

# **Universals as Respects of Sameness**

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## **Declaration**

I hereby declare that the work presented in this thesis is my own.

Signed: \_\_\_\_\_ Date: \_\_\_\_\_

## Abstract

This thesis argues for realism about universals — the view that, in addition to particular things, there exist universals instantiated by those particular things. The first half presents a positive case for realism. Here it is claimed that universals are needed in our ontology to serve as the respects in which things are the same, and the features or characteristics that things have in common. This argument is defended against nominalist responses, first, that our apparent ‘ontological commitment’ to features and characteristics is not genuine; and second, that the same theoretical work can be achieved by treating respects of sameness as sets of particulars or sets of tropes rather than *sui generis* universals. The second half of the thesis defends the realist against the most serious objections to an ontology of universals. These are the problems arising from the realist’s obligation to ascribe referential function to predicates, and the family of difficulties known as “Bradley’s Regress”. By addressing both the reasons to believe in universals, and the alleged reasons *not* to believe in universals, it is hoped that a coherent case for realism is achieved.

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# Preface

Our attempts to describe similarities between objects seem to embody a theory about the nature of resemblance itself. Confronted with two similar objects, we say that they are (qualitatively) ‘the same’; that they ‘have something in common’ or ‘share some common feature’; that one has the ‘qualities’ or ‘characteristics’ of the other; that they are ‘identical in some respects’. How should we understand this talk of ‘sameness’ and ‘features shared in common’? The apparent quantification over ‘respects’ and ‘features’, and the lack of any obvious alternative to treating sameness as *identity*, tempt us to recognize entities which are shared between similar objects, whose presence we advert to by talk of their ‘shared features’ and ‘sameness in some respects’. The question of whether we must recognize the existence of a special kind of entity that objects share when they exhibit qualitative sameness, or share some common feature, is the subject-matter of the debate between nominalists and realists. Realists claim that it is necessary to recognize a special kind of entity — universals — shared between qualitatively similar things; nominalists deny that belief in the existence of universals is warranted. In the following chapters, I argue in favour of a realism that recognizes a shared universal wherever objects exhibit genuine qualitative sameness or objective similarity in some respect.

Before entering the debate, it would be well to ask, what *is* a universal, anyway? Without some answer to that question, we cannot know what claim we make when we say that universals exist. The first thing to say is that a universal is a special kind of entity that is literally identical across its instances, although it is shared by numerically distinct things. This serves to distinguish realism from some radical nominalisms, and from some trope theories, for the trope theorist considers the redness of *a* and the redness of *b* to be distinct entities, and may deny that there is any one thing *shared* between *a* and *b* as a universal is supposed to be shared between its instances. However, not every nominalist will deny that there is *something* literally identical shared between qualitatively similar things — for example, the Predicate Nominalist claims that what is shared is some actual or possible predicate, while the Class Nominalist claims that what is shared is a *set* containing the things that exhibit the similarity in question. For that reason, we must stipulate that universals are a *sui generis* kind of entity: they are not to be ‘reduced’ to one or another already recognized metaphysical kind, such as sets of individuals or tropes.

This characterization tells us more about what universals are *not* than about what they *are*. Can we supplement this with any positive account? It has already been observed that everyday discourse embodies a theory of universals in its vocabulary of characteristics,

features, attributes, qualities, properties, and relations.<sup>1</sup> All these words seem to pick out the same kind of entity; moreover they are entities which may be shared by different objects, for example when I am said to have some of the characteristics of my father. One of the reasons why it is rarely felt necessary to give a general account of what universals are is that the notion can be explained with reference to such usages in everyday discourse, and we may see our philosophical account of universals as clarifying and extending a theory already embodied in our everyday thought and talk. One element of everyday discourse about universals is that they are capable of combining with particulars: objects *have* properties, and *stand in* relations. This combination is not a matter of the particular being a constituent of the universal, in the way in which the Class Nominalist asserts that objects are constituents of the sets that are identified as their properties and relations. However, it is not an essential part of realism to hold the converse, that universals are constituents (Armstrong 1989a: 5) or ‘non-spatiotemporal parts’ (Lewis 1986: 67) of some further objects, whether these are construed as ‘thick particulars’ (Armstrong 1989a: 60) or ‘states of affairs’ (Armstrong 1997: 125); nor is it mandatory to treat the combination between universals and particulars as an entity — the relation of *instantiation* — although I shall argue later that this is in fact the best approach for the realist.

A feature of philosophical characterizations of universals is that they are considered immune to causal influence; nothing we could do would change the intrinsic nature of a given universal. This is not to say that universals need be causally *inert*. Certainly we are able to affect which combinations hold between universals and particulars, for our influence in the world extends as far as changing which properties objects have, and which relations they stand in. Moreover, universals may be said to have causal influence on us, even if we cannot change their nature. It would be hard to state a theory of perception which did not describe us as perceiving either the properties of objects, or the properties of our sensory impressions, and such perception requires that these properties be capable of exerting a causal influence on the world. Interlinked with this conception of universals as immune to influence is the idea that universals are mind-independent; nothing about the nature or existence of universals is determined by the activity of the human mind. This stipulation serves to distinguish realism from *conceptualism*, the position that what qualitatively similar things have in common is some man-made concept under which they fall.

Finally, any account of universals should mention the question of their location. So-called ‘aristotelians’ say that universals are located at the same places as the particulars

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<sup>1</sup>On this point I am in agreement with Quine (1969: 15).

that instantiate them, while ‘platonists’ may say that universals lack spatio-temporal location, or may take the extreme view that universals are located in a ‘Platonic heaven’. A premature decision between ‘platonism’ and ‘aristotelianism’ should not be allowed to prejudice the discussion; for now we should observe merely that it cannot be assumed that universals must have spatio-temporal location. I offer a tentative suggestion about how the controversy may be resolved in my conclusion.

The present work falls into two parts: chapters 1-3 make the case for realism about universals, while chapters 4-6 address difficulties for the realist project. This division is necessary because nominalists attack both the realist’s reasons for introducing universals in the first place, and the coherence and plausibility of the theory of universals itself. Discussion of problems for a realist ontology is helpful also because it enables us to see more clearly what kind of realist theory we may accept. My positive case for universals is based on a development of the argument known since antiquity as the ‘One over Many’, while the problems I consider are the difficulties associated with the thesis that predicates refer to universals, and Bradley’s regress.

