and thus an additional category. Supposing she's not willing to paraphrase sentences containing this predicate into sentences containing predicates already in the ostrich's theory, the predicate — and the category it represents — counts as explanatorily basic.³²

We want to determine whether realism is more ontologically parsimonious than ostrich nominalism. We've argued that a theory that

32. Analogously, a theorist who says there are objects that are green before time t or blue after t doesn't introduce any explanatorily basic categories. By way of contrast, a theorist who applies a new basic predicate, say 'grue', to these objects and *refuses to paraphrase* statements containing 'grue' into statements containing 'blue' and 'green' thereby introduces a new, explanatorily basic sort to her theory. (Our example is taken from Goodman 1983).

invokes fewer explanatorily basic sorts is, *ceteris paribus*, more ontologically parsimonious than a theory that invokes more. However, we're not yet in a position to count the explanatorily basic sorts invoked by the realist's theory. The realist's theory makes use of an atomic dyadic predicate, and we've not yet considered how to determine the explanatorily basic sorts generated by the use of such a predicate.

We hold that an atomic dyadic predicate generates a commitment to things of two explanatorily basic sorts corresponding to the two positions accessible to quantification in the predicate. That is:

Dyadic Predicate

A theory that, for some atomic dyadic predicate ' Q ', entails ' $\exists x \exists y Q(x, y)$ ' incurs an explanatorily basic ontological commitment to things that Q other things. Moreover, a theory that entails ' $\exists x \exists y Q(y, x)$ ' incurs an explanatorily basic ontological commitment to things that are Q -ed by other things.

If a theory employs an atomic dyadic predicate, say 'loves', which it asserts to hold between various objects, then the theory incurs ontological commitments to things of two explanatorily basic categories: *lovers* and *beloved*.

One might say that this procedure undercounts the explanatorily basic ontological commitments generated by the use of an atomic dyadic predicate. The theory introduced above that contains the predicate 'loves' commits not only to lovers and beloved, but also to lovers of specific individuals and those beloved by specific individuals (*e.g.*, there are *lovers of John*, and *those beloved by Sally*, etc.). We agree that the use of 'loves' and other dyadic predicates may generate ontological commitments to things of these additional sorts. However, we hold that commitments to things of these sorts weigh less in the assessment of ontological parsimony. We argued above that commitments to specific individuals should count less in the assessment of ontological commitment than commitments to general sorts. In other words, a

theory's qualitative commitments matter more than its commitments to specific individuals. Taking each existential generalization over a relational predicate ' $\exists x Q(x, a)$ ' as generating an explanatorily basic ontological commitment means that each specific individual posited by a theory counts more than the general commitments. The reason is that each name deployed in the theory will presumably be the subject and object in multiple relational predications. The qualitative commitments of a theory that says that Sally loves John are to *lovers* and to *beloved*. If the theory's commitment to *lovers of John* carries the same weight as these two commitments, then we will again be conceding that quantitative parsimony weighs the same as qualitative. For this reason, we think that a theory's commitments generated using relational predicates (to *lovers of John* and to *those beloved by Sally*) simply weigh less in the assessment of overall parsimony than do the commitment to *lovers* and *beloved*.

VIII.

We can now compare the number of explanatorily basic ontological commitments had by the realist theory to the number of those had by the ostrich nominalist theory. Once again, the realist's theory contains exactly four existential sentences generating commitments to things of explanatorily basic sorts: two involving atomic monadic predicates, ' $\exists x \text{Universal}(x)$ ' and ' $\exists x \text{Particular}(x)$ ', and two involving an atomic dyadic predicate, ' $\exists x \exists y \text{IS}(x, y)$ ' and ' $\exists y \exists x \text{IS}(x, y)$ '. The realist, therefore, has things of four explanatorily basic sorts in her ontology.

By way of contrast, the ostrich treats each atomic predicate used in the ordinary description as explanatorily basic. Each sentence formed by applying an existential quantifier to one of these predicates generates an ontological commitment to an explanatorily basic sort. As Melia (2005) says (expanding on Devitt 1980: 97),

Now [...] 'a is charged', 'a is square', and 'a has mass' may all report metaphysically primitive truths — there may be no interesting constitutive account that can be given of

such truths. Another way of putting this is that, for the sensible nominalist [read: ostrich], there just are charged things, square things, and massive things, and there is nothing more to be said about the matter[...]. [71]

For this reason, we take the ostrich's theory of the box world to have things of six explanatorily basic sorts in its ontology: (1) *blue things*, (2) *cubes*, (3) *spheres*, (4) *orange things*, (5) *green things*, and (6) *cones*. Thus, by our count, the ostrich has a less ontologically parsimonious theory than the realist.

The ostrich might retreat at this point. She too might propose to explain or reduce the basic sorts (1)–(6) in terms of basic sorts in some underlying physical theory. She might attempt to explain the colors of objects in terms of their reflectance properties, and ultimately in terms of their mass, charge, spin, the various flavors of quarks, etc. As Melia (2005) says,

[The] apparent ontological diversity [of the ostrich's theory] may one day be explicable in terms of a simpler ontology [...]. But whether such a reduction can be effected depends upon serious theoretical and empirical work – if such work cannot be done, the sensible nominalist accepts the extra ontological commitment. [72]

We have no objection to such attempted reductions. The ostrich's explanatorily basic sorts correspond to explanatorily non-basic sorts in the realist's theory. But unless the ideal physical theory has four explanatorily basic sorts or fewer in its ontology, the ontology of the ostrich's theory will still be less parsimonious than that of the realist's theory.

IX.

Realism reduces a lavish ontology to a sparse one that includes only particulars, universals, instantiating things, and instantiated things as its explanatorily basic sorts. The realist's theory, therefore, offers a more

unifying explanation of our experience than the ostrich nominalist's does. This is not to say that realism triumphs over all its competitors. Other forms of nominalism, including class nominalism, resemblance nominalism, and trope theory, offer realism a run for its money. Our purpose is not to adjudicate among these views. It is merely to point out that ostrich nominalism does not have this advantage: it is not a unifying explanation of our experience. Among the proposed unifying explanations, Quine advises us to select the simplest reasonable one "into which the disordered fragments of raw experience can be fitted and arranged" (1953b: 16). However, the ostrich's proposal, insofar as we understand it, amounts to rejecting this project of offering a unified explanation of the disparate sorts invoked in ordinary and scientific theorizing, and thereby resting content with an overpopulated ontological slum.³³

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Nominalism, Trivialism, Logicism

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Nominalism, Trivialism, Logicism

Agustín Rayo*

May 1, 2014

This paper is an effort to extract some of the main theses in the philosophy of mathematics from my book, *The Construction of Logical Space*. I show that there are important limits to the availability of nominalistic paraphrase-functions for mathematical languages, and suggest a way around the problem by developing a method for specifying nominalistic *contents* without corresponding nominalistic *paraphrases*.

Although much of the material in this paper is drawn from the book—and from an earlier paper (Rayo 2008)—I hope the present discussion will earn its keep by motivating the ideas in a new way, and by suggesting further applications.

I Nominalism

Mathematical Nominalism is the view that there are no mathematical objects. A standard problem for nominalists is that it is not obvious that they can explain what the point of a mathematical assertion would be. For it is natural to think that mathematical sentences like ‘the number of the dinosaurs is zero’ or ‘ $1 + 1 = 2$ ’ can only be true if mathematical objects exist. But if this is right, the nominalist is committed to the view that such sentences are untrue. And if the sentences are untrue, it not immediately obvious why they would be worth asserting.

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A nominalist could try to address the problem by suggesting nominalistic *paraphrases* for mathematical sentences. She might claim, for example, that when one asserts ‘the number of the dinosaurs is zero’ one is best understood as making the (nominalistically kosher) claim that there are no dinosaurs, and that when one asserts ‘ $1 + 1 = 2$ ’ one is best understood as making the (nominalistically kosher) claim that any individual and any other individual will, taken together, make two individuals.¹

Such a strategy faces two main challenges. The first is to explain why mathematical assertions are to be understood non-standardly. One way for our nominalist to address this challenge is by claiming that mathematical assertions are set forth ‘in a spirit of make-believe’ (Yablo 2001). She might argue, in particular, that when one makes a mathematical assertion one is, in effect, claiming that the asserted sentence is true in a *fiction*, and more specifically a fiction according to which: (a) all non-mathematical matters are as in reality, but (b) mathematical objects exist with their standard properties. This proposal leads to the welcome result that fictionalist assertions of mathematical sentences can convey information about the real world. For instance, one can use a fictionalist assertion of ‘the number of the dinosaurs is zero’ to convey the information that there are no dinosaurs, since the only way for ‘the number of the dinosaurs is zero’ to be true in a fiction whereby mathematical objects have all the standard properties is for the fiction to entail that there are no dinosaurs, and the only way for such a fiction to agree with reality in all non-mathematical respects is for it to be the case that there are no dinosaurs.

The second main challenge is that of specifying nominalistic paraphrases for arbitrary mathematical sentences. It is perfectly straightforward to come up with plausible nominalistic paraphrases for toy sentences like ‘the number of the dinosaurs is zero’ or ‘ $1 + 1 = 2$ ’. But we need is a method that will work in general.

Say that a paraphrase-function (for language L with output-language L^N) is an effectively specifiable function that assigns to each sentence in L a paraphrase in L^N . One of the main

¹More carefully: $\forall x \forall y (x \neq y \rightarrow \exists!_2 z (z = x \vee z = y))$.

objectives of this paper is to show that finding a nominalist paraphrase-function is not as easy as one might have thought.

2 Constraints

What would it take for a paraphrase-function for the language of (applied) arithmetic to count as a *nominalist* paraphrase-function? I suggest the following three constraints:

1. *The Counting Constraint*

The paraphrase assigned to $\ulcorner \text{The number of the } F\text{s} = n \urcorner$ should have the same truth-conditions as $\ulcorner \exists!_n x (Fx) \urcorner$.²

2. *The Inferential Constraint*

Suppose that ϕ and ψ are arithmetical sentences, and that the truth-conditions of ϕ are at least as strong as the truth-conditions of ψ (for short: ϕ *entails* ψ). Then the paraphrase assigned to ϕ should entail the paraphrase assigned to ψ .

3. *The Triviality Constraint*

- (a) The paraphrase assigned to any true sentence of pure arithmetic should have *trivial* truth-conditions (that is, truth-conditions that would be satisfied regardless of how the world turned out to be).
- (b) The paraphrase assigned to any false sentence of pure arithmetic should have *impossible* truth-conditions (that is, truth conditions that would fail to be satisfied regardless of how the world turned out to be).

It seems to me that a paraphrase-function satisfying these three constraints (for short: a *trivialist* paraphrase-function) should be thought of as the ‘gold standard’ of nominalist

²As usual, we let $\ulcorner \exists!_0 x (\phi(x)) \urcorner$ be short for $\ulcorner \neg \exists x (\phi(x)) \urcorner$, and $\ulcorner \exists!_{n+1} x (\phi(x)) \urcorner$ be short for $\ulcorner \exists z (\phi(z) \wedge \exists!_n x (\phi(x) \wedge x \neq z)) \urcorner$.

paraphrase-functions. It is hard to see how a nominalist paraphrase-function could deserve the label ‘nominalist’ if it failed to respect the Counting Constraint, and it is clear that something important would be left out if it failed to respect the Inferential Constraint.

What about the Triviality Constraint? Although some nominalists might be willing to settle for a paraphrase-function that failed to satisfy it, I hope it can be agreed on all sides that the nominalist should *prefer* a paraphrase-function that satisfies the Triviality Constraint over one that does not. Notice, for example, that the fictionalist proposal we considered above presupposes that the Triviality Constraint ought to be satisfied. For a truth of pure mathematics will count as true in the relevant fiction *regardless of how matters stand in reality*. So our fictionalist is committed to thinking that a truth of pure mathematics can be correctly asserted ‘in a spirit of make believe’ regardless of how matters stand in reality.

It is also worth noting that the Triviality Constraint was satisfied by the nominalistic reading that I had earlier suggested for ‘ $1 + 1 = 2$ ’. For the paraphrase I suggested is a logical truth, and it is reasonable to assume that the truths of pure logic have trivial truth-conditions. One might be inclined to think that it would be desirable if the nominalistic reading of *any* truth of pure mathematics turned out to be a logical truth, and therefore had trivially satisfiable truth-conditions.

3 The Bad News

The bad news is that it is *impossible* to specify a trivialist paraphrase-function for the language of arithmetic. A little more carefully: there is a formal result that suggests that it is impossible to specify a paraphrase-function for the language of arithmetic that is *uncontroversially* trivialist.

Here ‘uncontroversial’ means three different things: (1) no controversial linguistic assumptions, (2) no controversial metaphysical assumptions, and (3) no controversial subtraction-assumptions. I will say a few words about each kind of assumption before turning to the

formal result itself.

3.1 Controversial Linguistic Assumptions

If the expressive resources of one's output-language—i.e. the language in which nominalistic paraphrases are given—are sufficiently powerful, it is straightforward to define a trivialist paraphrase-function.

There is, for example, a method for paraphrasing each sentence of the language of arithmetic as a sentence of an $(\omega + 3)$ -order language.³ Would this count as a trivialist paraphrase-function? Yes: *if* one assumes that $(\omega + 3)$ -order logic is 'genuine logic' (if one assumes, in other words, that any truth of $(\omega + 3)$ -order logic has trivial truth-conditions). That is, however, a highly controversial assumption.

The view that *second*-order logic is 'genuine logic' is increasingly popular amongst philosophers. But most philosophers seem to think that languages of high finite order—to say nothing of languages of transfinite-order—can only be made sense of as 'set theory in sheep's clothing' (Quine 1986), and many philosophers would conclude on that basis that the truths of higher-order logic have non-trivial truth-conditions.

My own view is that $(\omega + 3)$ -order logic is, in fact, 'genuine logic',⁴ and that our $(\omega + 3)$ -paraphrase-function is, in fact, a trivialist paraphrase function. But even I must concede that it is not *uncontroversially* a trivialist paraphrase-function. The linguistic assumptions one would need to justify such a claim are just too great.

3.2 Controversial Metaphysical Assumptions

There is a nominalistic paraphrase-function that I find very attractive. It draws its inspiration from Frege's *Grundlagen*, so I will refer to it as the *Fregean* paraphrase-function. The basic

³An $(\omega + 3)$ -order language has variables of all finite types, plus three levels of variables of transfinite type. For further details, see (Linnebo & Rayo 2012). For more on the relevant paraphrase-method, see (Rayo 2013, ch. 7).

⁴I believe, in other words, that the truths of $(\omega + 3)$ -order logic have trivial truth-conditions. I also believe, however, that the truths of pure set-theory have trivial truth conditions. See (Rayo 2013, ch. 3).

idea is that a sentence of the form ‘the number of the F s = the number of the G s’ is to be paraphrased as:

the F s are just as many as the G s

What about a *quantified* arithmetical sentence, such as ‘there is an $n > 0$ such that: $n =$ the number of the planets’? We first paraphrase the sentence as:

there are some things, the F s, such that: the number of the F s = the number of the planets

We then eliminate arithmetical-terms altogether, and say:

there are some things, the F s, such that: the F s are just as many as the planets.

It is easy to show that similar transformations can be applied to every sentence in the language of applied arithmetic (excluding mixed-identity statements such as ‘Caesar = 17’).⁵ The result is a nominalistic paraphrase-function that assigns to each arithmetical sentence a second-order sentence.⁶

Suppose we concede that second-order logic is ‘genuine logic’, and that the truths of second-order logic have trivial truth-conditions. Is this enough to conclude that the Fregean Paraphrase-Function is a *trivialist* paraphrase-function? No—at least not if what we’re looking for is an *uncontroversially* trivialist paraphrase-function. For consider ‘any number greater than 0 has a successor’, which is a truth of pure arithmetic. Its Fregean paraphrase is:

For any things, the F s, there are some things, the G s, such that: for some g amongst the G s, the F s are just as many as the G s distinct from g .

⁵I spell out the details in (Rayo 2002); for a similar proposal, see (Fine 2002, II.5).

⁶Alternatively, one could think of the paraphrase-function as assigning to each arithmetical sentence a sentence of a *plural language* which has been enriched with the atomic plural predicate ‘they are just as many as them’. Here I fudge the distinction between the two for expositional purposes. For more on plural languages, see (Boolos 1984) and (Linnebo 2004).

which will only be true if there are infinitely many objects (or none). So we have a violation of the Triviality Constraint. More guardedly: we have a violation of the Triviality Constraint *unless* we are prepared to accept the following (highly controversial) metaphysical thesis:

Trivialist Infinitarianism

Not only is it the case that the world contains infinitely many objects, it is *trivially* the case that the world contains infinitely many objects. In other words: to assume that the world is finite is to assume something worse than false, it is to assume something *absurd*.

I myself think that Trivialist Infinitarianism is true.⁷ So I believe that the Fregean Paraphrase-Function is, in fact, a trivialist paraphrase-function. But even someone like me, who thinks that Trivialist Infinitarianism is true, must concede that the Fregean Paraphrase-Function is not *uncontroversially* a trivialist paraphrase-function.

It is perhaps worth mentioning that the Fregean Paraphrase-Function is in good company when it comes to infinity assumptions. Many attractive nominalist paraphrase-functions will only count as trivialist paraphrase-functions in the presence of Trivialist Infinitarianism.⁸

3.3 Controversial Subtraction-Assumptions

Joseph Melia (2000) has argued for a satisfyingly straightforward nominalistic paraphrase-function. One is simply to paraphrase the mathematical sentence ϕ as:

ϕ , except for the part about mathematical objects

⁷Why think that Trivialist Infinitarianism is true? Because it follows from [NUMBERS]—see section ref-sec:logicism.

⁸This is true, in particular, of (Hodes 1984) and (Yablo 2002). It is also true of a form of if-then-ism whereby a sentence ϕ is paraphrased as the universal closure of $\ulcorner(A \rightarrow \phi)^*\urcorner$, where A is the conjunction of the second-order Dedekind-axioms and ϕ^* is the result of uniformly replacing arithmetical vocabulary for variables of appropriate type.

A potential worry about this paraphrase-method is that it relies on a non-trivial *subtraction*-assumption. Suppose, for example, that ϕ is a complex physical theory couched in a mathematical language—quantum theory, as it might be. Melia’s method presupposes that the operation of subtracting away the ‘mathematical part’ from the content of quantum theory yields a result which is both well-defined and non-empty. But it is not immediately obvious that this is so: it is not immediately obvious that extricating the mathematical part from quantum theory leaves an interesting remainder.

Mark Colyvan (2010) has a nice example to illustrate why extricability might be a worry:

J. R. R. Tolkien could not, for example, late in the Lord of the Rings trilogy, take back all mention of hobbits; they are just too central to the story. If Tolkien did retract all mention of hobbits, we would be right to be puzzled about how much of the story prior to the retraction remains, and we would also be right to demand an abridged story—a paraphrase of the hobbitless story thus far.

The worry here is not necessarily that the result of subtracting all mention of hobbits from *The Lord of the Rings* is ill-defined—it may well not be. The point is that even if the result is well-defined, one shouldn’t expect much of a narrative. It would be a bit like *Harry Potter* without the wizards: what we’re left with just isn’t unified enough to be much of a story.⁹ Similarly, a skeptic might worry that even if the result of subtracting the mathematical part from the content of quantum theory turns out to be well-defined, what we’re left won’t be unified enough to tell us anything very interesting about the physical world. (Field’s (1984) ‘Heavy Duty Platonist’ is presumably one such skeptic.)

Another way to see that extricability claims can be problematic is to consider the question of what would be left if one subtracted *someone is thirsty* from *I’m thirsty* (Yablo 2012); or the question of what would be left if one subtracted *the tomato is red* from *the tomato is scarlet* (Searle & Körner 1959, Woods 1967, Kraemer 1986, Yablo 2012)? It’s not clear that there are well-defined answers to be given—unless, of course, one is prepared to say ‘nothing’.

⁹Thanks here to Kevin Richardson.

A further example, which I find especially illuminating, concerns the notion of *narrow content*. Narrow contents are supposed to be the result of subtracting away certain kinds of environmental facts from the contents of our beliefs (Brown 1992). The narrow content corresponding to my belief that water is wet, for example, is supposed to be the result of subtracting from what I believe when I believe that water is wet the fact that items in my environment playing a certain theoretical role are composed of H₂O. Since the claim that narrow-contents are both well-defined and non-empty is a highly controversial philosophical thesis, one can use the debate between friends and foes of narrow content to underscore the fact that the operation of subtracting particular ‘environmental factors’ from the contents of our beliefs shouldn’t be assumed to deliver the intended results. (For illuminating discussion, see Yablo forthcoming.)

The most straightforward way of justifying the claim that mathematical content can be usefully extricated from mathematical claims would be to set-forth a nominalist paraphrase-function—one that does not itself rely on subtraction-assumptions. For one would then be in a position to claim that the result of subtracting away the mathematical part from the content of a mathematical sentence is simply the content of the sentence’s paraphrase. But it is not immediately obvious that a suitable paraphrase-function can be found. For although we want the result of subtracting away the mathematical part from quantum theory, say, to deliver a non-empty content, we presumably want the result of subtracting away the mathematical part from a truth of *pure* mathematics to be an empty content: a content that would be satisfied however the world turned out to be. So the relevant paraphrase-function had better be a *trivialist* paraphrase-function. And, as we have seen, it is not easy to find an uncontroversial example of a trivialist paraphrase-function. It is therefore not immediately obvious that the operation of subtracting mathematical content can be defined in a way that delivers interesting results.¹⁰

¹⁰Yablo (2012, forthcoming) has as sophisticated treatment of these issues, which yields an illuminating characterization of the circumstances under which the subtraction operation delivers results which are both well-defined and non-empty. On its own, however, Yablo’s account does not settle the question of whether

My own view is that Melia’s use of the subtraction-operation can, in fact, be defined so as to deliver the right results. But I don’t think that such a claim can be simply taken for granted: a substantial argument is required. (I will attempt to provide the missing argument in section 4.)

Melia’s paraphrase-function has the advantage of wearing its subtraction-assumption on its sleeve. But it is worth noting that similar assumptions are required by other nominalist paraphrase-methods:

Method	Paraphrase ϕ as ...	Subtraction-Assumption
Fictionalism	ϕ is true according to a fiction which is accurate in all non-mathematical respects but in which mathematical objects exist with the standard properties (Yablo 2001).	One would impose an interesting condition on the world by insisting that it agree in non-mathematical respects with a fiction that makes, say, quantum theory true.
Modalism	ϕ is true at the closest possible world which agrees with the actual world in non-mathematical respects but in which mathematical objects exist with the standard properties (Hellman 1989, Dorr 2007).	One would impose an interesting condition on the world by insisting that it agree in non-mathematical respects with the closest possible world that makes, say, quantum theory true.
Subject-Matterism	ϕ is true as far as its non-mathematical subject-matter is concerned, where a claim’s non-mathematical subject-matter is defined as the set of worlds which agree in all non-mathematical respects with a world at which the claim is literally true (Yablo 2012).	One would impose an interesting condition on the world by insisting that it agree in non-mathematical respects with quantum theory’s non-mathematical subject-matter.

It is instructive to note that each of these paraphrase-methods could be easily modified so as to deliver a ‘narrowist’ paraphrase-method, in which paraphrases are meant to capture the *narrow contents* of the original claims, rather than their nominalistic contents. (One could claim, for example, that the *narrow part* of ‘water is wet’ is the claim that ‘water is subtracting ‘the mathematical part’ from, say, quantum theory delivers results which are both well-defined and non-empty.

wet' is true according to a fiction that is accurate in all respects, except perhaps for the nature of the substance that plays the theoretical role that water currently plays in our cognitive lives.) Just as the resulting 'narrowist' paraphrase-functions would do nothing to move a foe of narrow contents, the nominalistic paraphrase-functions in the table above should do nothing to move a skeptic of Melia's subtraction-assumption.

3.4 The Formal Result

Earlier I claimed that it is impossible to specify a paraphrase-function for the language of arithmetic that is uncontroversially trivialist. I am now in a position to give a precise statement of the underlying formal result.

First some assumptions:

A₁ We shall assume that the logical resources of our output-language do not go beyond those of the simple theory of types (which is a language with n th-order quantifiers for each finite n).¹¹

A₂ We shall assume that our output-language contains no intensional operators.

A₃ Any trivialist paraphrase-function must, by definition, satisfy the Triviality Constraint of section 2, and therefore preserve truth-values when applied to sentences of pure arithmetic. (In other words: every truth of pure mathematics must get paraphrased as a truth of the output-language, and every falsity of pure mathematics must get paraphrased as a falsehood of the output-language.)

We shall assume that this condition can be met even if the output-language has a finite domain.

Think of A₁–A₃ as stating, respectively, that our paraphrase-function is to make no controversial linguistic assumptions, that it is to make no controversial subtraction-assumptions,

¹¹Since paraphrase-functions are, by definition, effectively specifiable, we may assume with no loss of generality that our output-language has a *finite* stock of non-logical predicates and terms.

and that it is to make no controversial metaphysical assumptions. More precisely:

- A_1 places an upper bound on the expressive resources of the output-language. Such a bound is justified by the fact that the use of a language more powerful than the simple theory of types would be a sure sign of a controversial linguistic assumption in the sense of section 3.1. (As I noted above, the use of a *third*-order language would be pretty controversial already, but I'm trying to keep my assumptions as weak as possible.)
- A_2 is a ban on intensional operators, and is intended to ensure that our paraphrase-function makes no controversial subtraction-assumptions. I have certainly not shown that any sensible paraphrase-method based on intensional operators will require controversial subtraction-assumptions. But as the table in section 3.3 illustrates, such assumptions are required by the most natural methods for supplying intensional paraphrases for language of applied arithmetic.
- A_3 is meant to ensure that our paraphrase-method does not rely on infinity assumptions. This is important because—unless one embraces Trivialist Infinitarism, which is a decidedly controversial metaphysical thesis—one should think that no trivialist paraphrase-function can presuppose an infinite domain.

Now that our assumptions are in place, it is easy to state the formal result:

Impossibility Theorem

No paraphrase-function for the language of arithmetic can satisfy A_1 – A_3 .

The proof is totally straightforward. By assumptions 1 and 2, our output-language has a finite lexicon and quantifiers of finite type; by assumption 3, our paraphrase-function can preserve truth-value over pure sentences even if the output-language is assumed to have a finite domain. But the set of truths of a language with a finite lexicon and quantifiers of

finite type on a finite domain is effectively specifiable. So our paraphrase-function would deliver a decision procedure for arithmetical truth, which we know to be impossible from Gödel's Theorem.¹²

To the extent that one is prepared to think of A_1 – A_3 as capturing the idea that there are to be no controversial assumptions, one can think of this result as showing that no paraphrase-function for the language of arithmetic can be uncontroversially trivialist.

4 A Way Forward

Let us take stock. We started out by noting that the nominalist faces a challenge: she needs to explain what the point of making a mathematical assertion might be. We then noted that the challenge might be addressed by offering nominalistic paraphrases for mathematical sentences, and going on to claim that the point of asserting a mathematical sentence can be to convey the content of its paraphrase.¹³

What should a nominalist paraphrase-function look like? I listed three constraints in section 2, and suggested that paraphrase-functions satisfying those constraints—i.e. *trivialist* paraphrase-functions—should be thought of as the ‘gold standard’ of nominalist paraphrase. We have seen, however, that that the Impossibility Theorem suggests that there is no way of specifying a trivialist paraphrase function for the language of arithmetic without making controversial assumptions.

What is the nominalist to do? She could embrace one of the nominalist paraphrase-functions we discussed above, and insist that it is a trivialist paraphrase-function by making a controversial assumption. Or she could settle for a nominalist paraphrase-function that falls short of the gold standard.

Here I will propose an alternative. It seems to me that the *real* reason to be interested

¹²Thanks to Vann McGee for pointing out a strengthening of the original result.

¹³I say ‘can be’ rather than ‘is’ because one might think that the truths of *pure* arithmetic have trivially satisfiable contents, and it is not obvious that conveying such contents would be particularly interesting. For further discussion of the point of mathematical assertions, see (Rayo 2013, Chapter 4).

in nominalistic paraphrases is that one can use them to claim that the *nominalistic content* of a mathematical sentence is the literal content of the sentence’s nominalist paraphrase—where the nominalistic content of a sentence is the requirement that the world would have to satisfy in order for a given sentence to be true ‘as far as the non-mathematical facts are concerned’.

What I propose to do here is cut out the middle man. I will argue that there is a method for specifying the nominalistic contents of arithmetical sentences that does not proceed via paraphrases. This alternative method has an advantage and a disadvantage. The advantage is that it delivers *trivialist* contents (i.e. contents satisfying analogues of the three conditions in section 2), and does so without making controversial philosophical assumptions of the kind discussed in section 3. The disadvantage is that the method is couched in mathematical language, and is therefore only available to someone who is prepared to engage in mathematical practice. As we will see in section 4.2, this places certain limits on the purposes for which the proposal can be deployed.

4.1 Outscoping

On the view I would like to discuss, one assigns nominalistic contents to mathematical sentences by way of a *compositional semantics*: an assignment of semantic values to basic lexical items, together with a set of rules for assigning semantic values to a complex expression on the basis of the semantic values of its constituent parts.

I will assume that the semantic value of a sentence is a set of possible worlds. Accordingly, a compositional semantics should allow us to prove a statement of the following form for each sentence ϕ of the object language:

ϕ is true at world w if and only if w is such that ...

The usual way of interpreting such a clause is as a specification of ϕ ’s *truth-conditions*, that is, as a specification of the condition that a world w would need to satisfy in order for ϕ to

count as true at w . Here, however, we will be using the compositional semantics to specify *nominalistic contents*, rather than truth-conditions. Accordingly we will interpret the clause above as supplying a specification of the condition that a world w would need to satisfy in order for ϕ to count as true ‘as far as the non-mathematical facts are concerned’.

Suppose, for example, that a compositional semantics delivers the following clause for ‘the number of the dinosaurs is 0’ (where ‘ $[\dots]_w$ ’ is read ‘at w , it is the case that ...’).

‘ $\#_x(\text{Dinosaur}(x)) = 0$ ’ is true at w if and only if $[\text{there are no dinosaurs}]_w$

The right-hand-side of this clause specifies the following condition on w : that it represent reality as being such that there are no dinosaurs.¹⁴ Accordingly, if the relevant semantics is thought of as a specification of nominalistic contents, one should interpret the clause as stating that what it takes for ‘ $\#_x(\text{Dinosaur}(x)) = 0$ ’ to count as true ‘as far as the non-mathematical facts are concerned’ is for the world to satisfy the condition that there be no dinosaurs.

The example above has the form:

ϕ is true at w if and only if $[p]_w$

where ‘ ϕ ’ is a sentence of the object-language and ‘ p ’ is a nominalistic *paraphrase* of that sentence in the metalanguage. A compositional semantics that only outputs sentential clauses of this form will be severely limited in its ability to specify nominalistic contents, since it presupposes that one is in a position to specify a nominalist paraphrase-function for the object-language in the metalanguage. And, as we have seen, there are good reasons for thinking that it is impossible to specify a trivialist paraphrase-function for the language of arithmetic without making controversial assumptions.

Fortunately, a compositional semantics need not be restricted to outputs of the above form. It can deploy *outscoping*. To see what outscoping is all about, it is useful to contrast the following semantic clauses:

¹⁴What is it for a possible world w to *represent* reality as being such that p ? It is simply for it to be the case that p at w .

[WIDE]

‘ $\exists x(\text{President}(x) \wedge \text{Mustache}(x))$ ’ is true at world w if and only if [there is an x such that x is the president of the United States and x wears a mustache] $_w$

[NARROW]

‘ $\exists x(\text{President}(x) \wedge \text{Mustache}(x))$ ’ is true at world w if and only if there is an x such that x is the president of the United States and [x wears a mustache] $_w$

The only difference between [WIDE] and [NARROW] is the scope of the ‘[. . .] $_w$ ’ operator. But one can see that the difference is significant by considering the following question: How must a world w represent reality if it is to satisfy the right-hand-sides of each of the two clauses?

In the case of [WIDE] the answer is straightforward: w must represent reality as being such that there is an x such that x president of the United States and x wears a mustache. Accordingly, [WIDE] might be thought of as associating the following (unsurprising) condition with ‘ $\exists x(\text{President}(x) \wedge \text{Mustache}(x))$ ’: that there be an x such that x is president of the United States and x wears a mustache.

In the case of [NARROW], however, we get significantly different results. In the actual world, the president of the United States is Barack Obama. So, in order for a world w to satisfy the right-hand-side of [NARROW], it must represent reality as being such that Barack Obama wears a mustache, whether or not he happens to be president. Accordingly, [NARROW] might be thought of as associating the following condition with ‘ $\exists x(\text{President}(x) \wedge \text{Mustache}(x))$ ’: that Obama—the man himself—wear a mustache.

So, whereas [WIDE] specifies a content whereby the president—whoever that may be—wears a mustache, [NARROW] specifies a content whereby Obama—whatever his occupation—wears a mustache.

Let us now consider an arithmetical example. The following two semantic clauses for ‘the number of the dinosaurs is zero’ differ only in the scope of the ‘[. . .] $_w$ ’ operator:

[WIDE]

‘ $\#_x(\text{Dinosaur}(x)) = 0$ ’ is true at w if and only if [the number of x s such that x is a dinosaur = 0] $_w$

[NARROW]

‘ $\#_x(\text{Dinosaur}(x)) = 0$ ’ is true at w if and only if the number of x s such that [x is a dinosaur] $_w = 0$.

How must w represent reality in order to satisfy the right-hand-sides of each of these clauses? In the case of [WIDE], w must represent reality as being such that there is a *number* which numbers the dinosaurs and is identical to zero. So [WIDE] associates a *Platonist* content with ‘ $\#_x(\text{Dinosaur}(x)) = 0$ ’: a content whose satisfaction requires the existence of numbers. In the case of [NARROW], on the other hand, all it takes for the right-hand-side of the clause to be satisfied is for nothing to be such that w represents *it* as being a dinosaur. So [NARROW]—unlike [WIDE]—remains *neutral* on the question of whether w must represent reality as containing numbers.¹⁵

The crucial feature of [NARROW] is, of course, that all arithmetical vocabulary has been *outscooped*: it has been removed from the range of ‘ $[\dots]_w$ ’. So although one uses arithmetical vocabulary in the metalanguage to *characterize* a requirement on w , the requirement itself brings in no specifically arithmetical constraints: it is simply the requirement that nothing be counted by w as a dinosaur. The role of arithmetical vocabulary is to impose a metatheoretical *test* that will ensure that such a requirement is met: one asks, in the metalanguage, for the number of the objects that are counted as dinosaurs by w and demands that that number be zero. But because the test is performed while working *outside* the scope of ‘ $[\dots]_w$ ’, one doesn’t have to presuppose that the resources one uses to perform the test are present in w .

¹⁵Here I assume, for simplicity, that the domain of the metalanguage includes merely possible objects. Without this assumption—or, alternatively, without the assumption of necessitism (Williamson 2013)—[NARROW]’s right-hand-side will be satisfied by worlds which represent reality as containing dinosaurs but don’t represent *of any actually existing individual* that it is a dinosaur, and it therefore won’t succeed in associating with ‘ $\#_x(\text{Dinosaur}(x)) = 0$ ’ the condition that there be no dinosaurs. Happily, there is a technical trick that allows one get the right results without surrendering modal actualism (or contingentism); see (Rayo 2008), (Rayo 2012) and (Rayo 2013, Chapter 6) for details.

As it turns out, it is possible to give a compositional semantics that delivers suitably outscoped semantic clauses for *every sentence in the language of arithmetic* (see appendix). Not just that: the resulting clauses succeed in delivering the gold standard. They specify *trivialist* nominalistic contents, and do so with no need for controversial philosophical assumptions of the kind we discussed in section 3. (They do not, however, deliver a *paraphrase-function*, since our semantic clauses do not assign a non-mathematical sentence to each mathematical sentences. But, of course, the Impossibility Theorem suggests that that would be too much to hope for.)

Our semantics assigns every truth of pure arithmetic a trivial semantic clause (i.e. a clause whose right-hand-side will be satisfied by a world w regardless of how reality is represented by w), and it assigns every falsehood of pure arithmetic is assigned an impossible semantic clause (i.e. a clause whose right-hand-side will fail to be satisfied by w regardless of how reality is represented by w). The reason we get this result is that, when it comes to sentences of pure arithmetic, *everything* gets outscoped. The clause for ‘ $1 + 1 = 2$ ’, for example, will be:

‘ $1 + 1 = 2$ ’ is true at w if and only if $1 + 1 = 2$

in which *nothing* remains in the scope of ‘ $[\dots]_w$ ’. Since the right-hand-side of this biconditional is true (and contains no free variables), it will be satisfied by w regardless of how the world is represented by w . So our semantics will assign a trivial nominalistic content to ‘ $1 + 1 = 2$ ’.

4.2 What Outscoping Can and Cannot Do

Our trivialist semantics is couched in an arithmetical language. So use of the theory presupposes that one is able to understand arithmetical vocabulary. Not just that: in order to extract illuminating results from an outscoped semantic clause, one usually needs to prove an arithmetical claim in the metatheory. (In arguing that the semantic clause for ‘ $1 + 1 = 2$ ’

delivers a trivial content, for example, I made use of the fact that $1 + 1 = 2$.) So illuminating use of our semantic theory presupposes that one is able to prove arithmetical results. Either of these presuppositions would be utterly uncontroversial in a non-philosophical context. But it is worth considering how they play out in the present discussion.

It will be useful to start by seeing things from the point of view of a *mathematical Platonist*: someone who thinks that mathematical objects exist. Suppose, for example, that our Platonist is interested in the project of understanding which nominalistic contents a nominalist would wish to associate with arithmetical sentences. Since the Platonist feels comfortable using arithmetical vocabulary, she is in a position to set forth the trivialist semantics we have been discussing, and read off the nominalistic content of arithmetical sentences from the outscoped semantic clauses that are delivered by the semantics.

In doing so, our Platonist will have found a way around the Impossibility Theorem of section 3. For even if she lacks a general method for characterizing trivialist *paraphrases* for arithmetical sentences, our Platonist will have succeeded in finding a general method for characterizing trivialist *contents* for arithmetical sentences. It is true that she will have *used* arithmetical vocabulary in the process. But this is no threat to the project because the contents themselves will involve no specifically arithmetical constraints.

We started out assuming that the Platonist is interested in the project of characterizing nominalistic contents in order to better understand the nominalist, but she might also be interested in the project for a different purpose. Suppose she wishes to understand how arithmetical claims can be *relevant* to one's knowledge of the natural world (Steiner 1998, Yablo 2001). She might hypothesize that the answer is partly to do with the fact that an arithmetical claim like 'the number of the dinosaurs is zero' can impose non-trivial demands on the natural world, and see her outscoped semantic clauses as supplying a precise statement of the relevant demands.

Relatedly, our Platonist might wish to know whether the operation of *content-subtraction* delivers interesting results in the arithmetical case. Our Platonist will see the trivialist se-

mantic theory as decisive proof that the operation is well-defined, and delivers results of the right kind. For she will see the outscoped semantic clause corresponding to each arithmetical sentence as a precise statement of the result of subtracting ‘the mathematical part’ from the relevant arithmetical claim. (This is what I meant in section 3.3 when I reported thinking that Melia’s use of the subtraction-operation was, in fact, well-defined.)

We have been seeing things from the perspective of a Platonist. But what would a nominalist make of our trivialist semantics? It seems to me that the important issue is not whether one is a nominalist, but whether one is prepared to *engage in mathematical practice*. Suppose, for example, that our nominalist is also a fictionalist, and that she is happy to engage in mathematical practice: she proves mathematical theorems and uses mathematical vocabulary in making claims about the world; in her more philosophical moments, however, she insists that her mathematical assertions are set forth ‘in a spirit of make believe’ and that there really are no mathematical objects. A nominalist of this kind should have no difficulty working with the trivialist semantics that we have been discussing, and using outscoped semantic clauses to give a precise statement of the nominalistic content of her mathematical assertions.

A nominalist who *would* be barred from employing the trivialist semantics is what might be called a *nominalistic zealot*: someone who thinks that one cannot engage in normal mathematical practice. The zealot would not be prepared to assert ‘the number of the dinosaurs is zero’ in describing the world, even if she thought there were no dinosaurs; similarly, she would not be prepared to use a mathematically formulated semantic clause to characterize a nominalistic content. Our trivialist semantics is also unavailable to a mathematical *novice*: someone who is not competent in the use of mathematical vocabulary.

We set out to give a precise characterization of the nominalistic contents of arithmetical sentences. Had we been able to do so by way of an uncontroversially trivialist paraphrase-function, we might have been in position to satisfy both the zealot and the novice.¹⁶ But the

¹⁶Paraphrase-functions, like other functions, are mathematical objects. So whether or not a given paraphrase-

Impossibility Theorem shows that there are real limits to what can be done when it comes to giving paraphrases. Our trivialist semantics allows us to circumvent the theorem, but it is only available to someone who is prepared to engage in ordinary mathematical practice. It seems to me, however, that such an exclusion is not a particularly high price to pay, because neither the zealot nor the novice represent serious philosophical positions.

4.3 Beyond Arithmetic

We have seen that it is possible to give a compositional semantics that delivers suitably outscoped semantic clauses for every sentence in the language of (pure and applied) arithmetic. As it turns out, it is also possible to use the outscoping technique to characterize a trivialist semantics for the language of (pure and applied) *set-theory*. Full details are supplied in the appendix, but the basic idea is straightforward. In place of a standard homophonic semantic clause such as:

[WIDE]

‘Socrates \in $\{x : x \text{ is a philosopher}\}$ ’ is true at w if and only if [Socrates \in $\{z : z \text{ is a philosopher}\}$] $_w$

one uses an outscoped semantic clause such as:¹⁷

[NARROW]

‘Socrates \in $\{x : x \text{ is a philosopher}\}$ ’ is true at w if and only if Socrates \in $\{z : [z \text{ is a philosopher}]_w\}$

Although the outscoping technique happens to be available both in the case of arithmetic and in the case of set-theory, it is important to be clear that these results are not automatic:

function could actually be used to satisfy the zealot or the novice might depend on just how the function is presented to them. Consider, for example, the Fregean paraphrase-function of section 3.2. If such a function were to be described as a set of order-pairs, neither the zealot nor the novice would be moved. But one might get better results if one presents it a finite list of syntactic rules for transforming any given arithmetical sentence into the target second-order sentence.

¹⁷Here and below, I retain the simplifying assumption that the domain of the metalanguage includes merely possible objects.

there is no general reason to think that outscoping will be available whenever abstracta are used to describe features of the concrete world.

The best way to see this is to consider an example. Suppose that a mass of one kilogram is defined as the mass of N carbon-12 atoms (where ‘ N ’ is replaced by some particular numeral),¹⁸ and suppose that one wishes to specify a nominalist content for ‘Oscar’s mass-in-kilograms is 72’. One might suggest an outscoped semantic clause such as the following:

‘Oscar’s mass-in-kilograms is 72’ is true at w if and only if

$$\exists X((\#_x(Xx) = N \times 72) \wedge \forall x(Xx \rightarrow [^{12}\text{C-atom}(x)]_w) \wedge [\text{SameMassAs}(\text{Oscar}, X)]_x)$$

Although this clause has the right kind of flavor, it would presumably need to be refined in a number of ways. Notice, to begin with, that it presupposes that second-order quantification is nominalistically unproblematic, since a second-order variable occurs within the scope of ‘ $[\dots]_w$ ’. (It is possible to outscope the relevant variable, by making suitable mereological assumptions.¹⁹) Notice, further, that our clause presupposes that w contains enough carbon-12 atoms to establish an equal-mass comparison with Oscar. This won’t be a problem in this particular case, if Oscar is an ordinary-sized human and if w is a world roughly like our own. But it will be a problem if one wants to generalize the proposal to talk about, e.g. the mass of the entire universe. Perhaps one could amend the clause so as to allow for mass

¹⁸As of the time of this writing, a mass of one kilogram is officially defined as the mass of the International Prototype Kilogram, a particular artifact which is kept in a vault in the outskirts of Paris. An unhappy consequence of this definition is that every time the Prototype loses an atom, the mass of everything else in the world increases. The definition has nonetheless been kept in place because, until recently, we lacked the technology to produce more precise measurements using alternate definitions. It is likely that a new definition will be adopted soon, however. The definition I consider above is the simplest of the proposals under consideration.

¹⁹If one is prepared to countenance mereological sums, for example, then one can replace the right-hand-side of the original clause with the following:

$$\begin{aligned} &\exists X(\#_x(Xx) = N \times 72 \wedge \forall x(Xx \rightarrow [^{12}\text{C-atom}(x)]_w) \wedge \\ &\exists z([\text{SameMassAs}(\text{Oscar}, z)]_x \wedge \forall x(Xx \rightarrow [\text{PartOf}(x, z)]_w) \wedge \forall y([\text{Overlaps}(y, z)]_w \rightarrow \exists v(Xv \wedge [\text{Overlaps}(y, v)]_w)))) \end{aligned}$$

comparisons with different kinds of fundamental particles.²⁰ Even if that works, however, other problems might emerge. What should one do, for example, if one thinks that there are worlds in which the masses of fundamental particles differ from their actual masses?

The lesson of our example is the availability of outscoping is not automatic. Each new way of using abstracta to describe features of the concrete world calls for new type of outscoped semantic clause, and it is by no means obvious that suitable semantic clauses will always be available.²¹ On the other hand, the problem of finding suitable clauses is closely linked to the problem of better understanding the ways in which talk of abstracta conveys information about the way the world is. So limitations in our ability to outscope may sometimes reflect limitations in our understanding of the relevant subject-matter.

5 Logicism

When we discussed outscoping earlier in the paper, we were thinking of it as a means for specifying the nominalistic contents of arithmetical sentences rather than their literal truth-conditions. But there is room for arguing that our trivialist compositional semantics is, in fact, an accurate statement of *literal* truth-conditions. On such a view, all that is required of the world for ‘the number of the dinosaurs is zero’ to be literally true is that there be no dinosaurs, and *nothing* is required of the world for ‘ $1 + 1 = 2$ ’ to be literally true.

Should one conclude from this that arithmetical sentences don’t carry commitment to numbers? Not according to the version of the proposal I wish to consider here. I would like to consider a view whereby it is *both* the case that ‘the number of the dinosaurs is zero’ is

²⁰Here is a modified right-hand-side, where P_1, \dots, P_k is a list of all types of fundamental properties possessing mass, and, for each $i \leq k$, N_i particles of type P_i have a mass of one kilogram:

$$\exists X_1 \dots \exists X_k \left(\frac{(\#_x(X_1(x)))}{N_1} + \dots + \frac{(\#_x(X_k(x)))}{N_k} = 72 \wedge \right. \\ \left. \forall x \left(([\text{FundParticle}(x) \wedge \text{PartOf}(x, \text{Oscar})]_w) \leftrightarrow (X_1(x) \wedge [P_1(x)]_w) \vee \dots \vee (X_k(x) \wedge [P_k(x)]_w) \right) \right)$$

²¹For relevant discussion, see (Williams 2010).

committed to the number zero, *and* that all that is required of the world for ‘the number of the dinosaurs is zero’ to be literally true is that there be no dinosaurs.

The proposal escapes incoherence by endorsing the following claim:

For the number of the dinosaurs to be zero *just is* for there to be no dinosaurs.

and, more generally,

[NUMBERS]

For the number of the *F*s to be *n* *just is* for it to be the case that $\exists!_n x(Fx)$.

A friend of [NUMBERS] thinks that there is *no difference* between there being no dinosaurs and their number’s being zero, in the same sort of way that there is no difference between drinking a glass of water and drinking a glass of H₂O. More colorfully: when God created the world and made it the case that there was water to be drunk, there was nothing *extra* she needed to do or refrain from doing to make it the case that there was H₂O to be drunk. She was already done. Similarly, a friend of [NUMBERS] thinks that when God created the world and made it the case that there would be no dinosaurs in 2013, there was nothing *extra* she needed to do or refrain from doing to make it the case that the number of the dinosaurs in 2013 would be zero. She was already done.

An immediate consequence of [NUMBERS] is that a world without numbers would be *inconsistent*:

Proof: Assume, for *reductio*, that there are no numbers. By [NUMBERS], for the number of numbers to be zero *just is* for there to be no numbers. So the number of numbers is zero. So zero exists. So a number exists. Contradiction.

One might therefore think of [NUMBERS] as delivering a *trivialist* form of mathematical Platonism—the number zero exists, but its existence is a trivial affair. And, of course, it is not just the existence of the number zero that is a trivial affair: one can use [NUMBERS] to

show that each of the natural numbers must exist, on pain of contradiction, and to show that they are distinct from one another. (That is what I had in mind in section 3.2 when I reported thinking that Trivialist Infinitarianism is true.)

Someone who accepts [NUMBERS] can claim *both* that the truth-conditions of ‘the number of the dinosaurs is zero’ consist entirely of the requirement that the number of the dinosaurs be zero, *and* that they consist entirely of the requirement that there be no dinosaurs. She can make both these claims because she thinks that the proposed requirements are one and the same: there is *no difference* between there being no dinosaurs and their number’s being zero.

The trivialist semantic theory we set forth in the preceding section can be used to generalize this idea to every sentence in the language of arithmetic. One can claim that the literal truth-conditions of an arithmetical sentence are accurately stated *both* by a standard (homophonic) compositional semantics *and* by our trivialist semantics with outscoped semantic clauses. But the two semantic theories do not contradict one another because the truth-conditions they associate with a given sentence are, in fact, one and the same: there is *no difference* between what would be required of the world to satisfy the truth-conditions delivered by one semantic theory and what would be required of the world to satisfy the truth-conditions delivered by the other.²²

Consider ‘ $1 + 1 = 2$ ’ as an example. A standard (homophonic) semantics tells us that the truth-conditions of ‘ $1 + 1 = 2$ ’ demand of the world that it contain numbers. Our trivialist semantics tells us that the truth-conditions are trivial—that they will be satisfied regardless of how the world happens to be. But the two claims are consistent with each other because the existence of numbers is a trivial affair. ‘ $1 + 1 = 2$ ’ carries commitment to numbers, but this is a commitment that will be satisfied regardless of how the world happens to be.

What does this tell us about *logicism*—the view that mathematics can be reduced to logic? The Impossibility Theorem of section 3 suggests that the formal systems that contemporary

²²For a detailed defense of this view see (Rayo 2013, Chapters 1 and 2).

philosophers tend to think of as ‘pure logic’ are not expressive enough to capture basic arithmetic. So one might think of the theorem as a refutation of logicism: in an interesting sense, mathematics cannot be reduced to logic. But one could also think of the view developed in the present section as a certain kind of *vindication* of logicism. For it delivers the result that the truths of pure arithmetic—like the truths of pure logic—have trivially satisfiable truth-conditions, and the result that the falsehoods of pure arithmetic—like the falsehoods of pure logic—have impossible truth conditions. Admittedly, one also gets the result that a truth of pure arithmetic can carry commitment to numbers. But because the existence of numbers is a trivial affair, there is room for thinking of numbers as ‘logical objects’, as in Frege’s *Grundgesetze* .

6 Concluding Remarks

In this paper I have tried to shed new light on mathematical nominalism.

I began with the observation that the nominalist is committed to answering a particular challenge. She must explain what the *point* of making a mathematical assertion could be, if there are no numbers. One way of addressing this challenge is to argue that the point of mathematical assertions is not to communicate the literal content of the sentence asserted, but to communicate its *nominalistic content*: the requirement that the world would need to satisfy in order to make the sentence true ‘as far as non-mathematical facts are concerned’.

It is natural to suppose that one can specify the relevant nominalist contents by setting forth a nominalistic *paraphrase function*: an effectively specifiable procedure that assigns to each mathematical sentence a non-mathematical paraphrase in such a way that the nominalistic content of the mathematical sentence matches the literal content of its paraphrase. We have seen, however, that there is a formal result that suggests that it is *impossible* to specify a suitable paraphrase-function for the language of arithmetic, in the absence of potentially controversial assumptions.

One might have been tempted to think of the Impossibility Theorem as a decisive blow to the nominalistic dream of specifying nominalistic contents for arbitrary mathematical sentences. But we have seen that nominalistic paraphrase-functions are not the only way of specifying nominalistic contents. The method of outscoping makes it possible to construct a *compositional semantics* that assigns the right nominalistic contents to arbitrary arithmetical (and set-theoretic) sentences.

This result sheds light on nominalism in two different ways. First, it allows us to discard the idea that the case for nominalism ought to be linked to the availability of a nominalistic paraphrase function—a bad idea from the start, since it tied the *metaphysical* thesis that there are no numbers to potentially controversial *linguistic* theses concerning the legitimacy of particular expressive resources.

Second, and more importantly, our outscoped semantics shows that the notion of nominalistic content can be rigorously defined, and is therefore suited for serious philosophical work. We noted, in particular, that it can be used to address the question of how mathematical claims can be relevant to one's knowledge of the natural world. But we also noted that it can be used to reassess nominalism, by allowing one to give a rigorous characterization of a subtle variety of Platonism: a view according to which there is *no difference* between what would be required of the world to satisfy the nominalistic content of a given arithmetical sentence and what would be required of the world to satisfy the truth-conditions that would be assigned to that sentence by a homophonic semantic theory.

From a purely mathematical point of view, there is no particular reason to prefer Subtle Platonism over its rivals. But Subtle Platonism is philosophically significant because it casts doubt on Benacerraf's Dilemma: the idea that one must choose between holding onto the claim that mathematical assertions carry commitment to mathematical objects, and making contentious claims about our cognitive relationship to a causally inert realm of abstract objects (Benacerraf 1973). The Dilemma is sometimes construed as an argument for nominalism, since it seems to suggest that only the nominalist could have a sensible epistemology

of mathematics. But when Subtle Platonism is treated as a live option, we can no longer take for granted that commitment to numbers comes with epistemological costs. (The Subtle Platonist would argue, for example, that someone who has verified that there are no dinosaurs is thereby in a position to know that the number of dinosaurs is zero, since the fact that there are no dinosaurs is *already* the fact that the number of dinosaurs is zero.²³) If this is right, then the notion of a nominalistic content—which we first introduced in an effort to help nominalists answer a challenge—can also be used to cause trouble for nominalism, by allowing for rigorous development of a rival view.²⁴

²³For further discussion, see (Rayo 2013, Chapters 3 and 4).

²⁴For their many helpful comments, I am grateful to Duilio Guerrero, Bernhard Salow and Steve Yablo, to participants at MIT's Logic, Language and Metaphysics Reading Group, and to audiences at the University of Missouri, Kansas City, the Università Vita-Salute San Raffaele and Smith College.

Appendix

The material in this appendix is drawn from (Rayo 2013, Chapter 3), where I discuss further technical details. (As noted in footnotes 15 and 17, I assume, for simplicity, that the domain of the metalanguage includes merely possible objects; but the assumption can be avoided by appeal to the technique described in (Rayo 2013, Chapter 6). In the case of arithmetic, the details are spelled out in (Rayo 2008).)

1. A Trivialist Semantics for the Language of Arithmetic

We work with a two-sorted first-order language with identity, L . Besides the identity-symbol ‘=’, L contains *arithmetical* variables (‘ n_1 ’, ‘ n_2 ’, ...), individual-constants (‘0’) and function-letters (‘S’, ‘+’ and ‘ \times ’), and *non-arithmetical* variables (‘ x_1 ’, ‘ x_2 ’, ...), constants (‘Caesar’) and predicate-letters (‘Dinosaur(...)'). In addition, L has been enriched with the function-letter ‘ $\#_v(\dots)$ ’ which takes a first-order predicate in its single argument-place to form a first-order arithmetical term (as in ‘ $\#_{x_1}(\text{Dinosaur}(x_1))$ ’), which is read ‘the number of the dinosaurs’).

Let σ be a variable assignment and w be a world. $\delta_{\sigma,w}(t)$ will be our denotation function, which assigns a referent to term t relative to σ and w ; $Sat(\phi, \sigma, w)$ will be our satisfaction predicate, which expresses the satisfaction of ϕ relative to σ and w ; and $[\phi]_w$ will be our true-at-a-world operator, which expresses the thought that ϕ is true at w . Denotation and satisfaction are defined simultaneously, by way of the following clauses:

Denotation of arithmetical terms:

1. $\delta_{\sigma,w}(\ulcorner n_i \urcorner) = \sigma(\ulcorner n_i \urcorner)$
2. $\delta_{\sigma,w}(\ulcorner 0 \urcorner) = \text{the number Zero}$
3. $\delta_{\sigma,w}(\ulcorner S(t) \urcorner) = \delta_{\sigma,w}(t) + 1$

4. $\delta_{\sigma,w}(\ulcorner t_1 + t_2 \urcorner) = \delta_{\sigma,w}(t_1) + \delta_{\sigma,w}(t_2)$
5. $\delta_{\sigma,w}(\ulcorner t_1 \times t_2 \urcorner) = \delta_{\sigma,w}(t_1) \times \delta_{\sigma,w}(t_2)$
6. $\delta_{\sigma,w}(\ulcorner \#_{x_i}(\phi(x_i)) \urcorner) =$ the number of zs such that $Sat(\ulcorner \phi(x_i) \urcorner, \sigma^{z/\ulcorner x_i \urcorner}, w)$
7. $\delta_{\sigma,w}(\ulcorner \#_{n_i}(\phi(n_i)) \urcorner) =$ the number of ms such that $Sat(\ulcorner \phi(n_i) \urcorner, \sigma^{m/\ulcorner n_i \urcorner}, w)$

Denotation of non-arithmetical terms:

1. $\delta_{\sigma,w}(\ulcorner x_i \urcorner) = \sigma(\ulcorner x_i \urcorner)$
2. $\delta_{\sigma,w}(\text{'Caesar'}) = \text{Gaius Julius Caesar}$

Satisfaction:

1. $Sat(\ulcorner \exists n_i \phi \urcorner, \sigma, w) \leftrightarrow$ there is a number m such that $Sat(\phi, \sigma^{m/\ulcorner n_i \urcorner}, w)$
2. $Sat(\ulcorner \exists x_i \phi \urcorner, \sigma, w) \leftrightarrow$ there is a z such that $([\exists y(y = z)]_w \wedge Sat(\phi, \sigma^{z/\ulcorner x_i \urcorner}, w))$
3. $Sat(\ulcorner t_1 = t_2 \urcorner, \sigma, w) \leftrightarrow \delta_{\sigma,w}(t_1) = \delta_{\sigma,w}(t_2)$
4. $Sat(\ulcorner \text{Dinosaur}(t) \urcorner, \sigma, w) \leftrightarrow [\delta_{\sigma,w}(t) \text{ is a dinosaur}]_w$ (for t a non-arithmetical term)
5. $Sat(\ulcorner \phi \wedge \psi \urcorner, \sigma, w) \leftrightarrow Sat(\phi, \sigma, w) \wedge Sat(\psi, \sigma, w)$
6. $Sat(\ulcorner \neg \phi \urcorner, \sigma, w) \leftrightarrow \neg Sat(\phi, \sigma, w)$

2. A Trivialist Semantics for the Language of Set-Theory

We work with a two-sorted first-order language with identity, L . Besides the identity-symbol ‘=’, L contains the membership predicate ‘ \in ’, set-theoretic variables ($\alpha_1, \alpha_2, \dots$), urelement variables (x_1, x_2, \dots), and urelement predicate-letters (‘Philosopher(...)’).

As before, we let σ be a variable assignment and w be a world. $Sat(\phi, \sigma, w)$ will be our satisfaction predicate, which expresses the satisfaction of ϕ relative to σ and w ; and $[\phi]_w$ will be our true-at-a-world operator, which expresses the thought that ϕ is true at w . Satisfaction is defined as follows:

Satisfaction:

1. $Sat(\ulcorner \exists x_i \phi \urcorner, \sigma, w) \leftrightarrow$ there is a z such that $([\exists y(y = z)]_w \wedge Sat(\phi, \sigma^{z/\ulcorner x_i \urcorner}, w))$
2. $Sat(\ulcorner \exists \alpha_i \phi \urcorner, \sigma, w) \leftrightarrow$ there is a set β such that: (i) for any urelement z in the transitive closure of β , $[\exists y(y = z)]_w$, and (ii) $Sat(\phi, \sigma^{\beta/\ulcorner \alpha_i \urcorner}, w)$
3. $Sat(\ulcorner x = y \urcorner, \sigma, w) \leftrightarrow \sigma(x) = \sigma(y)$
4. $Sat(\ulcorner \alpha \in \beta \urcorner, \sigma, w) \leftrightarrow \sigma(\alpha) \in \sigma(\beta)$
5. $Sat(\ulcorner x \in \beta \urcorner, \sigma, w) \leftrightarrow \sigma(x) \in \sigma(\beta)$
6. $Sat(\ulcorner \text{Philosopher}(x) \urcorner, \sigma, w) \leftrightarrow [\sigma(x) \text{ is a philosopher}]_w$
7. $Sat(\ulcorner \phi \wedge \psi \urcorner, \sigma, w) \leftrightarrow Sat(\phi, \sigma, w) \wedge Sat(\psi, \sigma, w)$
8. $Sat(\ulcorner \neg \phi \urcorner, \sigma, w) \leftrightarrow \neg Sat(\phi, \sigma, w)$

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Nominalism and History

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The paper focuses on Nominalism in history, its application, and its historiographical implications. By engaging with recent scholarship as well as classic works, a survey of Nominalism's role in the discipline of history is made; such examination is timely, since it has been done but scantily in a purely historical context. In the light of recent theoretical works, which often display aporias over the nature and method of historical enquiry, the paper offers new considerations on historical theory, which in the author's view may solve some of the contradictions that have surfaced in recent times. The Nominalistic stance is argued against by disputing theorists such as Paul Veyne, who has made a case for Nominalism in history. A brief philosophical section introduces Nominalism in its metaphysical dimension and the discussion is speedily brought to its significance for history. The paper also proposes a solution to the misconstrued yet too often vague application of scientism in history, and offers theoretical grounds that might solve some of the 'stormy grounds' historiography finds itself today. Articles by Marcel Gauchet and *History and Theory's* Anton Froeyman and Bert Leuridan are engaged with, as well as Murray Murphy's books on the philosophy of history. Works by Georg Gadamer, Marc Bloch, Benedetto Croce, Hyppolite Taine, and Anthony Grafton crucially inform the discussion and brace the consequential conclusion.

Keywords: Nominalism; Particulars & Universals in History; Nomothetic History; Empiricism; Hermeneutics; History as Humanity vs Science; Historical Narrative

Introduction

The role of Nominalism in history has been seldom discussed, seldom considered, and even more seldom analyzed. Surely if asked, any historian would tender an opinion on the Nominalist/Realist antithesis, and favor one persuasion over the other as it relates their convictions about what is sound historical methodology. But most, I venture to say, would recognize that they have not given deep thought about how this crucial philosophical argument applies to their historical thinking. The dispute between Realists and Nominalists has raged in metaphysics for over two millennia—and is still the source of animated debates—but in historical circles the argument, save a few instances, has not been confronted directly on its own, diametrical terms. The aim of this essay is to present the reader with an assessment of the role of Nominalism in history, which, as far as I know, has not yet been wholly laid out for an outright historical readership. Although I shall present the various pertinent sources and their theories in order for readers to form an opinion of their own, I should like to point out that this essay's objective is to discredit the idea that strict Nominalism alone be an apposite stance in conceiving history. Still, I believe particulars to be the cornerstone for historical understanding; and yet, I am also convinced that historians who ignore universals and exclusively scrutinize particulars will find their work wanting of characteristics, which if overlooked, shall fatally compromise their historical apprehension. In other words, I wish to show that though particulars have a vital place in history—and we shall see why—Nominalism is epistemologically deficient,

especially in history, for universals are an inalienable aspect of human understanding, and thus are essential for a thorough conception of history and a comprehensive historical grasp: history's singularly extensive compass requires a broad vision that accepts both universals and particulars.

Robert Hume appositely stated that, "[t]he philosophy of history has long been a stormy ground, and it will probably remain so." (Hume, 1999: p. 13) In fact, philosophers of history are continuously examining and arguing over the ontology of history; its fractious, hybrid nature is ultimately an endless source of speculation and fervid discussion. The main point of contention—which is also fundamental for our analysis—seems to me the question of whether history is a science or a humanistic pursuit, and, if it is the latter, how and in what way does it differ from other humanistic disciplines, due to its para-scientific slant. While it is not the task of this essay—provisionally—to enter this acerbic dispute, our study of Nominalism and history must necessarily include a discussion of history as science and history as a humanity, because those who favor the Nominalistic stance tend toward the persuasion of history as a form of empirical knowledge, and thus view it through scientific lenses; on the other hand, Realists envision history as a discipline under the aegis of the humanities, whose epistemic tools are hermeneutics rather than Method.¹

I will show that despite the quagmire of opinions, currents, and theories, whether one regards history as a science or as a

¹As has been shown by Gadamer in *Truth and Method*. Gadamer, however, did not write about Nominalism at all.

humanistic pursuit strict Nominalism is fatally reductive to either conviction: it is in serious philosophical tension with the former and irreconcilable with the latter.

Since Nominalism and Realism's connection to history can best be judged with a clear understanding of their more abstract, philosophical perspective, I will first present the Nominalist/Realist antithesis in its purely philosophical dimension; this should provide the necessary understanding when the argument is applied to history. Next, I will cover the few, meaningful sources that pertain to our discussion; these shall be from different philosophies of history, which examine the question on a purely historiographical basis. I have already stated that the sources dealing directly with Nominalism and history are scanty: philosophers of history who labor either for the scientific or the humanistic view often do not confront Nominalism and Realism directly, and thus some degree of inference shall be required to locate their stance from their arguments.² I will then cover the most heated and productive dispute—that between Paul Veyne and Marcel Gauchet, over the legitimacy of historical Nominalism—which is the only modern debate directly centered on the philosophical and historiographical positions we are examining. Lastly, I will both attempt to present what seems to me the most sensible solution to the tendentious, Nominalist/Realist polemic and provide a sound argument against these unnecessarily polarized tenets and their role in history. I hope to provide at the very least, a certain degree of mental order—of food for thought—rarely furnished to the practicing historian, on the relation of Nominalism and history.

A Brief Philosophical Preparation

It is a well-known fact that the first to posit the theory of universals was Plato. This epistemological theory is of cardinal importance: it has engaged metaphysical speculation since its inception; its claims have been as fecund as any great question in philosophy. In fact, whether we know the world through its intelligible manifestations, through direct perception, or, whether we only really know the world through Forms, the unintelligible, is really the marrow of epistemology. Throughout his dialogues, Plato continuously alludes to what he refers to as “Forms”. The *Republic* is the classic, most quoted example: “shall we proceed as usual and begin by assuming the existence of a single nature or Form for every set of things which we call by the same name?” (Plato, 1961: p. 820) These words, pronounced by Socrates in the dialogue, are as ambiguous as they are famous. David Armstrong, who has devoted the most comprehensive modern survey of Nominalism and Realism in his two-volume work, *Universals & Scientific Realism*, points out the ambiguity of this passage: “But is Plato here arguing that the Form is required for the name to be meaningful? That is the way in which he is often interpreted. However, it is at least as plausible to suggest that the underlying argument is that sameness of name requires sameness of nature in the things named.” (Armstrong, 1978: Vol. 1, p. 98) Despite the ambiguity of the act of naming things³ in the statement quoted from *Republic*—whether sameness of name requires sameness of nature in

the things named or whether Forms are a prerequisite for the ability to name things—particulars are understood by Plato to be a subset of Forms (“the existence of a *single* nature or Form for every *set* of things which we call by the same name”). In *Parmenides*, Plato is more specific in detailing the discrete existence of Forms: “Do you believe that there is such a thing as likeness itself apart from the likeness that we possess? Certainly I do, said Socrates.” (Plato, 1961: p. 924) Here Plato is positing that universals and particulars exist as separate entities.