Chapter one discusses the ‘One over Many’ argument. Realists sometimes talk as though there is *one* ‘Problem of Universals’, some single problem to which the existence of universals provides a *prima facie* solution (Armstrong 1989: 5; Rodriguez-Pereyra 2002: 14), where the burden of proof falls on the nominalist to explain why universals are not, after all, required (Armstrong 1978a: 19). We should not assume that there is only one way to argue successfully for the existence of universals, and in fact the historical debates that may be subsumed under the Problem of Universals cover everything from accounting for the objectivity of mathematical truth to explaining how a predicate can be true of more than one thing (MacBride 2002: 27). At times, in the absence of adequate dialectic, realists have been known even to resort to violence and intimidation to promote their position.<sup>2</sup> Indeed, it would be strange to hold that universals are among the fundamental constituents of reality, yet deny that their existence could be established by more than one argument. The paucity of means of establishing their existence would itself count against the thesis that they exist.

Nevertheless, the tradition from Plato onwards retains a special place for the ‘One over Many’ argument, and it is not unreasonable to begin our search for a reason to believe in universals with consideration of what this argument is, and how it works. I argue that

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<sup>2</sup>See for example the behaviour of the people of Soissons, who, in persuading Roscellinus to renounce his nominalism in 1092, augmented the authority of a church council with the threat of stoning by an angry mob. (Russell 1946, p.404. See also Mansi, *sacrorum conciliorum nova et amplissima collectio* 20.740-1 and Anselm, *epistola de incarnatione verbi* I.18, and *Ep.*136.)

ancient and modern versions of the One over Many suffer from one of two flaws: either they presuppose the right to talk of a shared entity where there is sameness of type, or they establish the need to talk of such an entity on the basis of an analysis which they give the nominalist no reason to accept. I suggest an alternative argument which, I claim, suffers from neither of these flaws. The proposal is that we must employ quantification over *respects* in which things are the same, and features which they share in common, in order to achieve a satisfactory understanding of the logic of statements attributing qualitative sameness and difference.

Merely establishing that we cannot escape quantification over respects of sameness and shared features is not sufficient to establish realism, however, and objections will come from two distinct camps of nominalists. First are those who deny that such quantifications force us to recognize the existence of entities over which they quantify. In the language popularized by Quine, they deny that such quantification is ‘ontologically committing’ — they deny that the quantifications we (truly) employ are relevant to ontic decision. A second camp of nominalists accepts the ontological commitment of the quantifications as genuine, and hence agrees that some entities are needed to serve as the things over which we quantify; nevertheless they propose that the entities in question need not form a *sui generis* metaphysical category, but can instead be identified with some other kind of entity — sets of objects, or sets of tropes. Such a nominalist recognizes the existence of respects of sameness and shared features which objects have; however, he claims that this recognition does not force him to incorporate in his ontology a *new* category — that of universals. I devote my second and third chapters to these two kinds of nominalist response.

It might seem that the case for realism is complete. However, the realist can be attacked not merely by criticizing his positive argument, but also by attacking the coherence of his conclusion. In my fourth chapter I discuss the relationship between predicates and universals. Here the realist is threatened by an antinomy which is apparently resolved only by abandoning the conclusion that universals exist. On the one hand, there are good reasons why, given that universals exist, we should accept the further conclusion that predicates refer to universals; on the other hand, the thesis of predicate-reference faces problems which seem to render it untenable. In this chapter I attempt to resolve the antinomy by showing how the problems associated with predicate-reference may be avoided, of which the most serious is the worry about the ‘unity of the proposition’.

Realism is not yet free of worries about unity, however. Perhaps the greatest challenge to the coherence of an ontology of universals comes from the family of arguments known as ‘Bradley’s Regress’. Whereas the problem of the unity of the proposition concerned



the unity of particular and universal in *thought*, the Bradleyan arguments suggest that particular and universal can never be unified in *fact*, for the relatedness of particular and universal requires that they stand as terms to some further relation  $I$ , and the relatedness of  $I$  and its terms requires that all three entities stand as terms to some further relation  $I^*$ , and so on for yet further relations  $I^{**}, I^{***}, \dots$  each intervening to relate the entities already mentioned. There is a lack of agreement about precisely how this regress is problematic for the realist; I devote my fifth and sixth chapters to clarifying and answering three different kinds of regress argument. By establishing how the realist should respond to these problems, we achieve a better understanding of what form a satisfactory theory of universals should take. It is hoped that my discussion, taken as a whole, constitutes a compelling case for realism about universals.

# 1. The Problem of Universals

Approaching the debate about the existence of universals for the first time, we might be surprised by two features of the realist's presentation. One is the idea that appeal to a 'Problem of Universals' is a way of *motivating* realism. It would be more natural to suppose that the Problem of Universals is the problem of whether we have good reason to believe that universals exist (like the 'Problem of Other Minds'), or a problem *about* universals (like the 'Problem of Consciousness'). Instead, realists claim that it is a problem to which universals offer a solution (Oliver 1996: 47); our reason for believing in universals will be that universals offer the *best* solution to the Problem of Universals.

A second surprise is the prevalence of talk about '*the* Problem of Universals', with the concomitant assumption that there is only one such Problem to be found. I have already noted the strangeness of believing that there is only *one* way to establish the existence of universals, although they are among the most basic furniture of reality; in practice the arguments given by realists are irreducibly diverse. Nevertheless, realists persist in identifying the Problem of Universals with an argument known since antiquity as the 'One over Many'. There is general consensus that the One over Many sets up a problem about qualitative sameness between numerically different particulars (Armstrong 1980: 102; Oliver 1996: 47); however, there is no agreement about what this problem is, nor is there a canonical account of what it is about universals that enables them to solve the problem. Consequently, it is possible to distinguish several different arguments about sameness and difference which might deserve the name 'One over Many'. The discussion in this chapter falls into two halves: the first criticizes current versions of the One over Many; in the second I develop and defend an argument that, so far as I am aware, has been overlooked by modern realists. I claim that this argument succeeds where other One over Many arguments have failed.

The Problem of Universals is usually said to provide no more than *prima facie* support for an ontology of universals: a nominalist may recognize the problem as genuine but claim that his theory solves it in such a way that an ontology of universals is avoided. That statement of the dialectical position is also true of my positive proposal. I argue that we have to accept quantification over 'respects of sameness' or 'features shared in common' in order to explain qualitative sameness and difference. Such acceptance brings with it a *prima facie* commitment to the existence of universals, for the apparent situation is that the entities we talk about when we discuss the 'features' of objects are universals; however, the nominalist may respond that the quantification in question is not ontologically significant,

or that the same explanatory work can be done using entities that are not universals. Because it is necessary to answer these nominalist counter-claims, the positive case for realism will not be complete until the end of Chapter 3.