The Platonic theory of Forms is very complex due to the allusive, almost epigrammatic way which Plato scatters his dialogues with his references to them; Plato's idea of universals is open to a number of interpretations, which may lead to rather different conclusions. It is not our concern here to examine the theory of universals in all its metaphysical ramifications, but to lay the basic philosophical principles which will be engaged when discussing Nominalism and Realism and their role in historical perception; the reader must merely be made aware of the choices offered by this vital epistemological dilemma: does our knowledge stem exclusively through our direct perception of particulars or as an emanation of Forms? Do we accept or reject the existence of universals? This is the question which has fomented endless discussion—and in some cases derision: Wittgenstein famously claimed this problem to be a non-issue.

But let us now define with as much clarity as possible what Nominalists and Realists believe. I shall call upon David Armstrong's definition for both terms:

There are those philosophers who hold that when we say truly that two tokens are of the same type, then sameness is to be understood in terms of strict identity. The two different tokens have something strictly identical. [...] If, for instance, two different things have the same color, then this must be taken strictly. One and the same thing, the color, is a constituent of the two things. Historically, these philosophers are called **Realists** and are said to believe in the reality of universals.

On the other side there are philosophers who think that when we say that a number of tokens are all of the same type, then all that we are saying is that the different tokens are non-overlapping parts of some larger whole or unity (the tokens are all member of one class, or they all resemble each other in a certain way, or some other such formula). The sameness of the tokens is only loose and popular.

These philosophers hold with John Locke, that “all things that exist are only particulars”. There are no strict identities reaching across different tokens; there are no universals. Philosophers who hold such a view are traditionally called **Nominalists**. (Armstrong, 1989: p. 5)

In other words, Nominalism is the rejection of universals, while Realism is the belief in their existence. There are various forms of Nominalism—Concept Nominalism, Class Nominalism, Resemblance Nominalism, which all fall under the so-called heading of Predicate Nominalism—as well as various kinds of Realism: Immanent Realism and Scientific Realism. These distinctions are immaterial to our discussion and I shall relegate them to broader, strictly metaphysical discussions. It is now time to speak of the concepts outlined above in relation to history and see how they affect our conceiving history.

²Such inferences shall not be unmotivated and neither discretionary nor arbitrary.

³Plato dedicated his *Cratylus* to discussing the implications of naming things (especially at the end of the dialogue from sections 438 to 440), man as a name-giving creature, and language's relationship to truth.

Particulars and History

The first to proclaim that historians dealt above all with particulars, or “singulars”, as he referred to them, was Aristotle. In *Poetics* Aristotle distinguished the historian from the poet as follows:

The distinction between historian and poet is not in the one writing prose and the other verse—you might put the work of Herodotus into verse, and it would still be a species of history; it consists really in this, that the one describes the thing that has been, and the other a kind of thing that might be. Hence poetry is something more philosophic and of graver import than history, since its statements are of the nature rather of universals; the statements of history are singulars. (Aristotle, 1946: p. 1451b)

Aristotle thus inaugurated the Nominalist viewpoint in the discipline of history. This persuasion had enough thrust to persevere until today and has been the source of many fruitful debates in historiography, though most debates are not always conscious of the Nominalism/Realism antithesis at their root nor its implications when applied to the discipline of history; furthermore, the friction between universals and particulars is aggravated, when, as historians, we apply the indefeasible element of our discipline—temporality—in our consideration. For example, when we say that, “a certain person whom we saw today is the very same person that we saw yesterday [...] does that mean that the person today and the person yesterday are actually identical?” (Armstrong, 1989: p. 3) Thus we can see how temporality complicates particulars and their perception, for we can say with some confidence that the person yesterday is the same as the person today. But only loosely. Strictly speaking, “they are different temporal parts of a single four-dimensional entity, the person.” (Armstrong, 1989: p. 4) This ties itself to a principle—controversial in philosophy—called the Identity of Indiscernibles, which holds that “if a and b have all their properties in common, then a is identical with b. In other words, sameness of properties gives sameness of thing.” (Armstrong, 1989: p. 3) Universals, too, are affected by this principle, for in order to recognize them—just as we do particulars—we must apply the principle of Identity of Indiscernibles. In history, this involves the recognition of sameness through time for universals—if we believe in them, of course. But since history is the study of the *change* of human practices, particulars clearly bear greater significance for the historian, for it is by analyzing particulars that we can readily recognize change: temporality makes particulars dependent on their temporary instant, and therefore significant inasmuch as they reveal the historical moment we are examining. On the other hand, in the animated debate between Paul Veyne and Marcel Gauchet, we shall see that universals are not to be discounted. The dispute was centered on particulars—how, why, and to what extent they are meaningful in historical inquiry. In examining their claims, I think what should transpire is that particulars alone and the refutation of universals—Nominalism—severely limits the historian’s gaze.

Paul Veyne and the Chimera of the Nominalist Historian

Paul Veyne is the modern historian who wrote explicitly about Nominalism and history, and who argued for a Nominal-

ist outlook. He wrote about it in his theoretical writings on history. His first book, *Writing History*, is full of driving ideas about historical methodology as well as fruitful considerations about the ways a historian crafts his work. Though throughout the book Veyne offers a number of stimulating insights, he often stumbles in contradictions that mar the coherence of his thought: Veyne’s view of history is staunchly Nominalist, yet his statements are often incongruous with the implications of Nominalism. Very early he states that, “nothing is more reasonable than a Nominalist conception of history” (Veyne, 1984: p. 43) and explains the legitimacy of this position by stating that “we know historical types do not exist in themselves, that events are not reproduced with the constancy of living species, that the typical in history is a choice [...] in short, the types are infinite in number, since they exist only through us. Once again, we have to come to the conclusion of historical Nominalism.” (Veyne, 1984: p. 121) Veyne’s argument is essentially that the historian ought to look to particulars and reject universals—which here he calls “historical types”—since only the former can reveal the historical moment in its uniqueness, thus implying that the belief in universals hampers a historian’s understanding. In his most well known essay, *Foucault Revolutionizes History*, Veyne further expanded this idea:

In short, in any given era the set of practices gives rise, on a given material point, to a unique historical countenance in which we think we recognize what is called, in vague terms, historical science or religion; but what takes shape at that same point in another era will have its own unique and very different countenance and, conversely, a countenance vaguely similar to the earlier one will take shape at a some other point. This is what denying the existence of natural objects means: across the ages we do not encounter the evolution or modification of a single object that always appears in the same place. (Veyne, 1997: p. 171)

Again, Veyne makes a powerful and convincing case for the uniqueness of any historical moment, but he is less persuasive when he argues that across the ages we never encounter the same things. A few pages later, Veyne drives this point further: “there is no concrete trans-historical truth.” (Veyne, 1997: p. 174)

Marcel Gauchet fulminated Veyne for his extreme Nominalist position. In an article called *Le nominalisme historien. A propos de “Foucault révolutionne l’histoire” de Paul Veyne*, Gauchet faulted Veyne for the stringent Nominalism he displayed in his essay on Foucault, and claimed Veyne’s ideas to be the result of a naïve skepticism “scepticisme naïf” (Gauchet, 1984: p. 409), reminiscent of a second-degree scientism that could never allow authentic historicity. For Gauchet, the Nominalist epistemology is a “nullifying” philosophy: “[with his] strange epistemology, no time is seriously considered by Veyne to historical conditions of formation of this knowledge of historical fact according to standards of accuracy. This generalized genealogy excludes but one genealogy: *its own*. In other words, everything is historical, except history.” (Gauchet, 1986: p. 407)⁴ Furthermore, according to Gauchet, Veyne is ignorant of the foundations of historical methodology. He refers

⁴Or, étrange épistémologie, à aucun moment il n’est sérieusement réfléchi chez Veyne aux conditions historiques de constitution de cette connaissance du fait historique selon des normes d’exactitude. Cette vision généalogique généralisée n’exclut qu’une généalogie: la sienne propre. Tout est hiostorique, en somme, sauf l’histoire.

to Veyne's "authentic misunderstanding" citing Veyne's claim that historians of antiquity and the Middle Ages were a-critical, because they built their histories upon their predecessors'. According to Gauchet, this view is summary and erroneous, since Gauchet points out that historians before historicism were indeed critical⁵ but were so with completely different criteria from our own. For Gauchet, Veyne's ignorance creates a fatal blind spot in Veyne's historical epistemology; it is that spot, which accounts for the Nominalist's fortune.

The main task which an authentic epistemology of today must provide: dissolve the sophisms that naturally result in this renewed version of universal mobility. The fact that everything is historical does not mean that everything is relative, that history is made of nothing but radical heterogeneities and woven by singular, incomparable emergences. But it is precisely this challenge, that all is historical, that must be met. What does it mean, exactly? That death, tears, childhood, dreams, sexuality, folly are historical in their essence and not like natural objects always identical with themselves; but what does that truly mean? Since there is uncertainty about this point, skepticism and historical Nominalism arise and thrive. (Gauchet, 1986: p. 406)⁶

However Gauchet did explain particulars' own, rightful place in conceiving history, and wrote about it exemplarily: "History [is] the *emergence, the advent* of forms than cannot be *explained*, except by missing what matters in them, that is, what these forms have that is *incomparable*. From this comes the necessity of the historian's Nominalism, the only position that can adequately open him to the *inexplicable singularities* of a process of permanent innovation." (Gauchet, 1986: p. 403)⁷

Paying close attention to particulars to make the historian "open to the singularities" of a period is a process which we shall examine in its metaphysical dimension; but after Gauchet's sensible statement we have established particulars' exigency and that universals alone are insufficient for conceiving history. For example, if we consider the idea of "the State" we surely cannot find much in common among the Roman State in the first century AD, the State under Louis XIV, and, say, the bureaucratic Napoleonic State.⁸ Clearly, if we were to look exclusively through the lens of forms we'd make historically insignificant statements. (However, further on I shall present a more sophisticated concept of universals that discounts Nominalism in every epistemological maneuver.)

⁵He is quite right in saying so: see Nadel, 1964.

⁶[...] la tâche prioritaire que doit se proposer une authentique épistémologie historique aujourd'hui: dissoudre les sophismes qui paraissent naturellement découler de cette version renouvelée de l'universelle mobilité. Le fait que tout est historique ne signifie aucunement que tout est relatif, que l'histoire n'est fait que d'hétérogénéités radicales et tissé que des surgissements incomparables. Mais c'est très précisément à ce défi du *tout est historique* qu'il s'agit de répondre. Qu'est-ce au juste que cela veut dire? Que la mort, les larmes, l'enfance, la rêve, la sexualité, la folie soient d'essence historique et non pas autant d'objets naturels toujours identiques à eux-mêmes, qu'est-ce que cela véritablement signifie? Car c'est de l'incertitude sur ce point que naissent et prospèrent le scepticisme et le nominalisme historiques.

⁷L'histoire qui est le *surgissement, l'avènement* de formes qui ne saurient s'*expliquer*, sauf à manquer ce qui compte en elles, à savoir ce que elles comportent d'incomparable. D'où le nécessaire nominalisme de l'historien, seul à même de l'ouvrir adéquatement aux *singularités inexplicables* d'un processus d'innovation permanente.

⁸The example is Gauchet's.

Gauchet thus expounded brilliantly on the need to concentrate on particulars in historical enquiry. But the benefits of a composed Nominalism in conceiving history surely are not new: "Herder set a universal historical worldview against the Enlightenment's teleological view of history [...] to acknowledge that each period has its own right to exist, in its own perfection." (Gadamer, 2004: p. 198) I think it should now be evident that particulars do have a fundamental role in our understanding of a historical moment.

And yet, as Fustel de Coulanges said, "History is not the accumulation of facts and events of every sort that have been produced in the past: it is the science of human societies" (Bloch, 2005: p. 71)⁹ and as such, one must be aware that, as we saw above, contrary to Veyne's stating the contrary, "trans-historical truths" exist, because there *are* constants in human nature—vanity, rapacity, the wish for a better position in society, love, lust, etc. And the historian who disregards timeless human traits inevitably shall not set them against the period he is studying—which is of course exemplary and unrepeatable—thus finding his compass of vision considerably diminished by such heedlessness. Furthermore, Veyne's negation of trans-historical truths presents another, perhaps greater problem, especially for historians: a generation inherits certain beliefs and practices from a previous one; as that transference occurs, these beliefs and practices gradually change. By denying these constants the effects of temporality on man are ignored. That is nonsensical for an historian, whose charge is to be a most sensitive needle on the scale of change over time.

Let us remember Dilthey's precept that "we can explain things but we understand men". This important distinction shoulders us to what is probably the most insightful quote from Marc Bloch's *The Historian's Craft*, that undisputed masterpiece of twentieth-century historical methodology: "If men, who are the object of our study, fail to understand us, how can we feel that we have accomplished more than half our mission?" (Bloch, 1953: p. 86-87) Therefore, if we be understanding of men, how could we discount universals, which inexorably constitute his nature? And furthermore, the rightful insistence that these two great historians placed on *understanding over explanation* is really a charge that explanation in history has scientific inclinations, either by virtue of an unobtainable law-covering model, or by impossible empiricism for the causal explanations of events. And so, a crucial aporia rises before us here: a choice must be made between hermeneutic and scientific knowledge, for a hermeneut *cannot* be an empiricist. This was demonstrated most powerfully by Gadamer in *Truth and Method*: Gadamer revealed hermeneutics to be an ulterior form of knowledge bearing truth *outside* Method—the indefeasible foundation of natural science.

With this in mind, Veyne's contradictions begin to emerge, and they reveal to be problematic. Two contradictions in Veyne's *Writing History*, which diminish the efficacy of his theory considerably, are most pertinent to our discussion. The first is his statement that "[t]he historical explanation is not nomologic, it is causal; as causal, it contains something general". Is Veyne flirting here with Realism ("it contains something general") and contradicting his purported Nominalism

⁹La storia non è l'accumulazione degli avvenimenti d'ogni tipo che si sono prodotti nel passato: essa è la scienza delle società umane.

Marc Bloch cited de Coulanges's affirmation in his last, scattered papers on history, written just before being shot in 1944 by the Gestapo.

with the irruption of universals in historical explanation? Secondly, Veyne, argues that “history is not a science.” (Veyne, 1884: p. 144) But Veyne, in his essay on Foucault, praised him for his empiricism—for statements such as, “history becomes an empirical science of events and that radical mode of being that prescribes their destiny to all empirical beings, to those particular beings that we are” (Foucault, 1994: p. 219)—which according to Veyne made Foucault a better kind of historian. We have already seen the ontological tension that arises from these two positions standing side by side, for it is impossible for them to be bridged in any way.

Even in his most defensible apology for Nominalism Veyne finds himself hampered by this single-handed theoretical concern: “[...] historical Nominalism, the vague character of sub-ordinary causality, makes it that no order of causes constantly imposes itself as more decisive than the others.” (Veyne, 1984: p. 280) I think Veyne’s insistence on Nominalism, is the manifestation of a natural and widespread fear among historians—the fear of being faulted for not being sufficiently analytical. But analytical acumen is but one ingredient that makes a great historian. And so, Gauchet’s charge of Veyne’s relative ignorance of historical methodology seems correct to me: Veyne’s statement above, which upon its first reading seems sound, is really only valid for attempts at causal explanations of historical events, but it is utterly useless for broader notions of history, which as I suggested above, must also understand man, and gauge with great accuracy the change in human practices. It is worthwhile here to remember Vico’s celebrated dictum that history “discloses the realm of culture, not nature”: and so, the historian who handles culture must take into account human temperament, and the latter—it should be stressed again—has universals and forms that ought not to be dismissed. In addition, Vico’s statement is an excellent refutation—on its own—for using natural science’s practices (contra Veyne and his empirical stance).

Despite my quibbles with Veyne’s theoretical writing, I should like to point out that when he actually writes history, Veyne is a great historian, and practices history in the most integral fashion. But in his theoretical writings there is an underlying philosophical uneasiness, which stands in the way of his speculations. However, this should not diminish Veyne’s accomplishments in our eyes, since often people do something very well even if the theory they use to explain what they do is flawed.

Nominalism’s Attempts at a View of History Conforming to Scientific Knowledge

An interesting yet defective attempt for a philosophy of history analogous to science and that brims with sharp Nominalistic positions is offered by Murray Murphy. In his book, *Philosophical Foundations of Historical Knowledge*, Murphy seeks rather hopelessly to reconcile the theories of historical causal explanation put forth by Carl Hempel and Paul Oppenheim in their Deductive-Nomological model for scientific explanation—with which they sought to explain any given historical event with a series of “logically deductive premises” (Murphy, 1994: p. 98)—and the concept of “culture”, which according to Murphy must also be considered as an explanation for human actions. Murphy casts eight propositions about historical knowledge, which he believes to be verifiable; the last is also the thorniest, for it states that “human actions are causally explain-

able.” (Murphy, 1994: p. x)

For human actions to be causally explainable, Murphy upholds to the so-called “covering law”:

This [...] model of explanation [is] from the metaphorical idea that the general law “covers” the particular case. It involves certain presuppositions that should be noted. One is that all laws are general, that is, the law cannot contain any reference to a particular. This was seen as necessary to rule out “general” statements such as “All chairs in this room are made of wood”. For the same reason, Hempel and Oppenheim stipulated that laws could contain only purely qualitative properties, so that properties referring to particulars (e.g., “earthly”) are proscribed. (Murphy, 1994: p. 98)

At first this may seem to be a Realist position, since laws evince some form of ‘generality’ and are meant to be universal. But Nominalists hold that only physical particulars in space and time exist, and that universals, which do not, are at best subsequent to particular things; therefore general laws are brought into being by particulars, and, in the case of history, laws would provide predictability, which of course is unfeasible. Robert Hume, who, in his excellent book, *Reconstructing Contexts: The Aims and Principles of Archaeo-Historicism* labels himself an empiricist, attacks Murphy’s view by saying, “I think Murphy is overstating his case. To say that human action has causes is one thing; to say that we can identify them is something else” (Hume, 1999: p. 15). The last clause is apodictic: it is disarmingly obvious that every historian who has attempted to predict the future by using causal explanations for past events to forge laws for events, such as, say, revolutions, has always failed. Obviously, the covering law refutes universals categorically and places Murphy firmly in the Nominalist camp; Murphy accepts the covering law as an explanation for causality of events as well as action in history: “I believe there is no real doubt that the covering law model provides an explanation for an action [...]” (Murphy, 1994: p. 155). Hume expounds on the reasons for the mania of giving history a scientific footing to give it epistemological certainty as well as the deriving distortions of such attempts brilliantly.

Historians have long suffered from a dangerous hankering to be as precise and rigorous as physicists, and more than half a century ago history took a terrible turn when Hempel published “The Function of General Laws in History”. Historians spent the next thirty years trying to get out from under the demands that follow from Hempel’s attempt to impose on history the logical structure of explanation he found in physics. The gist of the “covering law model” is simplicity itself: explanation can be achieved “by subsuming what is to be explained under a general law”.

In the cold aftermath of repentance at leisure, this is manifestly a lunatic idea. If history has general “laws”, they are not of the sort to be found in classical physics. Physical science attempts to deal with something more or less available in the present; history attempts to explain the past now unrecapturable except via extrapolation from traces. The degree to which billiard balls can be used to explain human behaviour is evidently limited. More than a century ago Dilthey rightly distinguished between physical science (concerned with causal explanation of present

phenomena) and history (concerned with comprehension of a vanished past). (Hume, 1999: pp. 15-16)

That a self-proclaimed empiricist like Robert Hume states unequivocally the fatal pitfalls of scientism applied to history is significant; and lunacy is indeed what incites a statement by Michael Scriven, which Murphy quotes in defense of his eighth proposition, which as we saw above states that “human actions are causally explainable”, and which Scriven drives to its paroxistic locus: “causality is the most important explanatory function in history.” (Murphy, 1994: p. 102) Now I should like to know which historian, in Scriven’s or Murphy’s view, has fully *explained* the cause(s) of, say, the French Revolution, which is about the most written event in history. Can an arsenal of “scientists” redact a nomological system for the Revolution? Compared to these desiccated attempts, Hyppolite Taine’s overemphasized and infinitely figurative concept of “l’esprit classique” is indeed a tonic—for the *comprehension* of the spirit which animated the Revolution:

[Taine’s] thesis is that the philosophy of the eighteenth century was the product of the “classic spirit”, which was invented by Descartes and the essence of which was to pursue the absolute, and worship uniformity. When the French mind turned to politics it proceeded to prescribe according to the dictates of pure reason. This neglect of the individual, the concrete, the real, was the mark alike in literature, of the Philosophes and of the Revolution, and its predominance was the main cause of the tragedies of modern France. (Gooch, 1952: p. 228)¹⁰

Taine, like most historians of the nineteenth century, believed that history had both scientific and literary claims, which lent his history to a number of critical approaches. In his great work, *Les Origines de la France Contemporaine*, Taine had mastered the lessons of the august German school of history of the nineteenth century—that great efflorescence, which had produced unparalleled works written by men who engaged in the rigorous praxis of basing their histories on primary sources, as well as the necessity of understanding the reasons behind the actions of men. Accordingly, Taine employed a monumental archival knowledge with an almost unique, insightful psychological understanding; his great work is thus at the same time political philosophy, psychological history, social ethics, and, owing to its unique literary focus, literary criticism as well. For Taine, history was both an art and a science; his concept of the “esprit classique” sought—as Dilthey urged—also to *comprehend*, not just to *explain*. Bloch’s reiteration of this is thus noteworthy: “This faculty of understanding the living is, in very truth, the master quality of the historian.” (Bloch, 1953: p. 43)

The rich idea of “l’esprit classique” opposes Veyne’s much vaunted idea that Foucault’s merit—and supposed superiority—was that he was both an empiricist and a profoundly skeptical thinker “who believed only in the truth of facts [...] never in the truth of ideas” (Veyne, 2010: p. 1)¹¹ and that thanks to this supposedly sharpened, empiricist gaze, he managed to “peel away the banalities and notice that there is more to ex-

plain” (Veyne, 1997: p. 156) than what was previously understood about a period. But in arguing that Foucault revolutionized history Veyne forced the issue.¹² Again, the example of the French Revolution stands before us: if we are to explain it through facts, such as, among others, the failure of France to reflect “the change of the distribution of property and wealth [that] ceased to be the prerogative of a few” (Acton, 2000: p. 1), or the 1788 drought, we shall find that facts are not at all enough: I can think of a number of droughts and food shortages in numerous principalities in the eighteenth century, non of which resulted in a revolution, as neither did the iniquitous and anachronistic socio-economic conditions of the Kingdom of the Two Sicilies in the nineteenth century. Taine’s argument of the *thought* that permeated France has much import and cannot be discounted: unquestionably, ideas do exist and possess a truth just as facts do, much to Veyne’s discomfiture.

The imposing—and slanted—theoretical structure that Murphy is elevating, is irretrievably weakened by a fatal contradiction, which is his latest work, *Truth and History*, is most flagrantly evident:

History, as all historians agree [sic], is a form of empirical knowledge. Accordingly, the logic of history is similar to that of other forms of empirical knowledge. The basis of historical work is evidence, which as every philosopher of history, from Collingwood on, has agreed, consists of observations made on artifacts from the past. [...] It follows that the historian’s basic task is the finding and the interpretation of such artifacts. (Murphy, 2009: p. 177)

Interpretation? Is Murphy stretching his hand to hermeneutics? Again, as with Veyne, the empirically-leaning historian is faced with the irreconcilable, logical disjunction of being both an empiricist and a hermeneutist—a hopeless desire. Murphy also manifests a serious epistemological inconsistency when at first he states, “as an empiricist, I do not believe there is any way of knowing reality except through the theory that best explains our data, and I see nothing to be gained by the belief in an unknowable metaphysical entity.” (Murphy, 2009: p. 12) But only a page later, Murphy takes umbrage with Bas van Fraassen, whom he considers too severe an empiricist, due to his intransigence,¹³ which he considers it to be “an extreme form of empiricism that denies reality to anything not directly observable by us with our unaided senses.” (Murphy, 2009: p. 14) But empiricism *is* severe in that it must obey rigorous rules and it does not allow unobservable data to be admitted for theoretical purposes. It need be so: if we were to betray its framework—*Method*—our entire scientific knowledge would collapse.

Let me address Murphy’s statement that “History, as all historians agree, is a form of empirical knowledge”. Can empiricism, explanation, causality, and all these conceptual ingredients of the scientifically minded, yield statements of such profundity as the following by Johann Huizinga?

The great divide in the perception of the beauty of life comes much more between the Renaissance and the modern period than between the Middle Ages and the Renaissance. The turnabout occurs at the point where art and life

¹⁰For Taine’s own, extensive presentation of “l’esprit classique”, see Chapter 2 of Book 3 in: Taine, 1986.

¹¹Here Veyne is projecting his thought onto Foucault’s: in *The Order of Things*, Foucault evinces a clear regard for ideas and their veracity (See next footnote).

¹²Veyne misrepresents his interpretation of Foucault in a number of ways. For a closer look at Veyne’s flawed interpretation of Foucault, see Franchetti, 2011.

¹³See Fraassen, 1980.

begin to diverge. It is the point where art begins to be no longer in the midst of life, as a noble part of the joy of life itself, but outside of life as something to be highly venerated, as something to turn to in moments of edification or rest. The old dualism separating God and the world has thus returned in another form, that of the separation of art and life. Now a line has been drawn right through the enjoyments offered by life. Henceforth they are separated into two halves—one lower, one higher. For medieval man they were all sinful without exception; now they are all considered permissible, but their ethical evaluation differs according to their greater or lesser degree of spirituality.

[...] For the medieval man enjoyment per se is sinful. The Renaissance had managed to free itself from the rejection of all the joy of life as something sinful, but had not yet found a new way of separating the higher and lower enjoyments of life; the Renaissance wanted an unencumbered enjoyment of all of life. The new distinction is the compromise between the Renaissance and Puritanism that is at the base of modern spiritual attitudes. It amounted to a mutual capitulation in which one side insisted on saving beauty while the other insisted on the condemnation of sin. [...] Only after the Puritan worldview lost its intensity did the Renaissance receptiveness to all the joys of life gain ground again; perhaps even more ground than before, because beginning the eighteenth century there is a tendency to regard the natural per se as an element of the ethically good. Anyone attempting to draw the dividing line between the higher and lower enjoyments of life according to the dictates of ethical consciousness would no longer separate art from sensuous enjoyment, the enjoyment of nature from the cult of the body, the elevated from the natural, but would only separate egotism, lies, and vanity from purity. (Huizinga, 1996: pp. 40-41)

Can logical induction yield insights into human nature such as this by Fernand Braudel?

Pius V was indeed one of these “upstarts”, not a “princely” pope, not a man familiar with the ways of the world and prepared to make those compromises without which ‘the world’ would not go round. He had the passion, rigour and intransigence of the poor. (Braudel, 1995: p. 1027)

These excerpts whose breadth reveal a singular comprehension of the past and of the human spirit is not based on mere particulars: it is an understanding that springs from the profound knowledge of a period’s facts inspired with the impulse of universality and a deep understanding of a specific culture, which undoubtedly includes particulars, but goes beyond them. (Reading the work of such historians reveals Collingwood’s notion that history is really the history of thought applied to history as bracing.) These remarkable excerpts come from historical masterworks of the twentieth century; both works are still source of admiration—and discussion. Is it possible that a Nominalist outlook by itself power such statements? Could the rejection of universals ever produce such singularly penetrating insights? I do not think so. A view that sees no strict identities reaching across different tokens—particulars—as the sole source of knowledge could never achieve what a Huizinga or a Braudel has. Surely, I am obviously not concerned with resolving the dispute—it never shall be—over whether history is a

form of knowledge that is attained through *Method*, or whether its knowledge is *hermeneutic*, different and autonomous from science, and thus belonging to the field of the humanities, but, I am concerned about arguing against Nominalism’s inadequacy, whatever persuasion a historian may hold about the nature of the knowledge of history.

Murphy is a potent thinker, but he is trapped by an epistemology “that is an empirical discipline located within science, rather than an a priori discipline prior to science” (Murphy, 1994, p. xii)¹⁴ and as such, he has an ax to grind, the ax of empiricism—a most encumbering ax—and after reading his lucid but overwrought ruminations one parts from his books feeling that the ideologue has exerted himself far too much to wield this ax, which may just be too grueling for history.

The reason for a number of philosophers of history’s case for Nominalism—and there are a number of them—is, I think, the fear of the specter of arbitrariness. And so, to avoid being labeled relativistic, the insecure historian legitimizes his methodology behind a gray scientism. I hope this essay be a warning to lesser historians who are not a Veyne, and do not possess his capability of immersing himself in a period’s specificity—regardless of the way he says one must go about it—and take refuge in a clerical empiricism which shears all beauty, effect, and meaning to their writing: the rejection of anything universal is the death of anything really historical, because it circumvents the human element—through time, of course—the paramount object of the historian’s gaze.

Let us examine more closely the claim that predictive laws and the generalizations they allow, be they causal or non-causal, are functional or indeed even possible in history. I wish to look at this more closely, because so much literature has been devoted to devise some kind of lawfulness in history. In a recent article in *History and Theory*, Bert Leuridan and Anton Froeyman argue for the use of general laws in historiography without the more extreme leanings of a Hempel or a Murphy: in no place do they claim history to be an empirical science. However, they insist that “laws in history can be made [...] clear and fruitful” (Leuridan; Froeyman, 2012: p. 182) by applying “pragmatic laws”, a “milder” form of lawfulness developed by Sandra Mitchell, which essentially holds that “evaluation is context-dependent” (Leuridan; Froeyman, 2012: p. 177) and thus “scientific generalizations [...] will seldom be completely universal [...] the important question is *how* and *to what extent* they are contingent. This means that if we want to use a generalization, we need to assess the stability of these conditions. [...] Stability is a very important parameter for the evaluation of a generalization’s usefulness.” (Leuridan; Froeyman, 2012: p. 177) Through this context-dependent view Leuridan and Froeyman believe a scientific generalization resembling a law is believed to be possible in history.

This argument is astute but it suffers from an ontological fallacy—as we’ve seen repeatedly, that of using scientific practices to define, delineate, and *delimit* historical apprehension. These stilted and somewhat artificial efforts are again the result of the fear of relativism, as if without some scientifically postured grounding history would cease to be a sound form of knowledge, like the natural sciences. These strained efforts seem to me unnecessary. The sibylline pretensions of causality in history are the result of a confusion about the epistemology of history, which in my view is hermeneutics and *not* scientific

¹⁴Murphy follows Quine’s view of epistemology and plainly states so.

method. If historiographers and philosophers of history did not have misconceptions about historical knowledge, or if they did not harbor any uncertainty of any other form of truth that is not scientific, it seems to me that they would not keep spearheading historical theory down a spurious path; and a lot of intellectual energy would not be dissipated.

It is perhaps for this reason, too, that footnotes became such an essential part of historical writing in modern times; as Anthony Grafton admirably put it, “footnotes are the outward and visible signs of this kind of history’s inward grace—the grace infused into history when it was transformed from an eloquent narrative into a critical discipline.” (Grafton, 1997: p. 24) In other words, footnotes are the underpinning of a discipline that its practitioners and theorists are often fearful could drift into mendacious waters. Grafton explains that the origin of history’s uncertainty was, in fact, “the vogue for Cartesian philosophy and experimental science. That, in turn, explains why Bayle felt it necessary to argue, at length, against the fashionable view that mathematics had an advantage over historical knowledge, in that ‘it leads to truths not susceptible to doubt’.” (Grafton, 1997: p. 208) We ought to keep in mind that a number of thinkers, of no less caliber than a Pascal, a Leibnitz, a Spinoza, a Bayle, a Vico all reacted against this constricting view of knowledge and argued “that those pure mathematicians and physicists, who are ignorant of and despise all other forms of knowledge, are wrong” (Grafton, 1997: p. 210)¹⁵ and that “certitudes of history, though different from those of mathematics, were far more concrete, more applicable to human life, and even more certain in a metaphysical sense than the profound abstractions of mathematics.” (Grafton, 1997: p. 208)¹⁶

That it was an illustrious scientist, who in modern times peremptorily contrasted between the two forms of knowledge we have been discussing is ironic: in a lecture given at the commencement of the academic year at the University of Heidelberg in 1862, Herman von Helmholtz made the historic distinction between the natural sciences and the human sciences, declaring the latter to be of superior and humane significance.

It is not easy for a scientific man to convey to the scholar or a jurist a clear idea of a complicated process of nature; he must demand of them a great power of abstraction from the phenomena, as well as a certain skill in the use of geometrical and mechanical conceptions, in which it is difficult for them to follow him. On the other hand an artist will perhaps find the natural philosopher too much inclined to mechanical and material explanations, which seem to him commonplace, and chilling as his feeling and enthusiasm. Nor will the scholar or the historian, who have some common ground with the theologian or the jurist, fare better with the natural philosopher. They will find him shockingly indifferent to literary treasures, perhaps even more indifferent than he ought to be to the history of his own science. In short, there is no denying that, while the moral sciences deal directly with the nearest and dearest interests of the human mind, and with the institutions it has brought into being, the natural sciences are concerned with dead, indifferent matter, obviously indispensable for the sake of its practical utility, but apparently without any immediate bearing on the cultivation of the

intellect. (Helmholtz, 1873: p. 9)

Helmholtz also argued against employing natural sciences’ epistemological parameters—what he called “logical induction”—in the human sciences:

We might possibly, in opposition to logical induction which reduces a question to clearly-defined universal propositions, call the moral science’s kind of reasoning aesthetic induction, because it is most conspicuous in the higher class of works of art. It is an essential part of an artist’s talent to reproduce by words, by form, by colour, or by music, the external indications of a character or a state of mind, and by a kind of instinctive intuition, uncontrolled by any definable rule, to seize the necessary steps which we pass from one mood to another. If we do find that the artist has consciously worked after general rules and abstractions, we think his work poor and commonplace, and cease to admire. On the contrary, the works of great artists bring before us characters with such a lifelikeness, with such a wealth of individual traits and such an overwhelming conviction of truth, that they almost seem to be more real than the reality itself, because all disturbing influences are eliminated. (Helmholtz, 1873: p. 16)

Finally, Helmholtz made an unequivocal statement about “aesthetic induction”: “This latter kind of induction, which can never be perfectly assimilated to forms of logical reasoning, nor pressed so far as to establish universal laws, plays a most important part in human life.” (Helmholtz, 1873: p. 15) That a man who was first and foremost a scientist, a physicist of no less caliber than the teacher of Max Plank, the pioneer of Quantum Physics, wrote such resounding words emphasizing the humanities’ superior importance is a lesson to all of us, especially to those who doubt the *truth* that the humanities reveal. But Helmholtz was a man of immense breadth of vision and is a figure in a class of his own who transcended the boundaries of science and art.¹⁷

If we compare Helmholtz’s idea that “logical induction” is not applicable to human sciences with Veyne’s theory of “retro-diction”, Helmholtz’s superior footing from which he is looking at the humanities is evident: Veyne wrote that, “[h]istorical synthesis is nothing but this operation of filling in; we shall call it ‘retro-diction’, borrowing the word from the theory of incomplete knowledge that is the theory of probabilities. [...] So all ‘retro-diction’ calls into play a causal explanation and perhaps even a true law. To study historical synthesis, or ‘retro-diction’, is to study the part played in history by induction and in what ‘historical causality’ consists.” (Veyne, 1984: pp. 144-145) Once again, these statements are spurred from the view that “logical induction” yields a superior form of knowledge to that of “aesthetic induction”. But Helmholtz annulled this fallacy. If Veyne had been acquainted with the modern German school of

¹⁷In addition to the countless and fundamental contributions to science—the law of conservation of energy, the electromagnetic equation, the invention of the acoustics resonator, the invention of the ophthalmoscope, and much more—Helmholtz laid out ideas, which were later developed by Freud that were indispensable for his forming of the concept of the unconscious. Furthermore, this authentic polymath developed the “Helmholtz resonator”, which was able to identify the pitch and the frequency of any sound; this machine as well as the book he wrote called *On the Sensations of Tone as a Physiological Basis for the Theory of Music* influenced musicologists up until the twentieth century.

¹⁵The quote is from Spinoza.

¹⁶The quote is from Bayle.

hermeneutics, which was commenced by Schleiermacher in the early nineteenth century and culminated with Gadamer¹⁸ in the late twentieth, who knows what interesting flowering of ideas for historiography would have flourished from his pen!

A Broader Concept of Universals

I wish to show that universals are not an incongruity in history—and neither in science—if we look at them from a wider perspective.

I cannot understand these other ingenious theories of causation. If someone tells me that the reason why a given object is beautiful is that it has a gorgeous color or shape or any such attribute, I disregard all these explanations—I find them all confusing—and I cling simply and straightforwardly and no doubt foolishly to the explanation that the one thing that makes that object beautiful is the presence in it or association with it, in whatever way the relation comes about, of absolute beauty. I do not go so far as to insist upon the precise details—only upon the fact that it is by beauty that beautiful things are beautiful. (Plato, 1961: pp. 81-82)

In this excerpt from *Phaedo*, Plato postulates that universals are capable of acting upon particulars (“it is by beauty that beautiful things are beautiful”): “In *Phaedo*, Plato endowed his Forms with causal power. They act upon particulars, giving the latter their nature, to the extent that they have a nature.” (Armstrong, 1978: Vol. 1, p. 128) Armstrong’s just observation brings him to theorize that universals and particulars may be conciliated even in empirical sciences, “since universals match up with the fundamental particles that science tells us about.” (Armstrong, 1989: p. 88) That should settle the case for the importance of universals in history with those, like Veyne, who tirelessly advocate for Nominalism for conceiving history. Furthermore, Armstrong rightly notes that “particulars have properties that stand in relations” (Armstrong, 1978: Vol. 2, p. 133) thus echoing Plato’s *Parmenides*, “I see nothing strange in [...] a proof that all things are one by having a share in unity and at the same time many by sharing in plurality.” (Plato, 1961: p. 923) Throughout his two volumes, Armstrong’s thorough discussion of Nominalism and Realism has the aim of accounting for universals’ existence as well as their having a role compatible with empiricism; by having shown that universals themselves possess properties and relations, which constitute *laws of nature*,¹⁹ he revoked the incompatibility of universals with empirical knowledge.

That is a tonic against the most skeptical philosophical thinkers, and, for what concerns us, the skeptical historiographers whom we have been examining.

So if an ideal is responsible for a manifestation of a particular, how would that manifest itself in practice—in conceiving history? For example, the idea of a unified Christian Empire existed from Charlemagne to Charles V; but in 800 AD what was achieved was very different from what the empire came to be under emperor Frederick II; and that, too, was different during Charles V’s rule. Frances Yates, for example, pointed out that Charles’ abdication in 1555 was an implicit realization that

the figure of an emperor under whom a unified Christianity could exist was anachronistic: the *ideal* of a Christian Empire had vanished.²⁰ In fact, the only real case for the existence of a Christian empire may be made for the fourth century AD, as André Piganiol persuasively outlined in his *L’Empire Chrétien*.²¹ According to him, for seventy years, from 325—the year of the Council of Nicaea, the first ecumenical council of the Church (promoted by Constantine the Great), in which the Trinity, the relationship between God the Father and The Son, the drafting of the Credo Niceum, as well as other fundamentals of doctrinal orthodoxy were settled—to 395—when Theodosius, the last emperor to rule over both halves of the Christianized Roman Empire died and the empire splintered forever in East and West, with the latter soon disintegrating with the Goth invasions—according to Piganiol a unified, Christian empire did exist. The perspective of such works, so rich in historical comprehension, is certainly the consequence of a regard for universals *as well as* particulars. And that seems to me understanding of a vanished past of the utmost value, just like the magisterial examples of Huizinga and Braudel.

With this concept of “powered” universals, even Gauchet’s justification for Nominalism can be criticized. If history is the study of *change* of human practices through time, and if, as Gauchet suggests, we understand historical events or practice merely as particulars, and thus Nominalism is “the only position that can adequately open him to the *inexplicable singularities* of a process of permanent innovation”, how are we to compare these singularities with another, if we do not see particulars as standing in relations? Would it be possible to set, using our example above, the Council of Nicaea *in relation* to the idea of empire? How could we make historical conclusions, if not against the necessary, immovable fixity of the universal which the particular we are examining is a set of? And even in the differentiation of particulars themselves—a fundamental task of the historian, as we’ve seen in the examples of “the State” in different epochs—how can one do so without universals? “Lacking universals, a Nominalist cannot relate them! So he is nailed to the Humean or the Singularist cross.” (Armstrong, 1978: Vol. 2, p. 151)

Dilthey spoke most convincingly of the relationship, and consequently of the existence of universals when he wrote that, “The individual always experiences, thinks, and acts in a sphere of commonality, and only in such a sphere does he understand. Everything that has been understood carries, as it were, the mark of familiarity derived from such common features. We live in this atmosphere; it surrounds us constantly; [...] we ourselves are woven into this common sphere. This results in a reciprocal dependence the way we apprehend each particular of the human sciences within the communal, historical whole of which it is a part [...] In the progress of the human sciences, [...] we apprehend the human world around us [from] the reciprocal dependence of universal and singular knowledge.” (Dilthey, 2002: pp. 168, 174)

Conclusion

In closing this essay I wish to sum up briefly the conclusions we can draw about Nominalism in history, and, as a result of my discussion, offer a view of history that may settle the

¹⁸For a discussion of the progression of hermeneutics’ gradual appropriation of its foundational role in history, see Franchetti, 2013.

¹⁹See Armstrong, 1978: Vol. 2, p. 4

²⁰See Yates, 1975: pp. 20-28.

²¹See Piganiol, 1947.

“stormy grounds” of historiography.

Particulars, we have seen, are fundamental for the historian to decipher, discern, and distinguish the period or the event he is scrutinizing; but, the consciousness of universals, too, is essential for the apprehension of any human occurrence. Nominalism or Nominalistic stances are upheld in historiography due to the disquieting but false pretense that accepting universals will induce haziness to the historian’s gaze: it is time that historians lose this fear, for the historian recreates a tapestry of the past through many “a ‘track’, as it were—the mark, perceptible to the senses, which some phenomenon, in itself inaccessible, has left behind.” (Bloch, 1953: p. 55) And so, both particulars *and* universals are essential for us to make sense of the world—even more so for historians who have to make sense of a world that no longer exists except in traces of it. Armstrong stated it perfectly when he said that, “[t]he conclusion drawn is that particularity and universality, irreducible to each other, are both involved in all existence.” (Armstrong, 1978: Vol. 1, p. xiv) This statement relates just as well to history.

Therefore particulars are absolutely indispensable, but the rejection of universals, Nominalism, is absolutely dispensable. Especially if we accept Armstrong’s wider view of universals that I have presented above, which makes universals reconcilable with empiricism. And that buries Nominalism for a Veyne—who not only stresses empiricism in historical apprehension, but also openly reject universals—but also for a Foucault and a Murphy, whom we have seen fixed on a view of history as an empirical discipline.

As to the status of history as an empirical discipline, we have seen that even if we were to accept the foolish idea that history is nothing but empirical knowledge, universals still could not be discounted. I hope to have shown to some extent that Nominalism in history is a misconstrual of the historian’s methodology, since, though essential, the scrutiny of particulars cannot occur *without* the awareness of universals. The historian must therefore harmonize *both* an empirical stance—especially when sources from the past are faced—and hermeneutical understanding. It is for this reason that history is an enormously difficult discipline, which Macaulay justly acknowledged to have but few masters; and it is for this reason that we historians must constantly be able to shift our thought from a macro to a micro degree of understanding; historical knowledge is just that.

It is true that history is indeed a form of factual knowledge, but since it pertains to occurrences in the past that cannot be tested, it cannot be considered a purely empirical discipline; yet, of course, there is factual evidence that forbids statements such as, “Louis XIV waged war against China in the tenth century”. That satisfies one of history’s offices—that of making accurate, discernible statements about the past. But obviously history has—justly—much wider contentions. Where the latter lie, *interpretation* enters the historian’s arsenal. And it is here that we are faced with a crux, which has cast an endless conceptual problem of history’s nature: Murphy has justly stated that, “it is fair to say that the philosophy of history is currently something of a mess.” (Murphy, 1994: p. x) This “mess” is principally due to the difficulty for minds grounded in an age which accepts scientific facts as the only source of truth to bridge the gap between *Method* and *hermeneutics*.²² I think it is precisely here that the fulcrum of the problem—and the solution—of the philosophy of history lies.

²²Here lies Gadamer’s invaluable contribution to our field: the persevering and patient elucidation that hermeneutics is a practice and *not* a method.

Salient examples are the texts we have examined: Veyne and Murphy all proclaim in their introductions that “history is not a science”, but in their discussion of “historical truth” they cannot hold back from Nominalism or logical formulae with lots of P’s and Q’s. This rather clearly shows that these historiographers have not bridged the gap between Method and hermeneutics, our discipline’s most equivocal aspect, which Anthony Grafton so rightly defined as “that strange hybrid of science and art.” (Grafton, 1997: p. 235)

So why are so many theoreticians so attached to a Nominalist position? As I have already noted, I think it is because in their eyes the specter of arbitrariness is raised every time we make a statement that employs universals, for it seems to them that calling upon universals leads down the relativistic path.

“The generations just prior to our own, in the last decades of the nineteenth century and even in the first years of the twentieth, were as if mesmerized by the Comtian conception of physical science. This hypnotic schema, extending to every province of the intellect, seemed to them to prove that no authentic discipline could exist which did not lead, by immediate and irrefutable demonstrations, to the formulation of absolute certainties in the form of sovereign and universal laws.” (Bloch, 1953: p. 14)

I believe along with Bloch and many others, that there is no need to apply scientism to history for fear of not being taken seriously; I have read endless, dispiriting volumes²³ that not only blindly defend Nominalism but advance even more radical and misconceived ideas of empirical strictures and law-covering models for history. The Nominalist view restricts from a complete and exhaustive understanding of history, because no matter which view one has of history, Nominalism is deficient, since it is irreconcilable from a humanistic perspective, and, from a scientific standpoint, Armstrong has convincingly noted that “where there are laws there exist universals.” (Armstrong, 1978: Vol. 2, p. 151) But such attempts to apply natural sciences’ systems to history imply that history is incapable of unveiling or unveiling truths opposed to scientific understanding.

[...] the human sciences are a long way from regarding themselves as simply inferior to the natural sciences. Instead, possessed of the intellectual heritage of German classicism, they carried forward the proud awareness that they were the true representatives of humanism. The period of German classicism had not only brought about a renewal of literature and aesthetic criticism, which overcame the outmoded baroque ideal of taste and of Enlightenment rationalism; it had also given the idea of humanity, and the ideal of enlightened reason, a fundamentally new content. More than anyone, Herder transcended the perfectionism of the Enlightenment with this new ideal of “cultivating the human” and thus prepared the ground for the growth of the historical sciences in the nineteenth century. The concept of self-formation, education, or cultivation (*Bildung*), which became supremely important at the time, was perhaps the greatest idea of the eighteenth century [...] (Gadamer, 2004: p. 8).

I should like to offer at this point a most compelling example of the untenability of strictly empirical views of history, which

²³Mark Day’s *The Philosophy of History*; Georg Iggers’s *Historiography in the Twentieth Century; What is History Now?* Edited by David Carradine; to cite just a few.

touches ever so closely upon history's essence itself. Say, for example, that historian *a* has read exactly the same texts pertaining to the Renaissance as historian *b*. Their vision and understanding of that period shall inevitably differ considerably. How can a view of history as an empirical form of knowledge withstand, let alone explain any discrepancy at all? Here, in my view, lies the marrow of history's dilemma: that different accounts would spring from sources identical with each other is the ever fascinating aspect of history, for history is but the thought of different men. The very root itself of the word "history" points to this: its root "his" is the Indo-European "vid" which simply means "view", suggesting that the cardinal factor in history-making is indeed a historian's own, particular outlook.

The latter is achieved by integrating judiciously history's singular, three main elements, which are unique to it: philology, hermeneutics, narrative. These are, according to me, the critical, constitutive elements of history. The first element is the most scientific in nature. Peculiarly though, it did not have historical pretensions at first, or, as some would rightly claim, it did not propose—or expect—to inaugurate modern historiography: when Lorenzo Valla wrote his devastating *On the Donation of Constantine* in 1440, he did not expect that what was intended mainly as a linguistic feat aimed at proving the Vatican's forgery²⁴ of a document purporting the Church to be the inheritor of the Roman Empire by the hands of emperor Constantine the Great would lead to a fundamental branch of modern historiography. But it did and from a philological endeavor modern history sprung into being.²⁵ The second, hermeneutics, is, as I have been suggesting throughout this essay, the practice through which an historian *understands* history and is able to articulate it. Narrative is the third element of history, which a history inevitably calls for in writing it. Despite the Annales school of history displaced for a while a strictly narrative focus—which was the cornerstone of the vast, synthetic historical works until the nineteenth century—I agree with Paul Ricoeur that "the narrativist interpretation is correct in its clear perception that the specifically historical property of history is preserved only by the ties, which continue to connect historical explanation to our narrative understanding." (Ricoeur, 1984: Vol. 1, p. 228) And even Braudel's *The Mediterranean and the Mediterranean World in the Age of Philip II* which is perhaps the greatest work to come out of the Annales school, as well as the most argumentative work against narrative in history, in its entirety is experienced as "a grand narrative of the retreat of the Mediterranean from general history." (Ricoeur, 1984: Vol. 1, p. 217) Yet, the narrativist claim is quickly returned in favor and does not need much defense today: through Arthur Danto, Paul Ricoeur and the postmodernist stance of Hayden White, narrative in history has been restored, though, at times, with some gross exaggerations.

If we were to acknowledge that the historian's task is to coordinate this triangulation I have outlined, I think many misunderstandings and their resulting abuses, which disorient current historiography would be avoided.

Lastly, I should like to mention Benedetto Croce, who of late is overlooked in English-speaking historiography. Croce was an authentic philosopher as well as an historian, who did not exhibit aporias in his thought; he elicited Collingwood's admiration, who said of him that "it was the clean cut which he [Croce]

made in 1893 between the idea of history and the idea of science that enabled him to develop the conception of history so much farther than any philosopher of his generation." (Collingwood, 1946: p. 193)

Croce's argument toward an identity of history completely independent from empiricism was to unhook it from the idea of "universal history". Universal history was an inheritance of German idealism; it was an ideal shared by the German historical school of the nineteenth century, which believed there was a history that existed in itself that was an objective act of self-consciousness part of a wider, collective consciousness. This was a manifestation of a concept that had originated in the eighteenth century with Voltaire's *Essai sur les Moeurs*, but which sprouted fully in Germany with Hegel's concept of *Weltgeist*; its effects persisted in historians' thought throughout the nineteenth century. Ranke was a paradigmatic example, for he believed history to be composed of "spiritual beings" which in their totality would constitute "world history"; Droysen sought to understand the "inner essence" of things; Dilthey conceived of the historical world as a text to be deciphered. But Croce thought that "unless there is some way of knowing the real that is independent of our data, the postulation of such an independent reality leaves us with an unknowable *ding-an-sich*." (Croce, 1923: p. 14) Croce then spoke crucially on the "thing-in-itself" in history: "we know at every moment all the history that we need to know [...] that 'remaining' history is the eternal phantom of the 'ding-an-sich', which is neither the 'thing' nor 'in-itself', but only the imaginative projection of the infinity of our action and of our knowledge." (Croce, 1923: p. 55) This statement was Croce's way of dispelling the notion of universal history. But Croce made sure to add that "to negate universal history does not mean to negate the universal in history." (Croce, 1923: p. 59)

Nominalism—the rejection of universals—again just seems to be an unsuitable stance in any sound philosophy of history.

I do not believe that we live in a Fukuyama-like moment (at the "end of history") but I do believe that if we continue to abuse and stretch history's fabric with theoretical forcings, Marc Bloch's warning may come true:

It is not itself inconceivable that our civilization may, one day, turn away from history, and historians could do well to reflect upon this possibility. If they do not take care, there is danger that badly understood history could involve good history in its disrepute. But should we come to this, it would be at the cost of a serious rupture with our most unvarying intellectual traditions. (Bloch, 1954: p. 5)

It is our task to see that this not be so.

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²⁴See Valla, 2007.

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Nominalism, Trivialism, Logicism

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Nominalism, Trivialism, Logicism

Agustín Rayo*

May 1, 2014

This paper is an effort to extract some of the main theses in the philosophy of mathematics from my book, *The Construction of Logical Space*. I show that there are important limits to the availability of nominalistic paraphrase-functions for mathematical languages, and suggest a way around the problem by developing a method for specifying nominalistic *contents* without corresponding nominalistic *paraphrases*.

Although much of the material in this paper is drawn from the book—and from an earlier paper (Rayo 2008)—I hope the present discussion will earn its keep by motivating the ideas in a new way, and by suggesting further applications.

I Nominalism

Mathematical Nominalism is the view that there are no mathematical objects. A standard problem for nominalists is that it is not obvious that they can explain what the point of a mathematical assertion would be. For it is natural to think that mathematical sentences like ‘the number of the dinosaurs is zero’ or ‘ $1 + 1 = 2$ ’ can only be true if mathematical objects exist. But if this is right, the nominalist is committed to the view that such sentences are untrue. And if the sentences are untrue, it not immediately obvious why they would be worth asserting.

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A nominalist could try to address the problem by suggesting nominalistic *paraphrases* for mathematical sentences. She might claim, for example, that when one asserts ‘the number of the dinosaurs is zero’ one is best understood as making the (nominalistically kosher) claim that there are no dinosaurs, and that when one asserts ‘ $1 + 1 = 2$ ’ one is best understood as making the (nominalistically kosher) claim that any individual and any other individual will, taken together, make two individuals.¹

Such a strategy faces two main challenges. The first is to explain why mathematical assertions are to be understood non-standardly. One way for our nominalist to address this challenge is by claiming that mathematical assertions are set forth ‘in a spirit of make-believe’ (Yablo 2001). She might argue, in particular, that when one makes a mathematical assertion one is, in effect, claiming that the asserted sentence is true in a *fiction*, and more specifically a fiction according to which: (a) all non-mathematical matters are as in reality, but (b) mathematical objects exist with their standard properties. This proposal leads to the welcome result that fictionalist assertions of mathematical sentences can convey information about the real world. For instance, one can use a fictionalist assertion of ‘the number of the dinosaurs is zero’ to convey the information that there are no dinosaurs, since the only way for ‘the number of the dinosaurs is zero’ to be true in a fiction whereby mathematical objects have all the standard properties is for the fiction to entail that there are no dinosaurs, and the only way for such a fiction to agree with reality in all non-mathematical respects is for it to be the case that there are no dinosaurs.

The second main challenge is that of specifying nominalistic paraphrases for arbitrary mathematical sentences. It is perfectly straightforward to come up with plausible nominalistic paraphrases for toy sentences like ‘the number of the dinosaurs is zero’ or ‘ $1 + 1 = 2$ ’. But we need is a method that will work in general.

Say that a paraphrase-function (for language L with output-language L^N) is an effectively specifiable function that assigns to each sentence in L a paraphrase in L^N . One of the main

¹More carefully: $\forall x \forall y (x \neq y \rightarrow \exists!_2 z (z = x \vee z = y))$.

objectives of this paper is to show that finding a nominalist paraphrase-function is not as easy as one might have thought.

2 Constraints

What would it take for a paraphrase-function for the language of (applied) arithmetic to count as a *nominalist* paraphrase-function? I suggest the following three constraints:

1. *The Counting Constraint*

The paraphrase assigned to $\ulcorner \text{The number of the } F\text{s} = n \urcorner$ should have the same truth-conditions as $\ulcorner \exists!_n x(Fx) \urcorner$.²

2. *The Inferential Constraint*

Suppose that ϕ and ψ are arithmetical sentences, and that the truth-conditions of ϕ are at least as strong as the truth-conditions of ψ (for short: ϕ entails ψ). Then the paraphrase assigned to ϕ should entail the paraphrase assigned to ψ .

3. *The Triviality Constraint*

- (a) The paraphrase assigned to any true sentence of pure arithmetic should have *trivial* truth-conditions (that is, truth-conditions that would be satisfied regardless of how the world turned out to be).
- (b) The paraphrase assigned to any false sentence of pure arithmetic should have *impossible* truth-conditions (that is, truth conditions that would fail to be satisfied regardless of how the world turned out to be).

It seems to me that a paraphrase-function satisfying these three constraints (for short: a *trivialist* paraphrase-function) should be thought of as the ‘gold standard’ of nominalist

²As usual, we let $\ulcorner \exists!_0 x(\phi(x)) \urcorner$ be short for $\ulcorner \neg \exists x(\phi(x)) \urcorner$, and $\ulcorner \exists!_{n+1} x(\phi(x)) \urcorner$ be short for $\ulcorner \exists z(\phi(z) \wedge \exists!_n x(\phi(x) \wedge x \neq z)) \urcorner$.

paraphrase-functions. It is hard to see how a nominalist paraphrase-function could deserve the label ‘nominalist’ if it failed to respect the Counting Constraint, and it is clear that something important would be left out if it failed to respect the Inferential Constraint.

What about the Triviality Constraint? Although some nominalists might be willing to settle for a paraphrase-function that failed to satisfy it, I hope it can be agreed on all sides that the nominalist should *prefer* a paraphrase-function that satisfies the Triviality Constraint over one that does not. Notice, for example, that the fictionalist proposal we considered above presupposes that the Triviality Constraint ought to be satisfied. For a truth of pure mathematics will count as true in the relevant fiction *regardless of how matters stand in reality*. So our fictionalist is committed to thinking that a truth of pure mathematics can be correctly asserted ‘in a spirit of make believe’ regardless of how matters stand in reality.

It is also worth noting that the Triviality Constraint was satisfied by the nominalistic reading that I had earlier suggested for ‘ $1 + 1 = 2$ ’. For the paraphrase I suggested is a logical truth, and it is reasonable to assume that the truths of pure logic have trivial truth-conditions. One might be inclined to think that it would be desirable if the nominalistic reading of *any* truth of pure mathematics turned out to be a logical truth, and therefore had trivially satisfiable truth-conditions.

3 The Bad News

The bad news is that it is *impossible* to specify a trivialist paraphrase-function for the language of arithmetic. A little more carefully: there is a formal result that suggests that it is impossible to specify a paraphrase-function for the language of arithmetic that is *uncontroversially* trivialist.

Here ‘uncontroversial’ means three different things: (1) no controversial linguistic assumptions, (2) no controversial metaphysical assumptions, and (3) no controversial subtraction-assumptions. I will say a few words about each kind of assumption before turning to the

formal result itself.

3.1 Controversial Linguistic Assumptions

If the expressive resources of one's output-language—i.e. the language in which nominalistic paraphrases are given—are sufficiently powerful, it is straightforward to define a trivialist paraphrase-function.

There is, for example, a method for paraphrasing each sentence of the language of arithmetic as a sentence of an $(\omega + 3)$ -order language.³ Would this count as a trivialist paraphrase-function? Yes: *if* one assumes that $(\omega + 3)$ -order logic is 'genuine logic' (if one assumes, in other words, that any truth of $(\omega + 3)$ -order logic has trivial truth-conditions). That is, however, a highly controversial assumption.

The view that *second*-order logic is 'genuine logic' is increasingly popular amongst philosophers. But most philosophers seem to think that languages of high finite order—to say nothing of languages of transfinite-order—can only be made sense of as 'set theory in sheep's clothing' (Quine 1986), and many philosophers would conclude on that basis that the truths of higher-order logic have non-trivial truth-conditions.

My own view is that $(\omega + 3)$ -order logic is, in fact, 'genuine logic',⁴ and that our $(\omega + 3)$ -paraphrase-function is, in fact, a trivialist paraphrase function. But even I must concede that it is not *uncontroversially* a trivialist paraphrase-function. The linguistic assumptions one would need to justify such a claim are just too great.

3.2 Controversial Metaphysical Assumptions

There is a nominalistic paraphrase-function that I find very attractive. It draws its inspiration from Frege's *Grundlagen*, so I will refer to it as the *Fregean* paraphrase-function. The basic

³An $(\omega + 3)$ -order language has variables of all finite types, plus three levels of variables of transfinite type. For further details, see (Linnebo & Rayo 2012). For more on the relevant paraphrase-method, see (Rayo 2013, ch. 7).

⁴I believe, in other words, that the truths of $(\omega + 3)$ -order logic have trivial truth-conditions. I also believe, however, that the truths of pure set-theory have trivial truth conditions. See (Rayo 2013, ch. 3).

idea is that a sentence of the form ‘the number of the F s = the number of the G s’ is to be paraphrased as:

the F s are just as many as the G s

What about a *quantified* arithmetical sentence, such as ‘there is an $n > 0$ such that: $n =$ the number of the planets’? We first paraphrase the sentence as:

there are some things, the F s, such that: the number of the F s = the number of the planets

We then eliminate arithmetical-terms altogether, and say:

there are some things, the F s, such that: the F s are just as many as the planets.

It is easy to show that similar transformations can be applied to every sentence in the language of applied arithmetic (excluding mixed-identity statements such as ‘Caesar = 17’).⁵ The result is a nominalistic paraphrase-function that assigns to each arithmetical sentence a second-order sentence.⁶

Suppose we concede that second-order logic is ‘genuine logic’, and that the truths of second-order logic have trivial truth-conditions. Is this enough to conclude that the Fregean Paraphrase-Function is a *trivialist* paraphrase-function? No—at least not if what we’re looking for is an *uncontroversially* trivialist paraphrase-function. For consider ‘any number greater than 0 has a successor’, which is a truth of pure arithmetic. Its Fregean paraphrase is:

For any things, the F s, there are some things, the G s, such that: for some g amongst the G s, the F s are just as many as the G s distinct from g .

⁵I spell out the details in (Rayo 2002); for a similar proposal, see (Fine 2002, II.5).

⁶Alternatively, one could think of the paraphrase-function as assigning to each arithmetical sentence a sentence of a *plural language* which has been enriched with the atomic plural predicate ‘they are just as many as them’. Here I fudge the distinction between the two for expositional purposes. For more on plural languages, see (Boolos 1984) and (Linnebo 2004).

which will only be true if there are infinitely many objects (or none). So we have a violation of the Triviality Constraint. More guardedly: we have a violation of the Triviality Constraint *unless* we are prepared to accept the following (highly controversial) metaphysical thesis:

Trivialist Infinitarianism

Not only is it the case that the world contains infinitely many objects, it is *trivially* the case that the world contains infinitely many objects. In other words: to assume that the world is finite is to assume something worse than false, it is to assume something *absurd*.

I myself think that Trivialist Infinitarianism is true.⁷ So I believe that the Fregean Paraphrase-Function is, in fact, a trivialist paraphrase-function. But even someone like me, who thinks that Trivialist Infinitarianism is true, must concede that the Fregean Paraphrase-Function is not *uncontroversially* a trivialist paraphrase-function.

It is perhaps worth mentioning that the Fregean Paraphrase-Function is in good company when it comes to infinity assumptions. Many attractive nominalist paraphrase-functions will only count as trivialist paraphrase-functions in the presence of Trivialist Infinitarianism.⁸

3.3 Controversial Subtraction-Assumptions

Joseph Melia (2000) has argued for a satisfyingly straightforward nominalistic paraphrase-function. One is simply to paraphrase the mathematical sentence ϕ as:

ϕ , except for the part about mathematical objects

⁷Why think that Trivialist Infinitarianism is true? Because it follows from [NUMBERS]—see section ref-sec:logicism.

⁸This is true, in particular, of (Hodes 1984) and (Yablo 2002). It is also true of a form of if-then-ism whereby a sentence ϕ is paraphrased as the universal closure of $\ulcorner(A \rightarrow \phi)^*\urcorner$, where A is the conjunction of the second-order Dedekind-axioms and ϕ^* is the result of uniformly replacing arithmetical vocabulary for variables of appropriate type.

A potential worry about this paraphrase-method is that it relies on a non-trivial *subtraction*-assumption. Suppose, for example, that ϕ is a complex physical theory couched in a mathematical language—quantum theory, as it might be. Melia’s method presupposes that the operation of subtracting away the ‘mathematical part’ from the content of quantum theory yields a result which is both well-defined and non-empty. But it is not immediately obvious that this is so: it is not immediately obvious that extricating the mathematical part from quantum theory leaves an interesting remainder.

Mark Colyvan (2010) has a nice example to illustrate why extricability might be a worry:

J. R. R. Tolkien could not, for example, late in the Lord of the Rings trilogy, take back all mention of hobbits; they are just too central to the story. If Tolkien did retract all mention of hobbits, we would be right to be puzzled about how much of the story prior to the retraction remains, and we would also be right to demand an abridged story—a paraphrase of the hobbitless story thus far.

The worry here is not necessarily that the result of subtracting all mention of hobbits from *The Lord of the Rings* is ill-defined—it may well not be. The point is that even if the result is well-defined, one shouldn’t expect much of a narrative. It would be a bit like *Harry Potter* without the wizards: what we’re left with just isn’t unified enough to be much of a story.⁹ Similarly, a skeptic might worry that even if the result of subtracting the mathematical part from the content of quantum theory turns out to be well-defined, what we’re left won’t be unified enough to tell us anything very interesting about the physical world. (Field’s (1984) ‘Heavy Duty Platonist’ is presumably one such skeptic.)

Another way to see that extricability claims can be problematic is to consider the question of what would be left if one subtracted *someone is thirsty* from *I’m thirsty* (Yablo 2012); or the question of what would be left if one subtracted *the tomato is red* from *the tomato is scarlet* (Searle & Körner 1959, Woods 1967, Kraemer 1986, Yablo 2012)? It’s not clear that there are well-defined answers to be given—unless, of course, one is prepared to say ‘nothing’.

⁹Thanks here to Kevin Richardson.

A further example, which I find especially illuminating, concerns the notion of *narrow content*. Narrow contents are supposed to be the result of subtracting away certain kinds of environmental facts from the contents of our beliefs (Brown 1992). The narrow content corresponding to my belief that water is wet, for example, is supposed to be the result of subtracting from what I believe when I believe that water is wet the fact that items in my environment playing a certain theoretical role are composed of H₂O. Since the claim that narrow-contents are both well-defined and non-empty is a highly controversial philosophical thesis, one can use the debate between friends and foes of narrow content to underscore the fact that the operation of subtracting particular ‘environmental factors’ from the contents of our beliefs shouldn’t be assumed to deliver the intended results. (For illuminating discussion, see Yablo forthcoming.)

The most straightforward way of justifying the claim that mathematical content can be usefully extricated from mathematical claims would be to set-forth a nominalist paraphrase-function—one that does not itself rely on subtraction-assumptions. For one would then be in a position to claim that the result of subtracting away the mathematical part from the content of a mathematical sentence is simply the content of the sentence’s paraphrase. But it is not immediately obvious that a suitable paraphrase-function can be found. For although we want the result of subtracting away the mathematical part from quantum theory, say, to deliver a non-empty content, we presumably want the result of subtracting away the mathematical part from a truth of *pure* mathematics to be an empty content: a content that would be satisfied however the world turned out to be. So the relevant paraphrase-function had better be a *trivialist* paraphrase-function. And, as we have seen, it is not easy to find an uncontroversial example of a trivialist paraphrase-function. It is therefore not immediately obvious that the operation of subtracting mathematical content can be defined in a way that delivers interesting results.¹⁰

¹⁰Yablo (2012, forthcoming) has as sophisticated treatment of these issues, which yields an illuminating characterization of the circumstances under which the subtraction operation delivers results which are both well-defined and non-empty. On its own, however, Yablo’s account does not settle the question of whether

My own view is that Melia’s use of the subtraction-operation can, in fact, be defined so as to deliver the right results. But I don’t think that such a claim can be simply taken for granted: a substantial argument is required. (I will attempt to provide the missing argument in section 4.)