I should mention two simplifying assumptions I make. Neither seems controversial. One is that 'qualitative sameness', 'sameness of type', 'similarity' and 'resemblance' are different names of the same relation. I shall not, for example, question whether two objects could exhibit 'qualitative sameness' without resembling each other; it seems pointless to do so. A second assumption is that there is at most one kind of entity picked out by terms such as 'shared features', 'characteristics' and 'respects of sameness'.<sup>3</sup> I shall proceed on the basis that these terms may be used interchangeably, and that variation between them is merely stylistic, unless we encounter a reason to make distinctions within the category of entities that these terms introduce.

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<sup>3</sup>The situation is not so clear-cut with terms such as 'type' and 'kind'; for discussion see below, section on Armstrong's Arguments.

## Criticism of Current Proposals

### 1. The One over Many as an Appeal for Truthmakers

One question we might ask, regarding qualitative sameness and difference, is *in virtue of what* do two objects resemble each other? In particular, we might demand to know the *entity* that is responsible for the resemblance obtaining. This entity we might call the ‘truthmaker’ for the truth that the objects resemble each other. Such an approach to the One over Many is not uncommon (Campbell 1990; Rodriguez-Pereyra 2002; Armstrong 2004a); moreover there are some who believe that *all* ontological questions are to be answered by determining whether entities of the disputed kind are required as truthmakers (Heil 2003, Melia 2005, Cameron 2008b). Truthmaker principles may seem a plausible way of codifying intuitions about how truth and falsity depend on a reality external to us, or capturing the insight behind correspondence theories of truth. Nevertheless, I shall argue that considerations about truthmaking do not motivate realism about universals, and the realist should look for an alternative way of advancing his cause.

The reasons for this are threefold. First, it is not clear that there is any good argument from the principle that every truth be made true by something in the world, to the conclusion that universals exist. Such a principle could not support realism about universals directly, with the universals posited as the truthmaking entities for truths about which things instantiate them, for the existence of a given universal is compatible with *many* possible ways the world could have been (for example, worlds in which that universal is instantiated by different particulars); therefore it cannot be that the mere existence of that universal can *make it* or *necessitate that* it is true that the actual pattern of instantiation does obtain. If there is a route to the truthmaker principle to universals it is via the thought that universals are needed to function as constituents of truthmaking ‘states of affairs’ (Armstrong 2004a: 39ff).

Even if the need for states of affairs is conceded, it is unclear why universals must be posited among their constituents. An alternative is to construe truthmaking states of affairs, not as complexes of particulars *and* universals, but as entities whose only constituents are particulars (Van Cleve 1994: 587). Perhaps the realist will attempt to rule out such a theory, on the grounds that it lacks the resources to distinguish the different states of affairs *a*’s being *F* and *a*’s being *G*. On such a theory these states of affairs would have the same sole constituent, the particular *a* itself (Rodriguez-Pereyra 2000: 289). However, this criticism cannot be justified by appeal to a general principle that different states of affairs must have different constituents, for the advocate of states of affairs as complexes of universals and

particulars must hold the states of affairs  $aRb$  and  $bRa$  to be different, although both states of affairs contain the same three elements:  $a$ ,  $R$ , and  $b$ . Given that the states-of-affairs theorist cannot appeal to a *general* principle ruling out different states of affairs with exactly the same constituents, it is hard to see what argument he could provide against the nominalist hypothesis that the only constituents of states of affairs are the particulars involved in them.

Further, it might be doubted whether states of affairs are needed as truthmakers in the first place, for it may be that the requirement for truthmakers can be met with an ontology of tropes or ‘particularized properties’. On the supposition that the tropes,  $a$ ’s  $F$ -ness and  $b$ ’s  $F$ -ness, could not have existed without (i) being tropes of the very kind that they are ( $F$ -ness tropes), and (ii) belonging to the objects which they do, it may be claimed that these tropes jointly make it true that  $a$  resembles  $b$ , for every world in which the tropes exist will be one in which  $a$  and  $b$  resemble one another in point of  $F$ -ness (Maurin 2002: 101). The possibility that the truthmaking entities are Van Cleve’s nominalist states of affairs, or tropes, leaves us with no reason to believe that the need for truthmakers will require us to introduce universals. To reduce the argument for realism to a demand for truthmakers is to argue for universals on the basis of a problem to which universals are not even an *apparent* solution.

A second reason to keep our argument for realism independent of considerations about truthmaking is that we lack a cogent reason to accept the truthmaker principle itself. For the truthmaker principle to give decisive support to realism, it must be the ‘strong’ or ‘full-blooded’ principle,

If  $p$ , some  $x$  exists such that  $x$ ’s existing necessitates that  $p$ .<sup>4</sup>

Any weaker version of the principle – for example the maxim that truth should be determined by what there is and *how* it is (Lewis 1992: 216) – would not mandate the existence of universals, since it would be consistent with the nominalist hypothesis that there are only particulars, and that what ‘makes it true’ that  $a$  is  $F$  is simply the existence of a particular that is a certain way, i.e. is  $F$ . I postpone consideration of attempts to motivate the Strong Truthmaker Principle until my discussion of the regress arguments inspired by Bradley (Chapter 5); there I argue that none of the attempted justifications is compelling. Realists should state their case without employing premises which the nominalist has no reason to accept; yet any realism founded on the Strong Truthmaker Principle would be all too easy to ignore, provided the nominalist is disinclined to accept the Principle itself.

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<sup>4</sup>Fox 1987, p.189

A third, decisive reason for motivating realism without appealing to a truthmaker principle is the threat posed by Bradley's regress. In Chapter 5 I distinguish three kinds of Bradleyan regress argument; one kind challenges the realist to say what makes the difference between a particular instantiating a universal and this not being the case. I argue that this version of the regress does not trouble the realist who is not also a truthmaker theorist; however, once the Strong Truthmaker Principle is accepted, this version of the regress is fatal to realism. For that reason, more than any, the realist should make his case without appealing to truthmakers.

## 2. The Ancient One over Many

Aristotle, at *Metaphysics A9*, tells us that the followers of Plato defended their belief in Forms by using an argument called the 'One over Many', and the commentary by Alexander of Aphrodisias preserves an account of what this argument is, supposedly taken from Aristotle's lost work *Peri Ideon* ('On the Forms'). The first premise of the argument is that, when some things are *F*, there is some one thing that they share (Fine 1993: 104). Aristotle may well have been correct to say that this claim was central to the Platonic One over Many, for we also find it (albeit not presented as part of an extended *argument*) at *Republic 596a*, where the interlocutors agree that they are 'accustomed' (εἰωθήμεν)<sup>5</sup> to posit a Form whenever the same term applies to each of a multitude. Some debate is possible about whether Plato intended a Form to correspond to *every* predicate, as Aristotle alleged, or merely to general terms which mark genuine sameness of type (Fine 1980); however we need not involve ourselves in this controversy, for I shall claim that the ancient argument is unsatisfactory even when the principle for generating Forms is restricted to predicates which 'carve nature at the joints'.