Melia’s paraphrase-function has the advantage of wearing its subtraction-assumption on its sleeve. But it is worth noting that similar assumptions are required by other nominalist paraphrase-methods:

Method	Paraphrase ϕ as ...	Subtraction-Assumption
Fictionalism	ϕ is true according to a fiction which is accurate in all non-mathematical respects but in which mathematical objects exist with the standard properties (Yablo 2001).	One would impose an interesting condition on the world by insisting that it agree in non-mathematical respects with a fiction that makes, say, quantum theory true.
Modalism	ϕ is true at the closest possible world which agrees with the actual world in non-mathematical respects but in which mathematical objects exist with the standard properties (Hellman 1989, Dorr 2007).	One would impose an interesting condition on the world by insisting that it agree in non-mathematical respects with the closest possible world that makes, say, quantum theory true.
Subject-Matterism	ϕ is true as far as its non-mathematical subject-matter is concerned, where a claim’s non-mathematical subject-matter is defined as the set of worlds which agree in all non-mathematical respects with a world at which the claim is literally true (Yablo 2012).	One would impose an interesting condition on the world by insisting that it agree in non-mathematical respects with quantum theory’s non-mathematical subject-matter.

It is instructive to note that each of these paraphrase-methods could be easily modified so as to deliver a ‘narrowist’ paraphrase-method, in which paraphrases are meant to capture the *narrow contents* of the original claims, rather than their nominalistic contents. (One could claim, for example, that the *narrow part* of ‘water is wet’ is the claim that ‘water is subtracting ‘the mathematical part’ from, say, quantum theory delivers results which are both well-defined and non-empty.

wet' is true according to a fiction that is accurate in all respects, except perhaps for the nature of the substance that plays the theoretical role that water currently plays in our cognitive lives.) Just as the resulting 'narrowist' paraphrase-functions would do nothing to move a foe of narrow contents, the nominalistic paraphrase-functions in the table above should do nothing to move a skeptic of Melia's subtraction-assumption.

3.4 The Formal Result

Earlier I claimed that it is impossible to specify a paraphrase-function for the language of arithmetic that is uncontroversially trivialist. I am now in a position to give a precise statement of the underlying formal result.

First some assumptions:

A₁ We shall assume that the logical resources of our output-language do not go beyond those of the simple theory of types (which is a language with n th-order quantifiers for each finite n).¹¹

A₂ We shall assume that our output-language contains no intensional operators.

A₃ Any trivialist paraphrase-function must, by definition, satisfy the Triviality Constraint of section 2, and therefore preserve truth-values when applied to sentences of pure arithmetic. (In other words: every truth of pure mathematics must get paraphrased as a truth of the output-language, and every falsity of pure mathematics must get paraphrased as a falsehood of the output-language.)

We shall assume that this condition can be met even if the output-language has a finite domain.

Think of A₁–A₃ as stating, respectively, that our paraphrase-function is to make no controversial linguistic assumptions, that it is to make no controversial subtraction-assumptions,

¹¹Since paraphrase-functions are, by definition, effectively specifiable, we may assume with no loss of generality that our output-language has a *finite* stock of non-logical predicates and terms.

and that it is to make no controversial metaphysical assumptions. More precisely:

- A_1 places an upper bound on the expressive resources of the output-language. Such a bound is justified by the fact that the use of a language more powerful than the simple theory of types would be a sure sign of a controversial linguistic assumption in the sense of section 3.1. (As I noted above, the use of a *third*-order language would be pretty controversial already, but I'm trying to keep my assumptions as weak as possible.)
- A_2 is a ban on intensional operators, and is intended to ensure that our paraphrase-function makes no controversial subtraction-assumptions. I have certainly not shown that any sensible paraphrase-method based on intensional operators will require controversial subtraction-assumptions. But as the table in section 3.3 illustrates, such assumptions are required by the most natural methods for supplying intensional paraphrases for language of applied arithmetic.
- A_3 is meant to ensure that our paraphrase-method does not rely on infinity assumptions. This is important because—unless one embraces Trivialist Infinitarism, which is a decidedly controversial metaphysical thesis—one should think that no trivialist paraphrase-function can presuppose an infinite domain.

Now that our assumptions are in place, it is easy to state the formal result:

Impossibility Theorem

No paraphrase-function for the language of arithmetic can satisfy A_1 – A_3 .

The proof is totally straightforward. By assumptions 1 and 2, our output-language has a finite lexicon and quantifiers of finite type; by assumption 3, our paraphrase-function can preserve truth-value over pure sentences even if the output-language is assumed to have a finite domain. But the set of truths of a language with a finite lexicon and quantifiers of

finite type on a finite domain is effectively specifiable. So our paraphrase-function would deliver a decision procedure for arithmetical truth, which we know to be impossible from Gödel's Theorem.¹²

To the extent that one is prepared to think of A_1 – A_3 as capturing the idea that there are to be no controversial assumptions, one can think of this result as showing that no paraphrase-function for the language of arithmetic can be uncontroversially trivialist.

4 A Way Forward

Let us take stock. We started out by noting that the nominalist faces a challenge: she needs to explain what the point of making a mathematical assertion might be. We then noted that the challenge might be addressed by offering nominalistic paraphrases for mathematical sentences, and going on to claim that the point of asserting a mathematical sentence can be to convey the content of its paraphrase.¹³

What should a nominalist paraphrase-function look like? I listed three constraints in section 2, and suggested that paraphrase-functions satisfying those constraints—i.e. *trivialist* paraphrase-functions—should be thought of as the ‘gold standard’ of nominalist paraphrase. We have seen, however, that that the Impossibility Theorem suggests that there is no way of specifying a trivialist paraphrase function for the language of arithmetic without making controversial assumptions.

What is the nominalist to do? She could embrace one of the nominalist paraphrase-functions we discussed above, and insist that it is a trivialist paraphrase-function by making a controversial assumption. Or she could settle for a nominalist paraphrase-function that falls short of the gold standard.

Here I will propose an alternative. It seems to me that the *real* reason to be interested

¹²Thanks to Vann McGee for pointing out a strengthening of the original result.

¹³I say ‘can be’ rather than ‘is’ because one might think that the truths of *pure* arithmetic have trivially satisfiable contents, and it is not obvious that conveying such contents would be particularly interesting. For further discussion of the point of mathematical assertions, see (Rayo 2013, Chapter 4).

in nominalistic paraphrases is that one can use them to claim that the *nominalistic content* of a mathematical sentence is the literal content of the sentence’s nominalist paraphrase—where the nominalistic content of a sentence is the requirement that the world would have to satisfy in order for a given sentence to be true ‘as far as the non-mathematical facts are concerned’.

What I propose to do here is cut out the middle man. I will argue that there is a method for specifying the nominalistic contents of arithmetical sentences that does not proceed via paraphrases. This alternative method has an advantage and a disadvantage. The advantage is that it delivers *trivialist* contents (i.e. contents satisfying analogues of the three conditions in section 2), and does so without making controversial philosophical assumptions of the kind discussed in section 3. The disadvantage is that the method is couched in mathematical language, and is therefore only available to someone who is prepared to engage in mathematical practice. As we will see in section 4.2, this places certain limits on the purposes for which the proposal can be deployed.

4.1 Outscoping

On the view I would like to discuss, one assigns nominalistic contents to mathematical sentences by way of a *compositional semantics*: an assignment of semantic values to basic lexical items, together with a set of rules for assigning semantic values to a complex expression on the basis of the semantic values of its constituent parts.

I will assume that the semantic value of a sentence is a set of possible worlds. Accordingly, a compositional semantics should allow us to prove a statement of the following form for each sentence ϕ of the object language:

ϕ is true at world w if and only if w is such that ...

The usual way of interpreting such a clause is as a specification of ϕ ’s *truth-conditions*, that is, as a specification of the condition that a world w would need to satisfy in order for ϕ to

count as true at w . Here, however, we will be using the compositional semantics to specify *nominalistic contents*, rather than truth-conditions. Accordingly we will interpret the clause above as supplying a specification of the condition that a world w would need to satisfy in order for ϕ to count as true ‘as far as the non-mathematical facts are concerned’.

Suppose, for example, that a compositional semantics delivers the following clause for ‘the number of the dinosaurs is 0’ (where ‘ $[\dots]_w$ ’ is read ‘at w , it is the case that ...’).

‘ $\#_x(\text{Dinosaur}(x)) = 0$ ’ is true at w if and only if $[\text{there are no dinosaurs}]_w$

The right-hand-side of this clause specifies the following condition on w : that it represent reality as being such that there are no dinosaurs.¹⁴ Accordingly, if the relevant semantics is thought of as a specification of nominalistic contents, one should interpret the clause as stating that what it takes for ‘ $\#_x(\text{Dinosaur}(x)) = 0$ ’ to count as true ‘as far as the non-mathematical facts are concerned’ is for the world to satisfy the condition that there be no dinosaurs.

The example above has the form:

ϕ is true at w if and only if $[p]_w$

where ‘ ϕ ’ is a sentence of the object-language and ‘ p ’ is a nominalistic *paraphrase* of that sentence in the metalanguage. A compositional semantics that only outputs sentential clauses of this form will be severely limited in its ability to specify nominalistic contents, since it presupposes that one is in a position to specify a nominalist paraphrase-function for the object-language in the metalanguage. And, as we have seen, there are good reasons for thinking that it is impossible to specify a trivialist paraphrase-function for the language of arithmetic without making controversial assumptions.

Fortunately, a compositional semantics need not be restricted to outputs of the above form. It can deploy *outscoping*. To see what outscoping is all about, it is useful to contrast the following semantic clauses:

¹⁴What is it for a possible world w to *represent* reality as being such that p ? It is simply for it to be the case that p at w .

[WIDE]

‘ $\exists x(\text{President}(x) \wedge \text{Mustache}(x))$ ’ is true at world w if and only if [there is an x such that x is the president of the United States and x wears a mustache] $_w$

[NARROW]

‘ $\exists x(\text{President}(x) \wedge \text{Mustache}(x))$ ’ is true at world w if and only if there is an x such that x is the president of the United States and [x wears a mustache] $_w$

The only difference between [WIDE] and [NARROW] is the scope of the ‘[. . .] $_w$ ’ operator. But one can see that the difference is significant by considering the following question: How must a world w represent reality if it is to satisfy the right-hand-sides of each of the two clauses?

In the case of [WIDE] the answer is straightforward: w must represent reality as being such that there is an x such that x president of the United States and x wears a mustache. Accordingly, [WIDE] might be thought of as associating the following (unsurprising) condition with ‘ $\exists x(\text{President}(x) \wedge \text{Mustache}(x))$ ’: that there be an x such that x is president of the United States and x wears a mustache.

In the case of [NARROW], however, we get significantly different results. In the actual world, the president of the United States is Barack Obama. So, in order for a world w to satisfy the right-hand-side of [NARROW], it must represent reality as being such that Barack Obama wears a mustache, whether or not he happens to be president. Accordingly, [NARROW] might be thought of as associating the following condition with ‘ $\exists x(\text{President}(x) \wedge \text{Mustache}(x))$ ’: that Obama—the man himself—wear a mustache.

So, whereas [WIDE] specifies a content whereby the president—whoever that may be—wears a mustache, [NARROW] specifies a content whereby Obama—whatever his occupation—wears a mustache.

Let us now consider an arithmetical example. The following two semantic clauses for ‘the number of the dinosaurs is zero’ differ only in the scope of the ‘[. . .] $_w$ ’ operator:

[WIDE]

‘ $\#_x(\text{Dinosaur}(x)) = 0$ ’ is true at w if and only if [the number of x s such that x is a dinosaur = 0] $_w$

[NARROW]

‘ $\#_x(\text{Dinosaur}(x)) = 0$ ’ is true at w if and only if the number of x s such that [x is a dinosaur] $_w = 0$.

How must w represent reality in order to satisfy the right-hand-sides of each of these clauses? In the case of [WIDE], w must represent reality as being such that there is a *number* which numbers the dinosaurs and is identical to zero. So [WIDE] associates a *Platonist* content with ‘ $\#_x(\text{Dinosaur}(x)) = 0$ ’: a content whose satisfaction requires the existence of numbers. In the case of [NARROW], on the other hand, all it takes for the right-hand-side of the clause to be satisfied is for nothing to be such that w represents *it* as being a dinosaur. So [NARROW]—unlike [WIDE]—remains *neutral* on the question of whether w must represent reality as containing numbers.¹⁵

The crucial feature of [NARROW] is, of course, that all arithmetical vocabulary has been *outscooped*: it has been removed from the range of ‘ $[\dots]_w$ ’. So although one uses arithmetical vocabulary in the metalanguage to *characterize* a requirement on w , the requirement itself brings in no specifically arithmetical constraints: it is simply the requirement that nothing be counted by w as a dinosaur. The role of arithmetical vocabulary is to impose a metatheoretical *test* that will ensure that such a requirement is met: one asks, in the metalanguage, for the number of the objects that are counted as dinosaurs by w and demands that that number be zero. But because the test is performed while working *outside* the scope of ‘ $[\dots]_w$ ’, one doesn’t have to presuppose that the resources one uses to perform the test are present in w .

¹⁵Here I assume, for simplicity, that the domain of the metalanguage includes merely possible objects. Without this assumption—or, alternatively, without the assumption of necessitism (Williamson 2013)—[NARROW]’s right-hand-side will be satisfied by worlds which represent reality as containing dinosaurs but don’t represent *of any actually existing individual* that it is a dinosaur, and it therefore won’t succeed in associating with ‘ $\#_x(\text{Dinosaur}(x)) = 0$ ’ the condition that there be no dinosaurs. Happily, there is a technical trick that allows one get the right results without surrendering modal actualism (or contingentism); see (Rayo 2008), (Rayo 2012) and (Rayo 2013, Chapter 6) for details.

As it turns out, it is possible to give a compositional semantics that delivers suitably outscoped semantic clauses for *every sentence in the language of arithmetic* (see appendix). Not just that: the resulting clauses succeed in delivering the gold standard. They specify *trivialist* nominalistic contents, and do so with no need for controversial philosophical assumptions of the kind we discussed in section 3. (They do not, however, deliver a *paraphrase-function*, since our semantic clauses do not assign a non-mathematical sentence to each mathematical sentences. But, of course, the Impossibility Theorem suggests that that would be too much to hope for.)

Our semantics assigns every truth of pure arithmetic a trivial semantic clause (i.e. a clause whose right-hand-side will be satisfied by a world w regardless of how reality is represented by w), and it assigns every falsehood of pure arithmetic is assigned an impossible semantic clause (i.e. a clause whose right-hand-side will fail to be satisfied by w regardless of how reality is represented by w). The reason we get this result is that, when it comes to sentences of pure arithmetic, *everything* gets outscoped. The clause for ‘ $1 + 1 = 2$ ’, for example, will be:

‘ $1 + 1 = 2$ ’ is true at w if and only if $1 + 1 = 2$

in which *nothing* remains in the scope of ‘ $[\dots]_w$ ’. Since the right-hand-side of this biconditional is true (and contains no free variables), it will be satisfied by w regardless of how the world is represented by w . So our semantics will assign a trivial nominalistic content to ‘ $1 + 1 = 2$ ’.

4.2 What Outscoping Can and Cannot Do

Our trivialist semantics is couched in an arithmetical language. So use of the theory presupposes that one is able to understand arithmetical vocabulary. Not just that: in order to extract illuminating results from an outscoped semantic clause, one usually needs to prove an arithmetical claim in the metatheory. (In arguing that the semantic clause for ‘ $1 + 1 = 2$ ’

delivers a trivial content, for example, I made use of the fact that $1 + 1 = 2$.) So illuminating use of our semantic theory presupposes that one is able to prove arithmetical results. Either of these presuppositions would be utterly uncontroversial in a non-philosophical context. But it is worth considering how they play out in the present discussion.

It will be useful to start by seeing things from the point of view of a *mathematical Platonist*: someone who thinks that mathematical objects exist. Suppose, for example, that our Platonist is interested in the project of understanding which nominalistic contents a nominalist would wish to associate with arithmetical sentences. Since the Platonist feels comfortable using arithmetical vocabulary, she is in a position to set forth the trivialist semantics we have been discussing, and read off the nominalistic content of arithmetical sentences from the outscoped semantic clauses that are delivered by the semantics.

In doing so, our Platonist will have found a way around the Impossibility Theorem of section 3. For even if she lacks a general method for characterizing trivialist *paraphrases* for arithmetical sentences, our Platonist will have succeeded in finding a general method for characterizing trivialist *contents* for arithmetical sentences. It is true that she will have *used* arithmetical vocabulary in the process. But this is no threat to the project because the contents themselves will involve no specifically arithmetical constraints.

We started out assuming that the Platonist is interested in the project of characterizing nominalistic contents in order to better understand the nominalist, but she might also be interested in the project for a different purpose. Suppose she wishes to understand how arithmetical claims can be *relevant* to one's knowledge of the natural world (Steiner 1998, Yablo 2001). She might hypothesize that the answer is partly to do with the fact that an arithmetical claim like 'the number of the dinosaurs is zero' can impose non-trivial demands on the natural world, and see her outscoped semantic clauses as supplying a precise statement of the relevant demands.

Relatedly, our Platonist might wish to know whether the operation of *content-subtraction* delivers interesting results in the arithmetical case. Our Platonist will see the trivialist se-

mantic theory as decisive proof that the operation is well-defined, and delivers results of the right kind. For she will see the outscoped semantic clause corresponding to each arithmetical sentence as a precise statement of the result of subtracting ‘the mathematical part’ from the relevant arithmetical claim. (This is what I meant in section 3.3 when I reported thinking that Melia’s use of the subtraction-operation was, in fact, well-defined.)

We have been seeing things from the perspective of a Platonist. But what would a nominalist make of our trivialist semantics? It seems to me that the important issue is not whether one is a nominalist, but whether one is prepared to *engage in mathematical practice*. Suppose, for example, that our nominalist is also a fictionalist, and that she is happy to engage in mathematical practice: she proves mathematical theorems and uses mathematical vocabulary in making claims about the world; in her more philosophical moments, however, she insists that her mathematical assertions are set forth ‘in a spirit of make believe’ and that there really are no mathematical objects. A nominalist of this kind should have no difficulty working with the trivialist semantics that we have been discussing, and using outscoped semantic clauses to give a precise statement of the nominalistic content of her mathematical assertions.

A nominalist who *would* be barred from employing the trivialist semantics is what might be called a *nominalistic zealot*: someone who thinks that one cannot engage in normal mathematical practice. The zealot would not be prepared to assert ‘the number of the dinosaurs is zero’ in describing the world, even if she thought there were no dinosaurs; similarly, she would not be prepared to use a mathematically formulated semantic clause to characterize a nominalistic content. Our trivialist semantics is also unavailable to a mathematical *novice*: someone who is not competent in the use of mathematical vocabulary.

We set out to give a precise characterization of the nominalistic contents of arithmetical sentences. Had we been able to do so by way of an uncontroversially trivialist paraphrase-function, we might have been in position to satisfy both the zealot and the novice.¹⁶ But the

¹⁶Paraphrase-functions, like other functions, are mathematical objects. So whether or not a given paraphrase-

Impossibility Theorem shows that there are real limits to what can be done when it comes to giving paraphrases. Our trivialist semantics allows us to circumvent the theorem, but it is only available to someone who is prepared to engage in ordinary mathematical practice. It seems to me, however, that such an exclusion is not a particularly high price to pay, because neither the zealot nor the novice represent serious philosophical positions.

4.3 Beyond Arithmetic

We have seen that it is possible to give a compositional semantics that delivers suitably outscoped semantic clauses for every sentence in the language of (pure and applied) arithmetic. As it turns out, it is also possible to use the outscoping technique to characterize a trivialist semantics for the language of (pure and applied) *set-theory*. Full details are supplied in the appendix, but the basic idea is straightforward. In place of a standard homophonic semantic clause such as:

[WIDE]

‘Socrates $\in \{x : x \text{ is a philosopher}\}$ ’ is true at w if and only if [Socrates $\in \{z : z \text{ is a philosopher}\}$] $_w$

one uses an outscoped semantic clause such as:¹⁷

[NARROW]

‘Socrates $\in \{x : x \text{ is a philosopher}\}$ ’ is true at w if and only if Socrates $\in \{z : [z \text{ is a philosopher}]_w\}$

Although the outscoping technique happens to be available both in the case of arithmetic and in the case of set-theory, it is important to be clear that these results are not automatic:

function could actually be used to satisfy the zealot or the novice might depend on just how the function is presented to them. Consider, for example, the Fregean paraphrase-function of section 3.2. If such a function were to be described as a set of order-pairs, neither the zealot nor the novice would be moved. But one might get better results if one presents it a finite list of syntactic rules for transforming any given arithmetical sentence into the target second-order sentence.

¹⁷Here and below, I retain the simplifying assumption that the domain of the metalanguage includes merely possible objects.

there is no general reason to think that outscoping will be available whenever abstracta are used to describe features of the concrete world.

The best way to see this is to consider an example. Suppose that a mass of one kilogram is defined as the mass of N carbon-12 atoms (where ‘ N ’ is replaced by some particular numeral),¹⁸ and suppose that one wishes to specify a nominalist content for ‘Oscar’s mass-in-kilograms is 72’. One might suggest an outscoped semantic clause such as the following:

‘Oscar’s mass-in-kilograms is 72’ is true at w if and only if

$$\exists X((\#_x(Xx) = N \times 72) \wedge \forall x(Xx \rightarrow [^{12}\text{C-atom}(x)]_w) \wedge [\text{SameMassAs}(\text{Oscar}, X)]_x)$$

Although this clause has the right kind of flavor, it would presumably need to be refined in a number of ways. Notice, to begin with, that it presupposes that second-order quantification is nominalistically unproblematic, since a second-order variable occurs within the scope of ‘ $[\dots]_w$ ’. (It is possible to outscope the relevant variable, by making suitable mereological assumptions.¹⁹) Notice, further, that our clause presupposes that w contains enough carbon-12 atoms to establish an equal-mass comparison with Oscar. This won’t be a problem in this particular case, if Oscar is an ordinary-sized human and if w is a world roughly like our own. But it will be a problem if one wants to generalize the proposal to talk about, e.g. the mass of the entire universe. Perhaps one could amend the clause so as to allow for mass

¹⁸As of the time of this writing, a mass of one kilogram is officially defined as the mass of the International Prototype Kilogram, a particular artifact which is kept in a vault in the outskirts of Paris. An unhappy consequence of this definition is that every time the Prototype loses an atom, the mass of everything else in the world increases. The definition has nonetheless been kept in place because, until recently, we lacked the technology to produce more precise measurements using alternate definitions. It is likely that a new definition will be adopted soon, however. The definition I consider above is the simplest of the proposals under consideration.

¹⁹If one is prepared to countenance mereological sums, for example, then one can replace the right-hand-side of the original clause with the following:

$$\begin{aligned} &\exists X(\#_x(Xx) = N \times 72 \wedge \forall x(Xx \rightarrow [^{12}\text{C-atom}(x)]_w) \wedge \\ &\exists z([\text{SameMassAs}(\text{Oscar}, z)]_x \wedge \forall x(Xx \rightarrow [\text{PartOf}(x, z)]_w) \wedge \forall y([\text{Overlaps}(y, z)]_w \rightarrow \exists v(Xv \wedge [\text{Overlaps}(y, v)]_w)))) \end{aligned}$$

comparisons with different kinds of fundamental particles.²⁰ Even if that works, however, other problems might emerge. What should one do, for example, if one thinks that there are worlds in which the masses of fundamental particles differ from their actual masses?

The lesson of our example is the availability of outscoping is not automatic. Each new way of using abstracta to describe features of the concrete world calls for new type of outscoped semantic clause, and it is by no means obvious that suitable semantic clauses will always be available.²¹ On the other hand, the problem of finding suitable clauses is closely linked to the problem of better understanding the ways in which talk of abstracta conveys information about the way the world is. So limitations in our ability to outscope may sometimes reflect limitations in our understanding of the relevant subject-matter.

5 Logicism

When we discussed outscoping earlier in the paper, we were thinking of it as a means for specifying the nominalistic contents of arithmetical sentences rather than their literal truth-conditions. But there is room for arguing that our trivialist compositional semantics is, in fact, an accurate statement of *literal* truth-conditions. On such a view, all that is required of the world for ‘the number of the dinosaurs is zero’ to be literally true is that there be no dinosaurs, and *nothing* is required of the world for ‘ $1 + 1 = 2$ ’ to be literally true.

Should one conclude from this that arithmetical sentences don’t carry commitment to numbers? Not according to the version of the proposal I wish to consider here. I would like to consider a view whereby it is *both* the case that ‘the number of the dinosaurs is zero’ is

²⁰Here is a modified right-hand-side, where P_1, \dots, P_k is a list of all types of fundamental properties possessing mass, and, for each $i \leq k$, N_i particles of type P_i have a mass of one kilogram:

$$\exists X_1 \dots \exists X_k \left(\frac{(\#_x(X_1(x)))}{N_1} + \dots + \frac{(\#_x(X_k(x)))}{N_k} = 72 \wedge \right. \\ \left. \forall x \left(([\text{FundParticle}(x) \wedge \text{PartOf}(x, \text{Oscar})]_w) \leftrightarrow (X_1(x) \wedge [P_1(x)]_w) \vee \dots \vee (X_k(x) \wedge [P_k(x)]_w) \right) \right)$$

²¹For relevant discussion, see (Williams 2010).

committed to the number zero, *and* that all that is required of the world for ‘the number of the dinosaurs is zero’ to be literally true is that there be no dinosaurs.

The proposal escapes incoherence by endorsing the following claim:

For the number of the dinosaurs to be zero *just is* for there to be no dinosaurs.

and, more generally,

[NUMBERS]

For the number of the F s to be n *just is* for it to be the case that $\exists!_n x(Fx)$.

A friend of [NUMBERS] thinks that there is *no difference* between there being no dinosaurs and their number’s being zero, in the same sort of way that there is no difference between drinking a glass of water and drinking a glass of H_2O . More colorfully: when God created the world and made it the case that there was water to be drunk, there was nothing *extra* she needed to do or refrain from doing to make it the case that there was H_2O to be drunk. She was already done. Similarly, a friend of [NUMBERS] thinks that when God created the world and made it the case that there would be no dinosaurs in 2013, there was nothing *extra* she needed to do or refrain from doing to make it the case that the number of the dinosaurs in 2013 would be zero. She was already done.

An immediate consequence of [NUMBERS] is that a world without numbers would be *inconsistent*:

Proof: Assume, for *reductio*, that there are no numbers. By [NUMBERS], for the number of numbers to be zero *just is* for there to be no numbers. So the number of numbers is zero. So zero exists. So a number exists. Contradiction.

One might therefore think of [NUMBERS] as delivering a *trivialist* form of mathematical Platonism—the number zero exists, but its existence is a trivial affair. And, of course, it is not just the existence of the number zero that is a trivial affair: one can use [NUMBERS] to

show that each of the natural numbers must exist, on pain of contradiction, and to show that they are distinct from one another. (That is what I had in mind in section 3.2 when I reported thinking that Trivialist Infinitarianism is true.)

Someone who accepts [NUMBERS] can claim *both* that the truth-conditions of ‘the number of the dinosaurs is zero’ consist entirely of the requirement that the number of the dinosaurs be zero, *and* that they consist entirely of the requirement that there be no dinosaurs. She can make both these claims because she thinks that the proposed requirements are one and the same: there is *no difference* between there being no dinosaurs and their number’s being zero.

The trivialist semantic theory we set forth in the preceding section can be used to generalize this idea to every sentence in the language of arithmetic. One can claim that the literal truth-conditions of an arithmetical sentence are accurately stated *both* by a standard (homophonic) compositional semantics *and* by our trivialist semantics with outscoped semantic clauses. But the two semantic theories do not contradict one another because the truth-conditions they associate with a given sentence are, in fact, one and the same: there is *no difference* between what would be required of the world to satisfy the truth-conditions delivered by one semantic theory and what would be required of the world to satisfy the truth-conditions delivered by the other.²²

Consider ‘ $1 + 1 = 2$ ’ as an example. A standard (homophonic) semantics tells us that the truth-conditions of ‘ $1 + 1 = 2$ ’ demand of the world that it contain numbers. Our trivialist semantics tells us that the truth-conditions are trivial—that they will be satisfied regardless of how the world happens to be. But the two claims are consistent with each other because the existence of numbers is a trivial affair. ‘ $1 + 1 = 2$ ’ carries commitment to numbers, but this is a commitment that will be satisfied regardless of how the world happens to be.

What does this tell us about *logicism*—the view that mathematics can be reduced to logic? The Impossibility Theorem of section 3 suggests that the formal systems that contemporary

²²For a detailed defense of this view see (Rayo 2013, Chapters 1 and 2).

philosophers tend to think of as ‘pure logic’ are not expressive enough to capture basic arithmetic. So one might think of the theorem as a refutation of logicism: in an interesting sense, mathematics cannot be reduced to logic. But one could also think of the view developed in the present section as a certain kind of *vindication* of logicism. For it delivers the result that the truths of pure arithmetic—like the truths of pure logic—have trivially satisfiable truth-conditions, and the result that the falsehoods of pure arithmetic—like the falsehoods of pure logic—have impossible truth conditions. Admittedly, one also gets the result that a truth of pure arithmetic can carry commitment to numbers. But because the existence of numbers is a trivial affair, there is room for thinking of numbers as ‘logical objects’, as in Frege’s *Grundgesetze* .

6 Concluding Remarks

In this paper I have tried to shed new light on mathematical nominalism.

I began with the observation that the nominalist is committed to answering a particular challenge. She must explain what the *point* of making a mathematical assertion could be, if there are no numbers. One way of addressing this challenge is to argue that the point of mathematical assertions is not to communicate the literal content of the sentence asserted, but to communicate its *nominalistic content*: the requirement that the world would need to satisfy in order to make the sentence true ‘as far as non-mathematical facts are concerned’.

It is natural to suppose that one can specify the relevant nominalist contents by setting forth a nominalistic *paraphrase function*: an effectively specifiable procedure that assigns to each mathematical sentence a non-mathematical paraphrase in such a way that the nominalistic content of the mathematical sentence matches the literal content of its paraphrase. We have seen, however, that there is a formal result that suggests that it is *impossible* to specify a suitable paraphrase-function for the language of arithmetic, in the absence of potentially controversial assumptions.

One might have been tempted to think of the Impossibility Theorem as a decisive blow to the nominalistic dream of specifying nominalistic contents for arbitrary mathematical sentences. But we have seen that nominalistic paraphrase-functions are not the only way of specifying nominalistic contents. The method of outscoping makes it possible to construct a *compositional semantics* that assigns the right nominalistic contents to arbitrary arithmetical (and set-theoretic) sentences.

This result sheds light on nominalism in two different ways. First, it allows us to discard the idea that the case for nominalism ought to be linked to the availability of a nominalistic paraphrase function—a bad idea from the start, since it tied the *metaphysical* thesis that there are no numbers to potentially controversial *linguistic* theses concerning the legitimacy of particular expressive resources.

Second, and more importantly, our outscoped semantics shows that the notion of nominalistic content can be rigorously defined, and is therefore suited for serious philosophical work. We noted, in particular, that it can be used to address the question of how mathematical claims can be relevant to one's knowledge of the natural world. But we also noted that it can be used to reassess nominalism, by allowing one to give a rigorous characterization of a subtle variety of Platonism: a view according to which there is *no difference* between what would be required of the world to satisfy the nominalistic content of a given arithmetical sentence and what would be required of the world to satisfy the truth-conditions that would be assigned to that sentence by a homophonic semantic theory.

From a purely mathematical point of view, there is no particular reason to prefer Subtle Platonism over its rivals. But Subtle Platonism is philosophically significant because it casts doubt on Benacerraf's Dilemma: the idea that one must choose between holding onto the claim that mathematical assertions carry commitment to mathematical objects, and making contentious claims about our cognitive relationship to a causally inert realm of abstract objects (Benacerraf 1973). The Dilemma is sometimes construed as an argument for nominalism, since it seems to suggest that only the nominalist could have a sensible epistemology

of mathematics. But when Subtle Platonism is treated as a live option, we can no longer take for granted that commitment to numbers comes with epistemological costs. (The Subtle Platonist would argue, for example, that someone who has verified that there are no dinosaurs is thereby in a position to know that the number of dinosaurs is zero, since the fact that there are no dinosaurs is *already* the fact that the number of dinosaurs is zero.²³) If this is right, then the notion of a nominalistic content—which we first introduced in an effort to help nominalists answer a challenge—can also be used to cause trouble for nominalism, by allowing for rigorous development of a rival view.²⁴

²³For further discussion, see (Rayo 2013, Chapters 3 and 4).

²⁴For their many helpful comments, I am grateful to Duilio Guerrero, Bernhard Salow and Steve Yablo, to participants at MIT's Logic, Language and Metaphysics Reading Group, and to audiences at the University of Missouri, Kansas City, the Università Vita-Salute San Raffaele and Smith College.

Appendix

The material in this appendix is drawn from (Rayo 2013, Chapter 3), where I discuss further technical details. (As noted in footnotes 15 and 17, I assume, for simplicity, that the domain of the metalanguage includes merely possible objects; but the assumption can be avoided by appeal to the technique described in (Rayo 2013, Chapter 6). In the case of arithmetic, the details are spelled out in (Rayo 2008).)

1. A Trivialist Semantics for the Language of Arithmetic

We work with a two-sorted first-order language with identity, L . Besides the identity-symbol ‘=’, L contains *arithmetical* variables (‘ n_1 ’, ‘ n_2 ’, ...), individual-constants (‘0’) and function-letters (‘S’, ‘+’ and ‘ \times ’), and *non-arithmetical* variables (‘ x_1 ’, ‘ x_2 ’, ...), constants (‘Caesar’) and predicate-letters (‘Dinosaur(...)'). In addition, L has been enriched with the function-letter ‘ $\#_v(\dots)$ ’ which takes a first-order predicate in its single argument-place to form a first-order arithmetical term (as in ‘ $\#_{x_1}(\text{Dinosaur}(x_1))$ ’), which is read ‘the number of the dinosaurs’).

Let σ be a variable assignment and w be a world. $\delta_{\sigma,w}(t)$ will be our denotation function, which assigns a referent to term t relative to σ and w ; $Sat(\phi, \sigma, w)$ will be our satisfaction predicate, which expresses the satisfaction of ϕ relative to σ and w ; and $[\phi]_w$ will be our true-at-a-world operator, which expresses the thought that ϕ is true at w . Denotation and satisfaction are defined simultaneously, by way of the following clauses:

Denotation of arithmetical terms:

1. $\delta_{\sigma,w}(\ulcorner n_i \urcorner) = \sigma(\ulcorner n_i \urcorner)$
2. $\delta_{\sigma,w}(\ulcorner 0 \urcorner) = \text{the number Zero}$
3. $\delta_{\sigma,w}(\ulcorner S(t) \urcorner) = \delta_{\sigma,w}(t) + 1$

4. $\delta_{\sigma,w}(\ulcorner t_1 + t_2 \urcorner) = \delta_{\sigma,w}(t_1) + \delta_{\sigma,w}(t_2)$
5. $\delta_{\sigma,w}(\ulcorner t_1 \times t_2 \urcorner) = \delta_{\sigma,w}(t_1) \times \delta_{\sigma,w}(t_2)$
6. $\delta_{\sigma,w}(\ulcorner \#_{x_i}(\phi(x_i)) \urcorner) =$ the number of zs such that $Sat(\ulcorner \phi(x_i) \urcorner, \sigma^{z/\ulcorner x_i \urcorner}, w)$
7. $\delta_{\sigma,w}(\ulcorner \#_{n_i}(\phi(n_i)) \urcorner) =$ the number of ms such that $Sat(\ulcorner \phi(n_i) \urcorner, \sigma^{m/\ulcorner n_i \urcorner}, w)$

Denotation of non-arithmetical terms:

1. $\delta_{\sigma,w}(\ulcorner x_i \urcorner) = \sigma(\ulcorner x_i \urcorner)$
2. $\delta_{\sigma,w}(\text{'Caesar'}) = \text{Gaius Julius Caesar}$

Satisfaction:

1. $Sat(\ulcorner \exists n_i \phi \urcorner, \sigma, w) \leftrightarrow$ there is a number m such that $Sat(\phi, \sigma^{m/\ulcorner n_i \urcorner}, w)$
2. $Sat(\ulcorner \exists x_i \phi \urcorner, \sigma, w) \leftrightarrow$ there is a z such that $([\exists y(y = z)]_w \wedge Sat(\phi, \sigma^{z/\ulcorner x_i \urcorner}, w))$
3. $Sat(\ulcorner t_1 = t_2 \urcorner, \sigma, w) \leftrightarrow \delta_{\sigma,w}(t_1) = \delta_{\sigma,w}(t_2)$
4. $Sat(\ulcorner \text{Dinosaur}(t) \urcorner, \sigma, w) \leftrightarrow [\delta_{\sigma,w}(t) \text{ is a dinosaur}]_w$ (for t a non-arithmetical term)
5. $Sat(\ulcorner \phi \wedge \psi \urcorner, \sigma, w) \leftrightarrow Sat(\phi, \sigma, w) \wedge Sat(\psi, \sigma, w)$
6. $Sat(\ulcorner \neg \phi \urcorner, \sigma, w) \leftrightarrow \neg Sat(\phi, \sigma, w)$

2. A Trivialist Semantics for the Language of Set-Theory

We work with a two-sorted first-order language with identity, L . Besides the identity-symbol ‘=’, L contains the membership predicate ‘ \in ’, set-theoretic variables ($\alpha_1, \alpha_2, \dots$), urelement variables (x_1, x_2, \dots), and urelement predicate-letters (‘Philosopher(...)’).

As before, we let σ be a variable assignment and w be a world. $Sat(\phi, \sigma, w)$ will be our satisfaction predicate, which expresses the satisfaction of ϕ relative to σ and w ; and $[\phi]_w$ will be our true-at-a-world operator, which expresses the thought that ϕ is true at w . Satisfaction is defined as follows:

Satisfaction:

1. $Sat(\ulcorner \exists x_i \phi \urcorner, \sigma, w) \leftrightarrow$ there is a z such that $([\exists y(y = z)]_w \wedge Sat(\phi, \sigma^{z/\ulcorner x_i \urcorner}, w))$
2. $Sat(\ulcorner \exists \alpha_i \phi \urcorner, \sigma, w) \leftrightarrow$ there is a set β such that: (i) for any urelement z in the transitive closure of β , $[\exists y(y = z)]_w$, and (ii) $Sat(\phi, \sigma^{\beta/\ulcorner \alpha_i \urcorner}, w)$
3. $Sat(\ulcorner x = y \urcorner, \sigma, w) \leftrightarrow \sigma(x) = \sigma(y)$
4. $Sat(\ulcorner \alpha \in \beta \urcorner, \sigma, w) \leftrightarrow \sigma(\alpha) \in \sigma(\beta)$
5. $Sat(\ulcorner x \in \beta \urcorner, \sigma, w) \leftrightarrow \sigma(x) \in \sigma(\beta)$
6. $Sat(\ulcorner \text{Philosopher}(x) \urcorner, \sigma, w) \leftrightarrow [\sigma(x) \text{ is a philosopher}]_w$
7. $Sat(\ulcorner \phi \wedge \psi \urcorner, \sigma, w) \leftrightarrow Sat(\phi, \sigma, w) \wedge Sat(\psi, \sigma, w)$
8. $Sat(\ulcorner \neg \phi \urcorner, \sigma, w) \leftrightarrow \neg Sat(\phi, \sigma, w)$

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ARTICLE

The ontology of words: Realism, nominalism, and eliminativism

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Abstract

What are words? What makes two token words tokens of the same word-type? Are words abstract entities, or are they (merely) collections of tokens? The ontology of words tries to provide answers to these, and related questions. This article provides an overview of some of the most prominent views proposed in the literature, with a particular focus on the debate between type-realist, nominalist, and eliminativist ontologies of words.

Consider the word “omnishambles.” Famously used by Malcolm Tucker on the political satire “The Thick of It,” the word means (from the OED): “A situation that has been comprehensively mismanaged, characterized by a string of blunders and miscalculations.” The writers of the show came up with this word. They wrote it down, an actor spoke it, and the word has passed into common usage. But, a curious ontologist might ask what kind of entity did the authors bring into existence through their activity (or even if they did at all)? That is, what is a word?

The literature on the ontology of words has mainly focused on words as kinds or types—as things that can have instances or tokens. I will follow that trend here, outlining what the competing views take such kinds to be, and how these ontologies subsequently affect the answers to two interrelated questions that have dominated much of the literature. First, how should we individuate word kinds (or types), and, second, when is it the case that two token (or particular) words are instances of the same word-kind or type (A note on terminology. I will use “kind” and “type” interchangeably here.)

1 | CRITERIA FOR AN ONTOLOGY OF WORDS

We have a general practice of discussing and conceiving of words as entities in the world, and those words having certain characteristics or properties. Words have spellings, meanings, pronunciations, etc. Words play various roles

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in our lives. Some insult, some inspire, and words are central to communication. The aim of an ontology of words is to determine what entities, if any, can play those roles and possess (or instantiate) these properties. To do this, especially for those new to the debate, it would be useful to have some criteria in mind when assessing an ontology. Here are some initial proposals, drawn from (what I think are) common intuitions about words, with two caveats.

First, I have tried to phrase the criteria in a relatively neutral way. However, complete neutrality is likely impossible, and supporters of some ontologies may endorse a variation of some or all of the criteria rather than accept them as stated here. Others may reject some or all of the criteria entirely. This is fine as I do not intend them to be sacrosanct, but rather as a place to begin the discussion. It is also the case that other considerations such as theoretical virtues may also be important to theory choice. I therefore leave it open that parsimony, simplicity, elegance, or any other theoretical virtue might push us away from any criterion stated here.

Second, some of these criteria could be applied to token words and word-types. I take it that, ideally, we would have an ontology that accounts for the nature of both token words *and* word-types, and this may influence how we interpret any of the criteria.

The first criterion is that of *creation*.

Creation: whatever words are, they should be entities that account for the phenomena of “invention” or “coining.”

Words are created by people, perhaps for certain purposes, and within certain historical contexts. Creation speaks to the intuition that words exist only through the action of an agent. Raindrops forming patterns on my window, ants moving through spilt sugar, swamp words, waves forming patterns in the sand, and other bizarre natural phenomena are intuitively not instances of a word, and hence not sufficient to create a word (either qua token word or word-kind).¹

Second, *expressibility*:

Expressibility: whatever words are, they should be expressible through some means of externalization (speaking, writing, signing etc.)

Words are in some important sense social entities that are shareable through various forms of externalization. This makes word-kinds, at least to some degree, social kinds.² Importantly, expressibility is neutral as to how words are expressed. The most common form of expression (or externalization) of words in humans is through spoken language, but it is also done through writing, signing, and potentially other forms of linguistic communication.³

As stated, this is a requirement only that words could be expressed, not that they are. But, we might debate the scope of “could.” Some (e.g., Hawthorne & Lepore, 2011) hold that derivational morphology suggests that there are words that are composed of so many suffixes that those words would not be expressible, at least by humans with finite lifespans.⁴ Whether such words exist is an example of the debate about uninstantiated words that will come up again later in this paper. However, while the full details of expressibility are up for debate, the commonly accepted idea is that even if *some* words are not expressible, it must be the case that words can (in one sense of can) be expressed as it seems that some clearly are.

Third, *evolution* (or perhaps, *change*):

Evolution: whatever words are they should account for the apparent change of words (e.g., being spread, forgotten, changing meaning, spelling, or pronunciation).

Words are not static. They change their meanings, or at least *can* change their meanings, spellings, and pronunciations. As will be discussed below, it is difficult to find a property that some token of a particular word-type has that is also had by all other tokens of that word-type. Words are also lost. Intuitively, words from certain extinct languages that have no written record no longer exist.

This is to say nothing about how much change a word can undergo while remaining the same word. Different ontologies will be able to accommodate differing amounts of change, just as different accounts of the metaphysics of ordinary objects can accommodate differing amounts of change in medium-sized dry goods.

One last consideration is the relationship between the ontology of words and linguistics. Though it has been argued otherwise (e.g., Balletta, 2019), I will assume here that the aim of our ontology of words is to provide a conception of “word” that satisfies all of the scientific, philosophical, and everyday demands on it. That is, a conception that can provide answers to philosophical (and in this context primarily metaphysical) puzzles, whilst being (minimally) coherent with the empirical evidence, and maintaining as far as possible our ordinary way of talking about words. Perhaps this is not possible, with each of these ways of talking about words requiring its own specific notion of “word,” suggesting some form of ontological pluralism about words. However, the ontology of words typically proceeds with a unified single notion of “word” as the aim, with each theory attempting to provide the best balance between philosophical rigor and alignment with the empirical data. It remains debated, naturally, as to how well the ontologies achieve this aim.⁵

2 | TYPE-REALIST VIEWS

There are many different ways we could classify views about the ontology of words. Here, I will do so through the lens of (probably) the most discussed distinction, between “realist” and “nominalist” ontologies. In brief, the distinction is between views that posit the existence of “kind” or “type” level entities, and those that deny the existence of such entities. For each theory, the two interconnected questions I raised at the beginning of this paper will arise, and how satisfying an answer the theory provides to these questions will greatly influence how plausible the ontology. As we will also see, though, there is much disagreement amongst adherents of views within these broad categories.

2.1 | Platonism

Platonism, broadly understood, holds that words are abstract eternal types, which have instances—standardly, either physical instances (e.g., written or spoken tokens) or mental instances.⁶ Thus, following the convention to use lower case for particular entities, and capitals for type-level entities, a particular word, “table,” is an instance of the abstract word-type, “TABLE.” These types are genuinely existing entities, distinct from their instances, and (typically) Platonists posit a relation of instantiation as holding between word tokens and these abstract word-types (Wetzel, 2009).

Naturally, there are Platonic type-realist views that vary from this attempt to characterize the view, and in particular reject this “instantiation model” of the type-token relation. Katz’s Platonism, for example, holds that tokens are composite objects, composed of the abstract type and the some physical or psychological particular. Katz argues that it is a “tokening” relation, rather than instantiation (2000: chapter 5) that holds between the types and tokens.

Platonists (and many non-Platonic realists; see Section 2.2) are more united in arguing that we need to posit types in order to understand the truth of various ordinary and scientific claims about words. For example, when I say that “Paris’ contains 5 letters”, this is not standardly read as being a claim about some particular word, but the word-type “PARIS”. Granting that the claim is true, it is a true claim about some type-level entity, not just some particular instance(s).⁷ Hence, word-types must exist. Indeed, some Platonists (notably Katz, 1981, 2000; Postal, 2003, 2009; see also fn. 16) go further to argue that this evidence supports the view that the proper subject matter of linguistics must be abstract entities.

A major benefit of Platonism is that it provides a simple answer to the question of when it is the case that two particular words are tokens of the same word: “color” and “colour” are tokens of the same word because they are both instances of the same abstract type. As with Platonic views in other domains, the view allows that tokens of the same type need not share the same properties. This ensures that despite the difference in

spelling (and, in other cases, differences in pronunciation and meaning), “color” and “colour” are tokens of the same abstract type.

However, while Platonism allows for our intuition that there can be differences between tokens of the same type, what ensures that tokens are tokens of some type? What are the necessary and sufficient conditions on some token being a token of a type? The problem for the Platonist is that it may be that no good answer is forthcoming.

Platonists are quick to reject spelling and pronunciation as providing the identity conditions of words. This is because of the need to accommodate variation of tokens of a type, as shown by countless examples of alternative spellings and pronunciation of intuitively the same word. Semantic properties also cannot seem to provide a criterion of word identity. There are many cases where (at least intuitively) instances of the same word have distinct semantic properties, beyond changes arising from the circumstances of use, or where words have significantly changed their meaning over time, and yet are ordinarily thought to be the same word. For these and other reasons, there has been no major defense of a form of Platonism that appeals to phonetic, orthographic, or semantic properties in order to say when two tokens are tokens of the same type.

This difficulty in finding some shared property had by all tokens of a type have led some to appeal to intentions as necessary for the tokening of some type (e.g., Katz, 2000:153; cf. Kaplan, 1990, 2011 and discussion in Section 2.2). Others argue that there is no property that all tokens of the same type share except that of being tokens of the same type (Wetzel, 2002; 2009:106–7). If this later view is correct, then tokens are tokens of a type if they stand in the appropriate instantiation relation to the type, with no resemblance requirement either between tokens or between the token and the type. Somewhat relatedly, Hawthorne and Lepore argue for what they call an “abstracta-articulations” model. On their view, though words are abstract entities, the model “breaks with the standard type-token model’s picture of the relevant abstracta as pattern-like” (2011:38). On how to provide a criterion of identity for words, they are not sure that a positive answer can be given, suggesting instead that we should be “sloppy realists” wherein “there either are facts we may never know or simply no facts at all about the myriad borderline cases left unresolved by our capacity to settle questions in the area” (2011:36).

How persuasive sloppy realism or the appeal to the instantiation relation are, I think, depends on our antecedent commitment to a realism about word-types. Those with more nominalist tendencies will likely not find an appeal to the property of “being a token of the same type” convincing. To many nominalists this claim looks brute, possibly even ad hoc, and providing little scope for us to discover when it is the case that tokens are of the same type given that instantiation relations cannot be studied empirically. If types are posited to explain the sameness of words, then positing a “being a token of the same type” property looks like it is simply positing into existence a primitive that solves the initial problem (see Miller 2019c).

Moving on to other issues, Platonism about words faces versions of various familiar problems raised against other forms of Platonism. For example, granting the standard assumptions that abstract types are noncausal, the Platonist about words needs to be able to explain how it is that we can come to know words qua abstract eternal types.⁸ However, rather than rehearse familiar debates here, I will focus on some problems that are more specific to the debate about words.

One strong intuition that we have is that words are created entities—that we can and do “coin” new words, and that Shakespeare invented a “multitude” of “new-fangled” “auspicious” words. The problem for a Platonic account is that if words are abstract, eternal Platonic types, then how can we account for the sense that we create words? Indeed, many Platonists instead hold that we *discover* words (Katz, 2000:134; 168; see also the discussion in Begley, 2019). This strikes many as deeply counter-intuitive, and goes against our normal way of speaking about words as created.

Relatedly, we might object that taking words to be eternal entities suggests that there exist untokened word-types. That is, that words that have yet to be tokened already exist, and are waiting in some abstract realm to be first instanced. This, as well as being counter-intuitive, could have further consequences for the view that words are social entities created by, and for the use of, communities of speakers.

The typical response is to refine creation to distinguish between the creation of new tokens and the discovery of some eternal type. New tokens are created, and so are the ways of expressing abstract word types, and it is this that we mean when we say that Shakespeare created new words. He created new ways to express already existing eternal types, which prior to that point may have been untokened. Hawthorne and Lepore (2011) also argue that the findings of derivational morphology makes untokened abstract types less implausible than they initially seem, and in fact required in order to fully account for ordinary speakers to be able to understand new tokens that a speaker has not previously encountered.

This shift to tokens also typically explains word change in these kinds of realist accounts. As words are abstract eternal types, the words themselves cannot change. Instead, what changes are the ways in which words are expressed, and the relations that hold between particular sounds or ink patterns, and word-types. This means that for the Platonist, words evolve in the sense that the same sound may, over time, come to stand in an instantiation relation with a different word-type than previously.

2.2 | Non-Platonic realist views

Platonism is committed to words as eternal, unchanging, abstract entities. It is possible, though, to be a type-realist, but reject Platonism, with such views most often motivated by a desire to include words within a (more) naturalistic account and, borrowing a phrase from Kaplan, hold that words “live in the world, not in Plato’s heaven” (1990:111).

One way to hold an alternative type-realist view is to keep parts of the Platonic picture intact, but deny certain problematic specifically *Platonic* features of word-types. That is, to hold that there are genuinely existing abstract word-types, but deny that such types have one (or more) of the features that cause problems for the Platonist.

We have seen one example of such a twist on Platonism already in Hawthorne and Lepore’s abstracta-articulations model. Another comes from Szabó (1999) who argues against positing an instantiation relation between types and token, suggesting instead a “representation” relation. Importantly, Szabó does this, in part, to avoid a Platonic conception of types as eternal and unchanging, but to maintain a realism about types that can account for the historical nature of types. Under his view, token words are “type-representations” arising from “our tendency to apply terms referring to abstract entities to their standard representations too” (1999:160). Thus, a type is represented by its tokens, allowing us to explain how empirical information about a token can inform us about the nature of the type. Szabó argues that this is not possible under the instantiation model as it relies on inductive generalizations to move from knowledge of the token to knowledge of the type.

Yet another can be found in Irmak (2019). Irmak posits words as genuinely existing abstract entities—hence he is in my terms a realist—but they are *created* abstract entities. If correct, this would allow us to resist the problems I raised above for Platonism concerning the claim that we “discover” words, and the concern about already existing uninstantiated words.⁹

A very different type-realist ontology comes from Kaplan. Kaplan (1990, 2011) begins by noting the same variation that I have touched upon above: that instances of the same word can vary in spelling and pronunciation, and argues that this is sufficient reason to reject the view that words have “some fixed and perfect Platonic form” (1990:100).¹⁰ Instead, Kaplan proposes that utterances and inscriptions are “stages” of words, with words themselves being “the continuants made up of these interpersonal stages along with some more mysterious intrapersonal stages” (Kaplan, 1990:98). Particular utterances or inscriptions are instances of the same word if they are nodes on “a single, continuous tree of utterances, inscriptions, and quiescent storage” (2011:510). Though Kaplan resists the analogy (2011:508), this has reminded many of four-dimensionalist views in the metaphysics of persistence.

The main strength of this view is that it accounts for how words change and evolve over time. This is because what makes a token a token of a certain type is not that it resembles that type. Instead, Kaplan suggests that words are more like families. Like families, word-types (or, more precisely, continuants) may happen to resemble in certain ways, but they need not. The stages (or tokens) of a continuant (or word-type) may vary hugely whilst still being

tokens of the same type, including varying over time as spelling, pronunciation, and meanings associated with words shift and change. Rejecting any appeal to resemblance, what connects tokens of a type are relations that are “historical in nature and not apparent to perception” (2011:509). That is, it is the shared historical connection to other tokens of that type (to stages of that continuant) that makes two tokens of the same type.¹¹

The appeal to historical relations additionally ensures that this ontology can maintain that words are created entities. Two utterances or inscriptions are of the same word in that they “descend from a common ancestor” (2011:509). This means that there had to have been that common ancestor—a first token or stage—for later tokens to have descended from. Given that that common ancestor will itself be an utterance or inscription, this makes word-types created entities and rules out the existence of noninstantiated words.¹²

However, Kaplan also argues that the historical connection is itself not sufficient to token a word. Invoking the notion of “repetition”, Kaplan argues that “a sincere subject, intending to repeat a word that has been uttered by an examiner, will, indeed, utter that word” (2011:518). This is important for Kaplan's account as without it, given that tokens of the same type need not resemble at all, it would be open that any token could, without the knowledge of the speaker, turn out to be a token of any type. Repetition and intention together explain the continuity between speakers and within communities, as speakers are able to say the same word by intending to repeat words that others have spoken—by intending to utter a new stage that bears the correct historical relations to other stages of that continuant.

This means that so long as certain minimal capabilities are present (i.e., that the person is able to speak and is not simply producing grunts or other mere noises; see Kaplan, 2011:519), if a speaker intends to express a word, then they will succeed in expressing a token of that word, irrespective of how much it resembles other tokens of that word, thereby making the historical connection between tokens significantly intentional.

This reliance on intentions has been a source of many objections to the view. Cappelen (1999), for example, has argued that intentions cannot be part of the individuation of words, arguing that the intention to utter a token of a word is neither sufficient nor necessary for being a word token. This is because, under intentionalist views like Kaplan's, for a listener to know if something is a token of a word, they would need to know the intentions of the utterer of that word, something that we are often not in the position to do (1999:97). Thus, according to Cappelen, the knowledge of certain nonintentional (but still conventional) semantic facts is a necessary condition for communication as we observe it.¹³

It is important to stress that a stage-continuant ontologist need not accept intentionalism, but there are other objections arise more directly from the proposed ontology that would remain. For example, Kaplan holds that continuants are made up of, or composed by, stages. This seems to imply that if any stages that compose a word were different, then the word would also be different in virtue of being composed of different stages. Hawthorne and Lepore (2011: 7-8) have argued that this makes stages necessary, contra our intuition that any token might have been uttered differently, or even not at all. A possible response might lean on counterfactualist responses to similar objections raised against four-dimensionalism elsewhere in metaphysics, but this has yet to be fully developed in this literature.

3 | NOMINALISM

Nominalists about words hold that “no explanatory work will be done by picking out some one abstract entity as the sign type. That's to say, it might be that reifying sign types would be explanatorily superfluous” (Cappelen, 1999:100). Type-talk need not be rejected, but what these views have in common is the rejection of *ontologically committing* type-talk. Mention of types is *mere* talk, and is only a way of talking about sets, collections, or classes of suitably resembling tokens. Thus, these views only countenance the existence of tokens, rejecting the existence of types. The tokens “table” and “table” are thus distinct, and are the “same word” only in the sense that they

are members of the same set, collection, or class of tokens. In the rest of this section, my use of the term “type” should thus be read in a suitably nominalistic fashion.

The ontological parsimony achieved by positing only tokens is often a main motivation for accepting nominalism. Naturally, though, parsimony is only a virtue if the ontology is (at least) equally as explanatory as less parsimonious ontologies, and hence much of the focus of nominalist accounts has been to show how genuinely existing types are not needed, or even that a lack of types better accounts for the phenomena we observe. For example, by restricting our ontology to only word-tokens, the nominalist may avoid problems concerning the created status of words and uninstantiated words. If word-types are merely collections of tokens then those collections do not exist without members, and come into existence only when the tokens that compose the collection do.

The nominalist, though, still faces one of the central questions from above: what makes tokens tokens of the same type? Or, in the nominalist's terms, what makes two tokens members of the same (nominalistic) set, collection, or class? The nominalist still needs an answer to this question, despite their insistence that type-talk is not ontologically committing, if only due to the need to be able to adequately explain all of the ways that we typically talk about words that do seem to invoke types.

Older forms of nominalism attempted to respond to this question by appealing only to the “shape” or “form” of token words. That is, at least as it is often portrayed in the literature, nominalism about words is the view that the only relevant property of tokens to assess whether tokens are members of the same type are the spelling or pronunciation properties of those tokens. This means that we can only say that tokens are the tokens of the same type if the tokens are (exactly) resembling in their spelling or pronunciation. Accordingly, this has become known as shape- (or form-) theoretic nominalism.

Shape-theoretic nominalism has normally been attributed to Quine and Goodman as part of their broader nominalism applied to linguistic entities,¹⁴ and Bloomfield (1933). A full account of the reasons behind the restriction to “shape” properties for each of these authors would require a longer historical analysis than can be provided here, but one reason is an independent commitment to (or at least sympathy towards) (reductive) physicalism and behaviorism. For example, Bloomfield held that language was nothing more than sounds and ink patterns, and that “meanings” were reducible to complex behavioral analysis. He writes that:

“Non-linguists (unless they happen to be physicalists) constantly forget that a speaker is making noise, and credit him, instead, with the possession of impalpable ‘ideas’. It remains for linguists to show, in detail, that the speaker has no ‘ideas’, and that the noise is sufficient” (Bloomfield 1936: 93)

Given these further commitments, we can see why only spelling and pronunciation are acceptable to a shape-theoretic account. The “noise is sufficient” because there simply are no other properties, or those other properties are themselves reducible to “physical” properties (spelling and pronunciation) or the causal effects of those properties cashed out as a pattern of behaviors.

Shape-theoretic nominalism faces a number immediate and well-known counterexamples. By restricting membership of a type to tokens that are exactly resembling in spelling or pronunciation, the view would seem to predict that the tokens “color” and “colour” are tokens of different types, not just different spellings of the same type. Analogously, the view *prima facie* struggles to account for different accents. The shape-theoretic nominalist seems to be committed to the absurd claim that two speakers, one from Liverpool and the other from Newcastle, utter tokens that are not members of the same class due to the fact that they have very different accents.

Wetzel has taken this line of argument even further, suggesting that shape-theoretic nominalism also cannot accommodate the common phenomena such as that of misspelling. If I were to write “Pareiss” then we would intuitively think I have misspelt “Paris.” But Wetzel argues that the shape-theoretic nominalist cannot say this as there is no sense in which the tokens “Paris” and “Pareiss” are members of the same type (Wetzel, 2000). As the tokens have different spellings (ignoring any phonetic properties for now), they simply are not members of the same type. Many take these consequences as enough to show that shape-theoretic nominalism is false.

There are, though, nominalist ontologies that reject this restriction to *only* spelling and pronunciation properties as a guide to type-membership. These nominalists hold that there can be also types whose membership is determined by the resemblance of members (i.e., the tokens) with respect to other properties, including, but not limited to, semantic, grammatical, and intentional properties possessed by the tokens.

For example, modelled after bundle theories of objects, Miller (2019c) has argued that words are nothing more than bundles of certain (linguistic) properties. Under this ontology, tokens are bundles of properties, and types are collections of tokens, determined by the resemblance of properties that partly compose those token words qua members of that type. This allows Miller to hold that there are types whose members resemble with respect to any properties that we might take token words to possess, avoiding the limits within shape-theoretic nominalism, and thereby accommodating a wide range of intuitions we have about the sameness of words.

Indeed, by positing only resemblance relations between tokens, and not type-identity relations, the nominalist can also accommodate cases of nonexact resemblance by holding that some types may even be such that the members of the type nonexactly resemble in virtue of one (or more) of the properties possessed by those tokens. This, Miller argues, is able to explain our frequent talk about word-types where the tokens of that type are *relevantly* similar, where relevance reflects the various aims and purposes to which we put type-talk in our ordinary and scientific language.

Another, still nominalist approach has been to appeal to what Bromberger calls “archetypes” or “models” (Bromberger, 1989; Bromberger, 2011). Like other nominalists, Bromberger argues for a view of words that is intended to be able to maintain type-talk, without positing the existence of abstract entities. However, whilst the nominalism sketched above holds that types are collections of tokens, for Bromberger, tokens are members of quasi-natural kinds, and types are archetypes (or models) of those kinds. Types are models which are “object[s] so designed that, by finding the answers to some questions about it, a competent user can figure out the answer to questions about something else” (Bromberger, 1989, 62). Thus, to talk of the word “table” is not to talk about some genuinely existing abstract type, but instead to talk of a model that can be used to understand various tokens that resemble that model.

Bromberger, like other nominalists, argues that this modelling is reflective of our interests rather than being some metaphysical absolute: “no pair of objects stands (or fails to stand) in the model/modelled relation absolutely, but only relative to specific sets of questions, pairings of questions, and algorithms” (Bromberger, 1989, 63). This, Bromberger states means that “speaker-writer mind-brains endowed with grammars and lexicons leave no need for abstracta” (2011:496), in line with the nominalist denial of such entities.

A major weakness, or for some people a major benefit, of nominalism, in all its forms, is that it allows for a greater number of collections or models, with no collection or model ontologically more significant than another. Put another way, nominalism makes word-types more conventionalist than many ordinary speakers typically assume. This results in there being far more collections than those we typically recognize and accept, and those we do accept being only significant in that they are the most important for our contingent communicative or explanatory aims.

It also means that whether tokens are tokens of the same type becomes a relative matter, not absolute. The answer to questions about the “sameness” of tokens will depend on which collection of tokens we are interested in. To see this, consider again two tokens, “color” and “colour”. Typically, we think of these as tokens of the same type. Under nominalist ontologies, whether the tokens are of the same type will depend on which collection of tokens we are considering, or which model. Using Miller's ontology to illustrate, if the relevant type is one whose members possess (exactly) resembling semantic properties, then the tokens will be tokens of the same type. But, if the relevant type is one whose members possess (exactly) resembling orthographic properties (i.e., spelling), then they are not the “same” word. “color” and “colour” would then be tokens of different types.

It is important, though, that whilst being more conventionalist, the nominalist need not accept that “anything goes” when it comes to types. Some types will still be more gerrymandered than others, tracking unimportant (or even nonreal) distinctions amongst tokens and their properties. Nominalists can accept that our empirical research into words aims to find those types that track genuine, objective similarities and differences amongst

tokens, or those that are most pertinent to our explanatory aims. How objectionable this all is will likely come down to how firmly we want to retain the sorts of strong intuitions that initially motivated Platonic accounts against the idea that tokens are tokens of the same type “merely” in virtue of resembling each other.

4 | ELIMINATIVISM

The debate between the realist and the nominalist focuses significantly on the ontological status of word-types. The views disagree about whether word-types exist, but agree on the existence of word tokens. The last view I will outline is one strictly only about *token* words, and is the view that token words, and indeed all “standard linguistic entities,” do not exist as *concrete* entities. Words as we typically think of them qua ink patterns or sound waves are only an illusion, or are “intentional inexistent.” This is therefore an eliminativism about words.¹⁵

Here is an argument for the view. First, words cannot exist in space-time; they cannot be physical entities. The reason for this is that when we look closely at ink patterns and sound waves, we simply do not find objects instantiating the properties that are essential to words. In particular, we do not see the complex syntactic properties that words are taken to have by our best linguistic theories. In fact, when we look at acoustic strings, we do not even see any breaks between what we perceive to be distinct words. The subject matter of linguistics, which intuitively would include words, cannot be “physical” entities as the physical entities in the world, including words, do not have the sorts of properties of features that linguists investigate.¹⁶

What then is happening when we speak if we are not producing token words? The proposed answer is that words are “intentional inexistent”—they are “‘things’ that we represent and think of as ‘out there’, but which do not exist” (Rey, 2008:177). Rey calls this the “folieist” view, “according to which it is a kind of ‘folie à deux’ in which speakers and hearers enjoy a stable and innocuous illusion of producing and hearing standard linguistic entities” (2008:177).

This means that words are like perceptual illusions. In perceptual illusions we seem to see certain figures or shapes, but there is nothing more than that illusion. Similarly, when we seem to be uttering a word, we simply do not produce anything with the same structure or properties that words are typically taken to have. Speakers do make sounds, but those sounds are not words. Rather there is a shared illusion of the existence of words, such that the speaker and hearer can both infer certain intentional contents from those illusions, and this recognition of intentional content is all that there is to communication.

The major argument for this view is that positing words (and other linguistics entities) as being anything more than an illusion is simply not theoretically useful. That “all that need be true for the noises a speaker makes to have their intended effect is that they be perceived to have the tree structure that the speaker intended; and [...] something can be perceived to have a structure without it actually possessing that structure” (2006:554). Taking such entities to be “real” is not needed to explain the behavior of such entities within linguistic theory. Instead, the sound waves cause us to enter into perceptual states that are stable across time and people such that we can happily proceed as if words were real.

Objections to this view often focus on these claims that the view cannot accommodate some aspect of communication. For example, Barber (2006) argues that eliminativism about words cannot explain that communication involves (at least sometimes) the transfer of knowledge. This is because it conflicts with a principle that Barber argues underlies knowledge through testimony: the “no-false-lemmas principle” that holds that “belief is knowledge only if it is not based on falsehoods” (2006:412). Devitt (2006: 187) also raises objections to eliminativism due to its consequences for communication. Devitt argues that the guesswork that it requires for successful communication to occur—the “happy accident” that Rey relies on for speakers to be able to correctly infer the intentional content of others—could not account for the complex forms of communication we observe.

Devitt also takes on the initial motivation for eliminativism more directly, arguing that the lack of easily *perceptible* syntactic properties does not mean that such properties are not instantiated by words (and other linguistic

entities). This is because many relational properties are hard to perceive, but standardly taken to exist nonetheless (2006:185). Linguistic utterances thus, according to Devitt, really do instantiate syntactic properties, and hence it really is the case that the entities we typically take to be words have the properties that are essential to them.

Rey (2008) provides responses to these criticisms (and others) that I do not have the space to outline here. However, even if those responses are persuasive, it is still certainly the case that eliminativism about words requires a radical shift from our intuitive understanding of the nature of words. The question then is about how far we should be motivated by such intuitions, particularly if those common-sense intuitions can be shown to be in tension with empirical theorizing.

5 | SITUATING WORDS IN THE METAPHYSICS OF LANGUAGE

There is much more that could be said about the ontology of words, and it is still a field in its relative infancy. Words have been studied by philosophers, across a range of topics, but devoted explicit work on words has been, to borrow a phrase from Alward, more of “a trickle than a torrent” (2005:172). Here, I have focused on certain core ontological issues that have been discussed in the literature to date, especially the debate between the realist and nominalist about word-types. There remains, though, huge scope to develop alternative ontologies that may prove fruitful to our conception of words. For example, some have begun to explore the idea of word tokens as being constituted by, but distinct from, the matter that composes them. These views, drawing inspiration from well-known theories in the metaphysics of ordinary objects and debates about the statue and the lump, might open up new ways for accounting for various linguistic phenomena, and could bring work on the ontology of words more into line with developments in the wider field of social ontology.¹⁷

There are also wider metaphysical questions about words. For example, questions about persistence and change of *token* words, and there is a need to connect work on word-kinds with the wider literature on the metaphysics of kinds. Can a token of one type *become* a token of another type? Some scenarios suggest they can.¹⁸ If we accept the existence of word-kinds, then how should we understand the nature of those kinds? Are they natural kinds? Are words, as has often been suggested, like species? If so, what this means for our ontology of words will depend on various additional commitments we have about the metaphysics of kinds.¹⁹ These are merely indicative questions, and certainly not exhaustive of the range of metaphysical issues concerning words. Words have been understudied in the philosophical literature, at least from a metaphysical approach, and there is a lot of room for new developments on a range of topics.

There are also important questions about what a commitment to a particular ontology of words might mean for our other commitments in other philosophical domains. For example, one aspect that I have not touched upon, but certainly deserves research focus in the future, is the relationship between these ontologies and existing topics and theories in the philosophy of language. It would be extremely strange if our ontology of words did not have consequences for our broader philosophy of language and traditional issues therein. These connections have so far been very underexplored.²⁰

Looking at a bigger picture, the ontology of words should, in my view, be just one part of a broader metaphysics of language, as the investigation into a wide variety of questions arising from the nature of linguistic entities, be they linguistic objects like words, sentences, phrases etc., but also linguistic properties (such as grammatical properties). There are metaphysical questions about phrases, sentences, morphemes, phonemes, grammatical relations, and languages. All of these require their own specific ontological treatment, and any putative ontology of words should be minimally coherent with ontologies of these other linguistic entities.²¹

As touched upon above in passing, there also remains the central issue of how far an ontology of words (and metaphysics of language) should align with our linguistic theorizing. I suggested in this paper that there should be minimal coherence with empirical theorizing, but this leaves upon the question of coherence with which theories? Linguists, naturally, disagree about many aspects of language relevant to the ontology of words, and hence any

ontologist working on a theory about the nature of words may (implicitly or explicitly) make assumptions that cohere with some theories but conflict with others. It is beyond the scope of this paper to talk extensively about the relationship between philosophical work on words and linguistic theories that concern words, but the need for work on this connection is clear. Those working on the ontology of words (and the metaphysics of language more broadly) need to have some grasp on developments in the relevant areas of linguistics, and, going the other way, work in the ontology of words may uncover implicit metaphysical assumptions within linguistic theorizing. Personally, I am sceptical about attempts to read our ontology off of our linguistic theory, and we need to recognize that the aims of linguists are (at least often) different from those of ontologists. But, the ontology of words as a topic calls out for interdisciplinary connections and research.

To conclude, the ontology of words gives rise to many questions that need be answered within the broader enterprise of the metaphysics of language, with connections to existing research in various philosophical domains including (at least) social and political philosophy, philosophy of mind, and philosophy of science. The interdisciplinary potential is also clear to see. Language is studied, in various ways and to various ends, in a wide range of sciences, and there are unanswered questions concerning both how that work intersects with philosophical work on words and other linguistic entities, and about possible philosophical assumptions that lie within those endeavors. The correct ontology of words will ultimately be just one piece of this wider metaphysics of language.

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ENDNOTES

¹It is also the case that not all intentionally created patterns are words. It is more disputed whether there can be non-intentionally created words; see Munroe (2016) for a discussion of this in the context of theories in psycholinguistics. Accepting creation as stated is intended to require no commitment about what intentional acts or agency are, and what creatures are capable of such actions or possess agency.

²See Mason (2016) for an overview of the debate about the metaphysics of social kinds.

³I leave aside the difference between linguistic and non-linguistic communication as, *prima facie*, while non-linguistic communication may be complex, it does not involve *words*.

⁴The suggestion is that we can iteratively add suffixes and/or prefixes, *and* that the addition of each new suffix or prefix involves the creation of a new word. At some point these newly created words would be inexpressible - some could be infinitely long. The new words may not be accepted by ordinary language speakers, in part due to the cognitive difficulty of parsing them. However, this inability seems likely to be an issue of the working memory of humans, and may not be a good guide to the *existence* of these words.

⁵Due to space restraints, the precise details about how each theory is meant to be coherent with the empirical data cannot be discussed in this paper. For the most direct discussion of this, see Wetzel (2009) on Platonism and linguistics, Miller (2019c) on nominalist views and linguistics, and Nefdt (2019a) on structuralist accounts of words and linguistics.

⁶A relatively underdiscussed point in the literature is what mental tokens of words are in the sense of what sort or type of mental state we should take such words to be, or whether they are a distinct sort of mental state from those discussed elsewhere in the philosophical literature. This is an open question for both realist and nominalist ontologies, as supporters of both views are generally happy to posit token words as being either physical or mental.

⁷See Wetzel (2009: chap. 1) for an extended discussion of a range of data that the Platonist argues warrants the positing of types.

⁸See Wetzel (2009: chap. 2) for a response to this concern that leans heavily on responses developed in the context of Platonism in the philosophy of mathematics

⁹Of course, this requires a major shift in how we have typically thought about abstract entities. For more on this, see Irmak (2020).

¹⁰Note that Kaplan argues for this view as an alternative to what he calls “type-token models,” which is a form of Platonism that appeals to spelling and/or pronunciation as the criterion of word identity. The terminology is also tricky here. Kaplan dislikes the using the term “type,” but only due to its prior association with other ontological views. In this exposition, I am using the term “type” more neutrally, and hence we can equate types with Kaplan’s “continuants,” and tokens with his “stages.”

¹¹Similar views on the importance of history or origin on the individuation of word-types can be found in Irmak (2019), Millikan (1984: 74–75), and Sainsbury & Tye (2012: 4). See Miller (2019a) for an argument against taking historical properties as the correct criterion of individuation for words.

¹²Though Kaplan does hold that there are untokened sentence-types (and other linguistic types), holding that those types are abstract entities (2011:511).

¹³Though see Alward (2005) for a response to Cappelen’s concerns, but also further problems for Kaplan’s ontology relating to the role of words in communication.

¹⁴See in particular, amongst their other work, Goodman and Quine (1947), and Quine (1960, 1987).

¹⁵Again, this is a claim about the ontological status of token words, not types. The view is neutral about the existence of types; see Rey (2006, 2008: 181).

¹⁶See Nefdt (2019b) and Stainton (2014) for in depth discussion of the subject matter of linguistics specifically; here I focus only on how it contributes to the motivation for certain views about the ontological status of words. It is, though, worth noting that many writing in the nominalist-realist debate more recently have normally tried to stay neutral with respect to a related debate about the subject matter of linguistics.

¹⁷For discussion of these views concerning words and other social entities, see Epstein (2009, 2015) and Evnine (2016).

¹⁸For example, imagine a sentence written on a blackboard, reading “A bank is a financial institution.” Now someone erases all of the tokens, except the token “bank,” and then inscribes new tokens such that the sentence on the board now reads “The bank was home to many small creatures.” Intuitively, the token “bank” in the second sentence is of a different type than the token “bank” in the first, and difference in type indicates a difference in identity. However, *prima facie* the only changes to the token in this scenario are extrinsic, and changes in extrinsic properties are standardly taken to be insufficient for genuine change.

¹⁹See Wetzel (2009) for a defense of the view that words are like species. See Miller (n.d.) for a wider discussion of the nature of word-kinds in the context of recent work on the metaphysics of kinds.

²⁰For an example of this kind of work, see Miller (2019b) on the ontology of words and theories of quotation.

²¹See Nefdt (2019a) and Jackendoff (2018) for work that comments on this connection to the ontology of other sorts of linguistic entities. See also Santana (2016) for a good overview and discussion of the ontology of language debate more broadly.

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Nominalism Meets Indivisibilism

JACK ZUPKO

Nominalists, it is said, are defined by their opposition to the needless multiplication of entities. For most fourteenth-century nominalists, parsimony was in the first instance a logico-semantic matter, raising the question of how one should explain the truth conditions of sentences without assuming any kind of strictly isomorphic relation between individual sentences and what makes them true.¹ In their analyses of the structure of continuous spatial magnitudes, this question was presented in an especially clear and unambiguous

1. Thus Calvin Normore argues that it is misleading to see medieval nominalism solely as a campaign against real universals. It would be more accurate to say that medieval nominalists sought to economize on entities, of which real universals were but one type. See his "The Tradition of Mediaeval Nominalism," in *Studies in Medieval Philosophy*, ed. John F. Wippel (Washington, D.C.: Catholic University of America Press, 1987), pp. 201–217. For a detailed discussion of the nominalist ontological program as found in the writings of William of Ockham, see Marilyn McCord Adams, *William Ockham* (Notre Dame, Ind.: University of Notre Dame Press, 1987), pp. 3–313. For a useful historical study of late medieval nominalism (whose author, it should be pointed out, "purposely disregards" what he calls "the traditional cliché of 'nominalism'," p. 152), see Damasus Trapp, "Augustinian Theology of the 14th Century: Notes on Editions, Marginalia, Opinions, and Book-Lore," *Augustiniana* 6 (1956): 146–274, esp. pp. 182–190. Though I am in general agreement with Normore's view, I won't take up here the controversial question of how medieval nominalism should be defined. Nor will any of the particular arguments given below depend on it.

form: "Is it necessary to posit indivisible entities to explain the truth conditions of sentences containing terms such as 'point', 'line', and 'surface'?" Affirmative answers offered one route to indivisibilism, the thesis that continua are divisible into finitely or infinitely many indivisible parts, or mathematical atoms.² But negative answers, besides leading to the opposing view that continua are infinitely divisible, also invited some account of how terms such as 'point', 'line', and 'surface' are to be understood, if not as standing for real mathematical points, lines, and surfaces (surfaces being indivisible in one dimension, lines in two dimensions, and points in three). The way in which such parsimonious ontologies were achieved in practice, however, shows us that nominalist methodology was anything but static in the later Middle Ages, as more and more sophisticated techniques were introduced and perfected to explain the relation between terms and what they signify. This essay is addressed to one small, though representative, part of that story.

I shall focus on an example, an argument, and the reply to that argument given by a series of fourteenth-century nominalist thinkers roughly contemporary with each other: William of Ockham (ca. 1285–1347), Adam Wodeham (ca. 1298–1358), and John Buridan (ca. 1295–1358).³ The example concerns an ideal sphere gradually descending onto a perfectly plane surface until they come into contact, or sometimes a sphere in contact with a plane surface and rolling across it. Its function was to raise a question: Would the sphere (first) touch the plane surface at a point? This example was a commonplace in medieval literature on continua, where it most typically arose as

2. These are mathematical atoms, not physical atoms. As John Murdoch has observed, "late medieval atomism was not intended, as was that of Democritus or Epicurus, as any kind of general system which might cover or explain the natural world. . . . It was intended rather as a single facet of natural philosophy, designed simply to explain the structure of magnitudes, and specifically of space, time, and motion as magnitudes." See his "The Development and Criticism of Atomism in the Later Middle Ages," in *A Source Book in Medieval Science*, ed. Edward Grant (Cambridge, Mass.: Harvard University Press, 1974), p. 313. Nicholas of Autrecourt is an exception insofar as he defended physical atomism, though his views are not fully understood.

3. For the date of Ockham's death as 1347 and not (as traditionally cited) 1349, see Gedeon Gàl, "William of Ockham Died Impenitent in April, 1347," *Franciscan Studies* 42 (1982): 90–95.

an argument for indivisibilism.⁴ Though he did not originate it,⁵ the sphere and plane example emerged as a standard topic in the medieval debate after it was discussed by the indivisibilist Henry of Harclay (1270–1317), who presents it in the following argument:

a sphere moved on a plane touches the plane at a point. Proof: because [between] a straight [line] and a circle, or a spherical body and a circular [body], there is nothing in common but a point, and contact [*tactus*] is always at something common. But the sphere is continuously moved on the plane. Therefore, it touches continuously, point after point, and through its motion describes a line. Therefore, point after point will be continuously in that line, and consequently, such a line is composed of points.⁶

We find a variation on Harclay's touch-at-a-point argument given somewhat later by the Franciscan indivisibilist, Walter Chatton (1285–1344):

God can make one thing truly plane in parts and another truly spherical in parts. Indeed, according to the Philosopher [*De caelo* 2.27.287a11–22],

4. Adam Wodeham, who is usually careful about naming his sources, refers to it only as “the common argument [*communis ratio*] concerning the touch of a plane by a spherical body.” Adam de Wodeham *Tractatus de indivisibilis* 2.3.3, ed. Rega Wood (Boston: Kluwer, 1988), p. 138, lines 26–27.

5. Rega Wood has indicated to me in correspondence that the probable first appearance of this common argument in the West is in Book 6 of an anonymous *Physics*-commentary preserved in Erfurt, Amplonian Q.312, at fols. 9^{va} and 10^{rb}. She believes that the commentary can be attributed to Richard Rufus of Cornwall. Richard, who knew Averröes's writings well, most likely found the argument in Averröes's commentary on *De caelo*. As Vassili Zoubov notes, in “Jean Buridan et les concepts du point au quatorzième siècle,” *Medieval and Renaissance Studies* 5 (1961): 61–62, the example is cited not only by Averröes (*De caelo* 1.32 [Venice, 1560] f. 27^r with reference to 1.4 [271a]), but also by Sextus Empiricus (*Adversus mathematicos* 3.27–28) and Plutarch (*De communibus notitiis* 40.7).

6. Henry of Harclay, as paraphrased in William of Alnwick's *Determinationes* 2, fol. 7^v (Latin text quoted in Wodeham *De indiv.* [Wood p. 290, n. 6]). For Alnwick as a reliable expositor of Harclay's views, see John Murdoch and Edward Synan, “Two Questions on the Continuum: Walter Chatton (?), O.F.M. and Adam Wodeham, O.F.M.,” *Franciscan Studies* 25 (1966): 212, n. 2. Harclay's argument is also paraphrased by Adam Wodeham in his *Tractatus de indivisibilibus* 1.2 (Wood p. 94), as well as in his *Quaestio de continuo*, ed. Murdoch and Synan in “Two Questions on the Continuum,” pp. 276–277, sect. 25. The latter may be an early draft of the former. Except for passages from Wodeham's *De indiv.* (which I quote from Rega Wood's edition and translation), all translations in this paper are my own.

the heavens are spherical, or there would be a vacuum in the recess of the indented or protruding parts. Since it happens that those two are together, the sphere touches the plane. I ask whether [they touch] at a number of parts, or only at one. [1] If at many, then it is not a sphere, because it will be molded to the plane in its parts; this I demonstrate [as follows]: those parts make a plane, or [else] some [of them] will penetrate the plane and there will be some parts protruding into the plane and others indented. [2] If only at one, I have what [the argument] proposed [to show]; for it follows formally: only one part, therefore not many, therefore indivisible by necessity.⁷

The gist of the argument seems clear: a sphere and plane must touch each other at *something*, but not at a divisible part, since any such part is divisible into further parts, and contact between more than one part of the sphere and plane would involve the compression and/or penetration of the one by the other; therefore, the sphere and plane must touch each other at a single, indivisible part, i.e., at a point.

The trouble with this argument, however, is that it offends against certain divisibilist principles also established by the Philosopher. At the beginning of *Physics* 6, Aristotle argues that continuous magnitudes cannot be composed of indivisible points because indivisibles have no *extremities* (i.e., first and last parts) by means of which they could be continuous (meaning that their extremities are one) or even in contact (meaning that their extremities are together).⁸ Furthermore, continua must be infinitely divisible, because if they were divisible into indivisibles, we would *per impossibile* have indivisibles in contact with each other, since the extremities of what is continuous must be in contact with each other.⁹

7. Wodeham *Quaestio de continuo* (Murdoch and Synan p. 249, sect. 68). Compare Chatton *Reportatio* 2.2.3.1,4 as in Paris, BN lat. 15887, fol. 93^{rb}–94^{va}. I have abandoned the editors' suggested emendation of *tenet* for *contingit* in the third sentence of this passage. There is a refinement which should be mentioned, but which need not detain us. Although both were indivisibilists, Chatton, unlike Harclay, argued that continua were composed of a finite number of indivisibles in consecutive contact. For the details, see John Murdoch, "Infinity and Continuity," in CHLMP pp. 571–578, and Rega Wood, "Introduction" to *De indiv.*, pp. 4–8.

8. See *Physics* 6.1 (231a21–b5).

9. *Physics* 6.1 (231b15–18). Although both Harclay and Chatton reject the first Aristotelian contact argument, only Chatton rejects the second. He does so by modifying Aristotle's definition of 'touch'. See second section, below, and Chatton *Reportatio* 2.2.3 (fol. 94^{vb}).

The indivisibilists who rejected Aristotle's contact argument did so for a variety of reasons: Harclay, to avoid the mathematical absurdity that would result if continua of unequal magnitude were composed of equally infinite parts; Chatton, as part of his defense of the theological doctrine of angelic motion;¹⁰ still others, because of difficulties they saw in Aristotle's own refutation of indivisibilism.¹¹ My aim here, however, is not to determine which theory, divisibilism or indivisibilism, is better suited to deal with various mathematical and theological constraints.¹² Rather, I am interested in ontological constraints, and specifically in the way in which one group of divisibilists tried to reply to the indivisibilist touch-at-a-point argument without multiplying entities, viz. mathematical atoms.

A point of clarification: although the ontological question raised by the fourteenth-century divisibilist-indivisibilist debate over the structure of continua seems clear enough, this is not to suggest that rejection of indivisible entities is what nicely separates the nominalist position from all others. Parsimony can be achieved in a number of ways, of course, so that it would not have been inconsistent for a participant in the debate to express traditional nominalist scruples about the existence of universals,¹³ while still embracing mathematical atoms on the grounds that we cannot do without them if we want to explain the structure of continuous magnitudes. Moreover, although indivisibilism

10. Aristotle goes on to argue in *Physics* 6.10 (241a6–14) that it is impossible for anything indivisible to be in motion, an argument with obvious applications to angels, which are indivisible beings. Duns Scotus, for example, offers a mathematical argument against indivisibilism in the context of a discussion of angelic motion in *Opus Oxoniense* 2.2.9.

11. For discussion of the various contexts in which medieval indivisibilism was defended, see Murdoch and Synan, "Two Questions on the Continuum," pp. 212–225; Murdoch, "Infinity and Continuity," pp. 575–577; and Wolfgang Breidert, *Das aristotelische Kontinuum in der Scholastik* (Munster: Aschendorff, 1970).

12. That task has already been embarked upon by others, and the story it reveals is in any case extremely complex. I direct the reader to John Murdoch's authoritative writings on this subject over the past quarter-century.

13. These scruples were often expressed in connection with Porphyry's first question about the nature of genera and species, "whether they subsist or are placed in bare [acts of] the understanding alone." See Boethius *In Isagogen Porphyrii* editio altera 1.10–11. Both Abelard and Ockham, for example, agree here that genera and species (1) exist in the understanding alone and (2) have no extramental significance except as conventional names. For discussion, see McCord Adams, *William Ockham*, pp. 3–12.

was certainly the minority view, there were indivisibilists, such as Harclay, who held that universal terms do not signify anything real outside the mind, and divisibilists, such as Walter Burley (ca. 1275–1345), who defended a moderate realist position on the nature of universals.¹⁴ What this means is that there is no reason why the divisibilist-indivisibilist debate could not have taken place as an in-house disagreement between one group of nominalists who thought it necessary to add indivisible entities to one's ontology for mathematical and/or theological reasons, and another group who rejected this. But at least by second quarter of the fourteenth century, it hardly ever did. Since most nominalists found it natural to identify with divisibilism,¹⁵ the ontological aspect of the debate was not usually separated from it in practice. Indeed, the popularity of the problem of the *existence* of indivisibles is distinctively medieval,¹⁶ and, as John Murdoch has suggested, may have been "to some extent a result of the kinds of questions about entities a particularist ontology urged one to ask."¹⁷

The three nominalist thinkers I shall be discussing—Ockham, Wodeham, and Buridan—all subscribed to a trio of doctrines characteristic, though (except for the first) by no means definitive, of fourteenth-century divisibilism:¹⁸

14. Although Harclay maintained that universal and particular terms are distinct only in reason, his view does represent, as McCord Adams has suggested, an "attempt to combine a nominalist ontology of singulars and concepts with a realist vocabulary" ("Universals in the Early Fourteenth Century," in CHLMP, p. 439). Accordingly, Harclay is best thought of as occupying a middle ground between moderate realists, such as Duns Scotus and Burley, and more radical nominalists, such as Ockham, who is unwilling to concede even that much. For discussion and references to the relevant texts, see McCord Adams, *William Ockham*.

15. The naturalness of the identification can be partly explained, no doubt, by the influence of Ockham's views on indivisibles. For discussion, see Murdoch, "Infinity and Continuity," pp. 574–575.

16. There is, for example, no direct treatment of the existence problem in Aristotle's *Physics*.

17. John Murdoch, "*Scientia mediantibus vocibus*: Metalinguistic Analysis in Late Medieval Natural Philosophy," in *Sprache und Erkenntnis im Mittelalter*, ed. Wolfgang Kluxen et al. (Berlin: Walter de Gruyter, 1981), p. 89, n. 43. Compare Murdoch, "William of Ockham and the Logic of Infinity and Continuity," in *Infinity and Continuity in Ancient and Medieval Thought*, ed. Norman Kretzmann (Ithaca: Cornell University Press, 1982), pp. 165–168 and 175–183.

18. For the variations on medieval divisibilism, see Murdoch, "Infinity and Continuity," pp. 571–584; Wood, *Adam de Wodeham*, pp. 10–15 (to whom the useful

- (1) *Divisibilism*: A continuum is not composed of atoms, but of parts divisible without end.
- (2) *Non-entitism*: Indivisibles do not exist in the physical world.
- (3) *Infinetism*: the composite parts of a continuum are infinitely divisible, or constitute a potentially infinite set.

The ontological question raised by the indivisibilist touch-at-a-point argument is especially worrisome for divisibilists who are non-entitists, of course, since it is not open to them to conceive of the point of contact between the sphere and plane as some kind of real limit. Despite their shared theoretical commitments, Ockham, Wodeham, and Buridan defuse the argument in surprisingly different ways. These differences cannot be explained, I think, solely by the various contexts in which the touch-at-a-point argument arose. Rather, as I hope to show, they demonstrate both the evolution and the increasing sophistication of explanatory methods used by fourteenth-century nominalist thinkers.

WILLIAM OF OCKHAM

William of Ockham discusses the indivisibilist sphere and plane example twice: once in the *Expositio Physicorum*, and once in the *Quodlibeta septem*.¹⁹ The first and more physicalistic context is in Book VI of the *Expositio*, a work directed in large part against

classificatory term 'non-entitism' is due). Duns Scotus, for example, rejected non-entitism, but is still classified as a divisibilist because he maintained that continua are not *composed* of indivisible entities. Scotus's arguments against non-entitism are cited verbatim and then attacked by Ockham in *Tractatus de quantitate* 1, in *Opera Theologica* 10, ed. C. A. Grassi (St. Bonaventure, N.Y.: Franciscan Institute 1986), pp. 26–45. The non-entitist form of divisibilism defended by Ockham, Wodeham, and Buridan was actually less common than the orthodox Aristotelian variety (whose defenders included Aquinas, Duns Scotus, and Giles of Rome), according to which indivisibles are to be understood as real limits, though not as constituent parts, of continua. In fact, the only non-entitist prior to Ockham seems to have been Peter John Olivi, for whom see Wood, *Adam de Wodeham*, p. 25, n. 44. The definition of divisibilism given here is that of Thomas Bradwardine (ca. 1295–1349), *Tractatus de continuo* (quoted in Murdoch, "Infinity and Continuity," n. 36): "continuum non componi ex atomis, sed ex partibus divisibilibus sine fine."

19. The argument is not mentioned in Ockham's other discussions of the structure of continua, namely in the *Summa logicae* and *Tractatus de quantitate*. The latter is his most comprehensive theological treatment of the continuum problem.

Giles of Rome, a divisibilist who conceived of indivisibles as *real limits* based on the assumption that quantity is a *res absoluta* distinct from substance and quality.²⁰ In Chapter 14, Ockham replies to Giles's view that, contrary to Aristotle, it is possible for indivisibles to be moved.²¹ Ockham notes that some have tried to refute this argument by proving that continua are composed of indivisibles, in connection with which he cites the following disjunctive argument for indivisibilism:

it is supposed that a completely spherical body touches an absolutely plane body. To which, I ask whether it touches at something divisible, or at something indivisible. The first cannot be given, because at whichever divisible you choose, there will be a curve, and consequently the whole [divisible] will not fit the plane, but there will be an intermediate body [*corpus medium*] between some part of the curve and that plane. If the second is given, we have what the argument proposes to show.²²

In other words, if we imagine a perfect sphere descending onto a perfectly plane surface beneath it, they must first touch at an indivisible point, because (ruling out compression or penetration) divisible

20. Giles was thus a divisibilist who rejected non-entitism. For discussion of the influence of Giles's views on Ockham in the *Expositio Physicorum*, see Ernest Moody, "Ockham and Aegidius of Rome," *Franciscan Studies* 9 (1949): 417–442. In contrast, Ockham denies that quantity is an absolute thing, distinct from substance or quality, and likewise rejects the notion that Aristotle meant to posit limits of continua really distinct from continua themselves. See Ockham *Expos. Phys.* 5.5.7, in *Opera Philosophica* 5, ed. Wood et al. (Bonaventure, N.Y.: Franciscan Institute, 1985), p. 382, lines 33–34. For discussion of Ockham's views here, see McCord Adams, *William Ockham*, pp. 201–213; Murdoch, "Infinity and Continuity," pp. 573–575; Murdoch, "Logic of Infinity"; and Eleonore Stump, "Theology and Physics in *De sacramento altaris*: Ockham's Theory of Indivisibles," in *Infinity and Continuity*, pp. 207–230, which takes issue with the interpretation offered in Murdoch, "Logic of Infinity."

21. Giles of Rome *Commentaria in octo libros Physicorum Aristotelis* 6.18 (Venice, 1502; rptd. Frankfurt a. M.: Minerva, 1968), fol. 160^{rb}. Aristotle had argued in *Physics* 6.10 (241a6–14) that there can be no motion of a point or any other indivisible because before anything moving can traverse a space greater than itself, it must first traverse a space less than or equal to itself; but since there can be no space less than an unextended indivisible, the notion of a *moving* indivisible is incoherent. It is perhaps worth noting that Ockham, Wodeham, and Buridan were not about to deny the existence of *immaterial* indivisibles (e.g., angels or human intellectual souls) or, for that matter, the possibility of their motion. The touch-at-a-point argument, of course, concerns the necessity of positing indivisibles to explain the structure of continuous *spatial* magnitudes.

22. Ockham *Expos. Phys.* 6.14.4 (Wood et al. 583.63–68).

curved parts do not “fit” divisible plane parts. Except for their single point of contact, there will always be space between them, and more space as one moves along the plane surface in any direction away from that point.

Ockham’s reply to this argument is quick and direct. He denies that any two absolutely spherical and absolutely plane bodies can be said to touch, if by that we mean that there is no intermediate body between them. This is for two reasons. First, the sphere and plane cannot touch each other as a whole, since both are divisible entities composed of parts *more* immediate to their place of contact. Second, they cannot touch each other at some part, because any first touching parts of the sphere and plane you choose will be further divisible into smaller parts that touch each other even more immediately. Thus, if we call the first touching parts of the sphere and plane *A* and *B*, respectively, Ockham says that it is “manifestly false” to suppose that there is nothing intermediate between any part of *A* and *B*. He argues for this as follows:

each would be divided into three equal parts, viz. *A* into *C*, *D*, and *E*, and *B* into *F*, *G*, and *H*. It is obvious that between *C* and *F* there is an intermediate body; for otherwise, they would be both curves or both planes. Therefore, *A* and *B* do not first touch each other. And so it can be proved of any parts that they do not first touch each other. . . .²³

Furthermore, to the counter-argument which supposes that a hard spherical body must immediately touch a soft plane bodily yielding easily to it (imagine a ball bearing dropped into a bowl of jello), Ockham replies that they would still not touch immediately, since there must always be an intermediate body between any sphere and plane parts you choose at the place of penetration.²⁴

Ockham’s reply here is interesting, to say the least. But one might argue that he arrives too quickly at what is, to be sure, a counterintuitive conclusion, namely, that “one must say, following Aristotle, that a purely spherical body cannot touch a purely plane body.”²⁵ For it seems a kind of philosophical overkill to reply to the indivisibilist touch-at-a-point argument by saying that the sphere and plane do not

23. Ockham *Expos. Phys.* 6.14.4 (Wood et al. 583.83–584.87).

24. Ockham *Expos. Phys.* 6.14.4 (Wood et al. 584.101–115).

25. Ockham *Expos. Phys.* 6.14.4 (Wood et al. 584.98–100).

touch each other at a point because they do not touch each other *at all*. Ockham is, of course, willing to say that the sphere and plane touch each other, if by that we mean that they touch “mediately,” or in such a way that there are always other extended bodies between them. He apparently does not think it necessary to take some of the counterintuitive edge off the divisibilist solution by further exploring in his reply the notion of mediate contact.²⁶ This is doubly unfortunate in view of Ockham’s non-entitism, since a more precise definition of contact would surely help to explain the truth conditions of sentences such as ‘The spherical and plane bodies are touching’. Unless predicates of contact can be assigned an interpretation that is both plausible and consistent with the assumption that indivisibles do not exist in the physical world, the indivisibilist query remains unanswered. If it is still true to say that continuous, divisible bodies touch at something, why not posit something, namely, indivisible points, at which they touch?

Ockham takes a more decided step towards addressing this issue in his reply to the touch-at-a-point argument in the *Quodlibeta septem*, a work composed after the *Expositio Physicorum*. Here his opponent

26. He does provide at least the beginnings of an account elsewhere in the *Expositio physicorum*, when he modifies Aristotle’s definition of contact (“Things are said to be in contact when their limits are together,” *Phys.* 5.3 [226b23]) to make it more amenable to his non-entitist brand of divisibilism. The reason seems clear. If by ‘together’ [rendered into Latin as *simul*] Aristotle means that their limits are in the same place, then the two things must be continuous. No two distinct things could touch each other in that sense without ceasing to be distinct. If two distinct things are to be in contact, then, their limits cannot literally be immediate, but only mediate. See Ockham *Expos. Phys.* 5.5.2 (Wood et al. 377.98–100 and 378.43–45). The conditional reflects Ockham’s view that although Aristotle sometimes speaks as if points are things distinct from bodies, he does not mean this literally, as implying the existence of indivisible entities. See Ockham *Expos. Phys.* 5.5.2 (Wood et al. 377.38–378.42). Such remarks are rather to be treated as *conditional propositions*. This is also the interpretation that Ockham suggests for mathematical propositions that mention indivisible points. Such points, he says, exist only in the imagination of the mathematicians. See Ockham *Expos. Phys.* 5.5.7, 5.7.1, 6.1.2 (Wood et al. 382.25–383.61, 402.28–31, 461.304–462.323). For discussion, see Murdoch, “Logic of Infinity,” pp. 175–179. As we shall see below, Wodeham and Buridan also make use of the notions of mediate contact and entities indivisible only *secundum imaginationem*, respectively, but (unlike Ockham) they do so specifically in the course of their own non-entitist replies to the touch-at-a-point argument.

is definitely Chatton, and his target is Chatton's version of the argument, which as we saw above concerns the hypothetical case of God placing completely spherical and completely plane bodies in contact with each other. To this argument, Ockham objects:

it is impossible and includes a contradiction [to say] that the sphere touches the plane, because if it does, since [it does] not [touch] at something indivisible, it must touch at a divisible part. And for any part of the spherical thing you choose, because it is part of something spherical, by necessity one part of it is ascending and another descending. And so by necessity there is some intermediate body [between them], say, the air, if it touches in the air.²⁷

Again, the curved surface of the spherical body will prevent it from being in contact with a plane at any of its *divisible* parts.

Yet Ockham also recognizes something in the *Quodlibeta septem* discussion that is present, but not made explicit, in his earlier treatment of the sphere and plane example: his conclusion that the sphere and plane would not touch is based on the *indivibilist* assumption that contact must be immediate. "Otherwise," he says, "it can be said (and perhaps better) that a spherical body touches a plane at some divisible part of it."²⁸ That the latter is Ockham's preferred definition of contact is evident in the next paragraph, where there is a reply to the objection that the divisible part of the sphere actually touching the plane would not itself count as a spherical body, presumably because it would lack a curved surface. Ockham says that this follows only if we assume that

. . . some first part is touching as a whole, such that each part of that part touches the plane, since then the argument would conclude by necessity that it would not be completely spherical.²⁹

It would follow, in other words, that no spherical part would touch any plane part if by 'touch' we mean contact between each and every divisible part of those parts. A glance at the curved surface

27. William of Ockham *Quodlibeta septem* 1.9 in *Opera Theologica* 9, ed. Joseph C. Wey (St. Bonaventure: Franciscan Institute, 1980), pp. 58–59, lines 200–205.

28. Ockham *Quodl.* 1.9 (Wey 59.205–207). For immediacy of contact as a characteristic thesis of indivibilism, see Wood, *Adam de Wodeham*, pp. 3–10.

29. Ockham *Quodl.* 1.9 (Wey 59.210–212).

of the sphere and the flat surface of the plane should be sufficient to confirm that.

Unable to talk about immediate contact between divisible parts, Ockham tries another strategy:

I now posit that [the sphere] does *not* touch by means of any first part of which each part touches the plane. Therefore, it does not touch by means of any first [part] that is prior to all other touching [parts]; but for any touching part you choose, still one half does not touch immediately, nor half of that half, and so on to infinity.³⁰

Ockham's view is that we can say that the sphere touches the plane as long as we do not mean that it touches immediately, or at any first part. His reasoning here reprises the argument from the *Expositio Physicorum*: any two parts of the sphere and plane taken to be immediate would be infinitely divisible into parts even more immediate, e.g., into halves, quarters, eighths, and so on.

Although Ockham considered these replies sufficient to refute the touch-at-a-point arguments offered by Giles and Chatton,³¹ his divisibilist and non-entitist successors do not seem to have regarded the issue as settled. Both Wodeham and Buridan take the indivisibilist sphere and plane example quite seriously, and, rather than merely repeating Ockham's arguments from the *Expositio Physicorum* and *Quodlibeta septem*, fashion their own *positive* accounts of how the sphere and plane may be said to touch. These seem intended to supplement Ockham's much briefer account in two ways: first, by precisely defining the divisibilist concept of mediate contact in the context of the touch-at-a-point argument; second, by turning the notion of the infinite divisibility of touching parts from a rough illustration into a quasi-mathematical procedure, thereby adding formal rigor to the non-entitist reply.

ADAM WODEHAM

Adam Wodeham focuses much more attention than Ockham on the indivisibilist sphere and plane example, quoting

30. Ockham *Quodl.* 1.9 (Wey 59.212–217), emphasis added.

31. See notes 22 and 7 above, respectively.

directly from both the Harclay and Chatton versions of the touch-at-a-point argument in his main discussion of the indivisibilist controversy, the *Tractatus de indivisibilibus*.³² Still, he prefers to call it a “common argument [*communis ratio*]” for the view that a point is an absolutely indivisible entity, a fact suggesting that, by the time Wodeham was writing, it had ceased to be associated with any particular indivisibilist thinker.³³ Indeed, Wodeham observes that the question of whether the sphere would touch the plane or not “is a great point of dispute between [adherents of different] schools [of thought], and they make the difficulty emerge nicely for each side.”³⁴

Wodeham argues, following Ockham, that either (1) it is not possible that the sphere touch the plane, or (2) if it does touch the plane, it touches it “at something infinitely divisible.”³⁵ The second alternative, of course, involves the divisibilist concept of mediate contact. But before considering that, Wodeham offers an argument in defense of the first alternative, namely, that there can be no contact at an indivisible point.

Unlike anything in Ockham, this argument seems designed to confront Chatton’s touch-at-a-point argument head on, even down to the way it sets up the sphere and plane example as a thought experiment about divinely produced ideal bodies. Wodeham asks us to imagine God placing a sphere at some distance above a plane in a medium of air, then causing the sphere to descend until it is prevented from descending further by the surface of the plane, but

32. See Harclay’s version in Wodeham *De indiv.* 1.2.4 (Wood); and Chatton’s in Wodeham *De indiv.* 1.2.16 (Wood 94.18–24, 100.14–17). The Chatton version quoted by Wodeham is not exactly the same as the version cited in note 7 above. It involves, like the Harclay version, a sphere being moved across a plane surface. For the source of the latter, see Chatton, *Reportatio* 2.2.3 (fol. 94^{va}); of the former, note 6 above. Both versions of the indivisibilist argument are likewise reproduced by Wodeham in his *Quaestio de continuo* (Murdoch and Synan, “Two Questions on the Continuum,” pp. 276, sect. 24; 280, sect. 34). Wodeham also considers the question of indivisibles and the composition of continua in his *Lectura secunda* 24.1–2, ed. Rega Wood and Gedeon Gál (St. Bonaventure: Franciscan Institute, 1990) 3:321–411, although the sphere and plane example is not discussed there.

33. Wodeham *De indiv.* 2.2.3 (Wood 138.26). Wood argues that it was composed between 1323 and 1331, probably closer to the earlier date (pp. 15–16).

34. Wodeham *De indiv.* 2.3.4 (Wood 146.7–8).

35. Wodeham *De indiv.* 2.3.3 (Wood 146.5).

without either body suffering compression or penetration. "This once accomplished," Wodeham argues, "the air interposed would still be continuous, although not everywhere uniformly or equally thick, but always thinner or more and more tenuous, as we approach the place of contact."³⁶ To demonstrate this conclusion, Wodeham embellishes his thought experiment as follows:

suppose that God annihilates that spherical body and the plane joined to it in the manner described above, without effecting any change of place in regard to the air, one part of which previously surrounded the spherical body and the other part of which touched the plane surface—[namely], the solid plane body previously withdrawn [below] the spherical body. Once this is done, I ask: will we find that that air is continuous or not? [1] [If it is continuous, then] this is what we proposed to show. [2] [If it is not continuous], then there will be a hole, either a divisible or indivisible hole. [2.1] [If it is divisible], then either [2.1a] there was a vacuum there before, which appears incongruous; or [2.1b] the plane was immediately touched by the sphere divisibly in a straight line, which is contrary to the nature of sphericity and of a straight line. [2.2] If [the hole is] indivisible, then it could be filled by an indivisible; or at least there would be an indivisible vacuous space, where before there had stood an indivisible belonging to the spherical body joined to the plane. And the opposite of this was proven above.³⁷

The problem with the indivisibilist option [2.2] is that since indivisibles cannot together make something continuous,³⁸ indivisible points cannot be part of any continuum, such as the sphere; hence, their annihilation would make no difference to the way in which the sphere touches the plane.³⁹

But even if the continuity of the air, in this example, shows that the sphere and plane cannot touch in the way Harclay and Chatton want them to touch, Wodeham still hopes to make sense of our intuition that they must touch *somehow*. Here he has a twofold

36. Wodeham *De indiv.* 2.3.6 (Wood 146.24–27).

37. Wodeham *De indiv.* 2.3.7 (Wood 146.32–35 and 148.1–10).

38. Wodeham *De indiv.* 2.1.7–8 (Wood 124.10–27). Wodeham refers to a preceding argument at *De indiv.* 1.3.1 (Wood 102.1–13), as well as quoting from Ockham's argument that an indivisible point cannot be posited as part of something existing *per se*. For the latter, see Ockham *Expos. Phys.* 6.1.2 (Wood et al. 454.57–65).

39. Wodeham *De indiv.* 2.3.3 (Wood pp. 144–146).

strategy. First, tempered by (of all things) Chatton's indivisibilism, he takes Ockham's notion of mediate touching as contact at some divisible part, but not at any first such part, and he refines it into a new account of touching which further modifies the Aristotelian notion of contact so that he can talk about divisibles being immediate to each other. Second, he introduces the quasi-mathematical procedure he calls "proportional division *ad infinitum*" to illustrate how the sphere and plane could be said to touch by *means* of a divisible, but in the *manner* of an indivisible. Wodeham begins by noting that in his argument against indivisibilism, it also follows that the sphere and plane do not touch at anything *divisible*. For the reason, he refers the reader first to Ockham's *Expositio Physicorum* argument that between any two "first" touching parts of divisible bodies, there will always be a *corpus medium*, or intermediate body.⁴⁰ For Wodeham, however, the real source of the problem is what he takes to be Aristotle's definition of contiguous contact. The sphere and plane do not touch each other, he says, if

we understand by 'touch each other' that their limits are together and in the same primary place according to the description of contiguous things laid down by Aristotle in *Physics* 5 and repeated in book 6.⁴¹

According to Aristotle, however, things are contiguous if they are (1) in succession (nothing of their own kind is intermediate between them), and (2) in contact, or touching (their extremities are together),⁴² suggesting that Wodeham has run together Aristotle's definition of contact (things are touching if their extremities are together) with his definition of continuity (things are continuous if their extremities are one).⁴³ No divisibles can be contiguous on this understanding of Aristotle because divisibles have spatially distinct parts, and it is not possible for two things having spatially distinct

40. See notes 23–24 above.

41. Wodeham *De indiv.* 2.3.11 (Wood 150.6–9).

42. Aristotle. *Phys.* 5.3 (227a9).

43. See Aristotle. *Phys.* 5.3 (226b21–227a9); 6.1 (231a21–29). Compare Wodeham *De indiv.* 2.3.11 (Wood 151.13–14): "no such [sphere and plane] limits are together in that fashion." The source of Wodeham's (perhaps deliberate) confusion here might well have been Ockham, who likewise found it necessary to reinterpret the Aristotelian notion of contact so that it would apply to non-continuous divisible bodies.

parts to occupy “the same primary place.” Nevertheless, Wodeham uses the occasion to offer his own, alternative definition of ‘touch each other’. This definition is interesting because it looks very much to have been inspired by his indivisibilist arch-rival, Chatton, who had tried to refute Aristotle’s contact argument with the novel assumption that continua can be composed of indivisibles not in the sense that they are in the same place (which is, after all, why Aristotle found indivisibilism to be absurd), but in the sense that they are next to each other such that whole touches whole without there being anything else in between.⁴⁴ With Chatton, Wodeham proposes that “things ‘touching each other’ or ‘contiguous’ are those whose limits are together, or [whose limits] are immediate [to each other]” by what he terms a “simultaneity or positive immediacy [*simultate seu immediatione positiva*].”⁴⁵ The sphere and plane would touch each other in this sense, he says, since each extends to the other without stopping short of, or extending beyond, the other.⁴⁶ But what, exactly, is the significance of them touching with “simultaneity or positive immediacy”?

Wodeham attempts to answer this question in a further step, illustrating the notion of contact between positively immediate limits by means of a procedure he calls “proportional division *ad infinitum*.” Although neither the sphere nor any part of it touches the plane “primarily and exactly [*primo et adaequate*],” he says, it does touch by itself and in its parts, viz. “by any part of it extending to and reaching the plane.”⁴⁷ Thus, the sphere and plane can be said to touch each other immediately if the sphere and each part of it is treated as a kind of *macro-indivisible*, extended towards the plane until

44. Chatton *Reportatio* 2.2.3 (fol. 94^{vb}): “. . . placet mihi quod totum tangat totum, id est quod nihil est medium inter ea.” Wodeham, who was intimately familiar with Chatton’s writings, quotes directly from this passage at *De indiv.* 1.1.24 (Wood 48.5–8). The source of Chatton’s alternative definition of contact (though not, of course, its application to indivisibles) could well have been Ockham. See Ockham *Expos. Phys.* 5.5.2 (Wood et al. 377.98–100, 378.43–45).

45. Wodeham *De indiv.* 2.3.12 (Wood 150.17–18). Chatton, of course, characterizes such contact negatively, namely as involving “nothing else in between” things that are touching, rather than positively, as is suggested by the notion of positive immediacy.

46. Wodeham *De indiv.* 2.3.12 (Wood 150.18–20).

47. Wodeham *De indiv.* 2.3.14 (Wood 150.34 to 152.1).

it can go no further without compression or penetration.⁴⁸ And there are infinitely many such positively immediate limits:

For example, [a sphere would touch a plane] by means of its [lower] half, constructed transversely; and by means of a half of that same [half] constructed in parallel—[that is], the lower half similarly reaching the plane, and so on *ad infinitum*, as can be proven by argument and also using the examples introduced above here.⁴⁹

We might illustrate Wodeham's procedure as follows: a perfect sphere and the plane on which it rests would, ruling out compression or penetration, touch each other immediately, since the sphere would be extended towards its place of contact with the plane. But if we were to divide the sphere by slicing it horizontally through its middle, thus removing its top half, no change would be effected in the way the remaining half-sphere touches the plane, and so it, too, would touch the plane immediately. But then we can use the same procedure to produce a quarter-sphere having the same manner of contact, and then an eighth-sphere, and so on *ad infinitum*. And furthermore, adds Wodeham, we can say the same thing "analogously regarding the parts of the plane touched by the sphere."⁵⁰ Wodeham's non-entitist reply to the indivisibilist touch-at-a-point argument is that although we must say, following Ockham,⁵¹ that the sphere always touches the plane either as a divisible whole or at some divisible part (and there are infinitely many such divisible parts in contact with the plane, as

48. I owe the term 'macro-indivisible' to Norman Kretzmann, "Adam Wodeham's Anti-Aristotelian Anti-Atomism," *History of Philosophy Quarterly* 1 (1984): 388.

49. Wodeham *De indiv.* 2.3.14 (Wood 152.1–5).

50. Wodeham *De indiv.* 2.3.15 (Wood 152.12–13). Wodeham contends that proportional division *ad infinitum* can likewise be applied to the diameter of the sphere perpendicular to the plane. See the passage at *De indiv.* 2.3.16 (Wood 152.14–22). Wodeham's solution here appears to involve the mathematical notion of asymptotic division to a limit. His assumption (via his definition of mediate contact) would be that angles of tangency have a finite minimum limit, whereas one would expect an indivisibilist to argue that such angles are of infinitesimal magnitude. For a discussion of curvilinear angles and their relation to the continuum problem, see Murdoch, "Infinity and Continuity," pp. 580–582.

51. Wodeham mentions Ockham by name in this context at *De indiv.* 2.3.18 (Wood 154.6–7). He appears to have in mind Ockham's handling of the indivisibilist touch-at-a-point argument in *Expositio Physicorum* 6.

the method of proportional division demonstrates) it does so *in the manner of an indivisible*.⁵²

What Wodeham has done here, I believe, is to combine, rather ingeniously, Ockham's thoroughly divisibilist and non-entitist account of continua with a definition of contact inspired by Chatton's explanation of how continua can be composed of indivisibles (*pace* Aristotle), in order to suggest a way in which *divisibles* can be said to touch each other positively and immediately.⁵³ It is as if Wodeham were saying, "I know that no two absolutely spherical and absolutely plane bodies can be said to touch each other if by that we mean that there is no intermediate body between them. After all, each is divisible into infinitely many parts that touch each other with greater and greater immediacy. But if we think of them just as wholes, as Chatton conceives of indivisible points existing next to each other in a continuum, then they can touch each other immediately in the positive sense that each is extended towards the other as far as it can go, without compression or penetration." What Wodeham borrows from Chatton is a way of thinking about the composition of continua which he then applies to divisibles. Though *de facto* divisible, continua (or their parts produced by proportional division) are to be thought of as indivisible wholes, a move which enables the non-entitist to talk about immediate contact between continuous bodies while shielding his claims from being reduced to absurdity by Ockham's argument.

But non-entitist and indivisibilist alike may object that it is wrong to speak of the sphere and plane touching each other immediately, especially in view of Wodeham's earlier concession that there will always be a continuous body of air between them. Chatton in particular might want to stress that, on his definition of contact, there is nothing between the immediately touching indivisible points of which continua are composed. But Wodeham could handle such objections

52. Wodeham *De indiv.* 2.3.18 (Wood 152.32–34).

53. Again, Wodeham not only knew Chatton's definition of contact, but quotes from it directly near the beginning of the *De indiv.* See note 44 above. In a note on Wodeham's second definition of contact, Wood assumes (correctly, in my view) that "'positive immediacy' means that the immediate things are together, as opposed to there being nothing between them." She does not, however, make the further suggestion (which I am making here) that Wodeham got the idea from Chatton.

with a neat *distinguo*. First, he would say, if you are talking about the sphere and plane as divisible entities, then of course they will touch each other only mediately because any sphere and plane parts you choose will have more immediate parts, to say nothing of the air, between them. But if, on the other hand, you are talking about the sphere and plane (or of any of their parts produced by proportional division) as wholes, then the divisibilist *reductio* argument no longer applies, and we must instead define immediacy in terms of their being so close to each other that, if they were any closer, their sphericity and/or planeness would be compromised by compression and/or penetration. That this latter, immediate sense of contact is compatible with the former, mediate sense is something Wodeham concedes in the final section of his discussion of the sphere and plane when he remarks, “and nevertheless, as was made clear [i.e., in the refutation of the indivisibilist touch-at-a-point argument], something mediates, or could mediate, between any part touching the plane in this fashion and the plane, when there is such contact.”⁵⁴ That is, the sphere and plane can still touch each other immediately as wholes, even though there is air between them.

The influence of this novel strategy for defusing the sphere and plane example is evident in the writings of Wodeham’s somewhat younger Parisian contemporary, John Buridan. Buridan’s writings suggest that he adopted not only Wodeham’s indivisibilist-inspired definition of immediate contact, but also the method of proportional division *ad infinitum* as an illustration of how the parts of divisible bodies can be said to touch each other. Yet Buridan did not embrace Wodeham’s reply to the indivisibilist argument without first augmenting and refining it. It is to that final part of our story that I now turn.

JOHN BURIDAN

Buridan is more interested than either Ockham or Wodeham in exploring the logico-semantic underpinnings of the debate over the structure of continuous magnitudes. He sees the indivisibilist touch-at-a-point argument primarily as presenting the problem

54. Wodeham *De indiv.* 2.3.19 (Wood 154.16–18).

of how a non-entitist should understand the terms occurring in it, i.e., ‘point’ and ‘touch’, rather than as raising questions about the mode of contact between ideal spheres and planes. The logical and mathematical aspects of the problem are related, of course, but the difference we see in Buridan is one of emphasis. “If only we could be clear about the signification of our terms,” he seems to be saying, “such mathematical questions about modes of contact will answer themselves.”

Buridan’s approach is best exemplified in Book 6 of his *Questions on Aristotle’s Physics*, where the sphere and plane example is mentioned in an argument on the affirmative side of Question 4, which asks whether points are indivisible things [*res indivisibiles*] in a line.⁵⁵ After presenting and defending his own divisibilist, non-entitist, and infinitist views on the question, Buridan proceeds to reply to the arguments on the opposing side. To the touch-at-a-point argument, he replies as follows:

As for the sphere placed on the plane, we say that the whole sphere touches the whole plane, taking ‘whole’ categorematically. But it is not the case that the whole sphere, or some whole part of the sphere, touches the plane, taking ‘whole’ syncategorematically. Indeed, no part of the sphere taken syncategorematically touches the plane, except the last [part] next to that plane. And we wish to signify these concepts [*intentiones*] when we say that it touches at a point.⁵⁶

So would a sphere placed on a plane surface touch it at a point? Buridan is willing to say, following Wodeham, that they touch each other as wholes, but only if the term ‘whole’ is understood in its categorematic sense, which he elsewhere says should be expounded as ‘having parts’.⁵⁷ In this way, the proposition ‘The whole sphere

55. The argument Buridan presents here looks like a minimalist version of the Harclay/Chatton touch-at-a-point argument. See Buridan *Quaestiones super octo physicorum libros Aristotelis* 6.4, in *Kommentar zur Aristotelischen Physik* (Frankfurt a. M.: Minerva, 1964), fol. 96rb. Buridan’s clipped rendering of the argument both here and in his other writings suggests that it had perhaps acquired in mid-fourteenth-century Paris the same status Wodeham had earlier ascribed to it in England, namely that of a “common argument” (*communis ratio*) for indivisibilism.

56. Buridan Q. in *Phys.* 6.4 (fol. 97^{vb}).

57. Buridan *Quaestiones in De anima* 2.7, ed. Peter Gordon Sobol, in “John Buridan on the Soul and Sensation: An Edition of Book II of His Commentary on Aristotle’s

touches the whole plane' is true if sphere and plane are both seen as wholes *having parts*, namely, as *divisible* wholes. But if 'whole' is understood in its primary syncategorematic sense,⁵⁸ where it is expounded as 'each part', it will distribute the predicate 'touches the whole plane' over each and every integral part of its subject. The proposition will thus be false, because only the last part of the sphere immediately next to the whole plane would touch it in that sense. Furthermore, since Buridan maintains that continua are infinitely divisible, nothing answers to the description, 'last part of the sphere', if by that we mean 'whole last part' in the latter, syncategorematic sense. Buridan explains this consequence in an argument reminiscent of Ockham's proof that infinitely divisible spheres and planes cannot have any first touching parts:

no whole part of any continuum is its limit [*terminus*], and I am [here] taking the name 'whole' syncategorematically. This thesis is obvious because no whole part is the first or last. For which reason, assume the opposite, viz. that some whole part of any continuum is its first or last part. It follows that each part of that part will be the first or last, and that it will be the limit of that continuum. And this is false, because if the part which is posited first were divided into A and B, it is certain that A will be before B, and so B will not be the first part.⁵⁹

Buridan therefore sees the problem of the sphere touching the plane in terms of the logical distinction between categorematic and syncategorematic words. We can talk about whole continuous entities in contact with each other as long as 'whole' is taken in its categorematic, divisibilist sense of 'having parts'. But we cannot do so if we assume, following the indivisibilists, that there is some whole indivisible part

Book of the Soul, with an Introduction and a Translation of Question 18 on Sensible Species," doctoral dissertation, Indiana University, 1984, pp. 103–104; *Tractatus de suppositionibus* 3.7, ed. Maria Elena Reina in *Rivista critica di storia della filosofia* 12 (1957): 326, lines 482–483.

58. In its primary syncategorematic sense, 'whole' effects a distribution over the integral parts of its subject. See (for Buridan's discussion of this sense) *Q. in De anima* 2.7 (Sobol pp. 102–105); *Tractatus de suppositionibus* 3.7 (Reina 326.490–495). For general discussion, see Norman Kretzmann, "Syncategoremata, Exponibilia, Sophismata," in CHLMP, pp. 230–240.

59. Buridan *Q. in Phys.* 6.4 (fol. 97^{ra}). This is the seventh thesis (*conclusio*) defended by Buridan in the main part of the question.

of the sphere in the syncategorematic sense that each part of it is in contact with the plane. This is because indivisibles by definition have no parts.

What is the point of such precision? In a comment just prior to his discussion of the sphere and plane example, Buridan reveals that his aim is to underwrite certain figurative modes of discourse. The logico-semantic problem here is that even if the last part of a continuum is called a point in the divisibilist, categorematic sense that some whole part of it, i.e., some part of it having parts, is its last part, “a point is commonly said by everyone to be indivisible.”⁶⁰ To this Buridan replies that a point is called an indivisible “not because it is so, or because it is literally true [*quia sit ita, vel quia sit verum de virtute sermonis*]” that a point is indivisible,⁶¹ but because it is treated as such in conventional usage. He gives several examples here, the first and foremost of which has to do with the practice of mathematics. Although points are not strictly speaking indivisible,

in one way, this is said in keeping with the imagination of mathematicians [*secundum imaginationem mathematicorum*], as if there were an indivisible point, not because they must believe that there really is, but because they revert to those assumptions in measuring, just as if it were so. For if an indivisible point is limiting a line, it is agreed that that whole line would be exclusively beneath it, and likewise, the whole [line] itself is exclusively beneath its last part.⁶²

Likewise, says Buridan, we observe that in commerce, a cloth merchant measures lengths of cloth from an imaginary first point.⁶³ In more philosophical contexts, we see that a point is sometimes called

60. Buridan *Q. in Phys.* 6.4 (fol. 97^{rb}).

61. Buridan *Q. in Phys.* 6.4 (fol. 97^{rb}).

62. Buridan *Q. in Phys.* 6.4 (fol. 97^{rb-va}). Compare Buridan *Quaestiones super libros quatuor De caelo et mundo* 1.22, ed. Ernest A. Moody (Cambridge, Mass.: Medieval Academy of America, 1942), pp. 105, 112–15; and Ockham *Expos. Phys.* 5.5.7 and 6.1.2 (Wood et al. 383.48–49, 462.320–323), discussed in note 26 above.

63. Cloth merchants aren’t worried about the structure of continua, of course, but (and this is Buridan’s point) the practice of measuring an ell of cloth clearly assumes the existence of indivisible first and last points. If such points are treated as infinitely divisible, how could they give rise to determinate and non-arbitrary measurements? For analogous remarks regarding the utility of other concepts of measurement, e.g., length, width, and depth, none of which Buridan supposes to be really distinct from quantity, see *Q. in De caelo* 1.2–3 (Moody pp. 10–16, esp. 15.29–33).

the indivisible limit of a line in the sense that “it is not divisible into parts of which each part is the limit of the line.” In other words, ‘point’ can refer to some whole last part of a line in the categorematic but not the syncategorematic sense of ‘whole’.⁶⁴ Alternatively, the first or last part of a line is sometimes treated as a single thing distinct from every other part, in which case it would acquire the qualitative or formal indivisibility Aristotle ascribes in *Metaphysics X* to that which is one.⁶⁵ Like the cloth-merchants, mathematicians and philosophers sometimes ply their trade on the assumption that points are indivisible.

Buridan’s sensitivity to the logico-semantic underpinnings of mathematical language enables him to appreciate an aspect of the sphere and plane example missed by both Ockham and Wodeham. This emerges in Buridan’s reply to the touch-at-a-point argument in his “*Quaestio de puncto*,” an independent treatise on the continuum problem.⁶⁶ He begins by citing Averröes’s comment that although natural bodies can touch only at a divisible part, a geometrically conceived sphere and plane surface would touch at a point.⁶⁷ Accordingly, Buridan concedes that there is indeed a sense in which continua must be assumed to be in contact at a point.

64. Buridan *Q. in Phys.* 6.4 (fol. 97^{rb}).

65. Buridan *Q. in Phys.* 6.4 (fol. 97^{va}). See Aristotle *Metaph.* 10.3 (1054a20–29).

66. The *Quaestio de puncto* asks essentially the same question as *Q. in Phys.* 6.4, namely “whether a point is some indivisible thing added to a line or body.” See Buridan *Quaestio de puncto*, ed. V. Zoubov in “Jean Buridan et les concepts du point au quatorzième siècle,” *Mediaeval and Renaissance Studies* 5 (1961): 63, line 3. Though there are similarities between the two discussions, the actual texts differ in both their structure and argument. The editor of the *De puncto* prefers to describe them as “complementary” (Zoubov p. 46). Unlike *Q. in Phys.* 6.4, the *De puncto* is directed against a certain “doctor venerabilis” who is not further identified. The author of the table of contents of the volume in which one of the manuscripts of the *De puncto* is found (Paris, BN lat. 16621) calls it a treatise written “contra magistrum de Montescalerio” (Zoubov, “Jean Buridan,” p. 43). Michalski has suggested that the *magister* in question is Burley. The editor of the *De puncto* regards this as a possibility, but also maintains (sensibly, in my view) that it cannot have been the entire aim of the *De puncto* to refute Burley. See Zoubov, “Jean Buridan,” pp. 50–52.

67. Buridan *De puncto* 3.2 (Zoubov 91.21–25). The editor of the *Quaestio de puncto* gives the reference to Averröes as *In De coelo* 1.32 (Venice, 1560), 5:27^r. The touch-at-a-point argument is presented in highly abridged form at *De puncto* 3.1 (Zoubov 85.10).

in order to verify the thought of the mathematicians, you must know that when a body is touching another body, it touches the other as a whole—taking ‘whole’ unitively [*unitive*]—because the body that is one whole touches the other body that is also one other whole, but not as a whole dividedly [*divisive*], because this would signify that each part would touch each part, which cannot be without penetration.⁶⁸

The upshot for the sphere and plane example is clear.

if a spherical body is placed on a plane, they would not touch each other in their parts by any division. And so they are said to touch at a point.⁶⁹

The similarity between these remarks and Wodeham’s view, namely, that spheres and planes conceived as positively immediate macro-indivisibles would touch each other at a point, can hardly be accidental. Like Wodeham, Buridan stresses that continua said to touch at a point must be thought of positively as *wholes*, rather than negatively as mere aggregates of parts producible by division. But this is not simply an endorsement of Wodeham’s position. Buridan adds sophistication to the non-entitist reply in two ways. First, like Wodeham, he shows that Ockham’s counterintuitive denial of contact is not the only option available to the non-entitist: by exploiting the logical distinction between the categorematic and syncategorematic senses of the term ‘whole’, one can show how the touch-at-a-point argument is actually compatible with divisibilist and non-entitist assumptions about the structure of continua. Second, Buridan sees this compatibility as exemplified in the practice of mathematicians, who use terms such as ‘point’ connotatively to refer not to a new class of entities, but to already existing entities in a certain, abstract way, namely, as quantities.⁷⁰

68. Buridan *De puncto* 3.2 (Zoubov 91.26–92.1).

69. Buridan *De puncto* 3.2 (Zoubov 92.14–15).

70. Buridan *In Metaphysicen Aristotelis quaestiones argutissimae magistri Joannes Buridani* 6.2 (Paris: 1588 [actually 1518]), rptd. as *Kommentar zur Aristotelischen Metaphysik* (Frankfurt a. M.: Minerva, 1964), fol. 33^{vb}. For discussion of Buridan’s philosophy of mathematics, see J. M. M. H. Thijssen, “Buridan On Mathematics,” *Vivarium* 23 (1985): 55–78. Though Buridan is parsimonious about positing entities, he sees nothing wrong (unlike Ockham) with proliferating modes of entities to account for the truth conditions of sentences concerning certain kinds of physical change. See *Q. in Metaph.* 5.8, *Q. in Phys.* 2.3. For discussion, see Calvin Normore, “Buridan’s Ontology,” in *How Things Are: Studies in Predication and the History and Philosophy*

That Buridan eventually came to develop his account into something of a fine art is evident in one of his last works: Book 3, Question 14 of the third and final redaction of his *Questions on Aristotle's De anima*.⁷¹ After discussing the ways in which indivisibilists and divisibilists nominally define the term 'point', he notes that "students have occasionally asked whether a sphere placed on a plane would touch it at a point." To this query, he offers a split reply, depending upon which nominal definition of 'point' is used.⁷² If 'point' is defined in the indivisibilist sense as 'an indivisible having position in a magnitude', he says, then a sphere placed on a plane surface would *not* touch it at a point, since "a point is nothing," and touching obviously involves something. Recall that this is essentially Ockham's reply to the argument: if contact has to be at a point, then there can be no contact. But if 'point' is defined in the divisibilist sense as 'the first or last part of a line', Buridan replies conditionally, stating that "if it touches, it touches it at a point in such a way that it touches at [the sphere's] last part." Not only that, it touches "at infinitely many last parts," since the sphere has infinitely many last parts, which may be produced, he says, "by dividing the sphere at circles parallel to each other and to the plane itself"—in other words, by slicing the sphere horizontally so that the cuts are parallel to the plane on which it rests. This is, of course, the method of proportional division *ad infinitum*

of *Science*, ed. James Bogen and J. E. McGuire (Boston: Reidel, 1985), pp. 189–203; and Zupko, "How Are Souls Related to Bodies? A Study of John Buridan," *Review of Metaphysics* 46 (1993):575–601. For Ockham's contrasting views on this point, see McCord Adams, *William Ockham*, pp. 178–186 and 277–285. Buridan also sees mathematical or, more properly, geometrical terms such as 'sphere' and 'plane' as referring to natural entities conceived in a certain abstract and generalized manner, namely, in so far as they exhibit the inherence of a magnitude. One might say that Buridan proliferates 'hows' rather than 'whats'. See, e.g., Buridan *De puncto* 1.1 (Zoubov 65.10–15). I should perhaps add that Buridan's strategy here would be of little use to Ockham, and not merely because Ockham balks at proliferating modes. The traditional interpretation of the role of connotative terms in Ockham's ontological program (as strictly synonymous with their nominal definitions, and hence as eliminable in mental language), has been recently, and very effectively, criticized by Claude Panaccio, "Connotative Terms in Ockham's Mental Language," *Cahiers d'épistémologie* [Montreal] no. 9016 (1990): 1–21.

71. For evidence suggesting that Buridan's *Q.* in *De anima* was composed after May 1347, see Zupko, "John Buridan's Philosophy of Mind," doctoral dissertation, Cornell University, 1989, pp. xxii–xxiii.

72. Buridan *Q.* in *De anima* 3.14 (Zupko 155.98–156.110).

Wodeham uses to illustrate how the notion of contact at some whole part of the sphere is compatible with the divisibilist assumption that the sphere has infinitely many such parts. Thus, says Buridan, a sphere sliced in half would touch the plane at the point which is its bottom half; the sphere sliced in quarters would touch the plane at the point which is its bottom quarter; and so on *ad infinitum*.

All of this is, Buridan concedes, subject to the condition that the sphere does touch the plane. Should the objector be dissatisfied with this hypothetical reply, and “ask categorically whether [the sphere] touches [the plane surface at a point],” Buridan has a second twofold reply, depending this time on the nominal definition of the word ‘touch’.⁷³ If the definition is based on Aristotle’s remark in *Physics* 5 that two things touch if their extremities are together,⁷⁴ then we need to ask about the nominal definition of ‘together’. Buridan says that ‘together’ can be defined in terms of ‘adjacent’. The latter term, it appears, has an ambiguous signification: it could signify either (1) two bodies such that no other body is between them; or (2) the situation of two bodies such that they could not be closer “without the penetration or compression of one of them.”⁷⁵

If ‘adjacent’ signifies in the second way, Buridan says, “the sphere and the plane would touch each other.” Contact of this sort is defined using Wodeham’s notion of positive immediacy: the sphere and plane are said to touch if they extended towards each other as far as possible without compression or penetration, regardless of presence of other, intermediate bodies (e.g., the air) between them.