The problem is this: the ancient argument starts from a position of presumed agreement that whenever a predicate (or at least, a predicate which makes for sameness of type) applies to a number of things, some entity exists which is common to all those things. The remainder of the Platonic argument then consists in the demonstration that the common entity in question is 'separate' and 'eternal', and therefore deserves to be described as a Form or universal (Fine 1980: 200). But the presumed agreement that there is an additional entity which is the 'One' over the 'Many' particulars is a mirage. Some nominalists accept the existence of a shared entity: the Class Nominalist, for example, will recognize a shared entity yet insist that this entity is a *set*, not a universal. The argument presented by Aristotle is relevant to such theorists. However, the ancient One over Many has nothing to say to

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<sup>5</sup>'*eiōthamen*'

those nominalists who, when  $a$  is  $F$  and  $b$  is  $F$ , recognize the existence only of the two particulars  $a$  and  $b$ . Such theorists simply *deny* the first premise of the ancient One over Many: they deny that when  $a$  is  $F$  and  $b$  is  $F$ , there is a third entity such that  $a$  and  $b$  share it. The argument presented by Aristotle is unsatisfactory because does nothing to persuade a recalcitrant nominalist to accept the existence of such a shared entity. Perhaps it is for the best that Plato himself presents the first premise of the One over Many as a general principle followed by Platonists rather than as part of an argument that *demonstrates* the existence of Forms. It would be charitable to suppose that the argument Aristotle attributes to the ‘Platonists’ was not heavily relied on by Plato himself.

The kind of nominalist in question, who refuses to acquiesce in speaking of a shared, common entity where there is sameness of type, might reasonably be described as a species of ‘Ostrich Nominalist’. The nomenclature originates with David Armstrong (1978a: 16), but its notoriety may be traced to a paper by Michael Devitt (1980). Devitt identifies the characteristic feature of an Ostrich Nominalist as the refusal to take seriously some problem advanced in support of realism. Thus one could be an Ostrich Nominalist by adopting a dismissive attitude to any realist attempt to formulate a Problem of Universals.<sup>6</sup> Here the problem advanced by the proponent of the ancient One over Many is how the nominalist can make sense of the ‘fact’ that two numerically different entities can nevertheless share some third, distinct, entity between them. The ‘Ostrich’ response is that no explanation or ‘making sense’ of this ‘fact’ is required because there is no reason to believe it to be true in the first place: no reason to believe the premise that two similar things share a common entity between them.

I have claimed that the ancient One over Many fails because it assumes the right to talk of a shared, common entity whose existence the nominalist sees no reason to grant. Far from assuming agreement that such a common entity exists, the realist should begin by giving reasons *why* our ontology should include any such entity. Other versions of the One over Many attempt to persuade the nominalist to recognize a shared entity by drawing his attention to certain facts about qualitative sameness which could only be the case if such common entities existed. We may distinguish three modern One over Many arguments which proceed on this basis. One appeals to facts expressed in sentences like ‘ $a$  and  $b$  have

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<sup>6</sup>Devitt preserves the pejorative sense of the term. According to him, one can be an Ostrich Nominalist only by ignoring a *genuine* problem. For that reason Devitt refuses to apply the term either to Quine or to himself; he does not believe that there is any genuine problem to ignore. More recently, however, the label of ‘Ostrich Nominalist’ has become adopted as a badge of honour by the very philosophers it was intended to impugn, and it is now a position which we might have a ‘fling’ with (Van Cleve 1994), without thereby admitting we ignore a *genuine* Problem of Universals.

the property  $F$ , where the nominalist is challenged to say what entity, if not a universal, is denoted by the phrase ‘the property  $F$ ’. Another argument appeals to the idioms which apparently involve us in first-order quantification over an entity which is a shared feature or characteristic, e. g. ‘ $a$  and  $b$  have something in common’. Third is a family of arguments about ‘sameness of type’, or ‘qualitative sameness’, where it is claimed that the nominalist owes us an account of the ‘sameness’ in question, which the realist takes at face value as *identity* of characteristic or feature shared between objects. The most eminent advocate of the first and third of these One over Many arguments is David Armstrong; although my discussion will concentrate on his presentation of the arguments, it is intended that the conclusions I draw will be entirely general rather than *ad hominem*. In each case I shall suggest that the One over Many argument under consideration will not succeed against the committed nominalist.

### 3. Discourse about Properties

In his early formulations of the One over Many, Armstrong suggests that the challenge for the nominalist is to analyse sentences of the form

‘ $a$  has the property  $F$ .’<sup>7</sup>

The need for nominalist analysis is obvious since the sentence is apparently about two things: a particular and a property. Any nominalist who seeks to restrict his ontology to concrete particulars alone must propose some analysis according to which ‘the property  $F$ ’ does not denote an entity. The realist is under no such obligation to give an analysis; he can take talk of ‘properties’ at face value, as being discourse about universals.

This version of the One over Many seems unconvincing, for the nominalist can claim to have no compelling reason to accept that discourse about the ‘properties’ of objects is apt to express truth. Surely a nominalist who denies the existence of properties will *deny* that ‘ $a$  has the property  $F$ ’ expresses a truth, precisely on the grounds that *there are no properties!* Certainly, he will concede, discourse about ‘properties’ is rife in many areas of philosophy outside metaphysics, for example when the philosopher of mind asks whether mental properties might be identical with physical properties of the brain. Nevertheless, the mere fact that property-discourse has been enthusiastically adopted by philosophers does not prove that the discourse is true and the entities exist, any more than the mere

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<sup>7</sup>This is the demand which is reiterated at the start of the discussion of each kind of nominalism in *Universals and Scientific Realism I*. See Armstrong 1978a, pp.15, 53 and *passim*.



prevalence of talk about such entities as propositions or facts could force us to conclude that such entities are part of the furniture of the world. The nominalist's response, then, is this: 'property' is a term of art, popularized if not invented by philosophers; property-theoretic discourse is not the expression of obvious truths about the world, but an artefact of a prior, and mistaken, belief in an ontology of properties. The nominalist's theory of property-discourse will be an error-theory: anyone who talks about properties is making a mistake, the mistake of thinking that there are such entities to talk about. If the realist cannot be sure of agreement that his proposed 'truths' about properties are in fact true, his argument is in no better shape than the ancient One over Many: again, the nominalist is under no compulsion to respond to the challenge, since the alleged 'fact' from which the argument starts is one which the nominalist is inclined to dispute.<sup>8</sup>

Perhaps the realist has a response. Let it be granted that property-discourse is not part of everyday life, outside the disciplines of those who already theorize *about* properties and could be accused of partisanship (although it will be noted that any competent computer-user knows what it is to modify the 'properties' of a file). Nevertheless, the realist will observe that discourse committing us to entities which play the role of properties need not use the word 'property'; instead we might point to talk about 'qualities', 'characteristics', 'attributes', 'respects' in which things are the same, and 'ways' things are alike. *These* words genuinely do form part of everyday speech of which the nominalist owes us an account. Indeed, the depth to which such discourse is embedded in everyday speech is suggested by the fact that a recent successful UK television show did not need to explain what it was looking for when it sought out the 'X Factor', a quality or attribute possessed only by those destined for greatness in the field of popular music.