But if ‘adjacent’ signifies in the first way, Buridan argues that the sphere and plane “do not touch each other” because then they would have to touch at a point, which is nothing.

if you place an actual sphere above an actual plane here in the air, then there will be air between them and between any and every part of them, because the air on the right would touch against the air on the left, for if

73. Buridan *Q. in De anima* 3.14 (Zupko 156.111–124). Compare Buridan *De caelo* 1.22 (Moody 107.31–32); Ockham *Expos. Phys.* 5.5.2 (Wood et al. 377.98–100); Wodeham *De indiv.* 2.3.11 (Wood 151.13–14).

74. See *Physics* 5.3 (226b23).

75. Although Buridan does not acknowledge his sources here, we can easily recognize the former as Chatton’s negative definition of contact between indivisibles, and the latter as Wodeham’s positive definition of contact between continuous wholes.

there were indivisible points as some imagine, those volumes of air would be separated only by a single indivisible point, which would not separate the parts joined to it. And since there would be nothing indivisible in the sphere or in the plane, and air is between any indivisible of that sphere and of that plane, it follows that there is nothing belonging to the one that is touching something belonging to the other in such a way that there is not some body between them, namely, the air.⁷⁶

In other words, if we imagine the cross-sectional view of an actual sphere *S* being lowered onto an actual plane *P*, *S* and *P* will be separated by a continuum of air—call it ‘*LR*’—as long as *S* is above *P*. But when *S* has been lowered as far as it can go, *L* and *R* would be separated, the case assumes, only by the point of contact between *S* and *P*. But since this same point is also the point of contact between *L* and *R*, *S* cannot really touch *P*, for the continuum *LR* still separates them. Therefore, *S* and *P* cannot touch at a single, indivisible point. To divide these volumes of air, that point would have to be extended and hence divisible—something Ockham and Wodeham also both recognized.

CONCLUSION

The replies of Ockham, Wodeham, and Buridan to the indivisibilist touch-at-a-point argument based on the sphere and plane example show, on a small scale, how medieval divisibilism evolved from a relatively unsophisticated defense of Aristotelian assumptions into a highly complex and subtle theory about the structure of continua. The portion of the story I’ve told appears to go something like this: in Ockham, the indivisibilist argument is addressed mostly in terms of definitions and arguments from Aristotle’s *Physics*, with little concern about its initial or intuitive plausibility. In Wodeham, that plausibility is both confronted and resolved in terms of an alternative account of contact (inspired, ironically, by the indivisibilists), and by means of the quasi-mathematical technique known as “proportional division *ad infinitum*,” which is used to illustrate how the sphere and plane can be said to touch by means of a divisible, but in the manner

76. Buridan *Q. in De anima* 3.14 (Zupko 156.124–157.134).

of an indivisible. Finally, in Buridan, the conclusion of the argument is treated like a sophism sentence and disarmed using logic, so that the paramount concern is to understand the signification of terms such as 'point' and 'contact'. The mathematical problem of explaining contact between ideal continuous bodies is raised only secondarily, almost as an afterthought. The divisibilist and non-entitist solution is hardly free of counterintuitiveness, of course, since it stipulates that if 'point' is to be a referring expression, it must pick out some divisible macro-object. From a dialectical standpoint, however, what really blunted the force of the indivisibilist touch-at-a-point argument was the development by Wodeham and Buridan of particular methods by which it could be interpreted without positing indivisible entities. In the fourteenth-century debate over the structure of continua, nominalism did indeed meet indivisibilism, and at least in the case of the touch-at-a-point argument, it emerged with its ontology intact.⁷⁷

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A Nominalistic Ontology of Music: Compositions as Concrete Particulars

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LOYOLA UNIVERSITY OF CHICAGO

A NOMINALISTIC ONTOLOGY OF MUSIC:
COMPOSITIONS AS CONCRETE PARTICULARS

A DISSERTATION SUBMITTED TO
THE FACULTY OF THE GRADUATE SCHOOL
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DOCTOR OF PHILOSOPHY

DEPARTMENT OF PHILOSOPHY

BY

SCOTT KENNETH TJADEN

CHICAGO, ILLINOIS

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To Michele

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CHAPTER I

INTRODUCTION: THE PROBLEM DEFINED

Preliminary Comments

To no small degree this dissertation is a thought experiment. It consists of a somewhat self-imposed theoretical puzzle regarding a specific problem in the philosophy of art. Having an interest in and preference for nominalism as an ontological perspective along with an interest in and some occasional experience with musical composition and performance, I have wondered whether the two interests could be combined into a coherent position: a nominalistic theory of musical compositions. Therefore, it should be made quite clear from the beginning that I am not searching for a conclusion, I already have one in mind; nor do I have pretensions of settling the question, What is the ontological status of musical works of art?. My purposes are, I believe, more modest. I am simply addressing the challenge of constructing an ontology of musical works that might be satisfying to those with a nominalistic bent or outlook. The question this dissertation seeks to resolve is this: if someone were inclined to believe that musical works are individuals, and that as individuals, they are, furthermore, better understood as

concrete particulars, what shape might a theory take in order to account for such presuppositions?

The hypothetical character of this thesis should not be taken as implying any lack of seriousness of motive on my part, nor should it be thought to imply that I do not think what follows is a viable theory. But it is important that readers recognize the actual goals of this essay. The starting point is a nominalistic perspective. I do not hope to convert non-nominalists to such a position; however, a greater measure of sympathy for the view would be desirable. It might be said then, that the minimal goal for this dissertation is providing a plausible ontology of music that is reasonably consistent with both nominalism and actual musical practice. The maximal goal for these efforts is developing a better theory than the alternatives, that is, determining what might be a "correct" description of musical compositions.

Thus, this dissertation seeks to be an investigation into the possibility and character of a nominalistic ontology of musical works. More specifically, this is a proposal for a theory of musical compositions that construes them as concrete particulars. A nominalistic ontology is, in simplest terms, a theory that countenances the existence of individuals only; or alternatively, it denies the existence of any sort of non-individual abstract entities or universals other than general words.¹ Therefore, the

proposed theory that follows will characterize musical compositions as individuals. But in addition and in keeping with the proposed notion of concrete particular and consistent with their status as artworks, compositions will also be regarded as physical or sensory artifacts, i.e., publically observable objects made by some person or persons.

In saying that concrete particulars of the sort I shall be discussing are physical objects, I am indicating that such an entity exists objectively in space and time. In saying that these concrete particulars are sensory (or phenomenal) objects, I am indicating that they are perceivable with the senses by perceivers. Thus, compositions, as concrete particulars, are entities existing independently and objectively of those who perceive them. Compositions qua compositions are therefore not imaginary nor conceptual entities. Dreams or hallucinations, for example, are phenomenal without being physical, (except as chemical or electrical brain phenomena, I suppose); whereas atoms are physical without being phenomenal, (under "normal" sensory conditions).

Another point regarding the claim that concrete particulars are physical and phenomenal has to do with the recognition that entities of the sort I am concerned with are describable in at least two basic ways. A physical description of something is an account of its extension in

space and related qualities. A phenomenal description of the same thing would amount to a report of someone's sensory experiences of the thing. For example, a musical performance described physically would be in terms of sound waves of certain frequencies; while a phenomenal account would be in terms of adjectives like "loud", "sonorous", and "high pitched". Therefore, compositions, since they are concrete particulars which are physical and phenomenal objects, can be described in either of these two ways.

A nominalistic ontology has little difficulty in accomodating the notion of concrete particular. Whether or not so-called "abstract particulars" are acceptable to nominalism is not so clear, and so this shall be left an open question, (although I shall touch on it later, especially in Chapter V). The present objective includes the development of a theory of compositions construed as concrete particulars. Under the view presented here, to say that something is concrete is to assert that it is non-abstract and "combined with, or embodied in matter, actual practice, or a particular example"²; it is spatially and temporally identifiable and is fundamentally describable as physical and/or sensory. To say that something is a particular is to assert further that it is non-universal and not general, it is a specific individual entity distinguishable and separable from other individuals. It is a singular entity that, unlike what is general or universal,

cannot exist as some sort of common nature or feature of something other than itself. A concrete particular has no existence prior to or independent of its spatial and temporal location. Thus, a nominalistic ontology of musical artworks, as developed in this dissertation, will construe them as a unique, physical and/or sensory individuals, and will avoid any description of them in terms of abstract or conceptual ontological categories.

Although some might insist that all artworks are essentially conceptual or abstract rather than physical or sensory, (for reasons to be discussed later in this chapter), others are likely to acknowledge that at least some of the arts are comprised of works of art that are each concrete particulars. A paradigmatic example would be painting. Paintings are usually regarded as unique individuals consisting of pigments on an actual physical surface, (e.g., canvas, wood, paper, plaster, etc.). They are each one-of-a-kind spatial and visible objects, whose basic aesthetic properties are visual. Other arts such as sculpture and architecture are likewise often taken to be comprised of artworks which are the physical artifacts we perceive as before us; i.e., statues and buildings.

In music, (as well as with others of the arts, e.g., dance, film, literature, drama, printmaking), the situation appears to be quite different. What sort of thing or object is a musical work of art? Paintings, sculptures, and

buildings are physical objects, pieces of matter, but musical compositions and performances are not so clearly material in this way. Musical sounds are thoroughly transient, and the works made of these transitory items lack the relative permanence that the other sorts of artworks mentioned above possess. We cannot point at musical works in the way we can point at paintings, sculptures, and buildings. Whereas daVinci's "Mona Lisa" exists in one place at a time, and Giacometti's "Standing Youth" and Frank Lloyd Wright's "Heller House" are likewise spatially and temporally identifiable; can we refer to Beethoven's "D Minor Symphony" in this manner?

First of all, Beethoven's symphony, taken as his work of art, his composition, is usually distinguished from any particular performance of it. A performance quite clearly consists of sensory experiences, and is spatially and temporally identifiable. But in what sense is Beethoven's work itself identifiable and sensible? Does it even make sense to ask such a question about such a thing as a musical composition? It is generally recognized that this symphony can be performed an indefinite number of times in an indefinite number of places. Does this imply that there are an indefinite number of "D Minor Symphonies"?; is any given performance actually Beethoven's work of art anyway, or do we never hear Beethoven's genuine work of art?; and what about the score, is there any sense in which this paper

filled with notations is the work of art? These and a host of other difficulties associated with music will be examined later in this dissertation.

These sorts of considerations have led some philosophers to conclude that the genuine musical work of art, the composition, is fundamentally some kind of abstract entity. One direction taken has been to regard the ontological status of the work as essentially conceptual, mental, or imaginary. Any physical or sensory manifestation of music, i.e., performances, (actual sound sequences), or scores, (notated symbols on paper), is merely a means of conveying the essential work to audiences. The "true" work is the object of mental aesthetic contemplation: either the artist's or the spectator's internal experience; or it is the intelligible form or design of the work. Another direction taken has been to construe the work as some sort of universal; that is, as something capable of multiple instantiations. From this perspective the work is evident in any number of individual performances, scores, recordings, etc., but none of these particular objects is the actual work itself. Musical compositions may then be comparable to other universals like "Redness", "Chair", or "Justice". Under this view, compositions, as with universals generally, are not only non-physical and non-corporeal, but they do not require physical embodiment in order to exist. A version of each of these two

directions will be addressed at length in Chapter IV of this dissertation.

It is my intention here to argue against such conceptions of musical works, and for, rather, a conception which construes them nominalistically as concrete particulars. The musical work will be described as an individual sensory object, (perceivable as sights, sounds, etc., depending on the medium in which it exists), and as a particular physical artifact made by some person or persons. This artifact is the sort of thing that can be copied or imitated in a variety of ways and in diverse media. Such copying of a physical artifact will be distinguished from such non-nominalistic descriptions as 'instantiating' or 'being an example of' as the fundamental relationship between composition and performance.

Therefore, under my proposal, a particular performance of Beethoven's "D Minor Symphony", for example, is not, strictly speaking, Beethoven's artwork, nor is it properly regarded as an instance or occurrence of his composition. Rather, such a performance is understood to be a new, yet only partly original, artifact derived from Beethoven's original work. A performance of the "D Minor Symphony" uses a copy of Beethoven's manuscript score as a set of instructions, a description, or a guide towards making some musical sound occurrence. The performance "earns" the title "D Minor Symphony" on the basis of an historical connection

with Beethoven's original musical composition. Chapter III will attempt to spell out these issues.

An assumption of this dissertation related to the larger theory will be the claim that not all works of art are admired in and of themselves or for their intrinsic sensory qualities. The significance of this subsidiary thesis may be seen in relation to the aesthetic value of manuscript scores as artworks. Paintings and sculptures are often taken to be aesthetically interesting and valuable for their immediate sensory characteristics: color, shape, texture, etc. But they may be valued also for their references, meanings, and representations beyond these directly experienced qualities.³ That a painting represents the Crucifixion or incorporates various symbols, for example, is usually taken to be somehow aesthetically relevant, if not essential, to an adequate appreciation of such pieces. In admitting this, someone may yet justifiably claim that the physical painting remains as the actual artwork.

Similarly, musical performances are quite clearly aesthetically important, in large measure, because of their directly experienced auidial qualities. Whether musical sounds and their arrangements can be or are meaningful beyond this sensory level is a matter of great controversy. But let us set that issue aside for the moment and note that musical works in manuscript form, (i.e., notational symbols

on paper), are most often aesthetically admirable for what they can do or be used for rather than for their intrinsic sensory properties. Very rarely, if ever, is a manuscript contemplated for the dots and lines that comprise its visual qualities, (in fact, to do so would probably amount to treating the manuscript like a painting or a print rather than as a musical score). Some people can "read" manuscript scores and imagine music consistent with the notation. This situation does not undermine the point here. A case of imagining music "in one's head" based upon a composer's manuscript should not be confused with a case of admiring the sensory qualities of the manuscript. The former is a case of using the score for something else, namely, imagining music. The essential point here is that the notational symbols derive their aesthetic value primarily from their ability to refer to or represent actual musical sound occurrences which are aesthetically valuable for themselves.

Compositions are the products of certain acts of composing. One of the chief tasks of the dissertation will be to describe how the making of compositional artifacts bears upon the ontological issues at stake. Composing is, most simply, the selecting and arranging of what I shall call "musical elements". Musical elements will be presented as existing in two basic forms: sounds or symbols for sounds. These symbols may be notational or linguistic; that

is, they are either written inscriptions or verbal expressions. Those musical elements that are musical sounds, (as well as sound correlates such as silences and accents), are generally associated with certain conventional names and these names can be written in symbols, (i.e., dots, lines, etc. on a staff), letters, (e.g., B, G#, etc.), or words, (e.g. "Key of C", "crescendo", "F Major chord", etc.). These names may be spoken as well as written. Later, they will be described as functioning in a manner comparable to other general words, and consistent with a nominalistic interpretation.

The artifact that is the result of certain acts of composing, namely the musical composition, is that object constructed from whatever musical elements that are chosen by the composer. Thus, the artifactual object of musical art-making is most often either a rather short-lived, transient sound sequence or a written manuscript having a considerably longer life-expectancy.⁴ In either case, the artwork is this concrete individual.

Obviously, not all musical occurrences are compositions. Compositions are original works of art, while performances and scores as such are usually, in some way or other, copies or derivations of compositions. Spelling out this fundamental and crucial distinction between composition and performance provides another important task for this dissertation. In addition, the relationships between

composition, score, and performance, as well as understanding the place of improvisation in these considerations will have to be addressed. I shall argue that all performances are themselves musical works; but while some performances are compositional works of art, most are not. I shall also argue that it is usually the case that improvisations should be regarded as genuine musical compositions.

Composing is not just any occasion of selecting and arranging musical elements--some selecting and arranging is not composing. Transcribing into notation a heard performance involves, strictly speaking, selecting and arranging notational musical elements, but this is not composing. Performing a piece of music from a score is an occasion for selecting and arranging musical elements, (i.e., making decisions about which sounds to make on or with a musical instrument or voice), but again this is not composing. A genuine compositional work is the product of some person's (a composer) intention to make original selections and arrangements. That is to say, there must be a belief on the part of the composer that the work under construction is, to some extent, original. The degree of intentional originality required is probably not clearly specifiable, but I shall look into this question at the appropriate point in my presentation. A rough-and-ready characterization of musical compositions, to be spelled out

in Chapter III, will thus be that they are intended originals consisting of selected and arranged musical elements.

Before I explain in further detail this nominalistic theory of musical compositions, I shall attempt to accomplish two preliminary tasks: (1) provide a sense of context by setting the specific issues of this dissertation within the broader context of ontology and the arts; and (2) examine nominalism as an ontological perspective. The first of these will be the focus of what remains of this chapter; the latter task will be presented in Chapter II.

Work of Art: Evaluative and Classificatory Senses

Among George Dickie's many valuable contributions to the philosophy of art is his clarification of two senses of the phrase "work of art". He distinguishes what he calls a classificatory sense from an evaluative sense.⁵ This distinction is helpful and important not only because it clarifies a significant ambiguity in the use of this phrase, but also because it effectively identifies a fundamental division between areas of inquiry in the philosophy of art. I shall discuss this division shortly; first, let us briefly consider Dickie's distinction.

The difference between the evaluative and classificatory senses of "work of art" is fairly straightforward. The evaluative sense of "work of art"

refers to uses of the phrase concerning judgments of aesthetic value. To say of a thing that it is a work of art in an evaluative sense is to say that it is aesthetically valuable. The evaluative sense is, to a certain extent, honorific or complimentary. It is not uncommon for someone to say of some painting, for example, "Now that is a work of art." The purpose of the statement is to acknowledge the noteworthy aesthetic value of the painting. In calling the painting a "work of art" the speaker is not merely identifying what sort of thing is hanging on the wall; rather, she is making, for the most part, a value judgement to the effect that the painting is somehow "good" from an aesthetic point of view. "Work of art", used in this way, is thus evaluative since it expresses an evaluation, and a positive one at that, of the object to which it refers.

The classificatory sense of the phrase "work of art", on the other hand, refers to uses that do not make such evaluative attributions. This sense is essentially descriptive; it identifies what something is; it classifies the thing as an art object rather than something that is not an art object. Under this use of the phrase, if someone says of the painting, "Now that is a work of art.", the speaker's purpose might be only to single out the piece as an example of an artwork or to inform someone that the thing before them is an artwork and not something else. No evaluative assertion is intended.

Dickie points out that classificatory and evaluative senses may be operative in a single statement. If someone says, "This Rembrandt is a work of art," "the expression 'this Rembrandt' would convey the information that its referent is a work of art in the classificatory sense, and 'is a work of art' could then only reasonably be understood in the evaluative sense."⁶

A partial justification for this distinction between descriptive and normative senses of work of art is evident in our readiness to talk about "good art" as opposed to "bad art". If "work of art" had only an evaluative sense of the sort described, then the phrase "a good work of art" would be redundant and the phrase "bad work of art" would appear to be self-contradictory. A junior high school art show may have no items of aesthetic merit, yet this does not conspire against its status as an art show. We might say then that it is an art show in the sense that it exhibits works of art in the classificatory sense.

Clarifying this distinction between senses of work of art is important on a number of counts. Disagreements following on the exclamation, "You call that a work of art!", may be more profitably pursued if care is given to noting which sense of "work of art" is being used. The statement might be a value judgement expressing disapproval of the purported work's positive aesthetic value. Thus, the exclamation is just another way of saying, for example,

"That painting is ugly." In this way, an effective response to the speaker's charge should address criteria of aesthetic value.

Alternatively, if the speaker's intention consisted in taking "work of art" in a classificatory sense, any subsequent debate would best consider the nature or definition of art. For example, the speaker may be attending a photography exhibit and actually believe that the photograph before her is quite beautiful, but she does not believe, (for whatever reason), that photography is a legitimate artform. An appropriate interpretation of the exclamation would then be something to the effect that, "Photographs cannot be works of art." The point of her utterance is not to make a value judgement as such, but to deny the photograph's classification as an artwork.

Interestingly enough, probably the most common use of the above exclamation includes both senses of work of art. Controversial works of art are often indicted by the exclamation, "You call that a work of art!" for both lacking in aesthetic value and for not being the sort of thing that can be an artwork. For example, Duchamp's "readymades" such as "Fountain", (a manufactured urinal), have been criticized by some people for being aesthetically unappealing and also for not being the appropriate sorts of objects or subject matter for legitimate works of art.

This dissertation will make use of the phrase "work of

art", (as well as "artwork" and "work"), in the classificatory sense exclusively. Whether or not an given musical composition or performance is aesthetically good is not my concern here.

I said that Dickie's distinction of senses identifies a fundamental division of problems within the philosophy of art. The point I would like to make is this: just as there are these two distinct senses of "work of art", there are two distinct contexts for aesthetic inquiry which parallel these senses. An evaluative approach to works of art inspires questions such as "what is good art?", or "what features of artworks make them aesthetically valuable?", or "what is the nature of aesthetic value judgements?" In other words, quite clearly the philosophy of art consists partly of an inquiry into evaluative considerations of art, questions of aesthetic value. But the philosophy of art is also concerned with what might be called "classificatory" issues, or questions involving the nature of artworks independent of evaluative considerations.

Classificatory uses of the phrase "work of art" serve primarily, as we have already noted, to distinguish artworks from non-artworks. Following on this understanding such questions as "what is art?", or "how are artworks different from things that are not artworks?", or "is there a definition of art?" arise.

In connection with these fundamental questions

associated with the classificatory sense of "work of art" an additional set of questions emerge. These may be referred to as "ontological" questions. Once we have decided which things we wish to regard as genuine works of art we may be interested in questions such as, what or where is the artwork?, what sort of thing is an artwork?, in what sense do artworks exist?; are artworks necessarily embodied public objects?, is the actual artwork that which we experience with our senses or is it something essentially mental, something imagined? A complex web of problems and issues revolve around such ontological considerations, as we shall see.

Ontological questions involve the classificatory sense of work of art because they are not directly concerned with evaluative claims about the artworks they examine. They are concerned with the nature of works of art whether they are "good" works or "bad" works, whether aesthetically valuable or valueless, and anything in between. Thus, in an ontological context, references to works of art may or may not imply anything with respect to the work's value. Ontological investigations may proceed regardless of the work's evaluative qualities.

This is not to say that classificatory contexts, including ontological considerations, are irrelevant to evaluative contexts. What I wish to make clear is that in the ontological investigations that follow, the focus will

be on works of art in a classificatory sense. I am not concerned here with questions regarding which artworks or artforms are aesthetically superior, nor do I intend to discuss criteria for aesthetic evaluation or interpretation. As interesting as these issues are, I shall attempt to keep them separate from the present analysis.

It should also be made clear that the ontological considerations I shall be concerned with are not classificatory in the sense that they aim towards a definition of art or determining what distinguishes artworks from non-artworks as such, although it may draw from or contribute to such considerations. What I wish to emphasize is that ontological questions are classificatory since they are essentially descriptive, rather than evaluative or interpretive. We may proceed ontologically from the position that there is an already established fund of artworks, a large group of things accepted as works of art. These may or may not have been determined on the basis of some sort of explicit classificatory theory which lays out the critical necessary and sufficient conditions for arthood. The ontological project I propose to undertake, in effect, begins after such definitions and determinations have been made and however they have been made. The proposed theory should be able to accommodate all musical works, regardless of whether their status as works is widely accepted or highly controversial. This project proceeds

under the assumption that there are musical works of art, and then attempts to clarify and articulate a description of their ontological status regardless of the controversies surrounding them.

Ontology and the Arts

That works of art exist is not controversial. The controversies begin once we attempt to describe how or in what sense works of art exist. Are works of art physical objects or are they some kind of abstract entities? Are they particulars or something more akin to universals? Where is the art object to be located: the artist's experience, the physical stimulus, or the spectator's experience? Can a distinction be made between the "art object", (the physical, phenomenal, or public thing we observe), and the "aesthetic object", (that which we admire or to which we attribute aesthetic value, and is thus, the "real" work of art)? What is the relationship between works of art and copies, reproductions, performances, etc. of them? In what sense can works of art be created or destroyed? The questions and problems are many.

The question of the ontological status of works of art in general may not be amenable to a single answer anyway. Painting, sculpture, architecture, literature, music, dance, drama, photography, and film all appear as quite diverse modes of artistic expression. They differ so significantly

from one another that a single ontology applying to all artworks may be inappropriate if not impossible. On first reflection, we may be inclined to distinguish the arts ontologically simply in terms of their different physical media. Paintings are made of paint; sculptures are made of clay, metal, wood, and other solid materials; literature consists of words spoken or written; music of sounds; photography and film are visual images produced by certain chemical reactions to light. From these distinctions of media an ontologist might conclude that the ontological status of a given work of art is a function of its medium of presentation, i.e., the physical or sensory object of a viewer's or listener's or reader's attention. The pigment on canvas is the work of art in painting; the sounds heard in the concert hall make up the symphonic work of art; the words printed on the page are, taken together, the literary artwork; and so on.

What lends some initial plausibility to this physicalist/phenomenalist interpretation⁸ is the generally acknowledged expectation that artworks be publicly accessible. In other words, it is usually thought that it is not enough for artists merely to have aesthetic ideas, they must make something that people can experience. Therefore, a number of philosophers of art have regarded artifactuality as a necessary condition for status as an artwork. We often think of artists as people who create things, (sculptures,

poems, films, songs, etc.), for aesthetic appreciation. Few if any artworks require the presence of the artist in order to be experienced; rather, the artwork is an object distinct and separate from the artist herself. It does not require a great logical leap to identify the object experienced as the artist's work of art. This is just another way of recognizing that artworks usually, if not always, exist in some medium or other. The physicalist thesis amounts then to the claim that the work of art is identical with its medium, or that the work of art is the artist's physical artifact.

Consider the way in which we identify a work of art in terms of title and artist. We say, for example, "This painting is called 'Composition with Two Lines' and it was painted by Mondrian." It is easy enough to regard the work of art as the painting which is a particular physical object located in space and time, composed of paint applied in a certain way to a flat, rectangular piece of canvas, as something that can be pointed to and experienced with the senses, and the product of some individual person's intentional efforts occurring at a definite place during a specific time. This is shown, to some extent, in the way we can and do sometimes talk about artworks: "The 'Two Lines' fell off the wall today."; or "The 'Two Lines' was stolen, but has since been recovered."; or "Mondrian's painting has been destroyed by fire." Each of these statements is

literally meaningful; and the title of the work seems to be identified with the physical artifact. Such talk would be very peculiar, if not impossible, if the 'Two Lines' were not a physical object. Physical objects, but not ideas, can fall off walls. Physical objects can be stolen and then recovered, but not ideas. And it's not clear how anything other than a physical object can be destroyed by fire.

Now such a conception is not without its problems and detractors, (I shall return to these later); but let us assume for the moment that this ontological description of paintings is adequate. Can a physicalistic ontology extend to other sorts of artworks? Consider woodcut prints, an artform not terribly distant from painting in terms of its artifactual product, (often a two-dimensional image on a flat surface), although the process of making the artifact is significantly different. The artist carves out areas of a block of wood, applies ink to the carved surface, and then presses the block to paper, or similar material, leaving the desired impression. Typically, the artist makes a run of these impressions ranging from a single print to an indefinite number of them. What is the work of art in this case? Seldom, if ever, is the wood block put on display or admired for its aesthetic qualities, this is because it is usually not considered to be the work of art as such. What is commonly taken to be the work is a print made from the block. If someone or some museum possesses an impression or

two of Durer's "The Four Horsemen of the Apocalypse", each of these is spoken of as a work of art, each is displayed under the title and referred to as "The Four Horsemen of the Apocalypse". It would seem then that there are many works of art with the same title, but we can no longer speak of the work of art as an individually identifiable object in these circumstances. If a print of the "Four Horsemen" is destroyed by fire, Durer's work with this title is not considered lost. If the wood block from which the impressions were made is destroyed or lost the work itself is not considered lost or destroyed.

The situation in the case of literature appears even more problematic. John Updike wrote Rabbit, Run in 1960, presumably with some writing implement, (pencil, pen, typewriter, or whatever), on paper; this artifact is referred to as his manuscript. (An author could even dictate his work into some recording device, or type it into a word processor; this would further intensify the problems to be discussed below.) This manuscript was prepared and typeset by a publisher and thousands of copies of the novel were printed and distributed. Now what is Updike's work of art? Not only are we again confronted with a problem of identity and diversity as with woodcuts, (is each copy of the book equally a work of art?; are they each to be identified as a work of art?; is the original manuscript the only genuine artwork?, if so, what is the status of an

individual copy of the novel?; do we have access to the work of art without the manuscript?); but an additional and somewhat different ontological problem gains prominence. The physicalistic/phenomenalistic interpretation does not seem quite so amenable to literary artworks as it did to painting or even woodcut prints. This is so because, in literature, it is not so much the visible inscriptions on the page that we take as the object of our aesthetic interests or attention, but instead what some might call the meanings of the words, or even more abstractly, the story that is told with the words. In other words, the medium of literature does not seem to be a physical or sensory one. What further complicates the matter is the possibility of translations of the novel into other languages, languages the author probably does not even know. What permits us to refer to each copy of the book and each translation as Rabbit, Run?

Thus, two sorts of ontological issues can be seen emerging with respect to works of art: 1) identifying or locating the actual works of art, i.e., where they exist; and 2) determining what artworks are made of, i.e., how they exist. If paintings are taken as relatively unproblematic in this regard, a physicalist's response to the two ontological questions might go something like this: 1) Mondrian's work of art known as "Two Lines" is an individual object painted in 1931 and is presently hanging on a wall at

the Stedelijk Museum in Amsterdam; and 2) "Two Lines" consists of patches of pigmented oil paints arranged on the surface of an approximately 40" square piece of canvas. Can a comparable response be given to questions concerning literary works such as Updike's Rabbit, Run? If it is possible, it is certainly more complicated and difficult. How would we answer questions like, where is John Updike's literary work Rabbit, Run?, and what is it made of? The most likely response would be that such questions are somewhat nonsensical since a novel is not the kind of thing that can be identified in this way; its ontological status is significantly different from that of a painting, sculpture, or a building. Not only does literature seem to be comprised of artworks that are not individual objects, but the aesthetically relevant characteristics of these works do not appear to be sensory or physical.

The so-called performance arts, drama, dance, and music, for example, provide further complications for the ontologist. In these arts a credible distinction can be made between a work and a performance of it. The object the artist makes, a script, score, or set of choreographic instructions, is not usually thought to be the actual artwork. These artforms are sometimes described as consisting of two-stage artworks: the instructions and the performance. Scripts and scores are somewhat incomplete as artworks; they require some performer or performers to act

or sound them out. But the performances themselves are not the complete artwork either. The character of their phenomenal features largely depend upon the script or score produced by the artist. What then is the work of art in these cases if not the script or score nor any particular performances of them? We speak of performances as "a performance of the artwork 'Hamlet'"; and we speak of scores as "a score of the artwork 'D Minor Symphony'". The implication seems to be that neither performances nor their instructions are the actual works of art. Attempting to describe artworks within the so-called performing arts in terms of individual physical objects does not seem plausible.

Each of the various artforms has its own attendant ontological problems. I have touched on a few, and it should be quite obvious that there are many more. Various theoretical proposals have been offered to provide either a general, unified theory accounting for all artworks in all artforms, or specific theories aimed at satisfying the ontological requirements peculiar to particular artforms.

An approach to a general theory that is an alternative to a physicalist theory might begin by making a distinction between the work of art and its artifactual embodiment. The "aesthetic object" is considered the true work of art, whereas the artifact, the physical/sensory object is merely a means of presenting, conveying, or communicating the

essentially non-physical artwork to a spectator or audience. This view, in effect, denies that artworks are physical objects and so attempts to avoid whatever ontological difficulties arise from this interpretation. For example, if the work of art is not a physical object, then the problem of identifying its spatial and temporal location is removed. Artforms are distinguishable from each other partly in terms of the different kinds of aesthetic ideas they seek to convey, and partly in terms of the different media they use to embody the work itself. But any given embodiment or form of embodiment is not to be confused with the actual work of art. Neither the manuscript nor any printed copy of Rabbit, Run is the literary work; each of these items is important for experiencing the work, but they only serve to reveal the work, to put it in a publically accessible form, not to be the work. The work itself stands somewhat independently of these physical manifestations, and so is not lost or destroyed if the manuscript or any printed copy is lost or destroyed. The relationship is thus not symmetrical. Under this view, the work can exist without physical embodiment, but the physically manifested artifact cannot exist without a previously existing work of art. Aspects and variations of this kind of approach will be critically examined in Chapter IV.

Another important proposed solution to the ontological difficulties associated with the arts is an application of

the type/token distinction to this context.⁹ Under this view, works of art are essentially types of which any number of individual tokens may exist. This is what permits copies, reproductions, performances, prints, etc. to each be called examples of a specific work of art. In talking about a print of Durer's "The Four Horsemen" as having a tear in the left corner or being printed on deteriorating paper, we are referring to a token of the work, an individual copy. But if we say that "The Four Horsemen" is dynamic or comprised of exceptional detail revealing great skill on the part of the artist, we are talking about the type, i.e., the work itself.

Musical compositions, as types, may have not only numerous individual tokens, but these may also exist in a wide range of media: sound performances, notational scores, recordings, verbal descriptions, etc. The view I propose will not adopt either the language or the categories of types and tokens to characterize the ontological status of compositions and their performances, scores, etc. First of all, since my intention is to construct an account of compositions which is admittedly physicalistic, and since the notion of a type precludes construing it as a physical object, the concept of type must be ruled out as a viable category within the theory. Secondly, type is the sort of abstract notion which I am seeking to avoid. More on these points later.

Music and Nominalism

The problems and perplexities resulting from ontological examinations of works of art are clearly many and widely diverse. This diversity is largely a result of the great variety of art media and artistic practices. Much has been written about this, and I have only touched on a few examples. My purpose in what has preceded has been primarily to give some sense of perspective to the specific intentions of this dissertation: an ontological analysis of musical works of art, especially compositions. In what follows, I do not propose a unified theory applicable to all the arts; instead, I intend to focus on music, or more precisely, musical artworks.

Music, it seems, is fundamentally a matter of sounds. To listen to music is to listen to certain sounds. To play music is make certain sounds. Sounds are, physically speaking, waves of moving air; sensorially speaking, they are audial experiences had by beings capable of hearing. Sounds also have the character of events. They are transient, they happen, they can be measured for duration in time, but presumably not for extension in space, (at least not in the ways we typically do so). Although they occur in specific places, they are not enduring spatial objects, (except as moving particles of air); they are quite short-lived. Since music is made of sounds, the ontological

characteristics of music might seem to be reducible to the ontological characteristics of sounds and events. If this were the case, music could be treated merely as a species of sound.

If the art of music consisted of nothing other than making and listening to musical sounds, then the ontology of musical works might be fairly straightforward. Musical works would be certain sound-sequence-events occurring at a specifiable time and place. Their ontological status would be comparable to other sorts of sound-sequence-events; we would only need to determine criteria for distinguishing musical from non-musical sequential sound occurrences. But musical practice is not quite that simple. Music, as a form of artistic activity, is not only the making and hearing of musical sounds.

For example, we often speak of someone 'writing' music or 'reading' music. Such reading and writing does not directly involve sounds. These uses of the term 'music' reflect practices in music which permit associating musical sounds with symbolic notation. Notating music seems desirable because it allows for the possibility of repeating certain musical sound-sequence-events or communicating musical ideas to others. Since musical sounds are transient, the only way to 're-experience' aesthetically satisfying musical sound-events is to make similar sounds. Before the development of sound recording and reproducing devices,

symbolic notation provided an effective way, beyond the use of memory alone, of communicating and 're-making' certain desired musical sound-events.

Notation also allows for music to be created without actually making sounds. Since persons can imagine sounds, and since these sounds can be associated with symbols, creators of music can write out musical 'scripts' or scores instructing other persons how to make musical sound-events. Complex music, (multiple musicians, diverse instruments, longer sequences, etc.), may be facilitated through the use of this standardized notation. Music does not have to remain solely a matter of improvization, (making it up on the spot as it is being played), or of remembering previously occurring musical events.

Reading and writing music thus amounts simply to making use of the symbolic notation of music. But what are the statuses of these notational arrangements in the ontological scheme of things? We say of the musical artwork 'D Minor Symphony' that it has been written by Beethoven. If he had had nothing to do with any particular sounding of this work, we would not cease to refer to it as his artwork. Therefore, it appears that, given the way we talk about music, making musical works of art is not dependent upon the maker of the work making musical sounds. Furthermore, what we call musical artworks need not consist of sounds. And so, the ontology of music cannot be reduced solely to the

ontology of sounds.

Nor can music be reduced solely to its symbolic notation. A great deal of music is never notated; often musical sounds and works are made without the use of notation; and 'reading' musical notation alone is not generally regarded as the preferred way of aesthetically appreciating music. Actual sound-events remain as the central elements in music and music making, but not the only ones.

Sound recording and reproduction technology permit repeated sound-events that are quite similar to previously occurring sound sequences. Music produced in this way consists of sounds occurring in an identifiable time and place; but these sound-sequence-events are a few steps removed from the playing of musical instruments, (including voices). Musical instruments are similar to electronic playback equipment in that both can produce musical sounds under certain circumstances, and both can be used to produce sound-events very similar to others that have occurred before. The former requires a competent musician to properly play the appropriate musical instrument, whereas the latter requires a copy of a recording, (magnetized plastic tape, grooved vinyl disc, etched light-reflecting disc, etc.), made from some original sound-event caused by a musical instrument. Although the two methods of producing facsimiles of a previous sound-event may result in very

similar audial experiences, the processes are obviously quite different. I shall explore some of the ontological relevancies of these differences in subsequent chapters

Recording media and techniques also provide another approach to making musical works of art. Multi-track tape equipment allows composers to construct musical works by manipulating and combining sounds with far greater control than otherwise possible. For example, a composer may play, by himself, each and every musical instrument required for the sounding of a musical work. These individual soundings may then be played back together forming the complete work. A composer can thereby construct a multi-instrumental work without dependence upon other musicians. In this way, not only is a composer able to provide a sounding of the work, but the compositional work is composed through the use of actual sounds rather than notational symbols. Thus, composers may compose musical works of varying degrees of complexity without the use of notation, and they may do so by the direct use of sounds. This phenomena of 'writing' music with sounds will also be explored for its ontological significance.

The practice of virtually every art involves an artist who makes an artwork. In music, composers are the artists who make artworks referred to as compositions. But music also accomodates another sort of artist, the performer who makes performances. Often the activities of composers,

(e.g., writing on paper), and performers, (e.g., playing a musical instrument), are quite different; and usually the artworks produced by these activities are also quite different, (notational scores and musical sounds respectively). But sometimes, as we shall see, the composer and performer of a work may be the same person; and sometimes compositions may be performances, (e.g., improvisation).

Another important feature of most art making is the identification of an artist's artwork by and with a title. The artist is associated with the artwork as its creator/maker, and the title, in effect, sets off the work from other works. That is, a work of art is distinguished from others partly in terms of who made it and partly in terms of its 'name' or title. A central problem facing this dissertation is the determination of the ontological character of titled musical artworks, (as well as so-called "Untitled" works). An important dimension of this determination is an analysis of this practice of naming or titling musical artworks. This effort must inevitably address the process of making musical compositions, the process by which a particular musical work is associated with a particular composer, and the process by which it is set off from other compositions through naming.

As has been noted earlier, musical compositions are closely associated with performances of them. An

interesting feature of performances, from an ontological point of view, is the way in which they are given the same name or title as the composition. This is largely a matter of a dependency relationship that typically holds between performances and compositions. The ways in which performances are usually dependent upon compositions will be examined in some detail in Chapter III

Not only do performances of compositions share names with the original compositions themselves, but written scores and sound recordings do as well. In addition, if someone merely imagines the sound of a composition "in his/her head", this too is referred to by the same name. We say such things as,

"Beethoven composed the 'D Minor Symphony' during the early nineteenth century."

"I listened to the 'D Minor Symphony' last night at Orchestra Hall."

"The teacher played the 'D Minor Symphony' on the school's new sound system."

"We studied the 'D Minor Symphony' from an old German textbook."

What permits each of the referents (a composition, a performance, a recording, and a score respectively) to warrant the title 'D Minor Symphony'? Are we speaking accurately when we call each of these items 'D Minor Symphony'?

Performances, scores, and recordings are fairly uncontroversially considered to be individuals. What is

controversial is the status of the composition. My claim is that they too are individuals; but others hold that compositional works are the sorts of things that can have multiple instantiations, or that they are somehow abstract in a way that precludes identifying them as in any way individual. Composers create or make compositions, but just what sort of thing do they make such that they appear to be capable of having performances, capable of being written out in symbolic notation, and capable of existing or being present in diverse recorded media? The nominalist, as someone committed to accepting only individuals as genuine existents, must show just how it is that compositional works are themselves individuals, and how the association of performances, scores, and recordings with a compositional work is fundamentally understandable in terms of language and individuality, rather than some essential relationship based on universality or in terms of abstract entities.

Before proceeding with the proposed nominalistic theory of musical compositions, I shall briefly discuss nominalism as a general ontological perspective.

CHAPTER II
NOMINALISM AND ONTOLOGY

Introduction

In this chapter I shall explain in general terms what nominalism is and how this conception will provide the background for my theory of musical compositions as concrete particulars. I shall not attempt to defend nominalism as a comprehensive ontology, nor shall I provide a systematic survey of the historical development of nominalistic positions. Instead, my purpose will be to characterize a nominalistic perspective that will be applied later to a specific group of entities, namely musical compositions and performances.

Naturally this effort will involve some defense of nominalism as a point of view, as an approach to philosophical problems, but the adequacy of nominalism as a complete and all-encompassing ontological theory cannot be addressed within the scope of this paper. The reason for this is that the present essay has as its primary objective an examination of the phenomena of musical compositions and performances in ontological terms. An analysis of nominalism provides some of the background for this examination, but is not itself the central subject of this

dissertation. Therefore, I shall leave the fuller defense of nominalism to others or for another time.

I do believe though that an important part of the larger philosophical project concerned with spelling out a complete and adequate nominalism is the development of plausible nominalistic characterizations of certain problematic entities. In light of this, the chief objective of this dissertation is an explication of how a nominalist might understand the ontological makeup of a certain part of the world; i.e., musical compositions. This is thus one proposal for a conception of musical artworks as individuals. Seeing each and every part of the world as individual is a nominalist's fundamental ontological commitment, and so, I shall attempt to present musical compositions in a way that is consistent with this commitment.

I am particularly interested in exploring at this time some of the ontological problems associated with music and to determine what a nominalistic theory of compositions might be like. As stated in the Introduction, I do not aim at converting non-nominalists to such a perspective; but I do hope to contribute a reasonable theory to the range of possible ontologies of music and to provide one that would be satisfying to the nominalistically-minded.

Nominalism is not a single, unified doctrine. Rather it represents a range of positions that cluster around some

basic ontological commitments or preferences. Some versions of nominalism are highly complex, formal, and technical in their formulation and application. But generally speaking, put one way, nominalism is an ontological perspective asserting that anything that can be said to exist is an individual. Put another way, a nominalist may prefer to emphasize a negative version of this thesis by rejecting the notion of any actually existing universals, abstract entities, classes, kinds, or any other non-individual. Alternatively, nominalism might be expressed as the view that all uses of universal, general, or abstract terms are devoid of reference. That is, although such words may serve a function in a language, they do not name or identify or refer to any existing thing. My own nominalistic approach regards all of these alternative conceptions acceptable and sufficiently compatible with one another as far as the present thesis is concerned.

In order to flesh out these broader characterizations in greater detail, I shall develop my presentation of nominalism by examining the following general descriptions of nominalistic systems and approaches. A nominalist might explicitly ascribe to all of these claims or to some varying combination of them. In any event, nominalism might be construed as, (1) a form of skepticism about what there is, often emerging from empirical tendencies; (2) a philosophical temperament expressing a preference for some

notion of ontological individuality; (3) a negative theory denying the existence of universals and/or abstract entities; (4) a positive theory affirming that all existents are individuals; and/or (5) a limiting theory describing the far extent to which particularity can be emphasized in an ontological system. My own view is that taken together these comprise the key features of an overall nominalistic outlook. In the first part of this chapter I shall explain what I mean by each of these variations on nominalistic themes. Later I shall describe how ontological problems arise in aesthetic contexts and how nominalism is relevant to them.

Nominalism as Skepticism

First, nominalism can be understood as a form of skepticism; that is, it may consist of a skeptical attitude towards existence claims regarding any purported non-individual entity. In general, skepticism is the view that either all knowledge claims or at least those of specified sorts are impossible, unreliable, or dubious. Skeptics differ from one another in terms of the scope and objects of their skeptical attitudes, but all tend to sharply limit what counts as genuine knowledge.

Nominalism is skeptical about ontological claims regarding universals, abstract entities, kinds, classes, or any other non-individual. Can there be knowledge of

existing universals? Can any abstract entity be known to exist? Is there sufficient evidence that the terms "universal" or "abstract entity" refer to any knowable thing? A nominalist would tend to answer no to these questions. Minimally, the nominalist as skeptic is an agnostic with respect to the conclusiveness of arguments for the existence of universals. Although most versions of nominalism, as will be seen later, go beyond this skepticism to conclude that such entities simply do not exist, cannot be believed to exist, or cannot be meaningfully referred to as existing, a cautious nominalist might wish simply to suspend judgement and remain skeptical about certain types of existence claims. The point is that, at the very least, nominalism consists in a doubtful attitude toward the existence of universals.

Empiricism and nominalism seem to be somewhat comfortable concomitants in this regard. Empiricism can provide either an epistemological foundation for or a motivation towards a nominalistic perspective; that is, a nominalist may adopt empiricist arguments to defend his ontology or an empiricist may develop a nominalist ontology as an outgrowth of her empiricist commitments. Since most versions of empiricism are generally critical of claims involving non-experiential evidence or conclusions, and since universals and abstract entities are usually taken to be non-experiential and non-corporeal sorts of things,

empiricism may share with nominalism an uneasiness about such entities.

Quite clearly, empiricism is a wide ranging epistemological perspective, with many versions and variations. Some forms may straightforwardly imply a nominalistic outlook; others, in a decidedly opposite fashion, may actually be used to provide a basis for accepting universals and abstract entities as genuine existents. But, to the extent that a nominalism might be in search of an epistemology, it may adopt some sort of empiricist point of view. More importantly for the present discussion, empirical commitments may serve as the motivation for a nominalistic skepticism.

Universals and abstract entities are usually understood, by those who affirm their reality, as essentially non-sensory and non-corporeal. Thus, an epistemology that discourages accepting claims that are not justifiable in terms of sensation and experience would tend to be skeptical about claims affirming the existence of universals and abstract entities. This is not to say that some empiricist might not infer the existence of such entities from an experiential basis; rather, an empiricism may provide the justification for a skeptical attitude towards universals and abstract entities, or it may be the reason why someone is a nominalist in the first place. A nominalist can appeal to an empiricism that is skeptical

about ontological assertions regarding such purported entities. Thus, for nominalists, existential claims concerning real universals or common natures are not considered to be legitimate candidates for knowledge claims, or are at the very least open to serious reservations.

In this way, nominalism can be recognized as reflecting the ontological implications of the sort of empiricism which will not accept inferential knowledge of universals and abstract entities. It is a view as to what entities can be affirmed by the epistemological theory and what alleged entities cannot. The limited claim that whatever cannot be positively confirmed on acceptable empirical grounds is unreliably believed is a large portion of the skeptical dimension of a nominalist position.

I am not arguing that empiricism implies nominalism, although some forms might; nor that all empiricists have advocated nominalism, clearly this has not been the case. Instead I am suggesting that nominalism can be easily accommodated with an empiricist epistemology that endorses a skeptical attitude towards the sorts of entities about which nominalists are uneasy. Further, it is probably the case that many nominalists have maintained some version of empiricist epistemology. For example, William of Ockham, recognized as one of the earliest nominalists, believed that "the evidential base of all knowledge is direct experience of individual things and particular events."¹ He argued

that we have no experience of any non-individual, and so we have no evidential basis for belief in anything other than individuals. Although Ockham's nominalism is more fully argued from a logical analysis of terms, his empiricism does initiate and support a certain degree of epistemological skepticism regarding the existence of universals; Ockham's epistemology and ontology seem consistent in this regard.

To the extent that empiricism generates uncertainties or epistemological problems for accepting abstract entities as genuine existents, it gives rise to the sort of skepticism that may underlie or motivate the development of a nominalistic perspective. Nominalism usually does not tend to remain merely a skeptical position. As we will see later, nominalism is often a positive, substantive theory that attempts to provide an adequate account of existence in terms of individuals alone. Skepticism and an accompanying negative theory critical of abstract entities provides most of the impetus for the development of such a positive ontology. Before I address the positive and negative characterizations of nominalistic theories, I shall briefly discuss another perspective that frequently accompanies or encourages a nominalistic outlook or commitment.

Nominalism as a Philosophical Temperament

As some writers have pointed out, nominalism may be,

in part, what amounts to a "philosophical temperament". By philosophical temperament I mean something much like what William James described in his essay "The Present Dilemma in Philosophy".² There he argues that lying behind much philosophical reasoning is "no conventionally recognized reason"³; rather there is a "bias" that "loads the evidence for [the philosopher] one way or the other".⁴ Much of a philosopher's efforts, under this view, aim at developing reasons and theories that support or conform to a given temperament. A philosophical temperament is a bias, a disposition, an intuition, a preference. It is a fundamental sense of what makes for an adequate philosophical theory, and functions, to some extent, as the starting point for much philosophizing. As such, the philosopher "trusts his temperament" and tends to regard those with opposite temperaments to be misguided or "out of key with the world's character."⁵

James appears a bit ambivalent about these temperaments. On the one hand, they clearly do not rationally justify a given philosophical approach or system; on the other hand, he seems to think they are inevitable and that we ought to acknowledge them and accept whatever valuable insights they might provide. He does not try to explain the source of these preferences, only their presence and apparent influence.

Although James does not discuss nominalism

specifically, note what W. V. Quine and Nelson Goodman, prominent twentieth-century nominalists, have to say about their own nominalistic tendencies. "Why do we refuse to admit ...abstract objects...? Fundamentally this refusal is based on a philosophical intuition that cannot be justified by appeal to anything more ultimate."⁶ In a similar vein Goodman claims "a philosopher's conscience gives him little choice in the matter."⁷ Elsewhere Quine observes that some philosophers simply "have a taste for desert landscapes",⁸ alluding to the nominalist's emphasis on and preference for ontological economy.

Whether or not such philosophical temperaments can or should be avoided in sound philosophizing are interesting and debatable questions, but not of concern here. I wish simply to recognize the role they may play in motivating certain philosophical efforts, and to describe how a philosopher's desire to defend a nominalistic view of the world and its constituents may originate in a philosophical temperament. Nominalism is, to some extent, a way of seeing the world. Reasons can and should be provided for such a perspective; but what may often motivate the pursuit of such reasons is an intuition, a general sense, that the world is a "world of individuals".⁹

Nominalisms both affirm and deny something. They affirm that anything that can be said to exist is an individual. What is meant by this claim will be discussed

later. At this point I shall attend to what nominalistic views deny.

Nominalism as a Rejection of Abstract Entities

Historically, nominalisms originated as theories rejecting the existence of universals, or the need for them in an adequate theory of knowledge or description of reality. In the medieval controversies over such matters, the pole opposite the realists', those who affirmed the notion of real universals, was the position represented by nominalists such as Ockham. The concept of a universal to which these nominalists reacted extends back at least to Plato and Aristotle.

For Plato universals represented true reality. Universals, (also referred to as Forms or Ideas), were essentially non-corporeal, non-particular, and non-sensible. They existed independent of and prior to human thought and to the particular things that were said to "participate" in them. Thus, universals provided the necessary basis for knowledge, language, and reality.

Aristotle also believed that universals were real, i.e., actually existing, however, he did not think they existed independently of the particulars they informed. Whereas, for Plato ideas existed outside of particular things, for Aristotle universals existed within individual substances. Aristotle defined a universal as "that whose

nature is such that it may belong to many".¹⁰ A universal was something that was common to many individuals, it was something that could be predicated of various subjects. An individual, on the other hand, was something that could not be so predicated.

A host of problems concerning these conceptions of universals were addressed during the medieval period. At this time, three main positions with respect to universals developed, of which nominalism was one. In order to clarify the distinctions between these positions, let us consider the following two questions that were of concern to the medievals. The first question was, "to what extent are universals mind-dependent entities"? The second question radically challenged the whole tradition of Plato and Aristotle: "are universals real entities at all"? The three views that emerged from differing responses to these questions are well known as realism, conceptualism, and nominalism.

These perspectives on the problem of universals are not thoroughly discrete doctrines. Variations within each and difficulties in characterizing boundary regions between the views make distinguishing them clearly one from the other somewhat controversial. It is not my purpose to settle any historical disputes over these matters; but it is helpful nonetheless to demarcate generally the theoretical territory concerning universals apparent at this time, since

similar distinctions have persisted down to the present. I shall distinguish the three ontological positions in terms of the two questions mentioned above.

Realism is the name for any view in this context that regards universals as entities existing independently of the mind. The common or general features of reality are discovered not constructed. Whether universals exist only within particulars or ultimately lie outside them is a question that distinguishes types of realism, not realism as such. According to realists then, general words name actually existing non-individual entities understood as universals, kinds, species, etc. Thus, realism answers the second question, "are universals real?", by affirming the existence of real universals; and answers the first question, "to what extent are universals mind-dependent?", by claiming that they exist independent of any minds.

Conceptualism alternatively answers the question concerning the extent of mind-dependency on the part of universals by claiming that generality is essentially a product of mental abstraction. That is, universals are wholly mind-dependent. They are thus essentially mental entities, concepts constructed by and existing only in human thought. Some sort of objective basis in things is typically acknowledged, but how this is to be understood without returning to realism is the chief difficulty for this position, and is treated variously by different

conceptualist thinkers. For conceptualists, although universals are completely mind-dependent, nevertheless, it can be properly said of universals that they exist.

Nominalism denies the existence of universals altogether. Only individuals exist. Universals are nothing other than general words or "syncategorematic" terms. As general words, universals do not refer to anything other than the particulars to which they are applied. Ockham regarded universal terms as mere signs; "...every universal is one singular thing. Therefore nothing is universal except by signification, by being a sign of several things."¹¹ As syncategorematic terms, universals do not refer at all. Such terms serve certain logical functions within language, but do not themselves signify anything. They are meaningless except insofar as they are associated with or linked to categorematic terms, i.e., terms that do refer to or signify something. Thus, for nominalists, universals are not existents, rather they are linguistic devices devised in human thought.

We should note three principal reasons why Ockham, and other medieval nominalists, rejected real universals. First of all, they are not evident to the mind in direct experience. What is apprehended through experience by the mind is always individual, and universals, (as conceived by realists), are not individuals, therefore, experience cannot give us knowledge of universals.

Secondly, universals are not needed to explain how individual things come to have the characteristics they have, nor are they needed in order to have knowledge of individuals and their descriptive features. According to nominalists, language and reality can be adequately accounted for with reference only to individuals, (individual objects or individuals signs). Such an ontology is preferable, according to Ockham, because it is simpler. Here is an important application of Ockham's famous "Razor" to his own philosophical efforts. Universals are problematic partly because, it is argued, they unnecessarily multiply the number of entities required to adequately describe reality. For the nominalist, there is a certain number of individual green leaves, for example. A realist's universe has the same number of green leaves, but has in addition a universal 'Leaf' and another universal 'Green'; increasing the size of the universe by two. But that is not the end of it. Additional constituents of the realist's universe include the universal 'Color' and 'Plant Part', and 'Living Thing', and 'Physical Object', and so on, and so on. This sort of ever multiplying universe is unbearably complex and unwieldy to Ockham and other similarly minded nominalists.

Thirdly, realism apparently leads to various self-contradictions. For example, a universal is either one thing or many things. If it is one thing than it is an

individual and cannot be common to many things. If it is many things than each thing is an individual and these cannot be common either.¹²

Subsequent to the medieval period, nominalism or nominalistic tendencies appeared regularly, often as a rejection of universals or abstract entities. Hobbes argued that although certain names may be "common names", (i.e. they may apply to more than one individual), these do not name any kind of entity other than the particulars to which they are applied. Thus, the only universals are words. "What Hobbes really meant was that universals only come into being with classifying, and that classifying is a verbal technique."¹³ The objects of the world are all individual and unique. General words simply allow us "to speak of many of them at one and the same time."¹⁴

The British empiricists, Locke, Berkeley, and Hume, whether they were, strictly speaking, nominalists or conceptualists, rejected any notion of universals as independent or outside of the mind. Their reasons for this rejection were largely born of their empiricism. Locke held "that general and universal, belong not to real existence of things; but are the inventions and creatures of the understanding, made by it for its own use, and concern only signs, whether words or ideas."¹⁵

Berkeley rejected abstract ideas as well as universals. He said, "... I deny that I can abstract from

one another, or conceive separately, those qualities which it is impossible should exist so separated; or that I can frame a general notion, by abstracting from particulars...."16 He later adds, "... it seems that a word becomes general by being made a sign, not of an abstract idea, but of several particular ideas...."17

For Hume, all ideas originate in impressions. Impressions are particular and immediate sensations. Ideas are copies or images of impressions. Therefore, as all impressions are particular and definite, so all ideas must be particular too. Hume accounts for what are called abstract general ideas by claiming that they are actually particular images that "may become general in their representation".18

Our various impressions of particular dogs, for example, recalled as ideas, appear as resembling each other to some degree or other. We refer to each of these resembling ideas by using the same name, "dog" in this case. When we consider the word "dog" we cannot think of every dog or every idea we have of a dog; each of these is distinct and individual. Rather, it is a habit of the mind to bring to mind some or any individual idea of a particular dog when the general word is used. The general word thus names only individuals; and generality or universality is only a result of a certain habit of the human mind and of language.

The rejection of universals has been motivated by or

defended for various reasons. As Rolf Eberle has pointed out, "According to their temper, nominalists have tended to rule out unwanted categories of entities on grounds that positing their existence is contradictory, non-sensical, devoid of explanatory power, lacking in simplicity, unverifiable, or just plain suspicious."¹⁹ As this has been the case historically, so it has been in this century.

Positivists rejected universals as meaningless notions, since they are empirically unverifiable. Philosophers of the latter half of this century have continued the tradition. D. F. Pears has argued that the claim "universals exist" is not only an unverifiable statement of fact, but also that realism is dependent on circular reasoning in affirming the existence of universals.²⁰ Quine and Goodman, in "Steps Toward a Constructive Nominalism" rejected all abstract entities.²¹ Later, Goodman revised his view, (as did Quine, but in a different direction), to argue that "Nominalism...consists specifically in the refusal to recognize classes."²²

Contemporary nominalists have also refused to accept, or have been uneasy about, entities such as concepts, meanings, propositions, and an infinity of objects.²³ Their alternative ontologies countenance only individuals. To the extent that these other alleged entities, (universals, abstract entities, kinds, classes, meanings, concepts, etc.), are not construed as individuals, they are not

acceptable constituents of an adequate description of reality.

My purpose in presenting this very brief review of some historically significant nominalists is twofold: first, to show how nominalism has been a perennial perspective; and second, to indicate some of the general reasons why some philosophers have rejected universals and other purported non-individuals. In summary, nominalisms have rejected any notion of real universals or abstract entities for the following basic reasons:

(1) only individuals can be experienced, therefore the existence of universals cannot be verified, (this typically presupposes some version of empiricist epistemology);

(2) universals are not needed in order to adequately account for knowledge or existence, (it is argued that other descriptions are possible);

(3) theories of universals needlessly multiply entities, (such theories are at odds with various criteria of simplicity or parsimony advocated by some philosophers as marks of theoretical adequacy).

(4) theories of universals are purported to contain or lead to contradictions, circularity, or infinite regresses of undesirable sorts.

Nominalism does not end with its rejection of universals, abstract entities, or the like. A principal program for nominalism is to account for what is in terms of

individuals alone. A nominalistic analysis of reality determines not only that universals or abstract entities are suspect or non-existent, but that whatever can be said to exist is always an individual. It may be said of a nominalistic theory: to be is to be an individual, (or a particular--I shall use the terms interchangeably in the present context).

Nominalism Affirms the Existence of
Individuals Only.

The main challenge for the nominalist is to construct an adequate ontology following on the conclusions that universals do not exist and that what does exist is always an individual. Nominalism must account for a crucial element of our experience of the world, namely, the apparent recurrence of qualities.²⁴

The items of experience appear distinct and individual largely because of their separation in time and space. In addition, they are described as having features, (color, shape, size, etc.), that further distinguish them one from the other. But it is readily apparent, in spite of the obvious uniqueness that holds between the constituents of our experience, that we also recognize recurrences of many of the features we observe.

Our language reflects these observed recurrences. General words provide us with terms that permit us to speak of various individuals grouped together according to

recognized recurrences. The challenge for the nominalist is to describe this linguistic practice and the experiences that give rise to it with reference to individuals only.

The apparently recurrent "green" we speak of with respect to the various leaves on a tree suggests different interpretations. Do we call each leaf "green" because they each have some thing in common? Or is this practice a result of a recognition of some fundamental resemblance between the experience of green in one leaf with that of another? Or is the "recurrence" ultimately reducible to the general applicability of the word "green"?

Each of these questions reflects a different view on the matter. The realist regards recurrences and the general words that name them as referring to something both objective and universal. Objective in the sense of being to some extent extra-mental; that is, the recurrences are part of the way the world is, they are not mere mental abstractions or creations of human thought. They are universal in the sense of being non-particular and having the capacity to be common to many things. Recurrences are recurrences of qualities that may be "present in distinct individual things at the same time."²⁵ They are, in effect, repetitions of essentially the same qualities. The realist approaches the issue of recurrence from the side of the objects which reveal recurrences. In this way recurrence is considered a phenomenon of objects.

At the other end of the spectrum is the nominalist who approaches the issue of recurrence from the side of language. Recurrence is a phenomenon of language not of things. That is, the instances of a recurrent quality "are related only by the fact that they are the objects of the applicability of one and the same general word."²⁶ The principal task for the nominalist in this context is to account for the applicability of general words.

One approach might be to leave this general applicability of certain words unexplained, attributing general use to nothing more fundamental than convention. Such an explanation implies that groupings under general terms are wholly arbitrary or, at most, merely practical. Calling the leaves on the tree "green" and "leaf" is nothing more than a convenient and customary way of referring to more than one thing at a time. This approach is not very satisfying since it denies that there is any sense of recurrence in our experience, and our experience certainly seems to involve some sort of recurrence. It also seems implausible because it fails to account for our ability to use general words in consistent ways when we encounter new objects. Our experience of what appear as recurrent properties and our use of language in consistent and predictable ways implies that what is going on is not entirely arbitrary.

A more promising strategy for accounting for apparent

recurrence is to show how all uses of general words can be translated into discourse in such a way that reference to universals or abstract entities is neither implied nor needed. Two versions of this approach are worth examining.

Quine and Goodman have argued that all statements using general words can be translated into equivalent statements in which these general words occur as predicates. It is maintained that these predicates are genuinely meaningful but are not names. That is, they do not refer, as such, and thus do not commit us to the existence of any entities. As Quine has pointed out "being a name of something is a much more special feature than being meaningful."²⁷ Something meaningful and true can be said of a leaf when we say of it that it is green without also claiming that "green" names some entity. In this way, names that purport to name attributes, universals, recurrences, etc., can be regarded as predicates and converted into descriptions. "Whatever we say with the help of names can be said in a language which shuns names altogether."²⁸

Quine has argued that the ontological commitment of a theory is measured by the logic of our discourse. That is, we must accept as entities in our ontology only those items the terms of which function as values of a variable in a quantificational translation of our language: "a theory is committed to those and only those entities to which the bound variables of the theory must be capable of referring

in order that the affirmations made in the theory are true." 29

"The leaf is green" is translated as "Something is a leaf and is green", $(\exists x)(Lx \cdot Gx)$. Such a statement commits the speaker only to those entities that must be substituted for the variable 'x' in order that the proposition above be regarded as true. Such a statement commits the speaker only to individuals in this case because anything that is a green leaf is an individual. No commitment is made, no acknowledgment given to the existence of either "leafness" or "greenness" insofar as these are taken to refer to abstract entities. The terms "green" and "leaf" are regarded as syncategorematic terms; that is, terms which serve a logical function in a language, are meaningful only in a context that includes categorematic terms, (terms that refer to objects), but do not name anything.

Now consider the statement "Green is a color". This appears to commit the speaker to abstract entities since this statement makes no reference to any individual but only to general qualities "green" and "color". The nominalist addresses this seeming problem through a use of the universal quantifier. "Green is a color" is translated as "Anything that is green is colored", or $(x)(Gx \rightarrow Cx)$. Again such a statement commits the speaker only to individuals since every instance of 'x' can be substituted with a reference to an individual object. In other words, saying

"Green is a color" really amounts to saying that anything that can be called green can also be called colored. There are only green things and colored things. "Green" and "color" do not refer to or name anything.

Not all statements are so easily dealt with by this strategy. For example, mathematics consists of various statements that are not nearly so amenable to the sort of translations that resist entailing commitment to universals. A statement such as "There is a prime number greater than one million", $(\exists x)(Px \cdot x < 1,000,000)$, says that "there is something which is prime and exceeds a million; and any such entity is a number, hence a universal."³⁰

The project of characterizing mathematical statements in purely nominalistic language is quite complicated and is an ongoing one. It is beyond the purposes of this paper to analyze these developments, but it worth acknowledging some of the ways in which nominalism has been only partially successful in providing the means for translating different areas of discourse. But it should be quite obvious that having not solved every problem is not the demise of a theory. Three things can happen as a result of the nominalist's efforts: (1) success in nominalizing a realm of discourse; (2) a setting aside of the problem area until more promising methods or strategies are discovered or developed; or 3) renouncing certain ways of speaking as

ontologically unacceptable.

For example, Quine apparently recognized the significance of option (3) mentioned above when he remarked that, "bound variables for classes or relations or numbers, if they occur in existential quantifiers or in universal quantifiers within subordinate clauses, must be renounced by the nominalist in all contexts in which he cannot explain them away by paraphrase."³¹

Another important contribution to nominalistic theory is Goodman's rejection of class-membership in favor of a part/whole relationship as a description of general terms. In an attempt to avoid appealing to universals as a way of accounting for recurrences, some have proposed instead making use of classes. "Red" is thus understood as a name referring to "the class of red things". To be called red is to be recognized as a member of this class. But for some nominalists, classes are no less problematic than are universals. Interestingly enough, Goodman has actually defined nominalism as that view which "consists specifically in the refusal to recognize classes."³² Whereas in earlier formulations Goodman, along with Quine, defined nominalism in terms of a rejection of abstract entities, later he preferred "to characterize nominalism as renouncing all nonindividuals"³³; and indicating further that classes are not genuine individuals. Goodman develops his distinctive position on this point by claiming that "while the

nominalist may construe anything as an individual, he refuses to construe anything as a class".³⁴ And further that, "whatever can be construed as a class can indeed be construed as an individual, and yet a class cannot be construed as an individual."³⁵ To satisfy these conclusions, Goodman has argued that what can be described in terms of classes can be better described in terms of discontinuous wholes. For Goodman, an individual "need not have personal integration".³⁶

For example, instead of speaking of "the class of red things", Goodman would have us recognize each red thing as merely a part of the discontinuous and scattered whole individual "red". As most any nominalist would have it, "red" does not name any existing universal nor any abstract entity such as a class. But for Goodman "red" need not be disregarded as a name, but can be understood rather as the name of an individual comprised of many heterogenous and widely separated parts. This indicates something of the meaning of his assertion that whatever can be construed as a class can be construed as an individual. The so-called "class of red things" is fundamentally made up of individual red things by means of the relation of membership. The notion of class depends upon this proposed relation between class members. Goodman replaces the relation of membership with a part/whole relation and thus eliminates any reference to classes.

Goodman acknowledges that such a conception of individuals may stretch the imagination, but he is not willing to concede that it requires any more imagination than that required to accept the "platonist's" thesis regarding classes and universals.

Goodman's rejection of classes follows from what he takes to be a fundamental nominalistic assertion: no distinction of entities without distinction of content. That is, no "two different entities can be made up of the same entities".³⁷ The countenancing of classes allows for the generation and multiplication of entities; in this case, classes of classes.

The platonist, (Goodman's name for anyone who accepts classes or any other nonindividual as an entity in his/her system), and the nominalist may actually agree as to the atoms, (basic individuals), that go to make up their respective systems. The nominalist recognizes as entities all individual atoms and any sums of atoms forming additional wholes, (construed as individuals). As long as each sum-whole is composed of different combinations of the atoms, each is accepted as a legitimate entity in the nominalist's ontology. These are all and the only entities the nominalist's system can "generate".

The platonist, on the other hand, recognizes these same individuals, except the platonist construes the sums as classes. So far so good for both nominalist and platonist;

that is, the sizes of their respective universes are essentially the same, and neither party has any significant objection to the other's conception of the universe. But the nominalist objects to what the platonist's system is now capable of generating; namely, additional entities comprised of the same basic atoms. The platonist countenances the generation, by means of the membership relation, of additional classes out of the initial classes formed of the system's atoms: classes of classes. And what is more, these classes of classes may generate classes of classes of classes, and, at least theoretically, so on ad infinitum.

Such a populous universe not only offends the nominalist's minimalistic temperament, but, more importantly, it results in a virtual infinity of non-individuals which are, in terms of content, essentially indistinguishable from one another. As we have seen, from a nominalist's point of view such as Goodman's, there can be no distinction of entities made up of the same atoms; this is redundant and unparsimonious. To permit classes of classes is to permit entities made up of the same atoms. Therefore, the nominalist should not accept classes as constituents of her ontological system.

I should like to describe one final approach to translating language which refers to recurrences. Such an approach attempts to rephrase all statements containing

general words in terms of "names of particular objects, forms of the verb 'to resemble', and the phrase 'as closely as' ".³⁸ In other words, this position recognizes resemblance or similarity as fundamental to our experience of objects. But in doing so, the nominalist making use of such an approach does not acknowledge that "resemblances" exist, nor does resembling refer to, or name, any thing. "To resemble" may be, thus, regarded as syncategorematic.

Under this view, a statement such as "'a' is blue" might be translated as "'a' resembles 'b', 'c', and 'd' at least as closely as 'b', 'c', and 'd' resemble each other," where 'b', 'c', and 'd' are the exemplars of the meaning of the word "blue".³⁹ Thus, general words are understood as convenient ways of referring to conventional or practical groupings of objects. But note that resemblance under this approach is a matter of comparison to a group of standard objects or particular exemplars, not in terms of "resemblance in some respect". To speak of "some respect" has been taken to imply or suggest a realistic interpretation of resemblances, and so is preferably avoided in nominalistic accounts. An opponent of the nominalist's appeal to resemblances might charge that resemblance is a derivative relation requiring some reference to a universal. The argument might go something like this:

whenever we say that A, B, and C resemble each other in a certain respect, we shall be asked "In what respect?" And how can we answer, except by saying "in respect of being instances of the universal θ " or "in respect of

being characterized by the characteristic Θ ? 40

The nominalist must avoid either of these alternative expressions. This is probably one of the greatest difficulties for nominalism: how to account for what appear as observed resemblances without falling into the realistic or platonistic language of "respects".

One attempt at solving this difficulty is to interpret resemblance, as has already been mentioned, in terms of a comparison to standard objects or exemplars, (which are themselves individuals), rather than in terms of some universal. It is important to recognize that, for the nominalist, if resemblance is to be admitted into the system at all, it must be construed either as a syncategorematic term or as an individual, not some abstract relation, which requires some resemblance-universal in order to explain the phenomenon.⁴¹ For instance, the particular green of one leaf is said to resemble the particular green of another at a particular time, under particular circumstances, (light, perspective, distance, etc.), by a particular observer, and so on. The experience of resemblance between the two leaves is an individual experience that has nothing "in common" with any other individual experience described in terms of resemblance.

But a greater problem arises from within the context of this noticed resemblance. As noted above, it seems that we recognize how things resemble one another only in terms

of some "respect": green leaf A resembles green leaf B in respect to their color. The green color of leaf A is not claimed to resemble the oval shape of leaf B. How can a nominalist account for this aspect of such observed resemblances?

One response is that our language reflects certain habits of classification. That is, the individual objects and experiences that make up the world we experience appear to us with certain discernable individual features, and we observe what appear to us as more or less similar appearances of these individual features. But language makes an important contribution to these observed similarities. Our language adopts predicates that serve to identify useful groupings of what appear to us as similar features. Similarity or resemblance are themselves features of our experience of things, but our language conditions us, to some extent, to look for certain similarities. Thus, "respects" are actually items of experience for which we have predicate terms. The two green leaves do look more or less alike, but we need not say that this is due to some common respect. Rather, green leaf A and green leaf B are experienced in ways describable as "similar" to some extent, and our language gives us predicate terms 'green' and 'leaf' that have proven useful in grouping and classifying such items of experience.

What allows us to link specific predicate terms with

specific individuals is the similarity we attribute to the new individuals and other individuals that we take to be exemplars of the relevant predicate terms. We have now returned to the earlier notion of translating general words as predicates indicating comparisons to groups of standard objects or individual exemplars.

Summarizing, some nominalists propose to translate or interpret the language of recurrences not in terms of universals or general characteristics, but instead in terms of the resemblances and similarities. But similarity is a significantly relative concept. Our experience of similarity is greatly affected by the predicate terms we have at our disposal, our interests and purposes for acknowledging or attributing similarities, our individual sensory apparatuses, etc. Such a nominalistic approach consists in arguing that similarity, (resemblance, recurrence, or whatever), has less to do with the way things are than with how we choose to describe our experiences with language.⁴²

Nominalism as a Limiting Theory

One final conception of nominalism I should like to consider briefly is the capacity for a nominalistic approach to serve the theoretical function of delimiting the range of possible ontologies. By this I mean that nominalism is quite clearly an extreme position. It represents the far

extent to which an ontological theory might go in restricting existence to individuality.

In any attempt to develop a theory accounting for some phenomena, one useful way to proceed is to determine the range of plausible theories that might do the job. From there, weaker theories may be eliminated and further investigation may proceed on to more promising ones. In ontological matters, one of the principal questions of concern is, to what extent is what exists individual or not? Nominalism is the theory that takes the extreme position that anything that can truly be said to exist must be an individual. Each thing that exists is a particular, there are no non-individuals, universals, abstract entities, kinds, etc., except as linguistic devices. Some of the reasons why some theorists have held out in favor of such an ontological theoretical perspective have been examined in what has preceded. My purpose at present is to point out the general theoretical value of and interest in developing a coherent limiting theory, a theoretical perspective lying on the far reaches of viable possibility and plausibility. Even if one chooses in the end to reject the perspective, I believe that the exercise is worthwhile nonetheless, if only to contribute to a process of elimination.

Nominalism helps to "lay out the territory" and establish a sense of perspective with respect to certain ontological problems. Any investigation into the

ontological make-up of some part or the whole of the world must take account of the various key alternatives. As was discussed in the Introduction, diverse strategies and approaches have been applied to the ontological status of artworks. A complete appraisal of the matter must include an assessment of a nominalistic perspective, as well as others. (Consistent with this belief, I shall examine three other theories that I take to be "limiting theories" in Chapter IV.)

For those already inclined towards nominalistic thinking, development of a nominalism with respect to the subject at hand will be the first order of business. But for those less attracted to nominalism, or even hostile towards it, I would suggest that consideration of this perspective might be compared to looking at the back side of a statue. Although the "best" view may actually be a frontal one, such a conclusion is better justified and more confidently maintained only after examining such seemingly strange perspectives as the back or even underside of the statue. In analogous fashion, nominalism may appear as an "underside" ontological perspective to some, but I think it profitable to take a look anyway.

Summary

My intention in providing this chapter is to give some further sense of context to the subsequent presentation of

aesthetic issues. The first chapter stated the ontological problems associated with the arts generally and music specifically. I have now drawn the focus upon a decidedly nominalistic outlook. What I take nominalism as such to be and to entail has been the purpose of the preceding sections. In what follows a nominalistic theoretical approach will be applied to an ontological problem area in music. So far I have tried to explain how nominalism arises partly from a skeptical response to claims regarding the existence of universals and abstract entities, and partly from what I have called a philosophical temperament, which consists of a general sense or philosophical intuition that whatever exists is an individual.

From a description of these initial tendencies or preferences I moved on to a brief survey of some basic theoretical reasons against belief in universals, and then to a characterization of some rudimentary means of construing existence and experience in terms of individuals alone. Finally, I described what I take to be part of the value of nominalistic theorizing: to search out and explore the limits of plausibility in constructing a particularist ontology in an area of interest that is somewhat unsettled.

This chapter has not been an attempt to defend nominalism per se, rather it sets the stage for what I will regard as a genuine defense of a nominalistic proposal. I hope now to provide in what follows a reasonable and

defensible nominalistic ontology of musical compositions.

CHAPTER III

MUSICAL COMPOSITIONS AS CONCRETE PARTICULARS

Introduction

In this chapter I shall develop in specific terms my proposal for a nominalistic ontology of musical compositions. As described earlier, in Chapter I, this will require a careful consideration and application of nominalistic presuppositions and standards to performances, scores, and recordings as well. So, although the focus is upon compositions, much will be said about the various other products of musical activity, and thus, about music as a whole.

A commitment to nominalism is, in large part, a commitment to descriptions of what there is in terms of individuals alone. Therefore, the sense in which the proposed theory is nominalistic is that it maintains the view that musical compositions, (and any other musical artwork, for that matter), are individuals and not any sort of purported non-individual such as a universal or other abstract entity. In addition to the claim that compositions are individuals, I am also claiming that compositions are concrete, that is, they are temporally and spatially unique individuals, and as such I thus maintain that they are

physical objects. Therefore, the approach developed here not only specifies an ontology of musical works which is particularistic, but one that is also physicalistic. In saying this, I am not claiming that a nominalistic ontology must be physicalistic, instead I am claiming that this nominalistic theory of musical works is to be understood in terms of particular physical objects.

The principal reason for holding this view is that I take it that all artworks are physical objects. A physical object is something that is spatial and temporal, it can be identified with some position in space and it can be described as beginning to exist at one time, existing for some duration of time, and ceasing to exist at some other time. Under this view, events will be treated as physical in the sense that they occur in some locatable place and at some identifiable time.

Physical objects can also be described as phenomenal objects; that is, such objects can be described in terms of the way in which perceivers perceive them. For example, a chair can be described as a physical object in spatial and temporal terms such as "the chair is three feet tall and was sitting in my room yesterday." The chair can also be described a phenomenal object in this manner, "the chair appears brown and feels comfortable."

The bearing this has on art objects is that they too can be described physically and phenomenally. "The

performance of Beethoven's 'Ninth Symphony' last night filled the concert hall with intense sounds." "Last night" gives the event its temporal location, "concert hall" gives it its spatial location, and "intense sounds" can be interpreted as describing the event as consisting physically of air particles moving at a certain frequency. The performance can also be described phenomenally in such terms as "The 'Ninth Symphony' was often loud, yet at times sounded quite soft." This latter statement describes how the event was perceived by some listener.

It is my position, then, that all artworks are publically accessible artifacts, that is, they are unique physical objects that are made by some person or persons, usually referred to as artists, and that these artifacts are perceivable, i.e., phenomenally describable entities capable of having spectators. Quite simply, I am arguing that without a perceivable artifact, artworks cannot be said to exist. Without going too far into the matter here, let me call on the plausible intuition that I cannot be said to have made an artwork if I "have nothing to show for it"; what could my artwork be if it is not presentable for the inspection by others. I made this point earlier in Chapter I, and I shall have more to say about it later. I shall not argue for the larger thesis regarding the physical status of all artworks, but I shall argue the case for physical musical artworks in what follows.

It could be said then, that the theory under construction, which regards musical artworks as concrete particulars, is actually formulated on the basis of two ontological commitments: a nominalism that countenances only individual entities and a physicalism with respect to artworks that regards all genuine artworks, in this case, musical artworks, as physical entities.

The plan for this chapter is as follows. First, the basic terminology which makes use of the words work, artwork, composition, and performance must be defined and clarified, along with other related terms such as score, manuscript, interpretation, improvisation, and recording.

The next main objective of the chapter will be to explain the notion of "musical elements". Musical elements are, most simply, the materials out of which musical artworks are made. In other words, they are the things which are combined or grouped together to make compositions, performances, scores, etc. More specifically, with respect to compositions, musical elements are the simpler particular items that composers select and arrange and combine together in order to compose their musical works, which are thus understood as compound particular wholes. Some musical elements will turn out to be sounds while others are not.

Following this, I shall begin to develop a characterization of the ontological status of musical compositions by first describing the process of composing a

musical work. As I shall explain it, composing a work consists of some person (or persons) intentionally selecting and arranging musical elements for the purpose of making what that person (or persons) believes to be an original musical composition. It will be pointed out that not all composing is composing a work, just as, for example, not all writing is writing a book. "Composing" is, generally speaking, an activity of selecting and arranging musical elements in what is believed to be an original way.

"Composing a work" includes the intention to create an original musical work. The details of this process will comprise a crucial portion of the specific nominalistic theory under consideration. This description of how a composition is made will of course be coordinated with a description of the product of this activity: the composition. The composition, under this view, will thus be the physical (and phenomenal) artifact resulting from some person's intentional effort to make what is believed by that person to be a (somewhat) original combination of musical elements into a musical work. Since each of these musical elements is itself an individual entity, the composition is itself a complex whole comprised of some specifiable number of simpler parts. This whole is itself a temporally and spatially unique (physical and phenomenal) individual.

Once the process of composition-making is described, the relationship between the product of this process, i.e.,

the composition, and other musical objects, (performances, scores, etc.), will be delineated in some detail. This relationship will be explained in terms of what I will call "copying" and "deriving". These notions will be understood primarily in terms of an intention on the part of some person or persons to make a musical object explicitly associated with some already existing composition. The "connection" between composition and performance, for example, thus will be defined in terms of temporal precedence on the part of the former entity and of beliefs about the extent of originality with respect to each, and not in terms of any essential similarity or common qualities. It is believed that such a characterization of these connections or relationships will avoid such anti-nominalistic language of universals such as "instantiation of", "participation in", or the language of classes such as "member of".

Further development of some of these features of the theory will be presented by way of comparisons with some other, variably different alternative theories of compositions in Chapter IV. By providing such comparisons it is hoped that the present theory will be better understood as situated within a range of proposed approaches to musical ontology.

Some Terminology and Distinctions

For the sake of clarity and consistency, I shall describe my use of certain key terms. In doing so, my chief concern is to explain them in a manner consistent with the overall nominalistic theory of music I intend to construct, and yet in a manner not particularly foreign to conventional usage. Thus, not only will I define some terminology, but in so doing I hope to present some of the basic conceptual framework required for the central thesis.

For the purposes of this dissertation, I intend to use the words "artwork" "musical artwork" "compositional work" "composition" and "musical composition" as interchangeable or synonymous terms. These will refer to the products, (artifactual objects), of a composer's act of composing a work. These terms are to be applied to those musical entities regarded as having the requisite degree of originality to qualify as genuine compositions. What is meant by "requisite degree of originality" will be discussed in the relevant sections of this chapter. Under this view, originality will be regarded as a function of belief on the part of the composer and not a matter of inherent novelty with respect to the composition.

"Work", "work of art", and "musical work", on the other hand, will be used as the more general terms for most any kind of musical object.¹ Musical works thus include compositions, as well as any other non-compositional musical objects that should be accounted for in a theory of music.

In other words, some or all of performances, recordings, scores, improvisations, etc., may be and have been regarded as musical works of art. In music, compositions are but one sort of musical work--not all musical works are understood to be compositions. For example, it is not uncommon to consider musical performances to be musical works of art of a sort. Put another way, the claim could be expanded by saying compositions are not the only musical objects that are art objects. Thus, it follows, given the present terminology, that while all musical compositions are musical works, not all musical works are musical compositions.

Perhaps the most important distinction within the practice of music is the one between composition and performance. As I shall describe them, they are not mutually exclusive terms. Although most of the time what is a composition is not a performance and what is a performance is not a composition, we shall find that sometimes a single musical work may be referred to properly as both a composition and a performance.

"Performance" will be the name used to denote certain individual musical works which are comprised largely, if not entirely of sounds. More specifically, performances are events consisting of actual musical sounds occurring in a particular sequence during some specifiable period of time. As such, a performance will often be referred to here as a sound-sequence-event. But not all occasions of musical

sounds are performances. Performances are those musical sound-sequence-events that are either compositions themselves or derived, (or copied) from a composition. A performance is thus always either identified with a composition, (i.e., it is a composition) or it is associated with a composition, (i.e., derived or copied from an already existing composition). Most performances turn out to be of the latter sort. The former consist largely of improvisations, (which I shall argue are best understood as compositions); although I would also hold that there are some compositions which are performances but are not improvisations.

This point can be made as follows: since compositional works are defined in terms of intended originality, and since improvisations are produced by musical performers who believe they are selecting and arranging musical sounds in a somewhat original way, it seems sensible to consider improvisors to be composers, and what they make to be compositions; (I shall return to this point below). Thus, improvisation is typically described as music that is created (composed) at the same time as it is performed; a kind of spontaneous or extemporaneous composition. But some composing gets done in a more piecemeal, deliberative fashion. A composer may compose a musical work by playing a musical instrument and by combining and recombining actual musical sounds made with

the instrument. Unlike improvising this composing activity need not occur all at one time, nor need it always result in a completed work at that time. It may involve working out parts of the work, remembering what was played, and eventually fitting the various parts together into a completed whole. I will suggest that it is the first playing of this whole sound-sequence that constitutes the composition.² Such a sound-sequence is a performance and a composition, but it is not an improvisation. (Further details concerning this sort of composing will be forthcoming.)

Examples of musical sound-sequence-events which are not performances would be recordings of performances, (recordings of performances are not generally regarded to be themselves performances)³; and mere practicings⁴, (e.g., a musician playing scales or warm-up exercises). The former are generally ruled out for not consisting of sounds made directly by the performer or performers, and the latter are ruled out for not being associated with a composition.

Performances are defined then in terms of the following necessary conditions:

A performance

- (1) consists of a musical sound-sequence-event;
- (2) is made by some person or persons with the intention of producing a performance;
- (3) is associated with a composition, i.e., a performance is either a composition or it is derived from a composition.

The first condition states that all performances consist of actually occurring musical sounds. The second condition refers to the artifactual status of any musical work of art. And the third condition distinguishes performances from other musical sound-sequence-events, i.e., those not associated with any compositional work.

"Score" will refer to a musical work which consists of musical inscriptions or notational symbols. "Manuscript" refers to a score which is a composition; it is the result of some composer's original selection and arrangement of musical symbols; it is the composer's autograph score. But most scores are not manuscripts, rather they are copies (or most often, copies of copies), of manuscripts. Still others are "transcriptions". Transcriptions are scores derived from performances. A transcription is made by someone who listens to a sounding of a work of music and represents what is heard in terms of musical notatational symbols. Transcribing is analogous to taking dictation. As such, a transcription may be derived from an improvisation or it may be derived from a performance of a composition or it may be derived from a recording of some musical work.

Although most scores are copies or derivations of compositions and are not manuscripts, (i.e., compositions), as it happens, most compositions are scores, (i.e., manuscripts). The most controversial claim of this dissertation is probably the claim presently under

consideration: that a score can be and often is a composition, or conversely, some compositions are scores. Since most composers write scores, their artworks, I shall argue, are their manuscripts. I shall argue that it is this written score that is the particular artifact that many composers produce through their compositional efforts, and so, these manuscript scores are to be properly regarded as their artworks.

Musical "recordings" must be accommodated in any account of music, but they create puzzling difficulties. These difficulties arise partly because of a certain ambiguity associated with term "recording". In one sense, recordings are physical objects, (usually magnetically charged plastic tapes, grooved vinyl discs, or etched plastic discs), made through the use of specialized sound-recording and -playback equipment. In another sense, "recording" is used to refer to the sounds produced by playing the recording in the appropriate machine. Recordings are the products of what is typically a mechanical process consisting of converting sound-events into some other, less transient physical medium. But this is not always the case. Interestingly, recordings can be made without producing any sounds at all. Sounds are waves of moving air that are perceivable by hearers; but some electronic musical instruments can generate electronic impulses which are not sounds, yet these electronic signals

can be recorded and subsequently "played back" as audible sounds. Thus, a whole recorded piece of music could be produced without making any sounds whatsoever, yet this recording can later be used to produce actual musical sounds.

Sometimes "recording" applies to the physical object which is capable of being used to playback musical sounds. But "recording" also is used to refer to the musical sounds that are played back. When I say I have a recording of Beethoven's "Ninth Symphony" I can do so either by pointing at my tape cassette, for instance, or by directing attention to the sounds coming out of my stereo. If I say that I am "listening to a recording" the object of my listening is not the piece of tape, but the sounds produced by running the tape through the proper sound-reproducing device.

These recorded artifacts are used to make musical sound-events without the immediate or direct use of musical instruments or voices. The sounds resulting from the use of recordings in conjunction with the appropriate mechanical sound equipment are actually copies of previously occurring sound events. This is one way in which recordings differ from performances. In a performance, the performers, (the person or persons making the performance), directly cause the musical sounds to occur by playing their instruments, (or singing). The sounds resulting from the playing of a recording are not directly caused by a performer, rather

they are replicas of previously occurring sounds. To be sure, the sounds of the recording are individual sounds occurring at the time of playback and some of their character is due to conditions present at this time, and thus have an identity distinguishable from the sounds which occurred at the time of recording. But the proximal cause of the sounds at the time of playback is the machine and the tape or disc inside of it and not any performer's contemporaneous efforts. To some extent, a sound recording of a performance is comparable to a film of an event: they both are entities causally connected to prior events in such a way that they can produce sounds and sights, respectively, of significant similarity to those prior events. But such a recording does not consist of the same sounds as those that caused it; the sounds of the original event no longer do or can exist. The recording consists of new and distinct sounds distally caused by earlier sounds, and thus are noticeably similar to them.


Again, recordings involve the process of converting transient sound events into relatively permanent physical objects; physical objects of the sort that subsequently may be used to make new sound events. With this fact in mind, I shall, for the sake of simplicity, use the term recording to refer to the sound-sequence-events produced through a proper use of the recorded tape or disc (or whatever). Thus,

recordings are sound-sequence-events that are causally related to some earlier occurring sound-sequence-event.

Musical Elements

Musical elements are the stuff of which all musical works are made. They are the media or materials of music. I say media rather than medium because musical elements exist as differing physical entities. Some are apprehended by hearing them because they are sounds; others are apprehended by seeing them because they are inscriptions. Some groups of musical elements are appreciated for themselves: musical sounds; others are appreciated for what they may refer to: notational symbols and spoken descriptions. Put another way, musical elements are all physical entities, i.e., they are concrete individuals located spatially and temporally. But they are not all of the same material. Some musical elements are sounds, especially musical sounds, some are written inscriptions of specialized musical notational symbols, and some are spoken words denoting either these notational symbols or musical sounds. An example of a musical sound element would be an actual sounding of a particular pitch. This pitch can be described as a physical event consisting of air waves at say 440 cycles. Phenomenally, this musical sound element may be described as hearing an 'A' pitched sound.

An example of a musical symbol element would be the

inscription  . This symbol is generally used to refer to an 'A' pitched sound, but this notational inscription has its own status as a physical entity, (it is an ink mark on this page of paper). Notational symbols such as the one above are not the only inscriptions that are musical elements. The written words "A sharp major" or "'A' pitch in key of C" are also musical symbol elements.

An example of musical symbol element that is spoken would be actually uttered words "A sharp major" or "'A' pitch in the key of C". Their physical and phenomenal status is comparable to that of musical sounds, except that spoken words are not themselves musical sounds as such, rather they are sounded symbols denoting actual musical sounds. Spoken musical symbols should not be confused with musical sound elements which are sung words. Sung words are musical elements not because they are words, but because they are voiced musical sounds, i.e., musical sounds made with vocal cords. That these sounds are also words is not relevant to their status as musical sounds.⁵

All musical works are made of some combination of musical elements. These musical elements are either sounds or symbols. These latter exist as either written inscriptions or as spoken words. As I shall eventually explain, musical works may consist of any combination of any musical elements.

But this does not mean that just any or every

combination or occurrence of musical elements is a musical work. Two issues bear on this point. First, a work is a completed whole. Admittedly, it is not clear what constitutes a completed whole. Somehow it is something which involves a purported explicit beginning and a purported explicit ending. The sounds preceding a concert consisting of the tunings and practicings of the musicians are all musical sound elements, but the collection of these sounds is not a work nor are they considered any part of the work to be performed. This issue concerning the notion of a completed whole will be addressed further in the section on composing.

A second issue related to the question as to when a group of musical elements is a work concerns certain evaluative notions of art status. To raise the question as to what makes any given collection of musical elements a work can be, in effect, to raise questions concerning the necessary and sufficient conditions for something being a work of art, and some consider standards of aesthetic value to be at least somewhat relevant.⁶ I shall not address these questions and associated problems here. Rather, I will attempt to frame a theory that will be able to account for whatever is accepted as a genuine musical artwork by anyone. I shall begin with the assumptions that there are musical works and that compositions are distinguishable from performances and other musical objects. I will then proceed

from whatever is taken to be a musical artwork and address its ontological status. Mine is a theory about what musical artworks there are or might be, not about how something qualifies as a musical work of art.

The reason for making this latter point is that there may be some disagreement about what specific things are genuine musical elements. Just which sounds are musical sound-elements, just which inscriptions are musical symbol inscriptions, and just which verbal expressions are musical symbol expressions are somewhat controversial matters. To a large extent what counts as a musical element is a matter of convention, that is, musical elements are whatever things musical composers intentionally combine in order to make their works. This seems to suggest that virtually any sound could be a musical element. As a matter of fact, this is the case. This can be argued on the grounds that virtually any sound has actually been used in music: from cannon roars to sounds of running water, from whale calls to glass breaking; (composers from Tchaichovsky to Cage have used such sounds in their works). What aesthetic merit or value these may or may not have is an important and interesting question, but one outside the issues to be addressed here. But I would at least suggest that there seems to be little if any reason to exclude any sound as a possible musical element, (any attempt to do so will probably be met with an attempt by some composer or

performer to include it within a musical work). What is more controversial and less clear is the question, which collections or groupings of sound elements are musical artworks? If it is true that any sound could be a musical element, it would seem to follow that any collection of sounds could be a musical artwork. In the context of musical works the problems are amplified. But whatever or however we are to distinguish mere sounds from musical sound elements, (i.e., the way in which a mere sound comes to be regarded as a musical sound), should also, in similar fashion, indicate something about the distinction between mere collections of sounds and musical artworks. In other words, since any sound could be used and regarded as a musical sound, it would appear that any grouping of such musically used and regarded sounds thus would be a collection of musical sounds. All that is left to argue is whether just any collection of musical sounds is to be regarded a musical work. What settles this latter question will be determined by one's art theory in so far as it relates to the problem of defining art.

As I have said, my concern at present is not to argue for a particular definition of art nor for a set of necessary and sufficient conditions for art status, as narrowly or widely as this may be done; rather, it should be made clear that what I am claiming here is that musical artworks consist of whatever are, as a matter of fact, taken

to be musical elements. The point is that certain things, certain sounds and/or notational symbols, are used as musical elements. I believe that these are or can be construed as concrete particulars, and so it follows that any combination of them are or can be construed as concrete particulars.

Composing

I have defined composing as the activity of selecting and arranging musical elements by some person (or persons) who intends to make what he/she (or they) believes to be an original musical work. As an activity, composing need not terminate or result in a completed composition, although this is the usual aim of such efforts. Composing is comparable to writing in this regard. Just as completing a book, a paragraph, or even a sentence is not a necessary condition for some activity counting as writing, neither does composing require any such completion. Composing is a way of acting which implies little if anything with respect to achievement. It follows then, that whereas the existence of compositions is dependent upon composing, composing is not dependent upon the existence of compositions. In other words, while all compositions are to be understood as produced by acts of composing, not all acts of composing produce compositions.

There are two key parts to the proposed definition of

composing: (1) the selection and arrangement of musical elements; and, (2) an intention and belief about originality. Individually, neither of these is a sufficient condition for composing, but taken together, I do think they provide the necessary and sufficient conditions for an act counting as compositional. Since "being composed", (i.e., being the product of some act of composing), is not a sufficient condition for something to be a composition, it will later be argued that a further condition is required for a compositional act to produce a composition. I shall begin the present analysis of composing by addressing separately these two conditional aspects of composing.

Selecting and Arranging

Composing always involves selecting and arranging musical elements. (Of course, this is not to say that all selecting and arranging of musical elements is composing. There must also be intended novelty on the part of the person doing the composing, as we shall see.) As has been indicated earlier in this chapter, musical elements may be either sounds or symbols. Consequently, there are two principal ways of composing: (1) by selecting and arranging actual musical sounds; or (2) by selecting and arranging musical symbols. Regardless of which approach is taken, whether with sounds or with symbols, composing always involves making choices about which musical elements to use

and how they will be ordered with respect to one another. Any composition produced as result of these selections and arrangements of elements will consist just of those particular elements so selected and arranged. (But, again, not just any selecting and arranging of musical elements produces a composition.)

One way in which a person may go about selecting and arranging musical elements and thereby composing is by making individual musical sounds with some musical instrument, (including one's own voice). An arrangement of these sounds in a particular way results in what I have called a musical sound-sequence-event. Some of these sound-sequence-events are compositions, (improvisations are the most notable examples), but it is not uncommon for composers to proceed in this way intending only to develop a new tune, theme, or mere part of a whole musical work. For example, this may happen while at a piano or by humming a series of notes in some desired way. While I do not propose to investigate very far at all into the nature of the creative process, it should be noted that such musical sound selecting and arranging may vary considerably to the extent that it might be deliberative and calculated or spontaneous and even haphazard. The important point here is not how composers actually decide which sounds to select and how they are to be arranged, but to claim quite simply first, that it is one of the conditions of genuine composing that

some selecting and arranging of musical elements be done, and second, that actual sounds are candidates or objects for such choices. Keep in mind that by actual sounds I mean concrete particulars, individual sounds that exist or existed in a specific time and place. These are what composers who compose with sounds are selecting and arranging. I am not referring to "kinds" of sounds. A composer is not "composing with sounds", in the sense I am explaining here, if she states her musical selections in terms such as "the first sound is a 'C', followed by an 'A' sound, followed by ...", and so on. Someone is not composing with sounds unless actual sounds are selected and made.

But this first sort of composing, selecting and arranging actual musical sounds, is significantly limited by the fact that sounds are inherently short-lived, transient things. The objects produced by efforts to compose in this way cease to exist almost as fast as they come into existence. Memory, and the limits thereof, is thus sometimes an important consideration in those cases when someone chooses to compose with musical sounds. This is so because, as noted earlier, if some person's composing consists of working out various parts of a relatively complex composition and subsequently combining them together into a complete sounded whole, a significant exercise of memory is needed. As the whole work becomes more complex, the

required level of effective memory goes up. At some point it becomes impractical, if not impossible, to remember all the selected sounds and the desired arrangements of them.

Memory is not the only such limitation on composing with sounds. If an individual person wishes to compose multi-instrumentally, he must know how to play each of the instruments. (Actually, every would-be composer who chooses to compose with sounds is limited by his or her instrumental skills. A person cannot very well compose on a flute if he cannot play one.) Also, if a composer wishes to select and arrange sounds in harmonies, then she must use a polyphonic instrument such as a piano or guitar, otherwise she must enlist the help of instrumentalists to make the desired musical sounds. It should be kept in mind at this point in the analysis that I am not discussing compositions made of sounds, rather I am presently describing the activity of composing with sounds. Compositions are understood as completed wholes, original musical works. Composing is just the act of making novel musical choices. Therefore, these limitations are not so great as they might be for making compositions, since, as wholes, compositions must be sounded out in their entirety, therefore all parts of the composition must be remembered and sounded. Mere composing does not require that all parts be sounded, only that whatever sounds are made are selected and arranged in what is believed to be an original way. As we shall see further,

the limitations on composing with sounds are magnified if the aim is to produce a composition made of musical sound elements.

Musical notation provides a means of getting around some of these difficulties, and permits achieving a very high degree of compositional complexity. Again, the person wishing to compose in this manner is limited by knowing how to use an appropriate notational system. But using any such system is no longer composing with sounds. Sound recording, on the other hand, does permit composing with sounds in a way that allows virtually any degree of complexity,⁷ and yet does not place such heavy demands on the composer's memory or instrumental abilities. I shall return to both of these sorts of composing a bit later.

Another important way that composing with actual sounds may be, and very often is, accomplished is by improvising. When composing with musical sound elements is done all at one time in a somewhat spontaneous fashion, then such composing may be called improvising. Improvising music consists of making selections of musical sound elements and arranging them into a sound-sequence-event. But it is something more than merely this. Improvising brings to light the other crucial constituent of composing, which is an intention to make these selections and arrangements in a more or less original way. I would argue that all improvising is composing. Before I address this intentional

component of the definition a few more points should be made.⁸

Composing by improvising is not limited by memory in the ways described, because most improvising is done with no intention of repetition. That is, improvisations are usually not intended to have copies or derivations. Furthermore, any composition created through improvisation simply is the improvised sound-sequence-event, but this sort of composition does not exist beyond the time it is made. Improvised compositions are, in this way, no different from any other compositions made from transient sounds. The composition as such perishes as quickly as it is made. Any subsequent attempt to "repeat" the original sound-sequence-event does not result in bringing the composition back into existence. Rather, the new sound-event is what I have called a "copy" or "derivation" of the composition, which is not itself a composition.

Composing by improvising may involve the efforts of an individual improvising alone or it may occur with either improvised or unimprovised accompaniment. If an individual improvises alone, her work counts as a compositional work, and she is the composer of that work. Any unimprovised accompaniment is not a part of her composition. The whole performed musical work is therefore only partly a composition; it is a heterogenous entity consisting of a composition combined with a musical work derived from some

other composition. If the accompaniment is also improvised, then the composition consists of all improvised sounds; and so each of the improvising musical contributors is a co-composer of the work produced.

Summarizing thus far, selecting and arranging actual sounds is one means of composing. Usually such composing takes the form of improvising, distinguished from other sorts of composing with sounds by the degree of spontaneity involved. Composing with sounds is limited by memory, instrumental skills, and the possible need for the cooperative efforts of other persons. These limitations probably account, in part, for the motivation to develop alternative techniques and materials for composing.

The larger portion of composing is not composing with sounds, but composing with symbols. These symbols, by convention, refer to or denote musical sounds. The composer is, in effect, indicating what sounds, if made and put together in the specified way, might produce a sound-sequence-event as he might want to produce it. But this last point is somewhat irrelevant anyway. That is, composing by way of notation does not require that sounds ever be made based on this written manuscript nor that the composer have any particular interest in this regard. Most composers probably do wish to have their scores performed as sound-sequence-events, and many composers actually take part in doing so. But it is no less an instance of composing if

the person doing so has no interest in having her selected musical symbols sounded out. By extention, a composition is no less a composition, it is no more or less complete, if it is never sounded out, and even if the composer, for some reason or other, has no wish for it ever to be performed.

Composing is not uncommonly referred to as "writing music". Such a comparison with writing that consists of linguistic symbols arranged into words and sentences is a sensible one because prose writing, for instance, also consists of selecting and arranging certain specifiable elements, but instead of musical symbols, words are the objects selected and arranged in the desired way. When used in this way the term "write" is synonymous with the term "compose", whether referring to writing prose or writing music. This somewhat metaphorical and colloquial sense of "writing" is used not only for the literal process of making inscriptions or marks on paper; instead, it means that someone is arranging words or musical symbols in some way. Even further removed from the literal sense of writing, is the sense that such writing need not make use of written inscriptions at all. For example, a prose writer may dictate his work as spoken words into a tape machine; while, as we have seen, a musical writer may make her work by using sounds.

Most Western musical composing with symbols makes use of a conventional, standardized notational system, i.e., the

familiar five line staff with its various attendant symbols for pitch, duration, rhythm, meter, key, etc. But composing with symbols need not make use of this system. Alternative symbols, systems, and schemes can be and have been developed.⁹ I mentioned earlier in the chapter that included among musical symbols are words such as "B flat", "key of G", "4/4 meter", and so on. The important thing about musical composing with symbols is that the symbols used are musical symbols, i.e., they denote musical sounds.

Composing with symbols is of course limited by the composer's knowledge of and facility with an appropriate symbol system. But such composing does not require that a composer be even a competent instrumentalist. A composer who uses symbols is freed from composing only for instruments he knows how to play well or play at all. What this means is that skill in musical composing may be regarded as independent of other sorts of musical skill, such as instrumental virtuosity. This fact is widely recognized. Many composers are not good instrumentalists, while many performers likewise do not compose nor possess skills in this area of musical activity. What is interesting about this is that a music composer who uses symbols could conceivably go about her business without ever making or hearing any musical sounds. A composer could be deaf and entirely unable to play any musical instrument and yet compose music, even what might be considered good music.

Thus, musical composition, under certain circumstances, can amount to nothing more than skillful symbol manipulation.

Intending and Believing

Compositions are not just any collection of selected and arranged musical elements. Strictly speaking, a performer working from a score is also selecting and arranging musical elements. Such a person "reads" the score and makes decisions about what notes to play on his instrument and what sequence they are to follow. But no one would consider playing music from a score, (or from memory for that matter), to be composing, though it does involve, in a sense, the selection and arrangement of musical elements. Nor is a person who transcribes a performance into notation properly considered to be composing. These are not acts of composing because it is generally believed that composing is, to a greater or lesser extent, a creative activity of a special sort. That is, composing is thought to include a notable degree of originality on the part of the composer.

Originality sometimes refers to the extent to which something is itself new, different, unique, or novel. This is to define originality from the side of the thing created. I shall refer to this sense of originality as "objective" originality. Objective originality refers to whatever is new about an object. In a most minimal sense, every object

that comes into existence is objectively original, since it is a "new" object in the world. So there are degrees of objective originality that are a function of the extent to which some object is unlike any already existing object.

An alternative sense of originality, and one that I would like to emphasize, would approach originality from the side of the maker or creator. Looked at this way, originality refers to the extent to which the person involved is "coming up with something of her own", making something without direct dependence upon or conscious reference to something with which the person is aware. Such originality bears upon the experience, belief and intention of the creator, rather than upon the dependence of the character of the thing made on the existence or non-existence of something supposedly like it. I shall refer to this sense of originality as "subjective" originality.

Given this latter, subjective sense of originality and under the view now being presented, the actual degree of originality on the part of the composition, (i.e., its "objective originality", or the novelty with respect to the work itself and the world within which it exists), is not relevant to that work's compositional status, nor to the sense in which the person who made it may be said to have composed it. Rather, what is decisive for compositional status is the originality believed and intended by the

composer, that is, its "subjective originality".

Suppose two composers, unknown to one another, are working on what they believe to be original musical compositions. Each proceeds by selecting and arranging musical elements, (let us say in this case, they are both inscribing conventional notational symbols on paper). As improbable as it may be, they produce a sequence of symbols that is from all appearances indistinguishable one from the other. The two composers publish their manuscripts as new and original musical compositions. Do we now have two compositions or one?

If we say there is only one composition, we are led to the strange consequence that two separate people created the same thing. Now two people can create the same thing if they act jointly, but in the above scenario, the two people did not act together.

To say instead that there are two separate compositions is not at all strange and leads to no peculiar consequences. After all, manuscripts are particular objects; there is no reason that two objects cannot be made that coincidentally look alike. Two people could independently build two separate chairs that turn out to look alike in virtually every detail, would we want to say that there is anything other than two chairs. It might be argued to the contrary that the two chairs manifest the same design, and so, the makers of the two chairs have actually

created or discovered this design. Thus, manuscripts should be likewise understood as manifesting the same compositional design. Under such a view, the design is, in effect, a universal, something capable of having multiple examples. What would not be clear, given this appraisal of the situation, is whether the design existed before it was doubly realized through the efforts of the two makers, or whether the two makers simultaneously created the same universal. In the former case the makers might be said to have discovered the universal, in the latter they might be said to have created it. If it is thought that the composers created the universal, we return to the original oddity of saying that two persons created the same thing, that two people independently created the same universal. (A further question is raised here regarding whether or not universals are the sorts of things that can be created.)

If, on the other hand, it is claimed that the design or composition was separately discovered by two different persons, much like two chemists independently discovering the molecular structure of some compound substance, this would seem to imply that all compositions, past, present, and future, always existed somehow waiting to be discovered. This way of describing the matter leads to the kind of talk that many nominalists object to, talk of so-called possible objects. It leads to the multiplication of entities so uncomfortable to nominalistic sensibilities. Not only is it

claimed that there exist as many compositions as have been composed, but there also exist all the compositions that will be composed and all the possible compositions that never happen to be composed (discovered).

Clearly, the nominalist wants to avoid such a conception of the situation, and would prefer to acknowledge that there are, in the present example, two and only two separate, actual compositions each composed separately by each of two separate composers. There were two distinct acts of composing, two distinct individual composers, and two distinct manuscripts. Therefore, it is the belief on the part of the composer that distinguishes a compositional selection and arrangement from non-compositional selection and arrangement; more precisely, the belief that what is being constructed is generally original, that it is not an attempt to copy something else.

Closely allied to this belief about other musical objects is an intention to create an original work. Intention on the part of composer is decisive within the present consideration of composing. I wish to maintain that the intention to compose is a necessary condition for a genuine act of composing, and since compositions are defined as the artifactual products of certain acts of composing, intentions are constitutive of compositions.

Suppose an avant-garde composer well-known for her peculiar combinations of traditional musical sounds with

sounds not typically regarded as musical, (breaking glass, cat mews, closing books, etc.), along with poly-rhythmic and arhythmic or nonrhythmic structures. She is a tape composer, meaning that she records these various sounds onto multi-track sound equipment and later mixes the sounds into a completed recorded composition.

One evening she is alone working in her studio. The tape machine happens to be running while she is setting up various musical instruments to be played and recorded. Before she has a chance to purposely make any sounds she wants to record, she is struck with a violent seizure that sends her thrashing about the room pressing keys on her electronic keyboards and piano, knocking into a drum set that crashes to the floor making all sorts of booming and pounding noises. This goes on for a few horrible minutes ending with her death. Most of the noises she made while in her death throes were recorded on tape.

Later that evening a colleague discovers the gruesome scene. The colleague was aware of the composer's intention to do some work that evening and so several days later he reviews the tape in order to listen to what he thinks is the composer's last composition. Unknown to the colleague, none of the recorded sounds were selected and arranged for the purpose of composing a musical work. But the colleague believes that the recording is a composition, and with some very minor editing releases the tape as the famous

composer's last great work.

Is this recording a composition? I do not believe so; at least it is not the work of the dead composer. The facts of its production would, if known, almost certainly undermine any belief that this recording is the composer's musical artwork.

In this second example, the would-be composer's intentions determine whether or not a group of musical elements is a musical work generically and a composition specifically. The first example of the two composers reflects the role of belief in distinguishing between making a compositional work of music from making a non-compositional work of music. The intention to compose a work requires the belief that the selection and arrangement of musical elements is original, that these selections and arrangements are not significantly related to nor dependent upon some already existing musical work.

In order to more fully understand the difference between original and non-original selection and arrangement of musical elements, it will be necessary to consider the nature of non-original selection and arrangement, what I shall call "copying" and "deriving".

Copying and Deriving

As noted in Chapter I, the chief difficulty for an ontology of music, (as well as for literature, printmaking,

drama and others), is characterizing the relationship that is presumed to hold between an original work of art and its purported examples. One way to characterize this difficulty is to ask, what allows for the same proper name to be assigned to a composition and to performances of it? How is it that an indefinite number of musical performances, scores, and recordings can each be identified as Beethoven's "Ninth Symphony"? Why are some performances, (or scores or recordings), called "Ninth Symphony" and others "Fifth Symphony", and still others McCartney's "Yesterday"?

My objective throughout has been to develop a conception of this relationship which countenances the existence of individuals only. In keeping with the sorts of nominalistic presuppositions and hypothetical restrictions placed on the proposed theory, such a conception cannot construe the apparent relationship in a way that requires or permits the existence of something that composition and performance are said to share or have in common. To permit this is to admit universals or non-individuals into the theory. Nor should the relationship be described in a way that takes performances, (or scores, recordings, etc.), to be examples, occurrences, or instances of some composition. Again, such is the language of universals and abstract entities. But more importantly, I don't think that such characterizations correctly account for what is going on when we call a performance by the same name as some

composition anyway. It is the purpose of this section to describe what I take to be a nominalistic account of the presumed relationship that is thought to hold between a composition and other objects that are assigned its name. I shall do so in terms of what I have called copying and deriving.

Since copying will be understood as a species of deriving, what I have to say initially about deriving will apply to both; so, for simplicity, I will describe what deriving means and it can be assumed that what is said about it also applies to copying. The differences between them will be discussed afterwards.

Deriving may be defined provisionally in terms of three conditions:

- (1) temporal precedence;
- (2) acquaintance with an already existing work;
- (3) intention to make a work more or less like the already existing work;

Thus, more precisely, "B is derived from A" means,

- (1) A is temporally precedent to B, (i.e., A exists before B);
- (2) the person (or persons) making B is acquainted with A, (i.e., the person is aware of A's existence and its temporal precedence);
- (3) the person (or persons) making B intends to make it more or less like A.

Let us examine each of these items in turn.

The first condition is quite straightforward and

probably not controversial. To say that something is derived from something else is to acknowledge that the derived thing came from, was caused by, or followed from something that preceded it. This first condition is virtually analytic with respect to the notion of "derive". The point here as it concerns compositions is that compositions, as noted already, are originals; and, as such, precede any of their so-called derivations, (e.g., performances, score-copies, recordings, etc.). Or, put another way, what I am calling derivations are distinguishable from compositions partly by the fact that they are not originals in the ways that compositions are. One of the ways in which compositions are originals is that they exist before any work that is derived from them; that is, any of a composition's derivations are works made subsequent to the making of the original composition with which the derived work is associated and with which it shares a name or title. This first condition of deriving accounts for the sense in which all compositions may be regarded as "objectively" original: the object which is the composition existed before any purported performance, score, recording, etc. of it. It is not being claimed that compositions are objectively original in the sense that they are the first objects to exist which possess a certain combination of characteristics or qualities. According to the theory under construction, compositions need only be

objectively original in the sense that they precede, (exist before), any of their derivations.

It should be made clear though that derived works need not be derived directly from a composition in order to be named by the same name as that composition. Quite often it is the case that derived musical works are derived from other derived works. For example, a performance of Beethoven's "Ninth Symphony" is usually derived from a score of the "Ninth Symphony". It is highly unlikely that this score is Beethoven's manuscript, instead this score is probably itself a derivation. In point of fact, the scores used to derive performances of this symphony are just about always some of the more recently derived works in a series of derivations of derivations which eventually terminate in Beethoven's manuscript.

The second condition follows upon what was claimed about those who make compositions, insofar as such persons who intend to make compositions believe that what they are doing is original, whereas those who make works derived from compositions do not have such a belief. Instead they are aware of the composition from which they are deriving their musical works. Obviously, if I derive B from A, I must be somehow acquainted with A; I must know of A's existence and something about what A is like. In addition, this condition compares favorably with what was stated earlier about a subjective sense of originality. This sense turns on

whether a person's efforts to make a work are in any way consciously influenced by some already existing work. Novelty (or originality) under this conception is thus a person-relative notion dependent upon the prior experience of the maker of a work. The relevant prior experience in this context is acquaintance with an already existing or formerly existent selection and arrangement of musical elements from which the present deriving is taking place.

The third condition for deriving is also indirectly related to this subjective sense of originality. This condition is a particularly decisive criterion for distinguishing acts of composing from acts of deriving. Subjective originality, as described earlier, is a function of what a person believes about the work he is making. This belief amounts to someone thinking that the musical selections being made are not significantly influenced in any detailed way by some already existing work of which one is aware. It is the belief that the present selection and arrangement of musical elements is a novel selection and arrangement of such elements. This sort of belief of subjective originality is largely constitutive for acts of composing. To derive a work, on the other hand, means not to have such a belief with respect to this sort of novelty. Deriving is thus selecting and arranging musical elements in a non-original way. But it is more than merely this. More precisely, it amounts to selecting and arranging musical

elements with the intention of basing these decisions on some already or formerly existing selection and arrangement. The act of deriving is a matter of purposely being influenced by some other musical work, (e.g., a score, a performance, or a recording). in the process of selecting and arranging musical elements. This condition thus reflects the intentional character of deriving and parallels a similar, yet opposite, condition for composing.

Deriving a work from another work usually consists of selecting and arranging musical elements with the intention of naming the resulting work by the same name as the work from which it is derived. I say "usually" because such illicit work-making as plagiarism lacks this intention. Plagiarism, quite simply, is deriving a work without acknowledging publically the fact that the work is derived, and going so far as to intentionally deceive others into thinking that what one has made is original when, in fact, it is not. It is to claim subjective originality about a work that is not subjectively original. It is not plagiarism to create a work that turns out to be quite similar, if not apparently identical, to some other work. This point was made earlier in the example concerning two composers who, inadvertantly and entirely unknown to one another, make works that look or sound virtually identical. Such occurrences would be acts of genuine composing. The decisive element distinguishing composing from plagiarizing

is therefore subjective originality. (Forgery is the reverse of plagiarism. It consists rather of attributing someone else's composership to a work that one has made oneself. This is also a form of illicit work-making since it involves an intention to deceive, although it is genuine composing. Whereas, plagiarism is a form of deriving improperly called composing, forgery is a form of composing falsely called deriving.)

Except under these illicit circumstances, deriving one work from another work involves the intention of being significantly, (most often maximally), influenced by the earlier work in the process of selecting and arranging musical elements; and then calling or naming the resulting work by the same name as the temporally precedent work that one is aware of and intends to derive the new work from. We shall see in further detail how names are attached to derived works in the next chapter.

Admittedly, this third condition of deriving gives rise to possible difficulties for the theory. For example, let us imagine that I had heard a performance or read the score of a particular musical work some time in the past, yet I have consciously forgotten about it. Several months later, I begin working on a composition of my own and unintentionally make a work that is very much like the performance I had heard earlier, (the assumption here is that I have somehow been unconsciously or subconsciously

influenced by the previously experienced work). Are such efforts acts of deriving? As a consequence of the theory I am attempting to articulate and defend, I believe that such acts are genuine acts of composing and not deriving. Again, the decisive criterion distinguishing composing from deriving is to be understood in terms of the intentions and beliefs of the person making the work. To the extent that a person sincerely believes that her efforts are compositional, (i.e., among other things, purposely novel or minimally influenced by some other work), they are compositional. After the fact, it may be discovered that the activity did not produce the sort of objectively original work that is admired in aesthetic contexts, and that the composer was somewhat self-deceived about the novelty of her selection, but I think these efforts and the products of them remain compositional nonetheless. What is really at issue then bears upon certain evaluative rather than ontological considerations. Since actual, objective originality is particularly valued in the artworld, the less-than-original results of someone's activity of the sort described probably will not be as highly regarded as the original work that preceded them. But to say of some activity or some object that it is compositional need not be taken as honorific nor need they be recognized as making an evaluative judgement; it need not be meant to say that it is "good" or "bad". Recall that the issue at stake here

concerning the ontological status of compositions involves a classificatory rather than evaluative sense of work of art.

A second difficulty arises, which creates a deeper problem for the description of deriving as it now stands. Suppose a person who intends to derive one work from another, "gets it wrong". That is, suppose that some person believes that she is performing a work derived from a score of someone else's musical composition, but she overestimates her ability to read musical notation and plays the wrong notes for several or even all of the symbols contained in the score. In such a case, she is aware of the original work and she intends to base her performance on it. Such a scenario satisfies the stated criteria for derivation, yet the resulting performance does not sound anything like what we would expect; the score is for, let us say, Bach's "Art of the Fugue", but it sounds more like McCartney's "Yesterday". Is it proper then, since the performance is derived from the score, to name the performance by the original work's title?

Yes and no. I shall describe how it is that works acquire their names or titles in the next chapter, but a question remains regarding the sense in which the resulting performance can be said to be derived from Bach's work. We could say of what has resulted that it is an exceedingly poor performance of Bach's original work, since, after all, a score of his work did provide the basis for the musical

selections made by the performer. Typically, poor performances have a noticeably significant number of mistakes, but in the present instance, virtually every note is a mistake. In the barest sense of derive, the performance in the present example is a derived work from Bach's fugue because the performer did not make genuinely original musical selections, (they were influenced and guided by some other work, albeit in a peculiar and distorted way), and the performer believed that what was being made was not original; that it was somehow derived from the Bach score. But although the performer has seriously misread or failed to "comply" with the score, it remains an act of deriving nonetheless, and it was derived from Bach's work. The question remains though whether this derived work should be called a performance of "Bach's 'Art of the Fugue'" or "McCartney's 'Yesterday'"? Though it does not seem improper to say of the performance that it was derived from Bach's work, it does seem quite awkward to claim that it was a performance of "The Art of the Fugue", even to say that was an optimally bad or inaccurate performance of it. The purpose of analyzing the notion of deriving a work is to get clearer about the way compositions are related to their purported examples. We want to know what it is about some musical works that justifies associating them with already existing compositions. Why is it that it seems so inappropriate to consider the

performance described above a performance of Bach's "Art of the Fugue"?

In order to address this sort of phenomenon, a fourth condition for deriving seems to be in order. I shall refer to it as "the compliance condition". The third condition describes an intention on the part of the person doing the deriving to comply with the selections already made in the original work by the composer. This fourth condition requires that the derived work actually comply with the work from which it is derived. What is meant here by "comply"? Before I explain this notion of compliance and spell out its implications for deriving a work, I shall first describe the difference between copying and deriving.

The basis of the difference between copying and deriving is this: in copying, the product of the process of copying exists in the same medium as the thing copied; whereas, in deriving, the product of the process of deriving exists in a medium different from the original object. In saying that copies are in the same medium as the thing they copy, I mean that they are each comprised of the same sorts of musical elements. For example, since a score consists of musical elements that are notational symbols, a copy of a score also consists of notational symbols.¹⁰ A copy of a performance, on the other hand, will consist of musical elements which are sounds because performances are comprised of musical sound elements.

Now the process of deriving one work from another, as distinguished from copying one, involves using musical elements of a different sort from the original. A scored transcription derived from a performance consists of notational symbols although the performance itself consisted of sound elements. A performance of a scored musical work is also a derived object because it consists of sounds rather than notations.

Recordings create special difficulties for this distinction. As we have already observed, the ontology of musical recordings is particularly perplexing. As a musical object, a recording is significant for the sounds it can produce; nothing about its status as magnetic tape or a plastic disc is of any direct musical value. Somewhat like a score, which is also of value largely for what it can be used for, (i.e., to denote a sound-sequence-event that could be performed), a recording has no musical value other than its capacity for being used to make musical sounds. I shall consider recordings of performances to be copies on the grounds that they are musically significant for the musical sounds that they themselves are capable of producing and because the way they are apprehended is by being listened to. We typically say that we "listen" to recording, whereas we "read" a score.

Copies and copying should not be understood solely in terms of similarity. Similarity is a problematic notion for

a nominalistic perspective, so such a relationship should be avoided in the present context. Admittedly, copies are often described as varying in degree of similarity with respect to the objects they copy, but similarity, under the view being considered, is not constitutive for "being a copy". Rather, "to copy a work" is, in addition to the stated conditions for deriving in general: (a) to make a work comprised of musical elements in the same medium as those comprising the work being copied, and (b) to attempt a certain degree of similarity, to intend that a certain desired degree of similarity be achieved, and to believe that a certain degree, although not necessarily the intended degree, has been achieved. Thus copying, like deriving, is fundamentally a matter of making musical selections and arrangements that are intentionally and maximally influenced by some other already existing work. But again, as with deriving, a question arises as to how we are to account for those occasions in which, inspite of the intentions by someone to make musical selections based on some other work, the resulting work is quite different from the original? We usually do talk about a comparison between a copy and an original in terms of some degree of similarity. How do we avoid calling what sounds like "Mary Had a Little Lamb" by the name "Row, Row, Row Your Boat" when the performer of the former intended the latter?

As indicated earlier, a similar problem arose in cases

of derivations as well, although in a different sense. Instead of referring to a score's similarity to a performance of it, the term of comparison I suggested with respect to a derivation was "compliance".¹¹ Since derivations are, by definition, of a different medium than the thing they are derived from, any sense of similarity attached to notions of deriving are metaphorical at best and substantially different from what goes on with copying. Sounds cannot be similar to inscriptions and vice versa. But there is an interest in having some term for comparison, since people often do talk about the degree to which a performance, for example, compares with a score from which it was derived. The term that I shall use to characterize such positive comparisons is, as already stated, compliance. To comply with a score in deriving a performance is to make sounds that are within the range of the denotations of the musical symbol elements contained in the score.

A performance of Beethoven's "Ninth Symphony" is almost always derived from a score. If the performers have Beethoven's manuscript, then the performance is derived from the composition. But in either case, the degree to which the performance is said to comply with Beethoven's work is the degree to which the performers base their selection and arrangement of musical sounds on the score associated with Beethoven's composition titled "Ninth Symphony". But there is considerable room for variation even under these

circumstances because Beethoven's work is nothing more than the inscriptions on the page. Compliance consists of making musical sound elements in a way consistent with the denotations of the musical symbol elements contained in the score. Making sounds as denoted by the symbols contained in the score can be done in numerous ways, and the results can vary greatly. What determines the degree of compliance is clearly not similarity, since sounds cannot be similar to inscriptions; nor is compliance a matter of actualizing the intentions of the composer or providing a sounding of musical ideas somehow "embodied" in the score. Compliance is a matter of basing selections and arrangements of musical sounds as directly as possible on the score. This "basing" amounts, in part, to the belief on the part of the performers that what musical choices they make are maximally influenced by the score, that they believe their selections minimally original and independent. In addition, and very importantly, compliance requires that the musicians actually produce musical sound elements that are within the accepted, (according to musical conventions), range of sounds denoted by the musical symbol elements.

For example, a symbol within a score may denote a certain pitch, say 'A' flat, for a particular instrument, say a bassoon, and other symbols may denote something about the dynamics, say 'forte' (loud), but, even given these specifications, there is not only one way to comply with

these symbol elements. Conventional musical practice does prescribe certain limitations for how to play this note, but even this involves going outside of the score to some extent; i.e., to make a selection with respect to a musical element that is other than what is explicitly evident in the score. The point is that the performance of the note complies with the composition in the event that the performer does what is indicated, but there is not only one way to do this. It should be noted that it is just this fact which allows for what is called interpretation.

We may now return to what I have called the "compliance condition". This may be understood as the fourth condition for deriving a musical work. Thus, what it means to say that "B is derived from A" includes this final criterion:

- (4) the person deriving B from A makes B in such a way that it complies, for the most part, with the musical elements contained in A; (i.e., the musical elements which make up B are within the accepted range of denotation of the musical elements that make up A).

But how might this condition be applied to copies?

I believe this may be accomplished by recognizing that as denotation may function across media, (e.g., a symbol may denote certain sounds), so may denotation function within a single medium. That is, a sound may denote other sounds, or a symbol may denote other symbols. Therefore, the compliance condition may be said to apply to both deriving

and copying. It is the condition that specifies the extent to which copies and derivations of musical works may be compared with their originals. The degree of compliance is a function of the extent to which the derivation or copy consists of elements lying within the range of musical elements denoted by the musical elements that go together to make up the original work.

Let me summarize what I think has been accomplished in this chapter. Musical works may be made in one of two fundamental ways: they may be composed or they may be derived. To compose a musical work a person must:

- (1) select and arrange musical elements, (either sounds or symbols); and
- (2) intend and believe that
 - a. this selection and arrangement produces a musical work, and
 - b. this work is original, i.e., not derived from an already existing work.

To derive a musical work a person must:

- (1) select and arrange musical elements, (either sounds or symbols); and
- (2) intend and believe that
 - a. this selection and arrangement produces a musical work, and
 - b. this work 'B' is related to an already existing work 'A' according to the following conditions:
 - i) 'A' is temporally precedent to 'B';
 - ii) the person making 'B' is acquainted with 'A';
 - iii) the person making 'B' intends to make it more or less like 'A'; and
 - iv) the musical elements of 'B' comply with, (are denoted by), the musical elements of 'A'.

Copying a musical work is one important way of deriving a work. It consists of deriving a work in the same medium, (i.e., the copy-work is made of the same sort of musical elements), as the original, copied work. If the work consists of musical elements in a medium other than the original work, it is said to be a derived work.

Most acts of composing produce musical compositions, usually consisting of musical notations, (typically manuscript scores), or musical sounds-sequence-events, (typically improvisations or multi-track tape recordings). Derivations, (including copies), are usually either musical performances, scores, or recordings. Whereas composed works are usually given a name or title that is intended to uniquely identify and distinguish them from other composed works, derived works are usually given the same title as the compositional work from which they were ultimately derived. That is, a composition's title sets off the composition from other compositions, and thereby functions as a proper name. On the other hand, a derivation's title associates the derivation with a specific composition; and as such is not a genuine name. The title, which is the name of the composition, attached to a derivation is thus non-designative; it is non-referential, or syncategorematical, and serves only to indicate that from which it was derived.¹² More about this latter point will be forthcoming in the next chapter.

In this chapter the focus has been on the constituent elements, the materials, of composing, and on the activity of composing as such. In the next chapter, I shall turn to a more explicit consideration of compositions, the musical artworks produced from these materials and by these activities. To accomplish this I shall describe my theory of compositions in terms of comparisons and contrasts with alternative views on the matter.

CHAPTER IV

MUSICAL COMPOSITIONS AS CONCRETE PARTICULARS CONTRASTED WITH THREE NOTABLE ALTERNATIVES

Introduction

As a way of further characterizing my theory of musical compositions as concrete particulars, I shall distinguish it from some notable alternatives. Of course it would be impossible to contrast my theory with all competing theories; there are simply too many. My approach instead will be to discuss three important theories chosen for the following reasons. First, they are each reasonable and important contributions in their own right to the ontological discussion of musical compositions. Second, each theory is described by its author in language and categories amenable to my own approach: we are all developing our theories from within an Anglo-American perspective. Third, each of the selected theories has the effect of highlighting significant contrasts with my construal of musical compositions. Whereas my theory emphasizes the physical and individual character of compositions, the others represent varying alternative combinations of physicality vs. non-physicality and individuality vs. non-individuality.

My intentions in the present chapter will be to describe each of these theories, to point out their key points of divergence from my theory, and to comment on their adequacy as descriptions of musical compositions. Following this, in Chapter V, I will summarize this discussion in terms of the categories of concrete, abstract, particular, and universal as a means of identifying a proposed range of possible ontological characterizations of musical compositions, of which my theory represents one extreme.

Music, as was observed in previous chapters, is an artform that presents special problems for the ontologist. The usual practice of distinguishing compositions from performances encourages taking the composition as some sort of abstract entity or universal. This is often the case because it is customary to talk of multiple performances of music, sound sequences of a certain sort, as instances or occurrences of some composer's work. The performance is usually given the same name as the composition from which it was derived. Thus, the composition, understood as the sort of thing capable of multiple instances, is often readily construed as a universal, a type, a kind, or some such non-individual.

Another tendency evident in discussions about the ontological nature of musical compositions is the description of compositions as essentially non-corporeal. This is the notion that the actual work of art, although

revealed through some sensory artifact, (usually a performance or score), is not essentially this corporeal artifact. Rather, the artwork is some sort of non-physical, abstract entity or phenomenon. Under such a view, the musical composition exists, in some sense, independently of any physical or sensory expression or occurrence. In clear contrast to these tendencies, my approach has been to cast the ontology of musical works entirely in terms of physical or sensory individuals, (i.e., concrete particulars).

As we shall see shortly, R. G. Collingwood and Nicholas Wolterstorff describe musical works as essentially non-physical entities. Later, we shall see that although William Webster's theory requires that works be physically realized, he and Wolterstorff will construe works as non-individuals. Much more will be said about this in Chapter V.

R. G. Collingwood: Compositions as Mental Entities

Philosophers of art have devised various theories aimed at working out the relationship between the composition and performance that call upon the language of universals and/or abstract entities. One such approach is to consider the composition as essentially an entity of consciousness. That is, the musical composition is essentially an idea, a mental construction that is the product of and existent in the mind of a composer. The

composition is not a physical, corporeal, or concrete entity at all, rather it is an abstract entity of thought.

R. G. Collingwood has delineated just such a conception of musical compositions that takes them as fundamentally and essentially non-corporeal mental creations. "The work of art proper is something not seen or heard, but something imagined."¹ As he makes clear, compositions are created in the mind of a composer and are then communicated or expressed through some performance or score, and thus made available to the minds of other persons. The work remains an entity or phenomenon of human consciousness, the performance or score is merely a vehicle for communication of the musical ideas of the composer.

To regard the musical composition as a concrete particular, as I do, is effectively to deny that it has genuine status as an artwork "in the mind of the composer". The genesis or creation of a composition may indeed involve mental activity, but my view is that the composition as artwork is essentially a corporeal artifact, and moreover, it is an individual. Compositions are in a certain sense something like prototypes. Prototypes may be understood, in this context, as individual objects that serve as things to be copied, or as models suggestive of others objects that could be made similar to the prototype. The point I wish to emphasize here is that prototypes are usually some original physical object serving as a model for making other

physical objects.

Let us return to Collingwood's theory of artworks as presented in The Principles of Art. As I have said, his is an example of the sort of view that takes musical compositions to be some sort of mental entities, an object of thought that is not physical or sensory. Collingwood describes the work of art as an "imaginary object"². The section of his book that deals explicitly with this claim conveniently uses the "making of a tune", or the composing of music, as his paradigm case for developing his broader theory of artistic ontology.

He begins by reminding us that, under the view developed earlier in his book, the making of "art proper" is not the making of an artifact; it is not a process of "fabrication".³ Such activity is characteristic of what he regards as mere "craft", rather than genuine artmaking. (Whatever the merits and weaknesses of this distinction, I shall not address the basis for his view on this issue in any detail. Rather, my concern here is to describe how Collingwood understands compositions. I am interested primarily in the implications of his theory as they might bear upon my central thesis regarding the ontological conception of compositions.)

What sort of "making" is involved in "art proper"? Strictly speaking, for Collingwood, artists do not "make" their artworks; instead, they create them. In order to

clarify this claim, he carefully lays out a distinction between making and creating. In so doing, he defines creating as the special sort of activity artists as artists perform. Creating, unlike mere making, does not aim at achieving a particular outcome or end, does not require any preconceived procedure, and does not impose "a new form upon a given matter".⁴ To do these is to "craft" or fabricate artifacts, not create artworks. Yet creating is, as Collingwood points out, conscious, voluntary, and deliberate, although the creator may not know quite what will come of his creating.