This move is a step forward for the realist, for it can no longer be claimed that the alleged 'facts' are 'property-theoretic', only to be accepted by those who are partisans for an ontology of properties. It is not merely philosophers who talk of 'qualities' and 'attributes', and it would not be unfair to conclude, as Quine does (1969a: 25), that attributes themselves are part of a shared 'community ontology' into which we are inducted from childhood. However, the nominalist might offer the same response as before: we cannot conclude, simply because a community persists in speaking of 'qualities' and 'attributes', that discourse

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<sup>8</sup>Rodriguez-Pereyra offers a different nominalist approach, admitting that facts mentioning properties are 'undeniable' (2002: 15), but claiming that he makes use of property-discourse 'without committing myself to the existence of any *entities* over and above...particulars in general (2002: 16). Such a position stands or falls on his ability to give a nominalistically-acceptable account of how we should understand statements quantifying over properties; see Chapter 3, section on Resemblance Class Nominalism, for criticism of Rodriguez-Pereyra's account.

employing such words expresses facts of which the nominalist owes us an account. Again, the nominalist's account of such uses is merely an error-theory: that our constant talk of attributes and qualities is simply a mistake, for there are in fact no such things. The only difference between 'property' and 'attribute' discourse is that the latter is community-wide rather than restricted to a small circle of those with a special interest. So far we have supplied no reason why such discourse is something the parsimonious nominalist cannot do without. The ancient One over Many and Armstrong's demand for analysis of '*a* has the property *F*' share a common flaw: they demand an analysis of sentences that many nominalists simply do not recognize as expressing truths. If it is possible to deny the truth of the 'facts' of which the realist demands an account, then there is no reason for the nominalist to abandon the characteristically 'Ostrich' position that he is under no obligation to give such an account.

#### 4. Having Something in Common

Is there a way to reduce the appeal of an error-theory directed towards discourse apparently about properties? In the previous section, a nominalist error-theory was plausible because, in cases such as '*a* has the property *F*', it is possible to suggest that 'has the property *F*' is an ontologically misleading way of saying that *a* is *F*. (The nominalist may even go so far as to claim that the replacement of 'has the property *F*' with 'is *F*' constitutes an *analysis* of the kind Armstrong demands.) The realist needs to provide sentences where no facile recasting is possible, for then there is no possibility of the error-theorist suggesting an alternative way of saying roughly the same thing as the original sentence, minus the objectionable reference to a property. An error-theory that suggests what we *ought* to say instead of the controversial sentences is more plausible than one which merely declares a whole class of sentences ill-suited to expressing truth, without suggesting a better way to get at the truth we want to express.

A better approach is suggested by a gnomic remark of David Lewis<sup>9</sup>, that a more serious challenge to the nominalist is to account for

'*a* and *b* have some common property'<sup>9</sup>

where the property in question is not specified. Without knowing *which* property we are talking about, the nominalist cannot say what we *ought* to have said instead of using the 'ontologically inflated' sentence that mentions properties. However, Lewis might still be

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<sup>9</sup>Lewis 1983, p.201

accused of begging the question against the nominalist: the proposed *analysandum* is a sentence that talks of ‘properties’, yet the legitimacy of property-discourse is precisely what the nominalist challenges. The realist’s proposed ‘fact’ should be one which makes no mention of ‘properties’, ‘attributes’ or ‘qualities’, for the nominalist can allege that anyone who talks of such entities does so only because a prior partisan allegiance to a realist ontology.

To evade these problems, the realist should appeal to statements of fact which make no attempt to categorize the entity in question. A good candidate is a sentence like

(SC) *a* and *b* have something in common.

This sentence is not tainted by the suspicion of partisan allegiance to properties, attributes or qualities, for it employs no such general terms; it may be understood and accepted even by someone who had never encountered the words ‘quality’ or ‘attribute’. It is plausible that many truths about the world are expressed in sentences of the form of (SC). Yet (SC), if it expresses a truth, lends powerful support to the realist, for both its surface form and its logical relationships with other sentences suggest that (SC) involves first-order existential quantification, thus:

(SC\*)  $(\exists x)$  *a* and *b* have *x* in common.

(SC\*) is a plausible account of the logic of (SC) not only because (SC) employs the natural-language quantifier expression ‘something’, but also because (SC)’s logical relations with other sentences are precisely what we would expect if it had an existentially quantified form: ‘*a* and *b* have something in common’ is entailed by ‘*a* and *b* have everything in common’, and also by ‘*a* and *b* have their shape in common’. If we must recognize quantification over shared entities, which seem to be universals, then there is a *prima facie* case for the nominalist to answer, for he will have to find a way of blocking the inference from the truth of the quantified idiom to the existence of universals.

However, the fact that different things which exhibit qualitative similarity may be said to ‘have something in common’ does not signal victory for the realist. The problem is the same as before: that it will be possible for the committed nominalist to urge an error-theory for such utterances. He will claim that although we may, to all intents and purposes, *treat* (SC) as expressing a truth, we would be mistaken in taking it as an undeniable or ‘Moorean’ fact about the world.<sup>10</sup> Certainly the use of the expression ‘have something in common’ is not

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<sup>10</sup>For the introduction of ‘Moorean’ facts see Armstrong 1980, p.102

confined to those who assume the right to speak of ‘properties’, ‘qualities’ or ‘attributes’, but why should the fact that everyone employs this locution force us to admit that we are correct to do so? This is the approach counselled by Quine, who recommends that we repudiate quantification of this kind as habitual falsehood: no more than a ‘popular and misleading manner of speaking’ (1980: 10).

Is Quine’s suggestion a genuine option for the nominalist in this case? It might seem that the nominalist who proposes an error-theory for the entirety of the discourse that commits us to features or characteristics simply misunderstands where the burden of proof lies. Quine’s own approach is that we should decide ontological questions by discerning the ‘ontological commitments’ (1960: 238) of our best theory; choice between theories may be informed by ontological considerations but is not to be entirely determined by them, for scientific method is the ‘last arbiter’ (1960: 23) of decisions between theories. In adverting to idioms of quantification apparently over universals, the realist has pointed out a kind of discourse that is a feature of every scientific theory, for scientific theories quantify over the features and characteristics of objects no less than any other kind of discourse. Van Inwagen offers an argument of this kind, appealing to statements such as ‘Spiders have some of the anatomical features of insects’ (2004: 114). This quantification over shared features, he believes, is beyond question by the nominalist, for it is a ‘simple fact of biology’ (2004: 116). Moreover, there is no hope of evading the commitment by ‘paraphrasing’ into a non-quantificational form, for this would leave us without an account of the logical interaction between different sentences involving quantification over anatomical features.<sup>11</sup> A plausible position for the realist is that *any* likely candidate to be our ‘best theory’ will quantify over features shared between objects; if our ontology is driven by wider considerations about which is our ‘best’ theory, then the nominalist ought not to be so willing to reject as false those portions of the theory which are not in accordance with his nominalism. Certainly, if the nominalist were right and there are no entities of the kind we quantify over, we would require an explanation of *why* it is that every kind of discourse consistently presupposes the right to employ such quantification.

The current proposal is this: the burden of proof does not lie with the realist, to demonstrate that quantification over features shared in common is ‘indispensable’; instead, given that every form of discourse presupposes the right to such quantification, the burden lies with the nominalist, to show why we *should* dispense with something that is a feature of all of our theories. The suggestion is that indispensability is the wrong criterion for the admission of an entity. If scientific method is to be trusted as the means by which theories

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<sup>11</sup>Van Inwagen 2004, p. 120. See below, Chapter 2, for more on paraphrase.

are judged against one another, then it may be claimed that the nominalist has no *right* to suggest that the best theories of science are mistaken in their willingness to quantify over the features and characteristics of objects. Let the scientists choose the theory; let the philosopher clarify the ontological consequences.

A nominalist who proposes an error-theory of the kind under consideration faces the accusation of unwarranted meddling in the affairs of scientists, for he must be prepared to attribute widespread error to scientific theories that quantify over features and characteristics, although the decisions that led to the adoption of such theories may be paragons of scientific method. Can he answer such a charge, and sustain his rejection of these entities? It seems the nominalist has a response. He can allege that scientific method, as practised by working scientists, is deficient in its inattention to ontological consequences. The reason all our current theories quantify over features and characteristics of objects is simply that the people who choose them are unaware of, or unconcerned by, the ontological significance of their own theory. What gives the nominalist the 'right' to interfere with scientific method is that, if they do not scrutinize the theories from an ontological point of view, then no-one does. The result is an unduly inflated ontology.

For that reason, it does not seem possible to rule out the nominalist's error-theory on the grounds that it requires him to question established scientific fact, for the 'facts' in question were established by a method that took little or no account of the ontological consequences of the choice between theories. The nominalist might take a similar approach with other statements which are claimed to be undeniable and irrevocably committed to features or attributes, such as 'Red resembles pink more than blue' and 'Red is a colour' (Pap 1959; Jackson 1977). Since these statements can only be understood as claims *about* the attributes *red*, *pink* and *blue*, the nominalist who repudiates such entities has no option but to deny that they express a truth. These sentences attempt to convey a truth about resemblance between the features that objects share, but in reality, the nominalist says, there are no such features, and the appearance that there is some quality, *red*, whose resemblance to other qualities may be assessed, is mere illusion. In so saying, the nominalist goes against common sense, but it is no surprise to discover that common sense embodies commitment to the features and characteristics of objects. Just as he rejected the right of scientific method to be the ultimate arbiter of scientific truth, on the grounds that scientific method is inattentive to ontological consequence, the nominalist will argue that he has the right to question 'truths' of common-sense, if these truths were only accepted in the first place through inattention to their ontological import. In any case, even the *realist* may need to reject such 'truths of common sense' as 'Red resembles pink more than blue', if the

features named are ones for which his theory can supply no corresponding universal (Lewis 1983: 196).

This is not to deny, however, that the nominalist's position is a difficult one. His ability to get by without giving an account of features and characteristics shared between objects depends on our willingness to accept that common sense and scientific theory are mistaken in their commitment to these entities. When we realize the scale and depth of that commitment, acceptance of the nominalist error-theory might not be feasible. Nevertheless, it would be desirable not merely to repudiate the views of the nominalist, but also to *refute* them. For that reason I shall continue to argue as though an error-theory of the kind he proposes is, at least, not an impossibility. The real objective for the realist should be to show that we *cannot* get by without a theory of shared features and common characteristics, however much common sense we are willing to sacrifice.

## 5. Armstrong's Arguments about Sameness of Type

Armstrong claims that the One over Many is an argument based on the 'fact of sameness of type' (1980: 102). But what are 'facts of sameness of type'? Markku Keinänen (2005: 101) has argued that the phrases 'type identity' and 'sameness of type' are ambiguous. We could use 'sameness of type' as a general term for any case in which two things are qualitatively similar; alternatively it could be limited to cases in which the objects fall under a certain sortal or share membership of a natural kind. When two things are both red they are 'same-typed' in the sense of being similar; when two things are both tigers they are 'same-typed' because they are both members of the natural kind *tiger*. We may add another distinction: it is not obvious from the name whether 'sameness of type' can be qualitative similarity of *any* kind, or whether it is to be limited to cases in which we are inclined to recognize the objects as 'tokens' of a 'type' in the sense in which two physical books can be tokens of the type *War and Peace*.

It is clear that, in talking of 'sameness of type', Armstrong intends to include both straightforward cases of qualitative similarity, and the more limited phenomenon of falling under some sortal, kind or type. He considers both the situation of

'two dresses being the same shade or colour,'<sup>12</sup>

where 'sameness of type' indicates qualitative similarity, and the situation where two different words — 'a' and 'the' — are of the same type because they both fall under the

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<sup>12</sup> Armstrong 1980, p.103

sortals 'grammatical article' and 'word' (1989a: 7). I shall adopt Armstrong's use of 'sameness of type', and say that two objects are 'same-typed' whenever they exhibit any kind of qualitative sameness or similarity, with no restriction to cases in which we treat objects as tokens of types, or members of natural kinds. I shall also employ his relaxed use of the term 'type' to denote the entities which are said to be shared in common between qualitatively similar or 'same-typed' things. Where confusion threatens, we may use the phrase 'proper type' to distinguish types in the more usual, narrow sense of thing that have tokens.