Underlying these claims is Collingwood's expression theory of art. Collingwood believes that genuine artworks, ("art proper"), are expressions of an artist's imagination and emotions. But an artist's expression of emotions through aesthetic creation must not to be confused with any intention to arouse or evoke emotions in others. This later intention is goal directed; it aims at producing a certain kind of response in an audience. Expressing emotions is not directed toward any preconceived goal or response; rather works of art express emotions for their own sake. They give articulation and form to feelings that often arise subconsciously and inarticulately. Purported works of art that are intended to arouse or evoke certain emotional responses are, for Collingwood, not products of artmaking, but are instead products of craftmaking.

Collingwood goes so far as to say that an artist does not know at the beginning of her artistic endeavors just what character the artwork will take.

No artist, therefore, so far as he is an artist proper, can set out to write a comedy, a tragedy, an elegy, or the like. So far as he is an artist proper, he is just as likely to write any one of these as any other....⁵

This is so because art as the expression of emotions is not essentially a matter of communicating these emotions to an audience or getting them to feel the same way. The artist's primary concern is the expression itself; the mode or form of the expressed feelings or imaginings is secondary.

For this reason the artist's expression, the artwork, need not be externalized in a physical or public art object at all. Since artworks are intended neither for communicating to others nor for evoking responses in others, no publically accessible object is required. The essential work of art is therefore sufficiently actualized in the mind of the artist. More specifically, the composer's musical expression, the composition, is the musical idea created by the composer. It may or may not ever be sounded as a performance or written out as a manuscript score.

In distinguishing it from mere making, Collingwood likens creating to a special sense of planning; a planning that does not require execution in the form of either something written down or any sort of fabricated object. Creating artworks is an activity of "imagination". That is,

it is a mental activity which does not require construction of real objects. Collingwood claims that "a work of art may be completely created when it has been created as a thing whose only place is in the artist's mind."⁵ He compares the creating of an artwork to an engineer's planning of a bridge. Before any drawings are made or specifications discussed with anyone, the plan for the bridge can be said to exist in the imagination of the engineer. Such a plan for a bridge thus could be referred to as an "imaginary bridge". If this plan were executed, then we could refer to a "real bridge". But the "real bridge" and the "imaginary bridge" remain as distinct entities, the latter having no dependence upon the former in any significant way.

Note how the conception of planning in this case has the marks of Collingwood's sense of creating, rather than mere making. First, an engineer can plan a bridge without any specific purpose or end product in mind. Such planning might be simply an engineering exercise, an example for a textbook, a proposal for a contract, and so on. No intention to actualize or build the bridge need occur in order to plan a bridge.

Second, the planning need not follow any preconceived procedure or specifications. Again, the result of the planning may be merely an example or simple musings without guiding parameters or specifications. The engineer may simply "start from scratch" and let her ideas come as they

will.

And finally, planning a bridge does not consist in imposing new form on given matter. As an act of imagination, no material is there to be formed. The execution of a plan for a bridge in the form of drawings or actual construction would count as an instance of "making", not "creating". Collingwood acknowledges the use of the word "plans" for actual drawings on paper; but he denies that these are the plan as such. They are only the means by which the engineer's ideas are communicated to others for practical purposes. The drawings are not the plan, rather they merely represent the "true" plan that exists in the mind of the engineer.

As goes an engineer's planning of a bridge, so goes a composer's composing of a musical piece:

When a man makes up a tune, he may and very often does at the same time hum it or sing it or play it on an instrument. He may do none of these things, but write it on paper. Or he may both hum it or the like, and also write it on paper at the same time or afterwards. Also he may do these things in public, so that the tune at its very birth becomes public property.... But all of these are accessories of the real work, though some of them are very likely useful accessories. The actual making of the tune is something that goes on in his head, and nowhere else.⁷

It should now be fairly clear as to what Collingwood means by his claim that the genuine musical artwork, a composition, is an "imaginary object". Composing is an act of "imaginative creation". Making a tune is a mental act on the part of the composer, the product of which is a mental

entity. As such, according to Collingwood, the artwork is essentially complete. This creative act does not require the fabrication of any object, the writing of a score, nor any sounding of instruments in order to constitute an instance of composing a musical work.

An important implication of this view is that the concrete public object, (score, performance, recording, etc.), is not only inessential to a composer's art-making, but such an object is not a genuine artwork at all. Rather, it is only a vehicle for communicating the musical ideas in the composer-as-artist's imagination.

the music, the work of art, is not the collection of noises, it is the tune in the composer's head. The noises made by the performers, and heard by the audience are not the music at all; they are only means by which the audience, if they listen intelligently (not otherwise), can reconstruct for themselves the imaginary tune that existed in the composer's head.⁸

This is a striking claim. Not only does it characterize the composition essentially as a mental entity, but Collingwood goes further in denying the status of music to the actual sounds of a performance. Music is thus fundamentally imaginary. Listening to music, it appears, is merely a matter of getting a composer's musical ideas into your own imagination. (Collingwood makes parallel claims for other art forms as well).

To defend his position, Collingwood compares listening to music to listening to a scientific lecture. The upshot of this argument by analogy is that no one confuses the

collection of noises uttered by the lecturer with the ideas or thoughts that are supposed to make up the actual content of the lecture. What we are supposed to "get out of" the lecture are the ideas not the lecturer's spoken sounds. We do not go to lectures to hear noises, but to "listen" to the lecturer's ideas. Likewise, according to Collingwood, we are not to get out of the performance of a musical work the sounds of music, but we are to "reconstruct in our own minds" the composer's imagined composition.

This comparison seems flawed. Collingwood's point concerning the utterances of lecturers seems sensible enough. Generally speaking, the sounds of a speaker's voice are, for the most part, irrelevant to a lecture's content. The sounds are words that have meaning or reference. A lecture which did not consist of meaningful spoken words would hardly be counted as a genuine lecture. But musical performances are surely quite different from lectures on this very point. It is not at all clear that musical sounds refer to or mean anything in the ways that spoken words do.

Moreover, one could plausibly argue that the relationship between imagined compositions and soundings of the compositions should be understood as quite the reverse of how Collingwood portrays it. That is, compositions exist for the purpose of aiding in the production of aesthetically valuable performances. It is not so much the ideas of the composer that interest us but the aesthetic satisfaction

that comes from hearing certain arrangements of musical sounds. Much composing is a deliberate effort to formalize an arrangement of musical sounds so that these sounds may be repeated in similar fashion over and over again. There is aesthetic satisfaction and interest in hearing actual musical sounds and sequences of these sound. In this way the composition is taken as the vehicle for making musical sounds, rather than the sounds as vehicle for communicating compositions. More on this point later.

Collingwood recognizes that some listeners attend performances only for the "sensual pleasures"⁹ of sounds. He does not regard this as genuine "listening". Furthermore, such attention to these sensual pleasures will impede the genuine appreciation of the musical artwork. Genuine appreciation requires educated listeners; those sorts of listeners capable of reconstructing and understanding the composer's musical ideas.

Must the experience of music be dichotomized into either sensual pleasure or imagined ideas? It seems contrary to general musical practice and experience to deny that educated, experienced appreciators of music do not, and indeed ought not, attend closely to the actual sounds of music for their intrinsic aesthetic value. Much of the aesthetic interest in and qualities of the various rhythms, melodies, harmonies, dynamics, timbres, etc. evident in performances depend on actual soundings of musical

instruments. The aesthetic value and appreciation of performed musical works is vastly different from merely remembering or imagining such works. Why listen to different performances of a composer's work if only to acquaint oneself with his/her ideas? Once I have gotten the composer's ideas in my head, why not simply re-imagine the piece instead of bothering with the costs and other hassles of attending a performance?

Is it only pure sensual delight that follows from carefully attending to the physical sounds of a performance? Or, as Collingwood argues, are these sounds only to be attended to for the purpose of imaginative reconstructions? Isn't the conscientious experience of musical performances richer than the former, yet not so rarified as the latter? Collingwood's dichotomy might be a false one.

Performers often work very hard at "getting the right sound". That is, they aim at aesthetically valuable actual soundings of their instruments. In addition, the various instruments presumably were developed and perfected for the purpose of creating interesting and diverse musical sounds. Why have such potential diversity, some of which are highly subtle, if only to aid listeners in thinking like the composer or becoming acquainted with his expressions? Part of "educated" listening is an improved ability to recognize high quality musical sounds produced on fine instruments by skilled performers.

The wide ranging differences between individual instruments and performers certainly seem to be aesthetically relevant, and composers usually care a great deal about such aspects of music, but under Collingwood's view these aspects are not and cannot be significant. The only real issue with respect to quality of performance is to what extent and how clearly the composer's expressive ideas are conveyed.

But composers, first of all, cannot convey with conventional notation the nuances of sound possible with different instruments and by different players of those instruments. Secondly, composers may not have any specific ideas or preferences in these regards anyway; they may and often do leave these sorts of decisions to performers' interpretations. Does Collingwood's position imply that such nuances are nothing but extraneous embellishments; or worse, distractions from the "true" composition? Is a high quality instrument played by a virtuoso described as such by Collingwood purely because it is better at aiding listeners in the reconstruction of the composer's ideas? Do performers ever count as artists?

Performances seem to be aesthetically valuable in part because they allow the exercise and experience of the art of virtuosity. Part of virtuosity is interpretive ability: the expansion of a composer's musical work beyond what is explicitly contained in its original presentation, usually a

score. At least some, if not all, of this interpretation is expressible only in soundings of a particular instrument by a particular performer. The aesthetic qualities of such performances are not merely a matter of "sensual pleasure", since not just any hearer of the sounds can detect their aesthetic value.

My point here is that Collingwood not only misconstrues the nature of compositions, but that he misrepresents and "short-changes" the nature of performance both from the side of the performer and the side of the listener. Performances do depend upon compositions in crucial ways; but compositions depend upon performances in ways that are not solely practical. Collingwood's so-called educated listener may attend to the actual sounds not merely for their sensual satisfactions, but for unique aesthetic experiences unavailable any other way, and these are experiences often inaccessible to the "uneducated", inexperienced listener.

Also the performer and her instrument do more than merely convey the composer's ideas to other listeners. The aim of virtuosity is not simply the pursuit of a more effective communication of someone else's ideas or the exquisite presentation of a composer's work. It includes an aesthetically motivated desire to make aesthetically valuable musical sounds in their own right. A fine instrument in the hands of an accomplished musician is a

worthwhile and sought after aesthetic experience somewhat independent of the specific composition performed.

Collingwood seems to miss this important part of musical experience. His theory fails to account for what is commonly taken to be a principal reason for public performances: listening to fine musical sounds.

There is another problem with Collingwood's conception of musical compositions that does not involve its implications for performance. If the actual musical artwork is essentially mental, something that originates and is said to properly exist in the mind of the composer, then it would seem that no one else can have access to the genuine artwork except the composer.

Mental phenomena as such are private and thus not available for public inspection. The representation, in whatever form provided by the composer (score, tape recording, etc.), will always be an approximation. This somewhat Platonic conception, the notion that the true and real existent is non-corporeal, the notion that what is physical and sensuous is inferior, creates certain problems when it comes to evaluative questions concerning the composition. It seems that we can never critique the actual musical artwork. One, because the work is, by Collingwood's definition, private and thereby fundamentally inaccessible. Two, unless the composer is at the performance to confirm the adequacy of the performance in representing his/her

ideas, we cannot know that we are hearing, and thus reconstructing in our minds, what the composer intended.

Does such a view force us to commit the so-called "intentional fallacy"? How can a musical work be evaluated or appreciated at all without continuously attempting to get at the composer's intentions? In trying to reconstruct the artist's ideas in our own minds we are actively and deliberately doing what the intentionalist criticism warns against. Collingwood's view, in effect, seems to be just the view that the actual artwork consists of the artist's intentions.

The intentional fallacy is purported to occur whenever someone takes the artist's intentions for an artwork as relevant, if not decisive, for an aesthetic interpretation and/or evaluation of that artwork. The critic of intentionalist approaches to works of art argues that intentions are "neither available nor desirable as a standard for judging the success of work...."¹⁰ The bases for this view are first, the recognition that artist's intentions are essentially private and thus virtually inaccessible to others. The artist may be unavailable because of death or absence. The artist may choose not to reveal his/her intentions. The artist might not effectively or accurately communicate his/her intentions for all sorts of reasons. Or finally, the artist might not be a good interpreter of his/her own work. As the originators of the

intentional fallacy thesis, Wimsatt and Beardsley, point out, "judgement of poems is different from the art of producing them."¹¹

A second basis for the intentionalist criticism is the claim that since intentions are external to the artwork itself, it follows that they are of no significance to any aesthetic interpretation of the work. Intentions are essentially private to the persons having them. As private they are external to the public artifact or public expression that makes up the artwork under this view. That which is external to the actual work of art should not intrude on an aesthetic interpretation of the work itself. The work should be evaluated only in term of what is internal to it; what constitutes the work itself.

Although this critique of the role of the artist's intentions is aimed explicitly at interpretive and evaluative considerations, it carries with it some ontological implications. First, the view implies that a genuine artwork must be a public object and not a private entity of consciousness. To require examination of intentions on the part of the artist suggests that the public artwork is deficient in itself, that it is somehow incomplete. The position that disregards intentions takes the work of art to be an entirely independent object wholly or sufficiently accessible to a spectator or audience. Such an object must be a thoroughly public object; it cannot be a

mental entity in any way.

A second implication of the intentionalist criticism is that since intentions are not relevant to the evaluation of an artwork, they cannot be constitutive of it either. If intentions comprised the "true" work of art, and if intentions are fundamentally irrelevant to interpreting such works, then there would be nothing left to interpret or evaluate.

Collingwood's view not only makes the somewhat uncontroversial claim that artworks somehow originate in the artist's imagination, but he goes much further in arguing that the essential artwork is this imagining on the part of the artist. In this way, Collingwood's view implies that the artworks consist of the artist's intentions; the artist's intentions constitute the real work of art. As we have seen, experiencing a work of art actually amounts to attending to these intentions.

In saying this I have not shown Collingwood's view to be false. Rather I have attempted to show its incompatibility with the intentionalist criticism. I have tried to show its fundamental incompatibility with any view that identifies artworks with a public, physical artifact. And, I have tried to show its incompatibility with any view that focuses aesthetic attention or interest on a sensory experience of this artifact. I shall return to Collingwood in Chapter V.

Nicholas Wolterstorff: Compositions as
Norm-Kinds

In Part Two of his Works and Worlds of Art, Nicholas Wolterstorff develops a theory of musical compositions that construes them as what he calls "norm-kinds". A chief feature of such a conception of musical works of art is that they are to be regarded as a sort of universal, that is, as non-individuals capable of multiple instances. Compositions construed in this way may have any number, (including zero), of particular expressions, examples, instantiations, or as Wolterstorff puts it, occurrences. Such occurrences may take the form of scores, performances, or recordings, and as such may each be referred to by the same name as the composition. Thus, "performance of Schoenberg's 'Verklaerte Nacht'" is to be interpreted as "occurrence- or instance of Schoenberg's 'Verklaerte Nacht'".¹²

From a nominalist's perspective such talk is unacceptable. In this section I shall attempt to show (1) that an alternative nominalistic mode of discourse referring to compositions and performances is possible and reasonable; and (2) that Wolterstorff's construal of musical works as norm-kinds leads to difficulties and misconceptions concerning the practices of composing and performing music.

Wolterstorff claims that "performing some work of art consists of bringing about a performance of it...."¹³ Later he says, "a performance of a work of art is an

occurrence of it...."14 He calls artworks such as these "occurrence-works", and then argues that "most if not all occurrence-works are universals."15 From these initial descriptions he will eventually develop his theory of compositions as norm-kinds. Before I comment on his notion of norm-kinds, I shall consider these initial descriptions.

By describing performances of musical works in the ways that he does, Wolterstorff appears to have prejudiced the analysis in favor of a universalistic interpretation from the beginning. That is, the language he uses to characterize performances presupposes that they are instantiations of universals, and the universals that they instantiate are the compositions from which they are derived. Let me explain.

Although I readily agree that performances are occurrences, I fail to see why such occurrences must be taken as occurrences of the work. To speak of performances as occurrences of the composer's work is to presuppose that compositions are universals or abstract entities of some kind. But we may wish to ask, is "occurrence of" a proper and necessary interpretation of "performance of"? Does "performance of" imply "occurrence of" in the context of music? I shall argue that it does not, and that an alternative interpretation is possible and preferable.

If performances are admittedly occurrences, what then

are they occurrences of? When I attend a performance I listen to the occurrences of certain sounds made in a certain way, in a certain sequence, at a specifiabile time and place. Performances are these actual occurrences of actual sounds. Therefore, musical performances are occurrences of musical sounds. What occurs during a performance is usually a performance of a musical work, not the musical work itself. (Although later I will argue that improvisations are compositions that are created as they are performed, thus improvisations would be performances that are the compositions themselves.) Simply, but somewhat awkwardly, put, performances are occurrences of performances.

To speak of a musical performance as an occurrence of the musical work, rather than the musical sound-events that go together to comprise the performance, is to presume that in hearing the sounds occurring during the performance a listener is actually hearing the musical work itself, the musical composition. If this presumption is made, then the composition's ontological status will tend to be resolved in terms of abstract entities. This is so because the musical composition will have to be construed as the sort of entity capable of being present in different places at the same time. (No one denies that it is possible if not likely that there may be more than one performance of "Beethoven's 'Ninth Symphony'" on any given date.) Thus a composition

will be construed as an universal, the sort of entity capable of such multiple occurrences.

I would like to propose a different way of interpreting the two phrases "performance of" and "occurrence of" in the musical context. Consider this distinction: Beethoven's "Ninth Symphony" and "Beethoven's 'Ninth Symphony'". The work of art that was composed by Beethoven can be referred to as Beethoven's "Ninth Symphony". The name Beethoven refers to the composer, identifying the maker/creator of the artwork "Ninth Symphony". Beethoven's "Ninth Symphony" names a unique object: the original composition.

Naming a performance is a somewhat different matter. To say, "Last night I listened to a performance of Beethoven's 'Ninth Symphony'", is to significantly under-represent the event; that is, it is an incomplete description of what actually happened. A performance is usually produced by some musician or musicians making musical sounds. (John Cage's "4'33'" notwithstanding. This is an extremely minimalistic piece comprised entirely of one musical element, namely, silence. See Chapter III on "musical elements".)

When an orchestra performs Beethoven's work, we are actually listening to the musical sounds intentionally produced by that group of persons. Thus, if The Chicago Symphony Orchestra, for example, performs the work mentioned

above, an appropriate description for this performance would be, The Chicago Symphony's "Beethoven's 'Ninth Symphony'". That is, the performance, what occurred last night, was made by The Chicago Symphony. In this case, the name The Chicago Symphony identifies the maker/creator of the musical event attended and attended to last night. Strictly speaking, "Beethoven" here does not refer to the maker of the object of our attention; rather, his name identifies the maker of the composition from which the performance happens to be derived. In effect, "Beethoven" becomes a part of the name of the performance. Whereas "Ninth Symphony" names a composition, "Beethoven's 'Ninth Symphony'" names a performance.

What occurs is not an occurrence of Beethoven's "Ninth Symphony", rather what occurs is a performance of The Chicago Symphony's "Beethoven's 'Ninth Symphony'". Thus, we never hear Beethoven's "Ninth Symphony", but only some musicians' performance of "Beethoven's 'Ninth Symphony'". We can only hear sounds, and the composition under consideration, (Beethoven's "Ninth Symphony"), is not itself made up of sounds, therefore this composition is not capable of being heard.

Occurrences are events, happenings in space and time. What happens during a performance of Beethoven's work are sounds and actions, but Beethoven's work doesn't happen during a performance because his work is not sounds and

actions, at least not at this time. Therefore, it appears that saying we can listen to an occurrence of Beethoven's work of art is imprecise, since his work is not occurring at all; it is not the sort of thing that can occur during a performance. What we mean when we do speak of listening to an occurrence of this piece is that we listen to an occurrence of "Beethoven's 'Ninth Symphony'", not Beethoven's "Ninth Symphony".

Now there is a legitimate sense in which we may speak of the occurrence of Beethoven's "Ninth Symphony". That would be when we refer to the occasion of Beethoven's composing, (the actual selecting and arranging of musical elements into an intended original). This is a dateable event or set of events, and resulted in a particular artifact: most likely a manuscript score. In other words, Beethoven's "Ninth Symphony" occurred when he made composed it; "Beethoven's 'Ninth Symphony'", on the other hand, occurs whenever a musical performance is created with the appropriate connections to the original compositional work "Ninth Symphony" by Beethoven. (These appropriate connections have been discussed earlier in Chapter III).

Does "performance of x" imply "occurrence of x" as Wolterstorff seems to think? I have been trying to show that the performance of an already existing musical composition consists of making a different object, (or more precisely, an event), distinct from the composition itself.

To perform a work is not to make the work occur; to result in an occurrence of the work. The work itself already occurred at the time of its creation by the composer. It may be said to exist, if the manuscript, (or whatever sort of artifact the original composition is, e.g., tape composition), has not been destroyed. We may say that the "Mona Lisa" occurred at the time daVinci painted it, but now, instead of saying that it occurs in the Louvre, we say that it exists there. In like manner, under my approach to musical compositions, Beethoven's "Ninth Symphony" occurred when he composed it, but now exists wherever the manuscript resides.

To perform a work is thus to bring about a sound-sequence-occurrence that is derived from an already existing musical work, i.e., a composition. (Improvisational compositions are modifications of this formula, as was described in the previous chapter, in the sense that, though they are performed musical works, they are not derived from some already existing work.) Performing a work consists, in part, of a cultural practice that amounts to admitting that the musical event being made, (occurring), is not fully original. The performance of a composition is not an occurrence of the composition, rather it is a unique entity, (i.e., an new individual ontologically distinct from the composition from which it was derived), resulting from using a composition as a basis

for making choices with respect to musical sounds.

Again, a comparison with painting might be helpful. If a painter were to make a copy of the "Mona Lisa", we do not speak of it as an occurrence nor as an instance of the "Mona Lisa". Instead it is called a copy, an imitation, or possibly a forgery, depending on the circumstances of its presentation. This painter may or may not intend to make an indistinguishable copy of the original. She may succeed in accomplishing these intentions to varying degrees. In any case, the result is a new artifact derived, to some greater or lesser extent, from an original work of art. This new artifact is not an instance nor an occurrence of the original, daVinci's work of art.

Performing a musical work is somewhat similar. The performers are making a new artifact acknowledged to be derived from some other musical object, the composition. These performers may or may not intend to perform the work precisely as the composer is believed to have wanted it. They may succeed in accomplishing their intentions to varying degrees. In any case, their efforts result in a new artifact, (event/object), derived from and, to some greater or lesser extent, compliant with, an original composition. It so happens that, in music, most performers do aim at a high degree of compliance in doing what the composer wanted. This is largely a matter of cultural practice, or certain customary expectations in the musical artworld.

I do not wish to stretch the comparison between painting and music too far. The practices and products of these two arts are obviously quite different, and these differences are important. But I do think, as I have explained earlier, that they are ontologically more similar than is usually thought. My thesis continues to be that musical artworks, (compositions and performances), are concrete particulars. I take it that paintings are less controversially so. (Clearly, there are those who will dispute even this claim. I am not concerned here with defending the notion that all artworks are concrete particulars. I am maintaining that if the reader grants that paintings, or any artworks for that matter, can be understood as concrete particulars, then musical artworks may be interpreted in a similar manner.) Painting thus provides a useful paradigm art for comparison.

An implication of Wolterstorff's view that performances are occurrences of the musical work of art, and that as such the work is a universal, is his claim that recordings of performances, although not themselves performances of the work, are nevertheless occurrences of the work.¹⁶ This is admittedly consistent with his "universalistic" interpretation of musical works. Once one has accepted that something other than the work itself can be an occurrence of it, (as Wolterstorff has done in the case of performances; he has not said that performances of a

work are the work, rather they are occurrences of it), then it seems evident that there may be various ways that works can occur.

But if speaking of occurrences of the work in this way requires accepting the notion that works are universals, how might a nominalist reinterpret the situation? I agree with Wolterstorff that recordings of performances are not themselves performances. Performances are intentional acts in which a musician or musicians produce actual sound-sequence-occurrences. As I have explained in Chapter III, performances of a composition consist of performers selecting and arranging musical sounds with the intention of making these selections based upon the composition. (Wolterstorff requires that the performance, in addition, exemplify "the properties normative within the work". I will discuss his notion of "normative within" when I consider his concept of "norm-kind" below). Recordings are not performances, and Wolterstorff would concur, because they are not themselves the results of this intentional selecting and arranging.

But I do not follow Wolterstorff in his assertion that a recording of a performance of "Beethoven's 'Ninth Symphony'" is an occurrence of Beethoven's "Ninth Symphony", the composition. Indeed, in my view, it is not even an occurrence of The Chicago Symphony's "Beethoven's 'Ninth Symphony'". A recording of a

performance is not an occurrence of the performance. To speak this way would be to construe the performance itself as an abstract entity, since it would permit multiple instances of this performance at the same time in different places. Also, since performances are events and thus transient, they no longer exist after they have occurred. What does not exist certainly cannot be said to occur.

A number of things seem to be clear about recordings of performances. We should distinguish between three senses of the term "recording": (1) the act of recording; (2) the physical product of that act; and (3) physical copies of this produced object. The first involves using certain sorts of electronic equipment at the time of the performance-event. As an action, or set of actions, a recording in this sense is an occurrence, an occurrence of recording a performance.

The second sense refers to the object that is made as a result of using the equipment. It is thus a physical "record" of the sound-sequence-occurrence that made up the performance. Usually today this object consists of a length of magnetic tape that has been micro-physically altered so as to be capable of reproducing a sound-sequence-occurrence that sounds, to varying degrees, much like the original performance from which it was made.

Finally, a recording may be a physical object that is

a copy or reproduction of the original product of the act of recording. In this sense, a recording may be another length of tape made by copying the original tape. Or this recording may be in a medium different from the original recording, (e.g., a grooved vinyl disc). In any case, this sense of recording refers to some object derived from the initial product of the act of recording which is also capable of reproducing a sound-sequence-occurrence that sounds, to varying degrees, much like the performance from which it was ultimately derived.

The phrase "from which it was ultimately derived" should be interpreted in terms of historical and causal connections. That is, the act of recording is an event occurring simultaneously with the performance. The product of this act is, as such, caused by these events. The recorded copies of this product are themselves caused by this product. Thus, there is an historical and causal chain of events and objects linking the performance with subsequent recordings.

The point here is that the relationships between performances and recording are not to be understood as any kind of instantiation of an exemplar. Something is a recording of a performance not because it is an instance of the performance, but rather because it is historically and causally traceable back to a performance. Furthermore, nor is the recording an instance of the compositional work,

since, as was explained in Chapter III, the relationships between performances and compositions are also historical and causal, the relationships between recordings and compositions are also describable in terms of an extended series of historical and causal connections.

Consider this example. If someone is said to be listening to "Beethoven's 'Ninth Symphony'" on a tape player, what is actually happening is that the person is listening to, for example, the playback of a copy of Philips' recording of "The Chicago Symphony's" performance of "Beethoven's 'Ninth Symphony'" Philips has made an object, (a recording), that was derived from a performance that was derived from a composition.

Describing the situation in this way does not require reference to any abstract entities. The copy of the recording, the recording, the performance, and the composition are each concrete particulars, (objects or events). They are each unique entities made by different persons through various means or processes and in different media. The practice of referring to each of these items by the same name, "Beethoven's Ninth Symphony", is a matter of customary shorthand.

For now, the point I am attempting to make is that recordings in the musical context are not instances, examples, nor occurrences of the work from which they were derived. Rather, recordings can function in the audial

mode the way that photographs can function in the visual mode. A photograph of the "Mona Lisa" is never considered an instance of the "Mona Lisa". A photograph of the "John Hancock Building" is not considered an example of it. A photograph can "record" some visually perceptible object or event; whereas a sound recording can "record" some audially perceptible object or event.

(It should be recalled that, under my view, some recordings, often on magnetic tape, are themselves compositions. Some composers produce "tape compositions"; that is, they compose their works by means of recording musical sounds directly onto tape. Their compositions are thus not comprised of notational manuscripts. If a score of this tape composition is made later, it does not constitute a composition, rather it would have a similar status as that of any scored copy of a manuscript--it would not be a composition. A tape composition as a concrete artifact is the composer's artwork.)

So far I have presented a possible nominalistic interpretation of discourse about musical compositions, performances, and recordings. Unlike Wolterstorff, I do not see a need to regard performances and recordings as occurrences or examples of the composition. I have not argued that one way of speaking, nominalistically or "universalistically", is inherently superior; only that a nominalistic interpretation of the sort of language

Wolterstorff uses is plausible and not problematic.

Next I shall address Wolterstorff's characterization of compositions as norm-kinds. My primary objective will be to show how this conception leads to some strange, if not undesirable, implications, and that this suggests that an alternative conception may be preferable. My own theory of compositions as concrete particulars is offered as just such a preferable alternative.

Wolterstorff believes that musical works are "norm-kinds". Not all kinds are norm-kinds. Norm-kinds are those sorts of kinds that permit the possibility of malformed examples of that kind. For example, the norm-kind 'Dog' allows for a meaningful distinction between properly-formed dogs and improperly-formed dogs, (a dog missing a tail is still a dog, and thus an example of the kind 'Dog', albeit a moderately malformed one). On the other hand, the kind 'Red Thing' is not a norm-kind, since it is nonsense to speak of an improperly red thing. It is not possible to assess a red thing in terms of the degree to which it is properly red.¹⁷

Another way to get at Wolterstorff's conception of a norm-kind is through an understanding of his distinction between properties that are "essential within" and those that are "normative within" a given kind. A norm-kind will be a kind that has among its constituent properties at least one property that is normative within it.

A property normative within a kind is a property that a properly-formed example of that kind must have.

Wolterstorff's formal definition is as follows:

The property P is normative within the norm-kind K = df K is a norm-kind, and it is impossible that there be something which is a properly-formed example of K and lacks P.¹⁸

By contrast, a property essential within a kind is formally defined this way:

(Having) the property P essential within K = df P and K are such that necessarily if something is an example of K it has P.¹⁹

The central feature of this distinction is represented by the qualifying phrase "properly-formed". In effect, a property essential within a kind is one that contributes to determining whether or not some entity is an example of the kind. Without this property a thing cannot be an example of the kind. But a property normative within a kind does not play any role in determining whether or not something is an example of the kind in question. It determines rather the degree to which it is "properly-formed". It has something to do with how "correct" an example it is.

Therefore, in the case of the norm-kind 'Dog', the property of "being an animal" is a property essential within it. Something cannot be an example of 'Dog' unless it possesses the property of "being an animal". The property "having four legs", on the other hand, is a

property normative within this kind. Lacking this property does not undermine a thing's status as an example of 'Dog', but the lack of this property does indicate that the example is less than properly formed, that it is deficient with respect to four-leggedness.

In other words, possessing the property "being an animal" is a necessary condition for something being counted an example of the kind 'Dog'; possessing the property "having four legs" is a necessary condition for something being counted a properly-formed example of the kind 'Dog'. It follows then that, "any property essential within a norm-kind will also be normative within it; but not so vice versa."²⁰ After all, another property necessary for being counted a properly-formed dog is "being an animal"; but as should be quite clear, it is not necessary that all dogs possess the property of "having four legs" in order to qualify as examples of 'Dog'.

What of so-called "accidental properties"?

Wolterstorff does not discuss any properties accidental within, and with good reason. The sorts of properties he has described are those that go toward characterizing kinds themselves. Any given kind will be determined only by properties necessary to its character. "Normative" and "essential" properties are properties within the kind; they are somehow constitutive of the kind. "Accidental" properties, on the other hand, are not the sort of

properties that are within the kind. Such properties would attach to examples of a kind, but not to the kind itself. It would appear then that kinds as kinds do not have properties accidental within. Therefore, norm-kinds have two sorts of properties: those essential within and those normative within. Examples of norm-kinds can have three sorts of properties: essential, normative, and accidental.

The norm-kind 'Dog' possesses the properties of "being an animal" and "having four legs". A dog must be an animal in order to be considered an example of 'Dog'; it must have four legs to be considered a properly-formed example of 'Dog'; but it may be virtually any color whatsoever, (essential, normative, and accidental properties respectively).

This theory of norm-kinds has application to music in the following manner. Musical works, or compositions, are norm-kinds, according to Wolterstorff. As such they possess properties essential within them and properties normative within them. Performances, which are examples, or occurrences, of musical norm-kinds, must possess all the properties essential within the work in order to count as performances of the work. Furthermore, they must possess the properties normative within the work in order to count as "correct" performances of the work. This last feature of the work/occurrence relationship is particularly important

to Wolterstorff. He takes it that an adequate theory of musical works and performances must be able to account for the generally accepted view that works may have incorrect performances. (His criticism of Nelson Goodman's theory centers on just this issue.²¹)

An incorrect performance, on Wolterstorff's theory of norm-kinds, would be any performance that lacks some property normative within its associated work, the composition. It should be clear by now that, unless some property normative within a work is also essential within it, some performance's failure to possess a normative property and thereby be an incorrect performance is not decisive against a performance's status as a performance of that work. An incorrect performance of a work is usually still a performance of it.

At what point divergence from the properties normative within a work a performance ceases to be an example of the work at all Wolterstorff does not specify. This seems sensible since it is not clear how it could be otherwise. Properties normative within a kind simply do not play a role in determining that a thing is an example of the kind. He does say that performing of a work consists, in part, of bringing about "a sound-sequence-occurrence ... which comes fairly close to exemplifying the acoustic and instrumental properties normative within that work...."²² (emphasis mine).

To summarize Wolterstorff's position thus far:

Musical works are norm-kinds, and as such possess properties normative within them that establish criteria for evaluating the correctness of examples of the work. Any example of the work may be more or less correct according to the degree to which the example manifests the properties normative within the musical work from which it is derived.

Let us now examine what Wolterstorff has to say about composing and performing. This will provide us with a fuller understanding of the implications of his theory. Some of these implications will provide points for criticism later on. Since musical works consist largely of properties normative within them, composing consists most simply of selecting such properties. More specifically,

in selecting a set of properties as criteria for correctness of occurrence the composer composes a work. And the work composed ... is that one which has exactly those properties and their prerequisites as normative within it.²³

In this way, a composer uniquely determines a musical norm-kind that may serve as a guide for making musical sound-sequence-occurrences, (variously described as examples of the kind or as occurrences of the work).

A few observations are in order here. Since musical works are norm-kinds and composers are those persons who select the properties normative within the work, Wolterstorff argues that composing consists of intentionally selecting properties for the purpose

of determining correctness of occurrence. What this implies then is that improvising is not composing. Improvisation, as was discussed in Chapters I and III, is the musical practice of performing musical sounds, to varying degrees, extemporaneously. That is, the performing musician makes sound selections at the moment of performance somewhat spontaneously and somewhat independently of the composition. She may go outside the specified elements of the composition by adding embellishments; she may alter, in large measure, various components of the piece, or she may even create completely new passages of music on the spot.

Improvising does indeed involve selecting musical properties, but it does not involve doing so for the purpose of determining correctness of occurrence for some subsequent sound-sequence-occurrence. Wolterstorff emphasizes this aspect of improvisation in his denial of its status as genuine composing. In his terminology, the improvising musician is not selecting properties normative within a kind, therefore she is not producing a norm-kind, and so she is not composing a work. Improvisations are thus never themselves compositions. I will have more to say about this conclusion later.

Another point to be noted is that performances are not the only sort of examples that a musical composition can have. Sound recordings may be examples of a work in so far as they come "fairly close" to exemplifying the properties

normative within the work. Thus recordings are evaluated for correctness of occurrence in the same way as performances. If a composer were to specify that "being performed" is normative within the work, does this mean, under Wolterstorff's view, that a recording of such a performance is not an occurrence of the work? Not at all; it would only follow that the recording was a less than correct occurrence. But what if a composer claims that the property "being a performance" is essential within the work? According to Wolterstorff's conception of composing, a composer cannot do this. Composing is essentially the selecting of properties which determine correctness of occurrence, i.e., properties normative within, not selecting properties essential within. Wolterstorff does not explain why composing should be limited only to selecting normative properties; it appears that he merely stipulates this. (Actually, he seems somewhat unclear on this point.)

Could a composer compose a work that is unperformable? An interesting and important feature of norm-kinds, as construed by Wolterstorff, is their complete independence from any examples. That is, a norm-kind does not need to have any examples in order to exist.²⁴ Therefore, a compositional work need never be performed, recorded, or otherwise instantiated to be counted as a genuine work. Such a claim is justified by Wolterstorff first, on theoretical grounds, that as a sort of universal, norm-kinds

have the crucial characteristics of universals. One such key characteristic of universals, as traditionally understood, is their capacity for existence independent of instantiation. Wolterstorff argues that just as the universal or idea of 'Dog' could be said to exist even if no actual dogs existed, or just as the kind 'Unicorn' exists uninstantiated, so a norm-kind such as 'Beethoven's Ninth Symphony' could be said to exist even if it were never performed.

A second justification he offers is the common sense recognition of the fact that composers sometimes have trouble getting their works performed, especially if these works are highly unconventional or the composer is little known. Our ability to meaningfully talk about and identify such unperformed works is evidence to their status as genuine works. The point is that composers can produce works of compositional music that yet go unperformed or unsounded. This is not an uncommon occurrence in the real world of musical practice. Therefore, we can acknowledge the existence of unperformed or even unperformable musical works.

We now arrive at a somewhat strange element in Wolterstorff's theory. He says on page 67, "we must not overlook the fact that there are musical works which were probably never composed." He gives as an example what he calls "works of indigenous folk music", which he claims,

"just emerged from performances."²⁵ In other words, the work is not the product of any identifiable person's act of selecting normative properties, instead the properties are recognized and used to determine correctness of performance or occurrence. Thus, he suggests distinguishing two sorts of originators of musical works: composers and practitioners. Further, he adds, "a work is always a work of somebody. Nothing is ever a work of music without, in one or the other of these two ways, being the work of some person or persons."²⁶

The first peculiarity here is Wolterstorff's claim that some works are uncomposed. It may be that some of the works he is referring to are not so much uncomposed, as they are the product of some unknown composer, forgotten or unidentified. Alternatively, folk music of the sort he describes may have resulted from the combined efforts of many individuals, again with lost identities. Such works could still be described as composed but again by unknown composers. Surely the inability to identify composers should not count against a work's status as composed. Wolterstorff does not clearly explain the alleged process by which practitioners generate musical works, so it is difficult to evaluate the plausibility of his suggestions on this matter. But if we grant that there actually are works that have simply "emerged" from the practice of making music without any explicit intention of selecting normative

properties, why must we regard these as uncomposed?

As with his objection to regarding improvising as a form of composing, it seems that Wolterstorff is determined to confine composing to the intentional selection of normative properties. That is, composers must have as part of their intention, as they go about the selection process, that the choices they make be taken as normative for subsequent occurrences. Improvisors and practitioners are not composers because they do not explicitly propose that their musical selections be used as instructions for future musical products.

Nevertheless, so-called practitioners apparently do produce musical works, describable as uncomposed norm-kinds; whereas, improvisors apparently do not produce works, rather they produce what might be referred to as uncomposed non-kinds. Although he does not say so, it would seem that if an improvisation were recorded or remembered and then imitated to some degree and repeatedly so, that such an improvisation may eventually become the sort of work that Wolterstorff regards as the product of practitioners rather than composers. Thus, in this way an improvisation may evolve into or become a work

Since, for Wolterstorff, works are kinds, and since kinds are the sorts of things that are capable of having examples, and since improvisations are unique individual events which lack the capacity for multiple instantiation,

(although we may wonder whether Wolterstorff would consider recordings of an improvisation occurrences of the improvisation and thus examples of it?); therefore, improvisations cannot be regarded as any sort of kind, and, as such, cannot be regarded as works. Thus, not only are improvisations not compositions, but they are not musical works.

Wolterstorff acknowledges that improvisations may become compositional works if the performer later goes about making a score based upon the performed improvisation. He says,

Suppose that someone has improvised on the organ. And suppose that he then goes home and scores a work of such a sort that his improvisation, judged by the requirements for correctness specified in the score, is at all points correct. In spite of that, the composer did not compose his work in performing his improvisation. In all likelihood, he did not even compose it while improvising. For in all likelihood he did not, during his improvising, finish selecting that particular set of requirements for correctness of occurrence to be found in his score.²⁷

This description of improvisation seems consistent with Wolterstorff's theory of musical works as norm-kinds. But it leads to a perplexing dilemma.

Suppose that some performer, or group of performers, improvises an extended musical passage with a clear beginning and ending. Someone else, by virtue of an excellent memory or a sound recording of the event, produces a detailed score of this musical performance. Clearly, under Wolterstorff's view, we have a musical work. The

score allows for the production of examples of sound-sequence-occurrences that can be judged for correctness according to the indicated normative properties contained within the score.

Who is the composer of this work? According to Wolterstorff's characterization, neither improviser nor score-writer can qualify as composer. The improviser cannot be the composer because he never intended that his musical selections be taken as normative for future performances. He may never consent to this scoring, (the scoring is unknown to him, he may die soon afterward, he may even refuse to authorize the scoring), and so never confirm the properties of his performance as normative.

The score-writer cannot be the composer, under Wolterstorff's view, because, although she may intend that the score be taken as describing normative properties for subsequent musical occurrences, she did not select these properties herself. (Besides, if Wolterstorff's theory did lead to the conclusion that the score-writer is the composer, so much the worse for the theory. Such a conclusion would be quite counter-intuitive and inconsistent with general musical custom.)

Not only is such a situation as described possible, but it is quite probable, especially within musical idioms that often emphasize improvisation, such as jazz, blues, and rock. It is not uncommon for performers in these styles to

be: (1) unable to score their own music, since they do not know or use musical notation; and/or (2) uninterested in setting their music down as compositional works.

Wolterstorff may simply argue that these works resulting from improvisational performances are uncomposed works. The notion of uncomposed works may be troubling in itself to some of us, but in this case what makes the claim seem particularly odd is that we can clearly identify the person or persons directly responsible for the existence of the work. Yet, because of certain criteria established by Wolterstorff, namely, the intention to make a work and the selection of normative properties, such persons are denied the status of composers.

I would now like to consider another peculiar consequence of Wolterstorff's theory of musical works as norm-kinds. On page 88, he says, "if the ontological principles in accord with which we have been conducting our investigation are correct, then no kinds come into or go out of existence." He then adds that since musical works are kinds, "a composer does not bring that which is his work into existence. Musical works exist everlastingly."²⁸ (We may note that by 'modus tollens', any suspicions concerning the consequent of the above stated conditional, i.e., "no kinds come into or go out of existence", would have direct bearing on the soundness of Wolterstorff's "ontological principles". In other words, if it is spurious that kinds

exist everlastingly, then the ontological theory upon which such a claim is dependent is also spurious.)

Instead of assessing the whole of his theory of kinds, I shall examine its specific application to music. Is it reasonable to accept the conclusion that "musical works exist everlastingly"? First of all, Wolterstorff seems to contradict himself on this point. As we may recall, he said that "a work is always a work of somebody."²⁹ How can something be the product of some person's efforts if it has always existed? If musical works exist everlastingly, then they predated the existence of the composer. Wolterstorff says,

What the composer does must be understood as consisting in bringing it about that a preexistent kind becomes a work--specifically, a work of his. To compose is not to bring into existence what one composes. It is to bring it about that something becomes a work....The only thing a composer normally brings into existence is a copy, a token, of his score.³⁰

I think this passage is somewhat confused. If musical works exist everlastingly, then how is it that a composer brings it about "that a preexistent kind becomes a work" if the work already exists? Something cannot become what it already is. If musical works exist everlastingly, then in what sense does a composer select normative properties? or is it really a matter of discovering them?

Additional strange implications of the claim that works exist everlastingly seem to follow:

- (1) All works past, present, and future have always

existed and will always exist; and a large number of these are uncomposed.

(2) There are an infinite number of existing musical works, the vast majority of which will never be composed, (i.e., no person intentionally selects properties normative within the work), nor heard. Since there would seem to be an infinite variety of possible combinations of musical properties, and it is possible that any of these combinations could be selected and arranged by some composer at some time, then each of these infinite combinations must be considered a work, and furthermore, as a work, each must be everlastingly existent.

(3) Since improvisations are not compositions and not works, and since compositions are not created by their composers³¹, does this mean that improvisations might qualify as genuinely created by their improvisors? Since an improvisation is not a kind, but instead an individual, there is nothing to preclude it from being regarded as a new and original created object. This does not imply that improvisations are created, only that it appears they might be. Could it be then that improvisors are creators, whereas composers are not? An odd consequence indeed.

As we have seen with Collingwood earlier and with Wolterstorff now, it is quite apparent that the ways in which musical compositions are construed ontologically can lead to interesting yet problematic implications.

Ontological commitments in musical contexts are not without aesthetically significant consequences. A principal purpose for my examination of the preceding views and the one to follow is not so much to show the views false, (although I am inclined to think they are less than adequate), nor merely provide theoretical contrasts with my own view. Rather, I hope to reveal the sorts of implications ontological commitments in music have, and leave the suggestion that theoretical accuracy in this context is partly a function of a theory's ability to fit musical experience and practice. So far, I think I have raised at least some important inadequacies in this regard with respect to the presented theories. Let us examine one more proposal.

William Webster: Compositions as
Abstract Particulars

One additional ontological characterization of musical compositions that I should like to consider is a theory developed by William Webster. His view consists in regarding musical works as "abstract particulars". Whereas Wolterstorff has described compositions as "norm-kinds", a sort of universal; and I have described them as "concrete particulars", a sort of individual; Webster prefers to describe musical works as "abstract particulars", an ontological category lying somewhere between individuals and universals. For Webster, compositions are not properly

understood as either individuals or universals.

To begin with, Webster distinguishes universals from individuals in terms of what I shall construe as four key features:

- (1) physical uniqueness and temporal specificity.
- (2) spatial and temporal continuity, (or contiguity).
- (3) capacity for multiple instantiations.
- (4) independence from individual realization.

Individuals are identifiable as manifesting features (1) and (2); universals manifest features (3) and (4); and as we shall see, abstract particulars manifest, in a sense, features (1) and (3). Note that for each of the three ontological categories, (universal, individual, abstract particular), two features are affirmed for that category, while the remaining two features are denied. For example, an individual may be defined positively as an entity physically unique, temporally specific, and spatially and temporally continuous. Or it may be defined negatively as the sort of entity that is not capable of multiple instantiations and is entirely dependent upon individual realization for its existence. Similarly, universals may be understood negatively as the sorts of purported entities that do not exist in any physical, temporal, or continuous, (i.e., contiguous), way.

The characterization of the distinction between universals and particulars in the above manner is

sufficiently consistent with what has preceded it in this paper that little more need be said about it. But what of Webster's notion of abstract particulars? Let us now examine how they differ from both universals and individuals within the context of the four features mentioned above.

As stated earlier, abstract particular may be understood as referring to the ontological category manifesting features (1) and (3). At first gloss it seems quite strange to say of a thing both that it is physically unique and temporally specific and that it is capable of multiple instantiations. What Webster means by such claims shall now be spelled out.

Under Webster's view, abstract particulars, like universals, are themselves neither spatially nor temporally continuous entities. That is to say, they may be understood as capable of existing in more than one place at more than one time. For example, the universal 'Dog' is supposed to be present, in some manner or other, in each of any number of spatially and temporally separate individual dogs. The abstract particular 'Beethoven's Ninth Symphony' is likewise existent somehow, according to Webster, in each of any number of spatially and temporally separate musical realizations that warrant the title "Beethoven's Ninth Symphony". (What permits such warranting of a title for Webster will be discussed below). How then is the notion of abstract particulars to be distinguished from that of

universals?

Unlike universals, abstract particulars, as described by Webster, do not exist independently of their realizations. Whereas the universal 'Dog' requires no instantiations in order to exist as a universal, 'Beethoven's Ninth Symphony' must be realized in some medium or other in order to properly say of it that it exists. As Webster puts it,

The distinguishing feature of an abstract particular is that it does not exist at all unless it exists at some place at some time, but its identity is independent of the continuity of times and places at which it exists. An abstract particular exists wherever and whenever it is realized, and unrealized does not exist at all.³²

Thus, abstract particulars manifest feature (1) in the sense that they must be physically and temporally realized in some medium or other. Webster mentions that in the case of music such realizations are usually either scores or performances; but other media are available: "topological makeup of the grooves on a record, radio waves traveling in space, magnetic patterns on tape, . . .,"³¹ and so on.

In these latter terms, Webster's and my views are somewhat compatible. It would seem safe to say that in certain respects Webster's theory of compositional works appears to be a physicalist theory, as is mine. When he says that, "there is no work independent of a realization and no realization without a medium,"³² and the examples he gives of the various media are all physical, it seems

justifiable to conclude that all musical works, under Webster's view, are physical entities. Actually, Webster is a bit unclear about this. He says that all works must be realized in some medium, and that each realization is an individual. He has already defined individuals, in part, as essentially "physically unique". Yet later on, in the context of his discussion of authoritative realizations, (which I will address shortly), he says, "the authoritative realization may be in the composer's mind."³⁴ It is not clear how such a mental realization could be regarded as "physically unique and temporally specific". Webster does not explain this.

But Webster and I diverge more significantly with respect to his claim that musical works manifest what I have called feature (3). That is, Webster believes that musical works, as abstract particulars, are capable of multiple instantiations, or as he puts it, "discontinuous existence". Discontinuous existence is essentially a denial of feature (2), a definitive characteristic of individuals. For Webster, whatever is an abstract particular is not an individual just because it can be said to exist in more than one place at more than one time.

An abstract particular may exist in realizations (which are individuals) which are discontinuous spatially and temporally with one another. All of its realizations are individuals, but the abstract particular is not identical with any of its realizations, and yet is identical in all of its realizations.³³

In this way, insofar as Webster and I might agree that musical works must be physically actualized, we disagree over his claims that, (a) a musical work is not identical with any single individual, and (b) a work may exist equally in multiple realizations.

It is on just these points that Webster's theory is not nominalistic, and thus distinct from my theory. As such this fact alone certainly does not count against its adequacy, unless of course one is a nominalist. My intention at this point is merely to clarify the essential distinctions between my approach and others; and to indicate that what we have in Webster's theory is an approach that appears to be physicalistic, like mine, (but unlike either Collingwood's or Wolterstorff's), yet at the same time, unlike mine, (but similar to Wolterstorff's), is not particularistic. (Although Webster calls musical works abstract particulars, I do not believe that his conception of works is actually particularistic, and so "abstract particular" may not be, in my view, an accurate name for the ontological category he develops. I shall examine this question of terminology in the next chapter.)

In order to more fully understand Webster's theory, it is necessary to examine some other aspects of his proposed conception of musical works.

Two elements of Webster's theory are specifically relevant and important to the context of my thesis. First,

his description of compositions as "two dimensional" abstract particulars. Second, his notion of "authoritative realization".

Webster states that "a work of musical composition is an abstract particular in two dimensions. The work exists whenever, and only whenever these dimensions are realized in some medium."³⁷ These two dimensions consist of relations organized tonally (or vertically) and rhythmically (or linearly). The relata of these dimensional relations may be virtually anything as long as "they are elements of a potential medium for work realization".³⁸ For example, a composition realized in the medium of sound as a performance would consist of sounds related to one another in terms of tonal intervals, (changes in pitch), and rhythmic intervals, (changes in duration and accents of sounds over time). A score, on the other hand, is a realization in the medium of notation in which various symbols, ("notational phenomena"), are related to one another in such a way that some symbols describe tonal intervals and others describe rhythmic intervals.

Webster wishes to emphasize, at this point, that musical elements, (the relata of dimensional organization), in whatever medium, are, by themselves, minimally significant. What is most significant are the relations or intervals between the elements. More simply, the identity of a composition is a matter primarily of the way in which

musical elements are arranged. A performance of a composition, for instance, is not merely the collection of certain musical tones and rhythms, but the proper sequence and arrangements of these certain tones and rhythms.

Now the identity of most any compound individual is to some extent a function of the arrangement and relations of its constituent parts. The relational arrangement of my body parts has much to do with my individual identity; not just any arrangement will do. Clearly, Webster is saying more than that the way the relata are related is crucial to some work's identity. He is indicating something about how these relations are decisive for identifying realizations of some given musical work, i.e., how each of several realizations are realizations of a single work.

In attempting to clarify just how this is so, he says, "every correct and complete realization of a work is isomorphic with some authoritative realization of the work with respect to the ordered sets of relations in each dimension."³⁹ Thus, a genuine realization is something that manifests a set of relations specific to that work. What are most important are the relations, not the relata.

Webster has now introduced his notion of "authoritative realization". Such a realization is defined as that realization which serves to identify "violations of, deviation from, and alternative work realizations."⁴⁰ Authoritative realizations function similarly to

Wolterstorff's norm-kinds: they help us distinguish correct from incorrect performances, scores, recordings, etc. As was partially described above, Webster's view is that whatever realization is taken as authoritative, any other realization which is isomorphic with it counts as a correct and complete realization of the work. It should be noted that Webster is not identifying the composition with its authoritative realization; rather, it serves only a practical function for identifying more or less accurate realizations of a work.

The following characteristics of authoritative realizations (henceforth AR) may help clarify Webster's notion further:

(1) ARs need not be temporally prior to any other realization. An AR is not necessarily the original or first realization of a work. Rather, the AR is whatever realization is given or taken to have the proper authority. Webster does not specify how this occurs; partly because there are any number of ways that it could. He does say that, "The justification for considering a particular realization to be authoritative is pragmatic, not logical."⁴¹ In other words, there are no specifiable rules or criteria for determining which realizations are ARs, such as temporal priority.

(2) The compositional work is not identified with the AR. The AR only provides the means for comparing and

contrasting various realizations of the work, but the AR is itself not the work.

(3) Any given composition may or may not have an AR. Or there may be more than one competing ARs. This may be so for various reasons. The composer may not have indicated what would count as the AR. The composer's AR may be lost or unknown. There may be conflicting views over which of two or more competing ARs is the true AR. And so on.

Earlier I mentioned that Webster claims that an AR could exist in the composer's mind. I suspect that a justification he might offer for this claim would be that such an occurrence qualifies as a realization in a medium to the extent that it is spatially unique and temporally specific insofar as it is a mental phenomenon in the mind of the composer.

I shall leave further commentary on Webster's theory for the next chapter, in which the issues and the perspective he presents will be discussed directly and in the context of the other two theories discussed in the present chapter. I shall set my proposed theory in opposition to these three by means of a typology using a specified conception of the terms of concrete, abstract, particular, and universal.

CHAPTER V
SUMMARY AND CONCLUSIONS

Introduction

I have characterized my proposal for a nominalistic theory of compositions in terms of what I have called "concrete particulars". I have done so not because I believe a nominalistic theory must consider them in such terms; rather, (1) since they are artifacts, I think musical compositions are best understood as concrete particulars; and (2) if compositions can be construed as concrete particulars, a nominalism with respect to musical entities is clearly attainable. After all, concrete particulars are necessarily and noncontroversially individuals; and a nominalism is just that ontological perspective that countenances only individuals.

If this first objective can be accomplished, an additional task, which would aim toward developing a fuller nominalistic treatment of music as a whole, would be to show how, in terms of individuality alone, these compositions-as-individuals are related to what are referred to as their performances and other so-called "examples", (scores, recordings, etc.). But the notion of musical compositions as individuals is the toughest nut to crack, so

to speak, on the way to a nominalistic ontology of music. The sense in which this is the case was discussed at length in Chapter I. Simply put, the apparent problem is that compositions have seemed to some to be related to their so-called examples in a way that suggests they are best construed as some sort of universals or other abstract entities, i.e., non-physical entities capable of multiple realizations or instantiations. Typically, performances or other occurrences of a composition appear as associated in such a way that it is presumed that the composition itself is somehow evident or present in its realizations. Therefore, it may seem that compositions just cannot be individuals in the way that, say paintings or sculptures seem to be. I have attempted to show otherwise.

I think I have provided, in what has preceded, a plausible account of musical compositions, (and music generally), that construes them as concrete particulars. If this is true, then I believe I have gone some distance in accomplishing a goal I set out for myself from the beginning of this dissertation. I have aimed at producing an alternative limiting theory, cast in nominalistic terms, that fits within an array of theories that has been produced by others. My hypothetical adventure consisted of developing a theory within certain parameters; that is, all musical entities were to be described as individuals.

The three theories of musical compositions discussed

in Chapter IV are also, I believe, limiting theories of sorts. In the present chapter, I shall attempt to show more explicitly how these four theories, (mine and the others), indicate four distinct directions a musical ontology may take. I shall do so by combining the specified uses of a set of four terms into four main positions that may then be applied to the sorts of ontological considerations I have been investigating. More specifically, these four positions will serve to identify or demarcate what I think are four principal ontological conceptions of musical compositions.

Four Ontological Categories

The four terms that shall provide the language for characterizing the four ontological categories under consideration are: 'concrete', 'abstract', 'particular', and 'universal'. As I shall attempt to indicate, these terms may be understood as fitting together in four important ways. Since 'concrete' and 'abstract' will be shown to be contrast terms, as will 'particular' and 'universal', the following, in no significant order, may be regarded as the possible alternative conceptions of the ontological status of something:

- (1) concrete particular;
- (2) abstract particular;
- (3) abstract universal;
- (4) concrete universal.

Let me begin by briefly summarizing points I have made earlier and throughout this dissertation about the four elementary terms from which these categories are derived. But before I go any further, I should make clear that although I will be describing certain ontological categories, such descriptions are to be understood as entirely theoretical. That is, I will be making no claims as to whether or not anything exists within any given category as described. Part of my purpose in presenting these categories is that I think an ontology is largely describable in terms of which categories are taken as identifying genuine existents.

Under the use I propose for the term 'concrete', something is said to be concrete if it is describable as physical or capable of intersubjective perception. It is identifiable as something composed of matter, (however matter is to be understood); and/or it is perceivable with the senses. As I explained before, I am simply assuming for the purposes of this dissertation that material objects exist and that there are sensory perceptions. My intention is not to solve or even address any of the well-known controversies over such matters as matter and perception. Nothing I say turns on any conclusions about such claims anyway. Rather, all that is required in the present context is the acknowledgement that we can and do at least talk about physical objects and intersubjective sensory

experience. My point here is that we can distinguish descriptions of physical objects and sensory experiences from other sorts of descriptions, and that such physical and phenomenal descriptions are descriptions of what I shall call concrete entities. Therefore, anything that can be described as physical (material) or capable of intersubjective sensory apprehension can be referred to, under the proposed terminology, as concrete.

Traditionally, the term concrete has had a use indicating some sense of being composite, compound, coming together, and so on. Thus, concrete has been used, for example, to refer to the way in which an abstract quality is united, combined, or embodied in substance or matter. Thus, a thing was said to be concrete, whereas a quality or attribute was said to be abstract. In order to avoid the complex metaphysics implied by such a characterization of the meaning of this notion, I am suggesting that the term has a genuine use referring simply to something just being material or sensory. Again, to describe something as physical or perceptual is to describe it as concrete.

One proviso with respect to what I am calling sensory or perceptual should be kept in mind. I wish to distinguish such notions from what have sometimes been referred to as "internal sensations". That is, I take dreams, hallucinations, mental images, and any other sort of experiences that are not purported to originate outside the

perceiver and to be accessible through the sensory organs, to be included among those things which I shall eventually describe as abstract rather than concrete. This is why I have said that sensory experience which is concrete is "intersubjective". That is to say, such experience is in some sense objective or publically accessible by means of the senses. But this is not to say that all concrete things are directly and publically accessible with the senses. For example, atoms would seem to be appropriately described as concrete, yet they are not sensory, strictly speaking. Thus, it would seem that anything that is physical may be said to be concrete, and anything that is intersubjectively perceptual may also be said to be concrete; but not everything that is said to be concrete is perceptual.

In contrast to 'concrete', the term 'abstract' will be used to refer to what is not describable as physical or intersubjectively perceptual. What we usually refer to generically as 'ideas' are prime examples of what I have in mind for the term abstract. I shall use the term abstract in such a way that it may serve to identify or describe such various things, (or purported things), as ideas, mental conceptions, forms, structure, spirits, souls, and so on. To the extent that such things are apprehended, it is not by means of the sensory organs; and to the extent they may be said to exist, they are not properly conceived as being

material.

For the purposes of the present discussion, 'particular' shall be understood as synonymous with the term 'individual'. Something is particular when it is singular and not general or universal. A key feature of a particular is that it can be located in a specifiable place and at an identifiable time. A particular does not exist in more than one place at one time; it has a definite spatial and temporal identity.

It should be pointed out that a particular is not necessarily concrete. Given what was said about the notion of abstract, and if there are such things, it could be that something can be both particular and abstract. For example, my memory of some event is an idea and so it is abstract, under my view; but the occasion of having that idea is identifiable with the time I have the memory, and with the place where I am when I have it; thus, it is a particular idea. The idea is occurring here and now with me, not nowhere at no time, nor elsewhere at many times. We do speak of different persons having the same idea, or the one person having the same idea twice. Whether such talk should be taken literally or not as implying multiple existence of the same thing is an important question. (As we shall see, such descriptions of ideas would class them as universals). But my only point here is that we can regard or describe certain ideas as abstract and particular. They would be

non-concrete things that are, in a sense, spatially and temporally locatable; thus they are particulars. Another example of an abstract entity which is also a particular might be a Cartesian human soul. Such a conception of a person's soul would claim that a soul exists during a period of time that is identifiable, and this soul can exist with a body at some identifiable place. I will say more about 'abstract particulars' shortly.

Finally, the term 'universal' shall be understood to refer to entities which are not identifiable with a specific time or place; rather, a universal is said to be capable of multiple existence or realization. A universal is purported to be the sort of thing that may exist in more than one place at one time. 'Universal' is thus the contrast term to 'particular'. It should be made clear that it is not necessarily the case that a universal as such, (or what is purported to be a universal), must actually be instantiated or realized in order to exist, only that it can so exist. Particulars cannot exist in this way at all.

We are now in a position to combine the elementary terms into the four basic ontological categories mentioned above. The four categories are again (1) concrete particular; (2) abstract particular; (3) abstract universal; and (4) concrete universal. These are the four meaningful combinations of the four terms thus far described. Given the ways that the terms were defined, such combinations as

'particular universal' or 'concrete abstract' are not possible, much like 'odd even'. Besides, the way that I am using the terms, 'particular' and 'universal' are generally used as nouns, whereas 'abstract' and 'concrete' usually function as adjectives.

A concrete particular is something which is a physical, singular individual, and it exists and is locatable in space and time. Often, but not always, concrete particulars are publically observable through an exercise of the senses. Some examples are such things as rocks, trees, chairs, animals, and atoms.

As I described above, an abstract particular, like a concrete particular, is also spatially and temporally locatable, but it is not a physical or publically observable entity. It exists independent of being itself material. Examples would be mental conceptions and images, as well as minds or souls.

An abstract universal is the sort of thing that would be, first, neither physical nor perceptual; and secondly, neither is it identifiable as spatial or temporal. It is capable of multiple occurrence, and can be attributed to many different individuals. Plato's Forms are the preeminent examples of abstract universals. Kinds, natures, essences, etc., are often conceived as abstract universals.

Finally, a concrete universal would be the sort of entity that exists physically, in some sense, yet is not

limited to individual spatial or temporal location. This appears to be a very peculiar notion. But it would seem that something akin to Aristotle's essences may be likely candidates for examples within this category. The reason I think this, is that a concrete universal would amount to a universal that depends on physical instantiation for its existence. For example, it might be argued that "white" should be regarded as a concrete universal because while many things may be white at the same time in different places and in the same respect, and so may count as a genuine universal; "white" must exist in some physical realization in order to exist at all. "White" has no meaning independent of white things, things capable of being perceived as white; though there may be an indefinite number of them. Therefore, so-called physical qualities may, under some views, be considered concrete universals rather than abstract universals. The latter may exist independent of any realization or instantiation, whereas the former must exist as physically realized in order to exist at all.

The four theories of musical compositions presented in this dissertation appear to correspond, more or less, with the four ontological categories just described. In the case of my own theory, I have purposely tried to develop a conception of compositions which construes them explicitly as concrete particulars. Musical artworks are thus original physical artifacts consisting of collections of either

actual musical sounds or inscriptions of musical notations, which are themselves concrete particulars. They are intended originals made by some person from some selection and arrangement of musical elements. If these elements are sounds, the composition itself is exceedingly short-lived; it ceases to exist after its sounding. Any future awareness of the composition is dependent upon the existence of musical works derived from it. Subsequent attempts to make scores or performances of a composition made of sounds result not in instances, examples, occurrences, or realizations of the composition. Rather, performances, scores, recordings, or any other entity comprised of the appropriate musical elements, are distinct and unique individual works that, owing to their status as genuinely derived from the composition are entitled to the title of the original artwork.

Compositions made from musical elements that are not sounds have whatever durability these constitutive elements possess. That is, a composition which is a manuscript score consisting of inscriptions on paper will survive as long as the manuscript score remains in existence. If it is destroyed, so is the composition. This is no different than if daVinci's "Mona Lisa" were destroyed by fire, and we would quite accurately say that the "Mona Lisa" no longer exists. Copies and derivations afford us some sense of the nature of the original composition, maybe even to a very

high degree of compliance, but the composition ceases to exist when its constituents musical elements, (which are themselves concrete particulars), cease to exist.

In sharp contrast to this proposed ontological characterization of compositions is Nicholas Wolterstorff's theory of musical artworks as "norm-kinds". As we have observed in the previous chapter, Wolterstorff's proposal consists in defining musical compositions as a kind of universal. More precisely, they are norm-kinds, an abstract entity capable of an indefinite number of occurrences in many different media. This norm-kind is distinguished from other kinds of kinds by its status as a standard for determining the correctness of any occurrence derived from it. The composition as norm-kind need never be instantiated; it exists independent of any actual occurrences. For example, any given musical composition need never be performed. According to the description developed by Wolterstorff, it seems appropriate to categorize compositions as abstract universals. They are abstract, rather than concrete, because they are not and need never be physically or perceptually existent; they exist independently of the material world. Compositions are universals, rather than particulars, simply because they are defined as the sort of thing which may have examples or multiple occurrences. A composition is neither to be identified with any given example nor is it dependent upon

these examples for its own existence.

William Webster has articulated a theory of musical compositions in which he refers to them as abstract particulars. It should be apparent by now that I find this category misapplied within his theory. I would argue that, from what he says about musical compositions, it would be more accurate to categorize them as concrete universals. For Webster, a composition does not exist independent of being realized in some medium. That is, a composition always exists as one or other of the following: a score, a sounded performance, grooves on a vinyl disc, radio waves, magnetic patterns on plastic tape, and so on. These are all physical media, so it seems evident that Webster takes compositions to be necessarily dependent upon physical existence. Therefore, compositions, as described by Webster, would seem to be concrete, rather than abstract entities.

Furthermore, and I think more significantly, he does not describe compositions in terms that I think justify categorization as particulars. Webster holds that the distinguishing mark between individuals as such and what he calls abstract particulars is that the former cannot have "discontinuous existence", whereas the latter can. But what he means by discontinuous existence has much to do with why I think his view is not about particulars, but instead about a kind of universal.

I have indicated that, under my view, to be a particular is to be, in some sense, spatially and temporally locatable. With this, Webster would agree. But I do not think this precludes at least one sort of discontinuous existence; one that Nelson Goodman suggests is consistent with certain individuals. As noted in Chapter II, Goodman argues that certain individuals may be construed as discontinuous wholes; i.e., individual parts spatially and temporally separated with each part going together to form a compound whole individual. (Though nothing about this description of individuals is at odds with my my own views, it should be noted that neither Goodman nor I claim that compositions are this sort of individual.)