Even if we may acquiesce in Armstrong's relaxed use of 'type' talk, we should take issue with his failure to make explicit another, more important, distinction. Sentences of the form '*a* and *b* are the same *X*' sometimes assert numerical identity between *a* and *b*, for example when we say

Everest and Gaurisankar are the same mountain.

Equally often, a sentence with this form is used to assert what we might call 'qualitative sameness' between two numerically distinct things, as in

My tie and her dress are the same colour.

Armstrong does not distinguish between these two ways of being 'the same *X*'. Consequently he does not distinguish explicitly between two different realist arguments, which we may call the 'Argument from Identity' and the 'Argument from Qualitative Sameness'. The first of these takes as its starting point statements about (numerically) the same *X*, where we appear to be talking about a type or universal, for example

The same word is written on the left of the page as on the right.

The second is an argument about statements where two numerically different particulars are said to be (qualitatively) the same in some respect. Here, instead of talking about one numerically identical thing which happens to be a type, we say of two distinct things that they exhibit qualitative sameness. I shall argue that neither argument is as convincing as the realist might hope. Armstrong's arguments are not decisive against the nominalist.

### **The Argument from Identity**

The Argument from Identity is most strongly advanced in *Universals, an Opinionated Introduction*. Here, Armstrong asks us to consider two inscriptions next to each other:

## THE THE

We are invited to count the words: one way of describing the situation is that there are two inscriptions, but only one word, 'the' (1989a: 1). The two inscriptions may truly be said to be inscriptions *of the same word*. It is clear that 'the same word' is to be understood in terms of identity: there is only one word here, and it is written both on the right and the left of the page. This observation generates a *prima facie* case for universals with admirable speed. There is one thing which is written on the left of the page and on the right of the page; moreover, this thing may be written in many different scripts, and in many different ways, in many different places. But this one thing cannot be identical with any of its instances, since these instances are individuated by the particular places and times they occupy. So the one thing we talk about when we talk about 'the same word' seems to be a universal, capable of occurring at multiple different locations and being exemplified by many different pieces of matter.

This interpretation of the argument helps us understand Armstrong's response to an objection of Michael Devitt's (1980). Devitt suggests that the nominalist can accept the truth of '*a* and *b* share some common property, *F*' without conceding the existence of properties, since this sentence may be analysed as '*a* and *b* are both *F*' which in turn may be transformed into '*a* is *F* and *b* is *F*', thus relieving the theorist of his apparent ontological commitment to properties. Armstrong replies that

'It is true that "*a* and *b* are both red" is an abbreviation of "*a* is red and *b* is red." But the abbreviation does not hold just for these particular sentences..., but is a rule-governed, projectible transformation which we are capable of applying to an indefinite multiplicity of sentences. And what is the rule? It goes something like this. Suppose we are given a sentence of the form "*a* is \_\_\_ and *b* is \_\_\_." If but only if the two blanks are filled by the same predicate, it is permitted to rewrite the sentence as "*a* and *b* are both \_\_\_," with that same predicate in the new blank. But "same predicate" here is a type-notion. It is not meant that the very same predicate-token be plugged successively into three gaps!'<sup>13</sup>

Armstrong's central claim is that the rule to which Devitt must appeal talks about 'the same predicate' being found in three different places in a formula. By 'the same predicate' Armstrong cannot mean that qualitatively similar predicate tokens are required in each gap of the formula, for such tokens could be qualitatively similar in virtue of colour or

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<sup>13</sup>Armstrong 1980, p.104. So too following quotes.



size, while not being tokens of the ‘same predicate’ in the sense that is needed for Devitt’s rule to work. So the sense of ‘same’ in ‘same predicate’ must be identity. Armstrong’s response to Devitt is that the proposed transformation rule talks of numerically the same predicate being found in three different places in a formula. But this one predicate cannot be a predicate-*token* – a physical manifestation of a predicate – for then it would have to be capable either of appearing in all three places at once (the preserve of a universal), or of moving physically from one gap to the next. This is the point of Armstrong’s insistence that the rule should not require that the same predicate-token ‘be plugged successively into three gaps’. The charge against Devitt is clear: he has provided a general method for dispensing with statements which commit us to the existence of properties, but only at the cost of making tacit appeal to a rule which mentions, and so is apparently committed to, predicate-*types*.

The objection to Devitt is of the kind Armstrong imagines at the start of *Universals and Scientific Realism*:

‘If, then, in the course of an attempted Nominalist analysis it should happen that covert appeal is made to the notion of property, kind, or type, the analysis has failed to achieve its purpose.’<sup>14</sup>

Devitt believes that he can eliminate all mention of types from his theory. If this elimination is unsuccessful, then the nominalist is obliged to explain what types are, if they are not universals. To undertake such a project is to accept that there is a genuine problem to answer, and hence to abandon Ostrich Nominalism. That is why Armstrong believes that Devitt has not succeeded in defending an Ostrich Nominalist position.

Now it is clear what the ‘Argument from Identity’ is, we can try to determine what, if anything, the realist succeeds in establishing with it. The nominalist is confronted with examples of identity-claims about types, for example

The word on the left is the same word as the one on the right.  
I drive the same car as my brother.

Here the realist might invoke Quine’s exhortation to ‘paraphrase or retract!’ (1969a: 100). Since these identity-statements are naturally understood as involving quantification over word-types and car-types, they apparently exhibit ontological commitment to entities — types — which are capable of multiple instantiation and hence *prima facie* are universals.

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<sup>14</sup>Armstrong 1978a, p.19

Like any other apparent ontological commitment, the nominalist must either ‘retract’ — deny that the statements are true — or ‘paraphrase’ — propose some other analysis which reveals that the commitment is merely apparent, and not genuine.

Again, a familiar problem arises: the nominalist may simply choose to retract the troublesome statements — to deny their literal truth. It seems too much to require the nominalist to accept that numerically the same sentence is written on two different pieces of paper, or that numerically the same symphony was played on two different nights. Notice that one way in which a nominalist might choose to express his ontological position would be to *deny* this kind of sameness-claim. We could imagine a nominalist explaining his view thus:

“Of course, we may say that the same (identical) argument is found in two different books, but we only do this when we are being lazy – indulging in the ‘community ontology’ rather than our more accurate sparse conception. Strictly speaking, it is just false to talk of identical things having instances in different places.”