Now Webster's view is not that compositions consist of realizations as parts of some one whole composition. Rather, each realization is itself an instance of the composition, but the composition cannot exist independent of any realization. In this way, Goodman's sense of discontinuous existence and Webster's sense of the same are quite different. What makes Webster's sense of discontinuous existence significant for his theory is that it allows multiple occurrences of the composition at the same time. Therefore, even under Goodman's characterization of this sort of individuality, Webster's view fails to count as properly individualistic.

Admittedly, Webster does not want to construe

compositions as individuals anyway; rather, they are to be considered, according to Webster, abstract particulars. Though Webster may choose to stipulate a specific use for his terms, I think to do so as he does is misleading. The importance he gives to setting so-called abstract particulars off from individuals is not so much because he thinks they have discontinuous existence, but because he thinks they may have multiple realizations. The only sense in which compositions as "abstract particulars" have "discontinuous existence", according to Webster, is in the sense of that the individual realizations of the composition exist literally unconnected to one another; yet each, by itself is a realization of the composition. It seems to me, that such a characterization is much more accurately understood as referring to a kind of universal; namely, a concrete universal, because they must exist concretely in order to exist at all.

Though I do think Webster is mistaken in how he chooses to name his view, my primary purpose for criticizing his terminology is to set it in context, by means of a reasonably consistent use of language, with the rest of the theories I am examining, including my own. Furthermore, though he describes his theory in terms of what he calls "abstract particulars", I want to anticipate and blunt any temptation to regard his theory as a candidate for a nominalistic theory. Even if every realization of a

composition is understood as an individual, which few would dispute, it still would have to be shown that the composition itself is an individual. As far as I know, Webster has no interest in considering his theory a nominalistic one; but this is somewhat beside the point. My purpose here is to provide examples of contrasting limiting theories about musical compositions. Insofar as Wolterstoff has provided a worthy example of a theory in which compositions are conceived as abstract universals, it seems to me that Webster's is a good example of a theory that construes compositions as concrete universals, regardless of how he chooses to name his view. It only remains for me to provide a theoretical example which is cast in terms of what I have defined as abstract particulars.

What might such a theory be like? As abstract, compositions would have to be described as essentially non-physical and non-perceptual entities. As particular, they would have to be regarded as identifiable with a specific location and time. I mentioned before that a given person's conscious ideas occurring at a given moment may be the sorts of things that would count as abstract particulars. Assuming that there are such things, they may be referred to as mental entities or the contents of mind or particular ideas. Thus, a theory of compositions that identified the essential musical work with certain ideas in

someone's mind could be regarded a theory that construed them as abstract particulars. Although I do not think it fits the bill perfectly, R. G. Collingwood's description of musical compositions seems reasonably close to one which effectively treats them as abstract particulars.

Artworks, for Collingwood, are not to be identified with any physical or sensory artifact; rather, the artwork is essentially an "imaginary object" existing in the mind of the person attending to it. He says that artworks are made in the mind of the artist and nowhere else. Musical compositions, as artworks, are thus created ideas composed in the mind of the composer. If these musical ideas are expressed by way of a written score or sounded on an instrument, it is not the composition that is being seen or heard. The composition-as-idea is not directly accessible to anyone other than the person whose idea it is. What is required to have any access whatsoever to the musical ideas of the composer is that the reader of the score or the listener of the performance re-construct from what is seen or heard an imaginary musical object of one's own. This set of musical ideas in the mind of the spectator is not the same set as those of the composer's--that is impossible. What is seen or heard are not themselves ideas, but occasions for recreating musical ideas in one's own mind.

According to Collingwood, the composition as it originates and exists in the mind of the composer is fully

actualized in this state. In other words, the composition lacks for nothing, and is in no way deficient in its status as a "mere" idea. It does not depend on being physically or perceptually realized at all. If anything it is the artifactual expression, (e.g., a score or performance), of the compositional ideas that is extraneous, and somewhat superfluous. There is little question then that Collingwood's notion of compositions is one that virtually defines them as abstract entities.

But is his view properly represented as being particularistic; that is, are his compositions essentially individuals? On this point Collingwood is not so clear, and I may be forcing my case a bit by attempting to characterize his position in terms of particulars. On the other hand, I would suggest that it is at least plausible to interpret what he says about compositions--that they are mental entities of a sort--as implying that they are abstract particulars. At least it is the theory of musical works that comes as close to such a conception as any of which I am aware. The reason that I think this is because no other theory so closely identifies the actual artwork with the actual ideas in the mind of the creator of the work; and then goes on to argue further that any given spectator's experience with a work is also essentially a matter of having certain ideas in his or her mind. When he makes this latter point he does not imply at all that spectators are

having the same ideas as those that go to make up the composition, i.e., the ideas in the composer's mind. Therefore, it would seem inappropriate, under his view, to regard compositions as any kind of universal. He does not describe the compositional work of art as consisting of whatever these ideas within various minds have in common. Rather, the ideas that he is describing are the sort that occur in some specific person's mind at some specific time. As such they would appear to be particulars. It is these particular ideas, these abstract particulars, that are constitutive of the musical composition.

Summarizing, according to the four ontological categories proposed at the outset of this chapter, musical compositions may be construed as

- (1) concrete particulars: compositions are physical or perceptual artifacts; they are singular individuals identifiable with a specific spatial location and temporal duration. This dissertation is an attempt to articulate the details and implications of just such a description.
- (2) abstract particulars: composition are essentially mental entities originating and existing in the mind of the composer, and subsequently as reconstructed, but different, ideas in the minds of spectators. Any physical or sensory artifact associated with the composition is inessential to the existence and nature of the musical artwork. I have offered R. G. Collingwood's theory of artworks as an example of this sort of conception.
- (3) abstract universals: compositions exist independently of any actual occurrences--they need never be performed or scored; yet a composition may exist in and be attributed to multiple examples. The composition itself is not spatially or temporally located. Nicholas Wolterstorff's theory of compositions as norm-kinds is provided as a noteworthy example of this approach.

(4) concrete universals: compositions must be spatially and temporally realized in order to exist at all; yet the composition is not to be identified with any one of these realizations. Thus, compositions may have multiple existence insofar as the composition is what is common to its many realizations. William Webster has articulated just such a view.

According to Collingwood and Wolterstorff, the existence of compositions does not depend upon their artifactual realization; whereas for Webster and me, compositions do not exist if their physical or perceptual realizations do not exist. Webster and Wolterstorff attempt to show how compositions may have multiple instantiations, how they may exist in different places at the same and different times. But Collingwood and I emphasize the uniqueness of a composition's existence, its identifiable place and its individuality at the time of its creation. Clearly, Wolterstorff's views and my own have little in common. They are limiting theories at opposite poles. On the other hand, they do share an interest in an explicit analysis of certain ontological issues in music.

I offer these four theories as what I hope are creditable alternatives demarcating the ontological landscape with respect to certain aesthetic entities. Undoubtedly, other, and maybe better, theoretical versions of each categorical type are possible; though I do take these to be excellent theories. I have let my preferences be known in a way I hope is clear and somewhat convincing. But I leave it to readers to judge between these

alternatives as suits their own preferences and philosophical lights.

I shall end this dissertation by commenting on its origins. It must be said that this dissertation was initially motivated by a desire to come to terms with Nicholas Wolterstorff's theory of musical works. I am indebted to his fine work in this area of inquiry, though his conclusions are ones with which I am temperamentally and philosophically at odds. My first encounter with his impressive theoretical considerations on these matters found them, on the one hand, attractive, insofar as he sought to address issues about artworks that are of great interest to me; namely, what sort of things are artworks. On the other hand, my own philosophical intuitions and commitments discouraged me from accepting his characterizations and conclusions. Thus, I sought to develop a theoretical approach that started with similar objectives as Wolterstorff's, but instead were worked out in terms more amenable to an ontology that I found preferable. It may be said then that without Nicholas Wolterstorff's work, this dissertation would not have been written. I only hope I have provided an alternative that comes close to a level comparable to its inspiration.

ENDNOTES

Chapter I

1. As will be explained further in Chapter II, many nominalisms acknowledge that terms or words can be used in a general fashion. The name "nominalism" itself expresses etymologically, (the Latin 'nominalis': belonging to a name), the nominalistic view that all generality or universality can be accounted for in terms of language alone.

2. The Oxford English Dictionary (Oxford: Oxford University Press, 1971), entry #5 under 'concrete'.

3. For a valuable discussion of a similar point, see Eddy M. Zemach, "The Ontological Status of Art Objects" in The Journal of Aesthetics and Art Criticism, Vol. XXV, (1966), pp. 147-148. Here the author describes a distinction between "displayed qualities" and "represented qualities". Very simply, the point is that the former elements in artworks refer somehow to the latter. He develops this distinction in a way he believes to be acceptable even to formalists.

4. Musical compositions may exist in other media, e.g., magnetic tape; these shall be discussed in Chapter III.

5. George Dickie, Art and the Aesthetic (Ithaca: Cornell University Press, 1974), pp. 21-27.

6. Dickie, Art and the Aesthetic, p. 26.

7. These three alternatives are considered briefly by Manuel Bilsky in his article "The Significance of Locating the Art Object," Philosophy and Phenomenological Research, 13, (1953). p. 531. He attempts to show how such differing conceptions of the art object's "location" can and do affect art theory. His point is that questions about the ontological status of artworks are important for aesthetics.

8. Two points about what follows: 1) I shall assume that physical objects exist; that is, I shall not be

concerned here with defending the notion that there are physical objects; and more importantly, 2) to the purposes of this dissertation I shall collapse the notions of physical objects and phenomenal objects together. Most simply, I understand by physical object something which is materially objective; by "phenomenal object" I mean something which is sensorially objective. Thus, it is possible to take these terms as referring to different aspects of the same thing. But the important point is that my use of the term 'concrete' throughout this paper is intended to encompass both notions without preference for either.

9. Proposals along similar lines have been offered by such philosophers as Haig Khatchadorian, Margaret Macdonald, Joseph Margolis, Ruby Meager, C.I. Stevenson, and Richard Wollheim. Critics of this approach include Jay Bachrach, Richard Rudner, and R.A. Sharpe.

Chapter II

1. Paul Edwards, ed. The Encyclopedia of Philosophy, (New York, Macmillan Publishing Co., 1972), s.v. "William of Ockham," by Ernest A. Moody.

2. William James, "The Present Dilemma in Philosophy" in Pragmatism (New York: New American Library, 1974).

3. James, "Present Dilemma in Philosophy," p. 19.

4. James, "Present Dilemma in Philosophy," p. 19.

5. James, "Present Dilemma in Philosophy," p. 19.

6. W. V. Quine and Nelson Goodman, "Steps Toward a Constructive Nominalism" in Nelson Goodman, Problems and Projects (Indianapolis: Bobbs-Merrill Co., 1972), p. 174; emphasis mine.

7. Nelson Goodman, "A World of Individuals" in Goodman, Problems and Projects, p. 170; emphasis mine.

8. W. V. Quine, "On What There Is" in From a Logical Point of View, 2nd edition (New York: Harper Torchbooks, 1963), p. 4; emphasis mine.

9. Goodman's phrase from title of his essay.

10. Aristotle, "Metaphysics" in Aristotle's

Metaphysics, trans. Hippocrates G. Apostle, (Grinnell, Iowa: The Peripatetic Press, 1979), p. 129.

11. William of Ockham, "Summa totius logicae" in Philosophical Writings: Ockham, trans. Philotheus Boehner, (Indianapolis: Bobbs-Merrill, 1979), p. 36.

12. Ockham, "Summa totius logiae," p.38.

13. Richard I. Aaron, The Theory of Universals, (London: Oxford University Press, 1967), p. 223.

14. Aaron, Theory of Universals, p. 223.

15. John Locke, An Essay Concerning Human Understanding, edited by Peter H. Nidditch, (Oxford: Clarendon Press, 1979), p. 414.

16. George Berkeley, A Treatise Concerning the Principles of Human Knowledge, edited by Colin M. Turbayne, (Indianapolis: Bobbs-Merrill, 1957), p. 10.

17. Berkeley, Treatise Concerning the Principles of Human Knowledge, p. 11-12.

18. David Hume, A Treatise of Human Nature, edited by L. A. Selby-Bigge, (Oxford: Clarendon Press, 1968), p. 24.

19. Rolf Eberle, Nominalistic Systems, (Dordrecht, Holland: D. Reidel Publishing Co., 1970), p. 7.

20. D. F. Pears, "Universals" in Logic and Language, ed. A.N. Flew, (Garden City, N.Y.: Anchor Books, 1965), pp. 267-281.

21. Quine and Goodman, "Steps Toward a Constructive Nominalism," p. 173.

22. Goodman, "A World of Individuals," p. 156.

23. Eberle, Nominalistic Systems, p. 8.

24. Much of what is discussed here with respect to the recurrence of qualities is derived in part from Panayot Butchvarov, Resemblance and Identity, (Bloomington: Indiana University Press, 1966), and H. H. Price, Thinking and Experience, (London: Hutchinson University Library, 1969).

25. Butchvarov, Resemblance and Identity, p. 7.

26. Butchvarov, Resemblance and Identity, p. 7.
27. Quine, "On What There Is," p. 11.
28. Quine, "On What There Is," p. 13.
29. Quine, "On What There Is," p. 13-14.
30. Quine, W. V. "Logic and the Reification of Universals," From a Logical Point of View, p.103.
31. Quine, "Logic and Reification," p. 128.
32. Goodman, "A World of Individuals," p. 156.
33. Goodman, N. The Structure of Appearance, (Dordrecht- Holland, D. Reidel Publishing Co., 1977), p. 27n.
34. Goodman, "A World of Individuals," p. 158.
35. Goodman, "A World of Individuals," p. 157.
36. Goodman, "A World of Individuals," p. 156.
37. Goodman, "A World of Individuals," p. 158.
38. Butchvarov, Resemblance and Identity, p. 49.
39. Butchvarov, Resemblance and Identity, p. 49.
40. Price, Thinking and Experience, pp. 19-20.
41. Price, Thinking and Experience, pp. 19ff.
42. Butchvarov, Resemblance and Identity, pp. 33ff.

Chapter III

1. This distinction between uses of "artwork" and "work" has been adapted somewhat from comparable applications of these terms by Nicholas Waterstorff in his Works and Worlds of Art, (New York: Oxford University Press, 1980), p. 41. As shall be indicated in Chapter IV, Waterstorff's theory of compositions is considerably different from the one presented here, and so, his description of the different uses of "artwork" and "work of art" is significantly different from mine, although there are some important parallels. He uses "work of art" to refer to artworks and their "examples", while "artwork" refers to compositional works only, those works that can

have examples or occurrences. Such a way of describing things is consistent with his ontology which takes compositions to be a kind of universal, whereas performances and the like are examples or instantiations of them. The similarity of our usages is evident in the way that work of art is the broader term applicable to compositions, performances, etc., whereas artwork applies only to compositions.

2. This is not much different from what a painter does in creating her artwork. A painting may take days or even years to complete; and may involve all manner of starting, restarting, working on parts of the canvas while other parts are considered done. Musical composers do something like this although their materials are highly transient as so rely on memory to a greater extent.

3. This does not preclude using recorded sounds as parts of performances, (e.g., a singer using recorded orchestral arrangements as accompaniment or a recording of cannon roars instead of real cannons in the "1812 Overture"), or possibly even using nothing but recordings to produce a performance: imagine some avant-garde performer switching various recording/playback machines on and off in a certain way and calling the resulting sound-sequence-event a musical performance. I am not uncomfortable with accepting this as a genuine performance. My description of it using my terminology would be that it was a whole performance comprised of recordings as parts, but that none of the individual recordings as such were the performance. I am not sure how to solve the puzzle which might be created by a "performer" who simply stands in front of an audience, turns on a single machine which plays back some recorded music and then at a certain point turns off the machine and regards the whole event to be a musical performance. One solution may be to regard genuinely musical performances to be those which include at least one performer who is actually and directly making musical sounds.

4. It may be desirable to distinguish "mere practicing" from rehearsing, with the former not being a case of performing whereas the latter is. Mere practicing also involves making musical sounds but with no intentional reference to a compositional work. Rehearsals are performances to the extent that they are "derived" from a compositional work of music. What I mean by "derived" will be discussed a little later in this chapter.

5. Admittedly, the words, lyrics, or libretto associated with much music is often regarded as part of a musical work. Many music composers "compose" these words for their works. I do not consider this to be musical

composition though. Writing words for musical works is actually a kind of literary writing, although, the tonal aspects of the words often have musical significance.

6. See Chapter I on the distinction between evaluative and classificatory senses of art.

7. For an interesting examination of the status of composition by way of recording techniques, see Linda Ferguson, "Tape Composition: An Art Form in Search of Its Metaphysics," The Journal of Aesthetics and Art Criticism, Vol. XLII, No. 1 (1983), pp. 17-27.

8. Philip Alperson has provided a valuable contribution to discussions about musical improvisation in his article "On Musical Improvisation," in The Journal of Aesthetics and Art Criticism, Vol. XLIII, No. 1 (1984), pp. 17-29. While his general view of music is not nominalistic, his analysis of improvisation is not at great odds with my own.

9. For example, John Cage and George Crumb have composed works by adapting additional symbols and arrangements of symbols to traditional notation, or by creating entirely new symbols and schemes.

10. Nelson Goodman makes a similar point with his well-known notion of "sameness of spelling". See Languages of Art, (Indianapolis: Hackett Publishing Company, Inc., 1976), pp. 115-117, 131-132.

11. Again, Nelson Goodman has contributed much to my thinking on this. See Languages of Art, pp. 143-148, 233-234.

12. See Richard Rudner, "The Ontological Status of the Aesthetic Object" in Philosophy and Phenomenological Research, 10, (1950), pp. 380-388.

Chapter IV

1. R. G. Collingwood, The Principles of Art (London: Oxford University Press, 1958), p. 142.

2. Collingwood, p. 139ff.

3. Collingwood, p. 125.

4. Collingwood, p. 129.

5. Collingwood, p. 116.

6. Collingwood, p. 130.
7. Collingwood, p. 134.
8. Collingwood, p. 139.
9. Collingwood, p. 141.
10. William K. Wimsatt and Monroe C. Beardsley, "The Intentional Fallacy" in The Verbal Icon, (The University of Kentucky Press, 1954), p. 3.
11. Wimsatt and Beardsley, p. 9.
12. Nicholas Wolterstorff, Works and Worlds of Art (New York: Oxford University Press, 1980), p. 35.
13. Wolterstorff, p. 34.
14. Wolterstorff, p. 36.
15. Wolterstorff, p. 36.
16. Wolterstorff, p. 90.
17. Wolterstorff, pp. 56-57.
18. Wolterstorff, p. 57.
19. Wolterstorff, p. 54.
20. Wolterstorff, p. 57.
21. Wolterstorff, pp. 98-105.
22. Wolterstorff, p. 81.
23. Wolterstorff, p. 64.
24. Wolterstorff discusses this characteristic of kinds in general and norm-kinds in particular throughout Part Two; see for example pp. 58, 68n., 88n.
25. Wolterstorff, p. 67.
26. Wolterstorff, p. 67.
27. Wolterstorff, p. 64.
28. Wolterstorff, p. 88.
29. Wolterstorff, p. 67.

30. Wolterstorff, pp. 88-89.

31. William E. Webster, "A Theory of the Compositional Work of Music" in The Journal of Aesthetics and Art Criticism, Fall 1974. p. 60.

32. Webster, p. 60.

33. Webster, p. 61.

34. Webster, p. 61.

35. Webster, p. 60.

36. Webster, p. 62.

37. Webster, p. 61.

38. Webster, p. 61.

39. Webster, p. 61.

40. Webster, p. 61.

41. Webster, p. 62.

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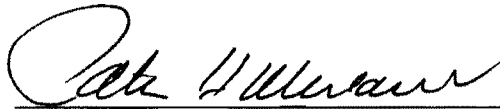
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The final copies have been examined by the director of the dissertation and the signature which appears below verifies the fact that any necessary changes have been incorporated and that the dissertation is now given final approval by the Committee with reference to content and form.

The dissertation is therefore accepted in partial fulfillment of the requirements for the degree of Ph.D.

4/16/91

Date



Director's Signature

Nominalism

In metaphysics, **nominalism** is a philosophical view which denies the existence of universals and abstract objects, but affirms the existence of general or abstract terms and predicates.^[1] There are at least two main versions of nominalism. One version denies the existence of universals – things that can be instantiated or exemplified by many particular things (e.g., strength, humanity). The other version specifically denies the existence of abstract objects – objects that do not exist in space and time.^[2]

Most nominalists have held that only physical particulars in space and time are real, and that universals exist only *post res*, that is, subsequent to particular things.^[3] However, some versions of nominalism hold that some particulars are abstract entities (e.g., numbers), while others are concrete entities – entities that do exist in space and time (e.g., pillars, snakes, bananas).

Nominalism is primarily a position on the problem of universals, which dates back at least to Plato, and is opposed to realist philosophies, such as Platonic realism, which assert that universals do exist over and above particulars. However, the name "nominalism" emerged from debates in medieval philosophy with Roscellinus.

The term 'nominalism' stems from the Latin *nomen*, "name". John Stuart Mill summarised nominalism in the apothegm "there is nothing general except names".^[4]

In philosophy of law, nominalism finds its application in what is called **constitutional nominalism**.^[5]



William of Ockham

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Ancient Greek philosophy

The opposite of nominalism is realism. Plato was perhaps the first writer in Western philosophy to clearly state a realist, i.e. non-nominalist, position:

... We customarily hypothesize a single form in connection with each of the many things to which we apply the same name. ... For example, there are many beds and tables. ... But there are only two forms of such furniture, one of the bed and one of the table. (*Republic* 596a-b, trans. Grube)

What about someone who believes in beautiful things, but doesn't believe in the beautiful itself ...? Don't you think he is living in a dream rather than a wakened state? (*Republic* 476c)

The Platonic universals corresponding to the names "bed" and "beautiful" were the Form of the Bed and the Form of the Beautiful, or the *Bed Itself* and the *Beautiful Itself*. Platonic Forms were the first universals posited as such in philosophy.^[6]

Our term "universal" is due to the English translation of Aristotle's technical term *katholou* which he coined specially for the purpose of discussing the problem of universals.^[7] *Katholou* is a contraction of the phrase *kata holou*, meaning "on the whole".^[8]

Aristotle famously rejected certain aspects of Plato's Theory of Forms, but he clearly rejected nominalism as well:

... 'Man', and indeed every general predicate, signifies not an individual, but some quality, or quantity or relation, or something of that sort. (*Sophistical Refutations* xxii, 178b37, trans. Pickard-Cambridge)

The first philosophers to explicitly describe nominalist arguments were the Stoics, especially Chrysippus.^{[9][10]}

Medieval philosophy

In medieval philosophy, the French philosopher and theologian Roscellinus (c. 1050 – c. 1125) was an early, prominent proponent of nominalism. Nominalist ideas can be found in the work of Peter Abelard and reached their flowering in William of Ockham, who was the most influential and thorough nominalist. Abelard's and Ockham's version of nominalism is sometimes called conceptualism, which presents itself as a middle way between nominalism and realism, asserting that there *is* something in common among like individuals, but that it is a concept in the mind, rather than a real entity existing independently of the mind. Ockham argued that only individuals existed and that universals were only mental ways of referring to sets of individuals. "I maintain", he wrote, "that a universal is not something real that exists in a subject ... but that it has a being only as a thought-object in the mind [objectivum in anima]". As a general rule, Ockham argued against assuming any entities that were not necessary for explanations. Accordingly, he wrote, there is no reason to believe that there is an entity called "humanity" that resides inside, say, Socrates, and nothing further is explained by making this claim. This is in accord with the analytical method that has since come to be called Ockham's razor, the principle that the explanation of any phenomenon should make as few assumptions as possible.

Critics argue that conceptualist approaches answer only the psychological question of universals. If the same concept is *correctly* and non-arbitrarily applied to two individuals, there must be some resemblance or shared property between the two individuals that justifies their falling under the same concept and that is just the metaphysical problem that universals were brought in to address, the starting-point of the whole problem (MacLeod & Rubenstein, 2006, §3d). If resemblances between individuals are asserted, conceptualism becomes moderate realism; if they are denied, it collapses into nominalism.^[11]

Modern and contemporary philosophy

In modern philosophy, nominalism was revived by Thomas Hobbes^[12] and Pierre Gassendi.^[13]

In contemporary analytic philosophy, it has been defended by Rudolf Carnap,^[14] Nelson Goodman,^[15] H. H. Price,^[14] and D. C. Williams.^[16]

Indian philosophy

Indian philosophy encompasses various realist and nominalist traditions. Certain orthodox Hindu schools defend the realist position, notably Purva Mimamsa, Nyaya and Vaisheshika, maintaining that the referent of the word is both the individual thing perceived by the subject of knowledge and the class to which the thing belongs. According to Indian realism, both the individual and the class have objective existence, with the second underlying the former.

Buddhists take the nominalist position, especially those of the Yogacara school; they were of the opinion that words have as referent not true objects, but only concepts produced in the intellect. These concepts are not real since they do not have efficient existence, that is, causal powers. Words, as linguistic conventions, are useful to thought and discourse, but even so, it should not be accepted that words apprehend reality as it is.

Dignaga formulated a nominalist theory of meaning called *apoha*, or *theory of exclusions*. The theory seeks to explain how it is possible for words to refer to classes of objects even if no such class has an objective existence. Dignaga's thesis is that classes do not refer to positive qualities that their members share in common. On the contrary, classes are exclusions (*apoha*). As such, the "cow" class, for example, is composed of all exclusions common to individual cows: they are all non-horse, non-elephant, etc.

Among Hindu realists, this thesis was criticized for being negative.

The problem of universals

Nominalism arose in reaction to the problem of universals, specifically accounting for the fact that some things are of the same type. For example, Fluffy and Kitzler are both cats, or, the fact that certain properties are repeatable, such as: the grass, the shirt, and Kermit the Frog are green. One wants to know by virtue of *what* are Fluffy and Kitzler both cats, and *what* makes the grass, the shirt, and Kermit green.

The Platonist answer is that all the green things are green in virtue of the existence of a universal: a single abstract thing that, in this case, is a part of all the green things. With respect to the color of the grass, the shirt and Kermit, one of their parts is identical. In this respect, the three parts are literally one. Greenness is repeatable because there is one thing that manifests itself wherever there are green things.

Nominalism denies the existence of universals. The motivation for this flows from several concerns, the first one being where they might exist. Plato famously held, on one interpretation, that there is a realm of abstract forms or universals apart from the physical world (see theory of the forms). Particular physical objects merely exemplify or instantiate the universal. But this raises the question: Where is this universal realm? One possibility is that it is outside space and time. A view sympathetic with this possibility holds that, precisely because some form is immanent in several physical objects, it must also transcend each of those physical objects; in this way, the forms are "transcendent" only insofar as they are "immanent" in many physical objects. In other words, immanence implies transcendence; they are not opposed to one another. (Nor, in this view, would there be a separate "world" or "realm" of forms that is distinct from the physical world, thus shirking much of the worry about where to locate a "universal realm".) However, naturalists assert that nothing is outside of space and time. Some Neoplatonists, such as the pagan philosopher Plotinus and the Christian philosopher Augustine, imply (anticipating conceptualism) that universals are contained within the mind of God. To complicate things, what is the nature of the instantiation or exemplification relation?

Conceptualists hold a position intermediate between nominalism and realism, saying that universals exist only within the mind and have no external or substantial reality.

Moderate realists hold that there is no realm in which universals exist, but rather universals are located in space and time wherever they are manifest. Now, recall that a universal, like greenness, is supposed to be a single thing. Nominalists consider it unusual that there could be a single thing that exists in multiple places simultaneously. The realist maintains that all the instances of greenness are held together by the exemplification relation, but this relation cannot be explained.

Finally, many philosophers prefer simpler ontologies populated with only the bare minimum of types of entities, or as W. V. O. Quine said "They have a taste for 'desert landscapes.'" They try to express everything that they want to explain without using universals such as "catness" or "greenness."

Varieties

There are various forms of nominalism ranging from extreme to almost-realist. One extreme is **predicate nominalism**, which states that Fluffy and Kitzler, for example, are both cats simply because the predicate 'is a cat' applies to both of them. And this is the case for all similarity of attribute among objects. The main criticism of this view is that it does not provide a sufficient solution to the problem of universals. It fails to provide an account of what makes it the case that a group of things warrant having the same predicate applied to them.^[17]

Proponents of **resemblance nominalism** believe that 'cat' applies to both cats because Fluffy and Kitzler resemble an exemplar cat closely enough to be classed together with it as members of its kind, or that they differ from each other (and other cats) quite less than they differ from other things, and this warrants classing them together.^[18] Some resemblance nominalists will concede that the resemblance relation is itself a universal, but is the only universal necessary. Others argue that each resemblance relation is a particular, and is a resemblance relation simply in virtue of its resemblance to other resemblance relations. This generates an infinite regress, but many argue that it is not vicious.^[19]

Class nominalism argues that class membership forms the metaphysical backing for property relationships: two particular red balls share a property in that they are both members of classes corresponding to their properties—that of being red and being balls. A version of class nominalism that sees some classes as "natural classes" is held by Anthony Quinton.^[20]

Conceptualism is a philosophical theory that explains universality of particulars as conceptualized frameworks situated within the thinking mind.^[21] The conceptualist view approaches the metaphysical concept of universals from a perspective that denies their presence in particulars outside of the mind's perception of them.^[22]

Another form of nominalism is trope nominalism. A trope is a particular instance of a property, like the specific greenness of a shirt. One might argue that there is a primitive, objective resemblance relation that holds among like tropes. Another route is to argue that all apparent tropes are constructed out of more primitive tropes and that the most primitive tropes are the entities of complete physics. Primitive trope resemblance may thus be accounted for in terms of causal indiscernibility. Two tropes are exactly resembling if substituting one for the other would make no difference to the events in which they are taking part. Varying degrees of resemblance at the macro level can be explained by varying degrees of resemblance at the micro level, and micro-level resemblance is explained in terms of something no less robustly physical than causal power. David Armstrong, perhaps the most prominent contemporary realist, argues that such a trope-based variant of nominalism has promise, but holds that it is unable to account for the laws of nature in the way his theory of universals can.

Ian Hacking has also argued that much of what is called social constructionism of science in contemporary times is actually motivated by an unstated nominalist metaphysical view. For this reason, he claims, scientists and constructionists tend to "shout past each other".^[23]

Mathematical nominalism

A notion that philosophy, especially ontology and the philosophy of mathematics, should abstain from set theory owes much to the writings of Nelson Goodman (see especially Goodman 1940 and 1977), who argued that concrete and abstract entities having no parts, called *individuals*, exist. Collections of individuals likewise exist, but two collections having the same individuals are the same collection. Goodman was himself drawing heavily on the work of Stanisław Leśniewski, especially his mereology, which was itself a reaction to the paradoxes associated with Cantorian set theory. Leśniewski denied the existence of the empty set and held that any singleton was identical to the individual inside it. Classes corresponding to what are held to be species or genera are concrete sums of their concrete constituting individuals. For example, the class of philosophers is nothing but the sum of all concrete, individual philosophers.

The principle of extensionality in set theory assures us that any matching pair of curly braces enclosing one or more instances of the same individuals denote the same set. Hence $\{a, b\}$, $\{b, a\}$, $\{a, b, a, b\}$ are all the same set. For Goodman and other proponents of **mathematical nominalism**,^[24] $\{a, b\}$ is also identical to $\{a, \{b\}\}$, $\{b, \{a, b\}\}$, and any combination of matching curly braces and one or more instances of a and b , as long as a and b are names of individuals and not of collections of individuals. Goodman, Richard Milton Martin, and Willard Quine all advocated reasoning about collectivities by means of a theory of *virtual sets* (see especially Quine 1969), one making possible all elementary operations on sets except that the universe of a quantified variable cannot contain any virtual sets.

In the foundations of mathematics, nominalism has come to mean doing mathematics without assuming that sets in the mathematical sense exist. In practice, this means that quantified variables may range over universes of numbers, points, primitive ordered pairs, and other abstract ontological primitives, but not over sets whose members are such individuals. To date, only a small fraction of the corpus of modern mathematics can be rederived in a nominalistic fashion.

Criticisms

Critique of the historical origins of the term

As a category of late medieval thought, the concept of 'nominalism' has been increasingly queried. Traditionally, the fourteenth century has been regarded as the heyday of nominalism, with figures such as John Buridan and William of Ockham viewed as founding figures. However, the concept of 'nominalism' as a movement (generally contrasted with 'realism'), first emerged only in the late fourteenth century,^[25] and only

gradually became widespread during the fifteenth century.^[26] The notion of two distinct ways, a *via antiqua*, associated with realism, and a *via moderna*, associated with nominalism, became widespread only in the later fifteenth century – a dispute which eventually dried up in the sixteenth century.^[27]

Aware that explicit thinking in terms of a divide between 'nominalism' and 'realism' emerged only in the fifteenth century, scholars have increasingly questioned whether a fourteenth-century school of nominalism can really be said to have existed. While one might speak of family resemblances between Ockham, Buridan, Marsilius and others, there are also striking differences. More fundamentally, Robert Pasnau has questioned whether any kind of coherent body of thought that could be called 'nominalism' can be discerned in fourteenth century writing.^[28] This makes it difficult, it has been argued, to follow the twentieth century narrative which portrayed late scholastic philosophy as a dispute which emerged in the fourteenth century between the *via moderna*, nominalism, and the *via antiqua*, realism, with the nominalist ideas of William of Ockham foreshadowing the eventual rejection of scholasticism in the seventeenth century.^[27]

Critique of nominalist reconstructions in mathematics

A critique of nominalist reconstructions in mathematics was undertaken by Burgess (1983) and Burgess and Rosen (1997). Burgess distinguished two types of nominalist reconstructions. Thus, *hermeneutic nominalism* is the hypothesis that science, properly interpreted, already dispenses with mathematical objects (entities) such as numbers and sets. Meanwhile, *revolutionary nominalism* is the project of replacing current scientific theories by alternatives dispensing with mathematical objects (see Burgess, 1983, p. 96). A recent study extends the Burgessian critique to three nominalistic reconstructions: the reconstruction of analysis by Georg Cantor, Richard Dedekind, and Karl Weierstrass that dispensed with infinitesimals; the constructivist reconstruction of Weierstrassian analysis by Errett Bishop that dispensed with the law of excluded middle; and the hermeneutic reconstruction, by Carl Boyer, Judith Grabiner, and others, of Cauchy's foundational contribution to analysis that dispensed with Cauchy's infinitesimals.^[29]

See also

- Abstraction
- Abstract object
- Conceptualism
- Concrete (philosophy)
- Idea
- Ideas Have Consequences
- Linguistic relativity
- Literary nominalism
- Object
- Problem of universals
- Psychological nominalism
- Realism (philosophy)
- School of Names
- Substantial form
- Universal (metaphysics)
- William of Ockham

Notes

1. Mill (1872); Bigelow (1998).

2. Rodriguez-Pereyra (2008) writes: "The word 'Nominalism', as used by contemporary philosophers in the Anglo-American tradition, is ambiguous. In one sense, its most traditional sense deriving from the Middle Ages, it implies the rejection of universals. In another, more modern but equally entrenched sense, it implies the rejection of abstract objects" (§1).
3. Feibleman (1962), p. 211.
4. Mill, J.S. (1865/1877). *An Examination of Sir William Hamilton's Philosophy*, volume II (https://books.google.com.au/books/about/An_Examination_of_Sir_William_Hamilton_s.html?id=5HBEAQAAMAAJ&printsec=frontcover&source=kp_read_button&redir_esc=y), Chapter XVII, page 50.
5. An overview of the philosophical problems and an application of the concept to a case of the Supreme Court of the State of California, gives Thomas Kupka, 'Verfassungsnominalismus', in: *Archives for Philosophy of Law and Social Philosophy* 97 (2011), 44–77, [PDF \(https://ssrn.com/abstract=2343741\)](https://ssrn.com/abstract=2343741).
6. Penner (1987), p. 24.
7. Peters (1967), p. 100.
8. "katholou" (<http://archimedes.fas.harvard.edu/cgi-bin/dict?name=lsj&lang=el&word=kaqo%2flu&filter=GreekXlit>) in [Harvard's Archimedes Project](#) online version of [Liddell & Scott's A Greek-English Lexicon](#).
9. John Sellars, *Stoicism*, Routledge, 2014, pp. 84–85: "[Stoics] have often been presented as the first nominalists, rejecting the existence of universal concepts altogether. ... For Chrysippus there are no universal entities, whether they be conceived as substantial [Platonic Forms](#) or in some other manner."
10. [Chrysippus \(Internet Encyclopedia of Philosophy\) \(https://www.iep.utm.edu/chrysipp/\)](https://www.iep.utm.edu/chrysipp/)
11. ["Meaning and the Problem of Universals" \(http://www.friesian.com/universl.htm\). www.friesian.com.](http://www.friesian.com/universl.htm)
12. [Thomas Hobbes \(Stanford Encyclopedia of Philosophy\) \(http://plato.stanford.edu/entries/hobbes/\)](http://plato.stanford.edu/entries/hobbes/)
13. [Pierre Gassendi \(Stanford Encyclopedia of Philosophy\) \(https://plato.stanford.edu/entries/gassendi/\)](https://plato.stanford.edu/entries/gassendi/)
14. ["Review of Gonzalo Rodriguez-Pereyra, *Resemblance Nominalism: A Solution to the Problem of Universals*" – ndpr.nd.edu \(https://ndpr.nd.edu/news/resemblance-nominalism-a-solution-to-the-problem-of-universals/\)](https://ndpr.nd.edu/news/resemblance-nominalism-a-solution-to-the-problem-of-universals/)
15. ["Nelson Goodman: The Calculus of Individuals in its different versions", Stanford Encyclopedia of Philosophy \(https://plato.stanford.edu/entries/goodman/supplement.html\)](https://plato.stanford.edu/entries/goodman/supplement.html)
16. [Donald Cary Williams, Stanford Encyclopedia of Philosophy \(https://plato.stanford.edu/entries/williams-dc/\).](https://plato.stanford.edu/entries/williams-dc/)
17. MacLeod & Rubenstein (2006), §3a.
18. MacLeod & Rubenstein (2006), §3b.
19. See, for example, H. H. Price (1953).
20. Quinton, Anthony (1957). "Properties and Classes". *Proceedings of the Aristotelian Society*. **58**: 33–58. [JSTOR 4544588 \(https://www.jstor.org/stable/4544588\)](https://www.jstor.org/stable/4544588).
21. Strawson, P. F. "Conceptualism." *Universals, concepts and qualities: new essays on the meaning of predicates*. Ashgate Publishing, 2006.
22. "Conceptualism." *The Oxford Dictionary of Philosophy*. Simon Blackburn. Oxford University Press, 1996. Oxford Reference Online. Oxford University Press. 8 April 2008.
23. Hacking (1999), pp. 80-84.
24. Bueno, Otávio, 2013, ["Nominalism in the Philosophy of Mathematics \(https://plato.stanford.edu/entries/nominalism-mathematics/\)"](https://plato.stanford.edu/entries/nominalism-mathematics/) in the [Stanford Encyclopedia of Philosophy](#).

25. The classic starting point of nominalism has been the edict issued by Louis XI in 1474 commanding that realism alone (as contained in scholars such as Averroes, Albert the Great, Aquinas, Duns Scotus and Bonaventure) be taught at the University of Paris, and ordering that the books of various 'renovating scholars', including Ockham, Gregory of Rimini, Buridan and Peter of Ailly be removed. The edict used the word 'nominalist' to describe those students at Paris who 'are not afraid to imitate' the renovators. These students then made a reply to Louis XI, defending nominalism as a movement going back to Ockham, which had been persecuted repeatedly, but which in fact represents the truer philosophy. See Robert Pasnau, *Metaphysical Themes, 1274-1671*, (New York: OUP, 2011), p. 85.
26. For example, when Jerome of Prague visited the University of Heidelberg in 1406, he described the nominalists as those who deny the reality of universals outside the human mind, and realists as those who affirm that reality. Also, for instance, in a 1425 document from the University of Cologne that draws a distinction between the via of Thomas Aquinas, Albert the Great, and the via of the 'modern masters' John Buridan and Marsilius of Inghen. See Robert Pasnau, *Metaphysical Themes, 1274-1671*, (New York: OUP, 2011), p84.
27. See Robert Pasnau, *Metaphysical Themes, 1274-1671*, (New York: OUP, 2011), p84.
28. See Robert Pasnau, *Metaphysical Themes, 1274-1671*, (New York: OUP, 2011), p86.
29. Usadi Katz, Karin; Katz, Mikhail G. (2011). "A Burgessian Critique of Nominalistic Tendencies in Contemporary Mathematics and its Historiography". *Foundations of Science*. arXiv:1104.0375 (<https://arxiv.org/abs/1104.0375>). doi:10.1007/s10699-011-9223-1 (<https://doi.org/10.1007%2Fs10699-011-9223-1>).

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Retrieved from "<https://en.wikipedia.org/w/index.php?title=Nominalism&oldid=1011895516>"

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Roscellinus

Roscelin of Compiègne (c. 1050 – c. 1125), better known by his Latinized name **Roscellinus Compendiensis** or **Rucelinus**, was a French philosopher and theologian, often regarded as the founder of nominalism.

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Biography

Roscellinus was born in Compiègne, France. Little is known of his life, and knowledge of his doctrines is mainly derived from Anselm and Abelard.

He studied at Soissons and Reims, was afterwards attached to the cathedral of Chartres and became canon of Compiègne. As a monk of Compiègne, he was teaching as early as 1087. He had contact with Lanfranc, Anselm and St. Ivo of Chartres.

It seems most probable that Roscellinus was not strictly the first to promulgate nominalistic doctrines; but in his exposition they received more definite expression, and being applied to the dogma of the Trinity, attracted universal attention.

Roscellinus maintained that it is merely a habit of speech which prevents our speaking of the three persons as three substances or three Gods. If it were otherwise, and the three persons were really one substance or thing (*una res*), we should be forced to admit that the Father and the Holy Spirit became incarnate along with the Son. Roscellinus seems to have put forward this doctrine in perfect good faith, and to have claimed for it at first the authority of Lanfranc and Anselm.

In 1092/1093, however, a council convoked at Soissons by the archbishop of Reims condemned his interpretation,^[n 1] and Roscellinus, who was accused of tritheism, recanted the doctrines attributed to him, but only out of fear of excommunication and even stoning to death by the orthodox populace, for later he returned to his early theories. He fled to England, but having made himself unpopular by an attack on the doctrines of Anselm, he left the country and repaired to Rome, where he was well received and became reconciled to the Catholic Church. He then returned to France, taught at Tours and Loc-menach (Loches) in France (where he had Abelard as a pupil), and finally became canon of Besançon. He is heard of as late as 1121, when he came forward to oppose Abelard's views on the Trinity. He was also sent a letter by Theobald of Étampes for having denigrated wrongfully the sons of priests.

Of his writings there exists only a letter addressed to Abelard on the Trinity, in which Roscellinus "belittles Abélard and makes merry over his castration."^[2] Hauréau brings forward his name in connection with a text: "Sententia de universalibus secundum magistrum R." ("Notices et extr. de quelques manuscr. lat.", V, Paris,

1892, 224), but this is a conjecture. We have as evidences of his doctrine texts of Anselm, Abelard, John of Salisbury, and an anonymous epigram. His share in the history of ideas and especially his nominalism have been exaggerated, his celebrity being far more due to his theological tritheism.

Roscelin's nominalism, or *Sententia Vocum*

According to Otto of Freisingen Roscelin *primus nostris temporibus sententiam vocum instituit* (*Gesta Friderici imp. in Monum. German. Histor.: Script.*, XX, 376) (Literally: "was the first in our times to institute the opinion/theory of words"), but the chronicler of the "Historia Francia" (cf. Bouquet, "*Recueil des hist. des Gaules et de la France*", XII, Paris, 1781, 3, b, c) mentions before him a "magister Johannes", whose personality is much discussed and who has not yet been definitively identified. What constitutes the *sententia vocum*? To judge of it we have besides the texts mentioned above which bear directly on Roscelin an exposition of the treatise *De generibus et speciebus* (thirteenth century), wrongly attributed to Abelard by Victor Cousin. The "sententia vocum" was one of the anti-Realist solutions of the problem of universals accepted by the early Middle Ages. Resuming Porphyry's alternative (*mox de generibus et speciebus illud quidem sive subsistent sive in nudis intellectibus posita sint*) the first medieval philosophers regarded genera and species (substance, corporeity, animality, humanity) either as things or as having no existence, and applying to this alternative a terminology of Boethius, they derived thence either *res* (things) or *voces* (words). To the nominalists universals were *voces* 'voices', which means: (1) above all that universals are not *res*, that is that only the individual exists: *nam cum habeat eorum sententia nihil esse praeter individuum ...* (*De gener. et spec.*, 524). Nominalism was essentially anti-Realist. (2) that universals are merely words, ***flatus vocis***, e.g., the word "homo", divisible into syllables, consonants and vowels. *Fuit autem, nemini magistri nostri Roscellini tam insana sententia ut nullam rem partibus constare vellet, sed sicut solis vocibus species, ita et partes ascribebat* (Abelard, *Liber divisionum*, ed. Cousin, 471); "[...] *Illi utique dialectici, qui non nisi flatum vocis putant universalis esse substantias, et qui colorem non aliud queunt intellegere quam corpus, nec sapientiam hominis aliud quam animam, prorsus a spiritualium quaestionum disputatione sunt exsufflandi.*" (Anselm, *De Incarnatione Verbi*, p. 285. Opera Omnia, vol. 1. Ed. F.S. Schmitt, 1938); "*Alius ergo consistit in vocibus, licet haec opinio cum Roscelino suo fere omnino evanuerit* (John of Salisbury, *Metalog.*, II, 17). The universal is reduced to an emission of sound (*flatus vocis*), in conformity with Boethius' definition: *Nihil enim aliud est prolatio (vocis) quam aeris plectro linguae percussio*. Roscelin's universal corresponds to what is now called the "universale in voce" in opposition to *universale in re* and *universale in intellectu*.

But this theory of Roscelin's had no connection with the abstract concept of genus and species. He did not touch on this question. It is certain that he did not deny the existence or possibility of these concepts, and he was therefore not a nominalist in the fashion of Taine or in the sense in which nominalism is now understood. That is why, in reference to the modern sense of the word, some call it a pseudo-nominalism. John of Salisbury, speaking of "nominalis secta" (*Metalog.*, II, 10), gives it quite another meaning. So Roscelin's rudimentary, even childish, solution does not compromise the value of universal concepts and may be called a stage in the development of moderate realism. However, because of his position as the first medieval philosopher to challenge medieval Realism, he has been invoked as a forefather of modernity.^[3]

Roscelin was also taken to task by Anselm and Abelard for the less clear idea which he gave of the whole and of composite substance. According to Anselm he maintained that colour does not exist independently of the horse which serves as its support and that the wisdom of the soul is not outside of the soul which is wise (*De fide trinit.*, 2). He denies to the whole, such as house, man, real existence of its parts. The word alone had parts, *ita divinam paginam pervertit, ut eo loco quo Dominus partem piscis assi comedissem partem hujus vocis, quae est piscis assi, non partem rei intelligere cogatur* (Cousin, P. Abaelardi opera, II. 151).

Roscelin was not without his supporters; among them was his contemporary Raimbert of Lille, and what the monk Hérیمان relates of his doctrine agrees with the statements of the master of Compiègne. Universal substances, says Hérیمان, are but a breath, which means *eos de sapientium numero merito esse exsufflandos*. He merely comments on the saying of Anselm characterized by the same jesting tone: a spiritualium

quaestionum disputatione sunt exsufflandi" (*P.L.*, 256a), and says that to understand the windy loquacity of Raimbert of Lille one has but to breathe into his hand (*manuque ori admota exsufflans* "*Mon. Germ. Hist.*", XIV, 275).

Tritheism of Roscelin

Roscelin considered the three Divine Persons as three independent beings, like three angels; if usage permitted, he added, it might truly be said that there are three Gods. Otherwise, he continued, God the Father and God the Holy Ghost would have become incarnate with God the Son. To retain the appearance of dogma he admitted that the three Divine Persons had but one will and power [*Audio ... quod Roscelinus clericus dicit in tres personas esse tres res ab invicem separatas, sicut sunt tres angeli, ita tamen ut una sit voluntas et potestas aut Patrem et Spiritum sanctum esse incarnatum; et tres deos vere posse dici si usus admitteret* (letter of Anselm to Foulques)].

This characteristic tritheism, which Anselm and Abelard agreed in refuting even after its author's conversion, seems an indisputable application of Roscelin's anti-Realism. He even argues that if the three Divine Persons form but one God, all three have become incarnate. There are therefore three Divine substances, three Gods, as there are three angels, because each substance constitutes an individual, which is the fundamental assertion of anti-Realism. The ideas of the theologian are closely linked with those of the philosopher.

Notes



1. Roscelin's writings and the council's acts have not survived and we know about them principally through the correspondence and writings of St Anselm.^[1]

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Citations

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3. Richard J. Utz, "Medievalism as Modernism: Alfred Andersch's Nominalist *Littérature engagée*," *Studies in Medievalism* 6 (1993), 76–90.

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- Cunningham, Francis (1836), *Text-book of Ecclesiastical History by J.C.I. Gieseler*, 3rd ed., Vol. II, Philadelphia: Carey, Lea, & Blanchard [A translation of the original German version]. (in English) & (in Latin)

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William of Ockham

William of Ockham (/ˈɒkəm/; also **Occam**, from Latin: *Gulielmus Occamus*;[[]^{9]}[[]^{10]} c. 1287 – 1347) was an English Franciscan friar, scholastic philosopher, and theologian, who is believed to have been born in Ockham, a small village in Surrey.[[]^{11]} He is considered to be one of the major figures of medieval thought and was at the centre of the major intellectual and political controversies of the 14th century. He is commonly known for Occam's razor, the methodological principle that bears his name, and also produced significant works on logic, physics, and theology. William is remembered in the Church of England with a commemoration on 10 April.[[]^{12]}

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William of Ockham

OFM



William of Ockham depicted on a stained glass window at a church

Born	1285 <div>Ockham, Surrey, England</div>
Died	1347 (aged 61–62) <div>Munich, Duchy of Bavaria, Holy Roman Empire</div>
Education	Greyfriars, London ^[1]
Alma mater	University of Oxford ^[2] ^[3]
Notable work	<i>Summa Logicae</i>
Era	14th-century philosophy <div>Medieval philosophy</div>
Region	Western philosophy
School	Scholasticism <div>Occamism</div> <div>Nominalism^[a]</div> <div>Theological voluntarism^[4]</div>

Life

William of Ockham was born in Ockham, Surrey in 1285. He received his elementary education in the London House of the Greyfriars.^[13] It is believed that he then studied theology at the University of Oxford^{[2][3]} from 1309 to 1321,^[14] but while he completed all the requirements for a master's degree in theology, he was never made a regent master.^[15] Because of this, he acquired the honorific title *Venerabilis Inceptor*, or "Venerable Beginner" (an *inceptor* was a student formally admitted to the ranks of teachers by the university authorities).^[16]

During the Middle Ages, theologian Peter Lombard's *Sentences* (1150) had become a standard work of theology, and many ambitious theological scholars wrote commentaries on it.^[17] William of Ockham was among these scholarly commentators. However, William's commentary was not well received by his colleagues, or by the Church authorities.^[18] In 1324, his commentary was condemned as unorthodox by a synod of bishops, and he was ordered to Avignon, France, to defend himself before a papal court.^[17]

An alternative understanding, recently proposed by George Knysh, suggests that he was initially appointed in Avignon as a professor of philosophy in the Franciscan school, and that his disciplinary difficulties did not begin until 1327.^[19] It is generally believed that these charges were levied by Oxford chancellor John Lutterell.^{[20][21]} The Franciscan Minister General, Michael of Cesena, had been summoned to Avignon, to answer charges of heresy. A theological commission had been asked to review his *Commentary on the Sentences*, and it was during this that William of Ockham found himself involved in a different debate. Michael of Cesena had asked William to review arguments surrounding Apostolic poverty. The Franciscans believed that Jesus and his apostles owned no property either individually or in common, and the Rule of Saint Francis commanded members of the order to follow this practice.^[22] This brought them into conflict with Pope John XXII.

Because of the pope's attack on the Rule of Saint Francis, William of Ockham, Michael of Cesena and other leading Franciscans fled Avignon on 26 May 1328, and eventually took refuge in the court of the Holy Roman Emperor Louis IV of Bavaria, who was also engaged in dispute with the papacy, and became William's patron.^[17] After studying the works of John XXII and previous papal statements, William agreed with the Minister General. In return for protection and patronage William wrote treatises that argued for emperor Louis to have supreme control over church and state in the Holy Roman Empire.^[17] "On June 6, 1328, William was officially excommunicated for leaving Avignon without permission,"^[15] and William argued that John XXII was a heretic for attacking the doctrine of Apostolic poverty and the Rule of Saint Francis, which had been endorsed by previous popes.^[15] William of Ockham's philosophy was never officially condemned as heretical.^[15]

He spent much of the remainder of his life writing about political issues, including the relative authority and rights of the spiritual and temporal powers. After Michael of Cesena's death in 1342, William became the leader of the small band of Franciscan dissidents living in exile with Louis IV. William of Ockham died (prior

Main interests	<u>Natural philosophy</u> · <u>metaphysics</u> · <u>epistemology</u> · <u>theology</u> · <u>logic</u> · <u>ontology</u> · <u>politics</u> .
Notable ideas	<u>Occam's razor</u> <u>Nominalism</u> <u>Empiricism</u> ^[5]
Influences	<u>Aristotle</u> , <u>Anselm of Canterbury</u> , ^[6] <u>Thomas Aquinas</u> , <u>Duns Scotus</u> , <u>Peter Abelard</u> , <u>Petrus Aureolus</u> , <u>Durandus of Saint-Pourçain</u> , <u>Henry Harclay</u>
Influenced	<u>Albert of Saxony</u> , <u>Jean Buridan</u> , ^[7] <u>Adam de Wodeham</u> , ^[8] <u>Gregory of Rimini</u> , <u>John Wycliffe</u> , <u>Gabriel Biel</u> , <u>Martin Luther</u> , <u>Henry VIII</u> , <u>John Calvin</u> , <u>Thomas Hobbes</u> , <u>René Descartes</u> , <u>Bertrand Russell</u> .



Sketch labelled "frater Occham iste", from a manuscript of Ockham's *Summa Logicae*, 1341

to the outbreak of the plague) on 9 April 1347.^[23]

Faith and reason

William of Ockham espoused fideism, stating that "only faith gives us access to theological truths. The ways of God are not open to reason, for God has freely chosen to create a world and establish a way of salvation within it apart from any necessary laws that human logic or rationality can uncover."^[24] He believed that science was a matter of discovery and saw God as the only ontological necessity.^[15] His importance is as a theologian with a strongly developed interest in logical method, and whose approach was critical rather than system building.^[25]

Philosophical thought

In scholasticism, William of Ockham advocated reform in both method and content, the aim of which was simplification. William incorporated much of the work of some previous theologians, especially Duns Scotus. From Duns Scotus, William of Ockham derived his view of divine omnipotence, his view of grace and justification, much of his epistemology and ethical convictions.^[26] However, he also reacted to and against Scotus in the areas of predestination, penance, his understanding of universals, his formal distinction *ex parte rei* (that is, "as applied to created things"), and his view of parsimony which became known as Occam's Razor.

Nominalism

William of Ockham was a pioneer of nominalism, and some consider him the father of modern epistemology, because of his strongly argued position that only individuals exist, rather than supra-individual universals, essences, or forms, and that universals are the products of abstraction from individuals by the human mind and have no extra-mental existence.^[27] He denied the real existence of metaphysical universals and advocated the reduction of ontology. William of Ockham is sometimes considered an advocate of conceptualism rather than nominalism, for whereas nominalists held that universals were merely names, i.e. words rather than extant realities, conceptualists held that they were mental concepts, i.e. the names were names of concepts, which do exist, although only in the mind. Therefore, the universal concept has for its object, not a reality existing in the world outside us, but an internal representation which is a product of the understanding itself and which "supposes" in the mind the things to which the mind attributes it; that is, it holds, for the time being, the place of the things which it represents. It is the term of the reflective act of the mind. Hence the universal is not a mere word, as Roscelin taught, nor a *sermo*, as Peter Abelard held, namely the word as used in the sentence, but the mental substitute for real things, and the term of the reflective process. For this reason William has sometimes also been called a "terminist", to distinguish him from a nominalist or a conceptualist.^[28]

William of Ockham was a theological voluntarist who believed that if God had wanted to, he could have become incarnate as a donkey or an ox, or even as both a donkey and a man at the same time. He was criticized for this belief by his fellow theologians and philosophers.^[29]

Efficient reasoning



Quaestiones in quattuor libros sententiarum

One important contribution that he made to modern science and modern intellectual culture was efficient reasoning with the principle of parsimony in explanation and theory building that came to be known as Occam's Razor. This maxim, as interpreted by Bertrand Russell,^[30] states that if one can explain a phenomenon without assuming this or that hypothetical entity, there is no ground for assuming it, i.e. that one should always opt for an explanation in terms of the fewest possible causes, factors, or variables. He turned this into a concern for ontological parsimony; the principle says that one should not multiply entities beyond necessity – *Entia non sunt multiplicanda sine necessitate* – although this well-known formulation of the principle is not to be found in any of William's extant writings.^[31] He formulates it as: "For nothing ought to be posited without a reason given, unless it is self-evident (literally, known through itself) or known by experience or proved by the authority of Sacred Scripture."^[32] For William of Ockham, the only truly necessary entity is God; everything else is contingent. He thus does not accept the principle of sufficient reason, rejects the distinction between essence and existence, and opposes the Thomistic doctrine of active and passive intellect. His scepticism to which his ontological parsimony request leads appears in his doctrine that human reason can prove neither the immortality of the soul; nor the existence, unity, and infinity of God. These truths, he teaches, are known to us by revelation alone.^[28]

Natural philosophy

William wrote a great deal on natural philosophy, including a long commentary on Aristotle's Physics.^[33] According to the principle of ontological parsimony, he holds that we do not need to allow entities in all ten of Aristotle's categories; we thus do not need the category of quantity, as the mathematical entities are not "real". Mathematics must be applied to other categories, such as the categories of substance or qualities, thus anticipating modern scientific renaissance while violating Aristotelian prohibition of *metabasis*.

Theory of knowledge

In the theory of knowledge, William rejected the scholastic theory of species, as unnecessary and not supported by experience, in favour of a theory of abstraction. This was an important development in late medieval epistemology. He also distinguished between intuitive and abstract cognition; intuitive cognition depends on the existence or non-existence of the object, whereas abstractive cognition "abstracts" the object from the existence predicate. Interpreters are, as yet, undecided about the roles of these two types of cognitive activities.^[34]

Political theory

William of Ockham is also increasingly being recognized as an important contributor to the development of Western constitutional ideas, especially those of government with limited responsibility.^[35] He was one of the first medieval authors to advocate a form of church/state separation,^[35] and was important for the early development of the notion of property rights. His political ideas are regarded as "natural" or "secular", holding for a secular absolutism.^[35] The views on monarchical accountability espoused in his *Dialogus* (written between 1332 and 1347)^[36] greatly influenced the Conciliar movement and assisted in the emergence of liberal democratic ideologies.

William argued for complete separation of spiritual rule and earthly rule.^[37] He thought that the pope and churchmen have no right or grounds at all for secular rule like having property, citing 2 Tim. 2:4. That belongs solely to earthly rulers, who may also accuse the pope of crimes, if need be.^[38]

After the Fall God had given men, including non-Christians, two powers: private ownership and the right to set their rulers, who should serve the interest of the people, not some special interests. Thus he preceded Thomas Hobbes in formulating social contract theory along with earlier scholars.^[38]

William of Ockham said that the Franciscans avoided both private and common ownership by using commodities, including food and clothes, without any rights, with mere *usus facti*, the ownership still belonging to the donor of the item or to the pope. Their opponents such as pope John XXII wrote that use without any ownership cannot be justified: "*It is impossible that an external deed could be just if the person has no right to do it.*"^[38]

Thus the disputes on the heresy of Franciscans led William of Ockham and others to formulate some fundamentals of economic theory and the theory of ownership.^[38]

Logic

In logic, William of Ockham wrote down in words the formulae that would later be called De Morgan's Laws,^[39] and he pondered ternary logic, that is, a logical system with three truth values; a concept that would be taken up again in the mathematical logic of the 19th and 20th centuries. His contributions to semantics, especially to the maturing theory of supposition, are still studied by logicians.^{[40][41]} William of Ockham was probably the first logician to treat empty terms in Aristotelian syllogistic effectively; he devised an empty term semantics that exactly fit the syllogistic. Specifically, an argument is valid according to William's semantics if and only if it is valid according to *Prior Analytics*.^[42]

Literary Ockhamism/nominalism

William of Ockham and his works have been discussed as a possible influence on several late medieval literary figures and works, especially Geoffrey Chaucer, but also Jean Molinet, the *Gawain* poet, François Rabelais, John Skelton, Julian of Norwich, the York and Townely Plays, and Renaissance romances. Only in very few of these cases is it possible to demonstrate direct links to William of Ockham or his texts. Correspondences between Ockhamist and Nominalist philosophy/theology and literary texts from medieval to postmodern times have been discussed within the scholarly paradigm of literary nominalism.^[43] Erasmus, in his *Praise of Folly*, criticized him together with Duns Scotus as fuelling unnecessary controversies inside the Church.

Works

The standard edition of the philosophical and theological works is: *William of Ockham: Opera philosophica et theologica*, Gedeon Gál, et al., eds. 17 vols. St. Bonaventure, N. Y.: The Franciscan Institute, 1967–88.

The seventh volume of the *Opera Philosophica* contains the doubtful and spurious works.

The political works, all but the *Dialogus*, have been edited in H. S. Offler, et al., eds. *Guilelmi de Ockham Opera Politica*, 4 vols., 1940–97, Manchester: Manchester University Press [vols. 1–3]; Oxford: Oxford University Press [vol. 4].

Abbreviations: OT = Opera Theologica voll. 1–10; OP = Opera Philosophica voll. 1–7.

Philosophical writings

- *Summa logicae* (Sum of Logic) (c. 1323, OP 1).
- *Expositionis in Libros artis logicae prooemium*, 1321–24, OP 2).
- *Expositio in librum Porphyrii de Praedicabilibus*, 1321–24, OP 2).
- *Expositio in librum Praedicamentorum Aristotelis*, 1321–24, OP 2).
- *Expositio in librum in librum Perihermenias Aristotelis*, 1321–24, OP 2).

- *Tractatus de praedestinatione et de prescientia dei respectu futurorum contingentium* (Treatise on Predestination and God's Foreknowledge with respect to Future Contingents, 1322–24, OP 2).
- *Expositio super libros Elenchorum* (Exposition of Aristotle's Sophistic refutations, 1322–24, OP 3).
- *Expositio in libros Physicorum Aristotelis. Prologus et Libri I–III* (Exposition of Aristotle's Physics) (1322–24, OP 4).
- *Expositio in libros Physicorum Aristotelis. Prologus et Libri IV–VIII* (Exposition of Aristotle's Physics) (1322–24, OP 5).
- *Brevis summa libri Physicorum* (Brief Summa of the Physics, 1322–23, OP 6).
- *Summula philosophiae naturalis* (Little Summa of Natural Philosophy, 1319–21, OP 6).
- *Quaestiones in libros Physicorum Aristotelis* (Questions on Aristotle's Books of the Physics, before 1324, OP 6).

Theological writings

- *In libros Sententiarum* (Commentary on the Sentences of Peter Lombard).
 - Book I (*Ordinatio*) completed shortly after July 1318 (OT 1–4).
 - Books II–IV (*Reportatio*) 1317–18 (transcription of the lectures; OT 5–7).
- *Quaestiones variae* (OT 8).
- *Quodlibeta septem* (before 1327), (OT 9).
- *Tractatus de quantitate* (1323–24. OT 10).
- *Tractatus de corpore Christi* (1323–24, OT 10).

Political writings

- *Opus nonaginta dierum* (1332–34).
- *Epistola ad fratres minores* (1334).
- *Dialogus* (before 1335).
- *Tractatus contra Johannem [XXII]* (1335).
- *Tractatus contra Benedictum [XII]* (1337–38).
- *Octo quaestiones de potestate papae* (1340–41).
- *Consultatio de causa matrimoniali* (1341–42).
- *Breviloquium* (1341–42).
- *De imperatorum et pontificum potestate* [also known as "Defensorium"] (1346–47).

Doubtful writings

- *Tractatus minor logicae* (Lesser Treatise on logic) (1340–47?, OP 7).
- *Elementarium logicae* (Primer of logic) (1340–47?, OP 7).

Spurious writings

- *Tractatus de praedicamentis* (OP 7).
- *Quaestio de relatione* (OP 7).
- *Centiloquium* (OP 7).

- *Tractatus de principiis theologiae* (OP 7).

Translations

Philosophical works

- *Philosophical Writings*, tr. P Boehner, rev. S Brown, (Indianapolis, IN, 1990)
- *Ockham's Theory of Terms: Part I of the Summa logicae*, translated by Michael J. Loux, (Notre Dame; London: University of Notre Dame Press, 1974) [translation of *Summa logicae*, part 1]
- *Ockham's Theory of Propositions: Part II of the Summa logicae*, translated by Alfred J. Freddoso and Henry Schuurman, (Notre Dame: University of Notre Dame Press, 1980) [translation of *Summa logicae*, part 2]
- *Demonstration and Scientific Knowledge in William of Ockham: a Translation of Summa logicae III-II, De syllogismo demonstrativo, and Selections from the Prologue to the Ordinatio*, translated by John Lee Longeway, (Notre Dame, IN: University of Notre Dame, 2007)
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- *Predestination, God's Foreknowledge, and Future Contingents*, translated by Marilyn McCord Adams and Norman Kretzmann, (New York: Appleton-Century-Crofts, 1969) [translation of *Tractatus de praedestinatione et de praescientia Dei et de futuris contingentibus*]
- *Quodlibetal Questions*, translated by Alfred J Freddoso and Francis E Kelley, 2 vols, (New Haven; London: Yale University Press, 1991) (translation of *Quodlibeta septem*)
- Paul Spade, *Five Texts on the Mediaeval Problem of Universals: Porphyry, Boethius, Abelard, Duns Scotus, Ockham*, (Indianapolis, IN: Hackett, 1994) [Five questions on Universals from His *Ordinatio* d. 2 qq. 4–8]

Theological works

- *The De sacramento altaris of William of Ockham*, translated by T Bruce Birch, (Burlington, Iowa: Lutheran Literary Board, 1930) [translation of *Treatise on Quantity and On the Body of Christ*]

Political works

- *An princeps pro suo uccursu, scilicet guerrae, possit recipere bona ecclesiarum, etiam invito papa*, translated Cary J. Nederman, in *Political thought in early fourteenth-century England: treatises by Walter of Milemete, William of Pagula, and William of Ockham*, (Tempe, AZ: Arizona Center for Medieval and Renaissance Studies, 2002)
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In fiction

William of Occam served as an inspiration for the creation of William of Baskerville, the main character of Umberto Eco's novel *The Name of the Rose*, and is the main character of *La abadía del crimen* (*The Abbey of Crime*), a video game based upon said novel.

See also

- Gabriel Biel
- Philotheus Boehner
- History of science in the Middle Ages
- List of Roman Catholic scientist-clerics
- List of scholastic philosophers
- Ernest Addison Moody
- occam (programming language)
- Ockham algebra
- Oxford Franciscan school
- Rule according to higher law
- Terminism


Notes

- a. However, Ockham has also been interpreted as a defender of Conceptualism

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