As we have seen, this need not be the last word on the argument. The realist may be able to claim that the nominalist’s retraction of such forms of speech is implausible: that we should not adopt an error-theory for discourse so widely used and so essential to the expressive power of the language.<sup>15</sup> Such arguments may be persuasive against the nominalist, but we cannot expect them to be conclusive. It is to be hoped that the realist can do better than merely complain about the extent of the revision of common sense that nominalism of this kind entails.

### **The Argument from Qualitative Sameness**

It would be surprising if there was nothing more to the ‘One over Many’ than the Argument from Identity. The statements which that argument deals with mention only one thing — a type — which is said to be numerically the same wherever it is found. But, given the name of the argument, we would expect it to pose a problem about one thing combined with *many* other things. A recurrent theme in discussion is that the One over Many is a problem about the qualitative sameness of numerically different things. H. H. Price contends that the main justification for realism is that we notice ‘constant recurrences and repetitions’ (1953: 7) between different things in the world — for example where several

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<sup>15</sup>For an argument of this kind see Strawson 1979 pp.57-8.

objects are the same colour, or where several groups of things exhibit the same 'pattern or mode of arrangement' (1953: 7). Likewise, Armstrong points out that

'we... are continually talking about the sameness of things. And most of the time when we talk about the sameness of things we are talking about the sameness of different things.'<sup>16</sup>

This suggests that, as well as being concerned with statements about (numerically) the same type, the realist can provide an argument about sentences in which two or more numerically different things are said to be (qualitatively) the same.

Why should the phenomenon of two different things being the same shape or same colour constitutes a '*prima facie* case for postulating universals' (Armstrong 1980: 102)? Armstrong seems to think that talk of two particulars' qualitative sameness, or 'sameness of type', is *prima facie* to be understood as asserting that both particulars, though numerically different from each other, share something literally identical between them. This way of looking at things motivates his original statement of the One over Many in *Universals and Scientific Realism*:

'Apparently there can be something identical in things which are not identical. Things are one at the same time as they are many.'<sup>17</sup>

We are now in a position to give an outline of the Argument from Qualitative Sameness:

- (i) Two numerically different things, *a* and *b*, are qualitatively the same.
- (ii) If *a* and *b* are qualitatively the same, they share numerically the same type; therefore
- (iii) Types exist.

Once laid out in this form, the argument is not as persuasive as it might originally have seemed. Here, the problem for the realist is *not*, as it was before, that the nominalist can deny the truth of the sentences he is challenged to analyse, for no nominalist should deny that two things can be qualitatively the same. Instead, the problem is that there is no reason to believe the claim of premise (ii) that qualitative sameness must be understood in terms of an entity shared between the two objects.

Why should we believe that qualitative sameness depends on the sharing of something literally identical? It might be claimed, with Price, that we experience something identical

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<sup>16</sup> Armstrong 1980, p.102

<sup>17</sup> Armstrong 1978a, p.11

in objects which are numerically different, but a claim about the phenomenal character of perception should not be expected to sway a nominalist. If it were *obvious* that we perceive numerically the same universal in different objects, we might expect there to be far fewer nominalists. An alternative strategy would be to appeal to idioms of ‘*sameness of type*’. It seems that ‘*a and b are qualitatively the same*’ means the same as ‘*a and b are of the same type*’. Perhaps the realist can argue that ‘*a and b are of the same type*’ really means ‘*a and b have, or exhibit, the same type*’ — i.e. that sameness of type is a relation between particulars and a further entity, a type.

It is hard to see why the nominalist must accept that ‘*a and b are of the same type*’ is more accurately expressed as ‘*a and b have the same type*’, as the realist proposes. The nominalist can respond that statements of ‘*sameness of type*’ do not assert that two particulars are related in some way to a third entity — a type; instead he can say that ‘... and ... are of the same type’ is a two-place predicate in which the argument-places are to be filled by expressions referring to objects. Since the realist is willing to say that two things ‘are of the same type’ whenever they exhibit qualitative sameness, the nominalist can respond that ‘*a and b are of the same type*’ says merely that *a* and *b* are qualitatively the same, or qualitatively similar. The nominalist will say that we have been misled by the phrase ‘the same’. In fact, he will claim, such predications do not involve *identity*; the only ‘sameness’ here is similarity. It seems that, once again, the realist’s argument does not succeed. Realists urge that sameness of type is a matter of having the same (identical) type; the nominalist, for whom there are no types, will say that sameness of type is simply a matter of satisfying a relational predicate — being same-typed or qualitatively similar — and of course it cannot be assumed against the nominalist that predicates themselves are ontologically committing. If realism is to be established decisively, we need a better reason to believe it than current One over Many arguments provide.

## An Alternative Proposal

The mere fact that we can talk of the qualitative ‘sameness’ of objects does not oblige the nominalist to recognize that there is ‘something identical’ — some *entity* — shared between things that exhibit qualitative sameness. The nominalist has an alternative account: that this ‘sameness’ can adequately be captured by a two-place resemblance predicate that is true of pairs of qualitatively similar things. In response the realist might urge that resemblance between two things is *not* a dyadic relation, since an account of the resemblance between two things will not be complete until we specify the *respect* in which two things are the same. A natural way of speaking is that, when two things are qualitatively the same, they are so in respect of some feature, characteristic or quality that they share; indeed the ‘sameness’ between them will be a consequence of sharing this feature. If there is good reason to think that resemblance between objects must always be construed as resemblance *in some respect*, the realist can pose a genuine challenge to the nominalist: how does a nominalist account for the entities — shared features or respects of sameness — over which we quantify when explaining resemblance between objects? The *prima facie* situation is that these entities are universals.

Is there any good reason to believe that resemblance between objects is always resemblance *in some respect*? This realist claim is considered by Loux, who discusses

‘the alleged incompleteness of resemblance-claims... sentences of the form “*a* resembles *b*” are always incomplete... If we are to complete their sense we have to indicate the respect in which the resembling objects are alike’<sup>18</sup>

How should we understand the thesis that resemblance-claims are ‘incomplete’? Loux’s remarks suggest that this is to say that a resemblance-claim does not express a complete *sense* unless we specify some respect in which the resemblance holds. Such an interpretation might be opposed: it clearly is not the case that we have failed to express a ‘complete sense’, in the sense of failing to express a complete thought, by saying that ‘*a* resembles *b*’. Better, then, to understand ‘incompleteness’ as the claim that what is said by ‘*a* resembles *b*’ goes beyond what is suggested by the surface form of the language; that since (according the realist) resemblance is always resemblance *in some respect*, a complete account of what is said by a resemblance-claim must specify that it is resemblance *in some respect or other*. On this account, resemblance-claims are ‘incomplete’ because a *full* account of what is

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<sup>18</sup>Loux 1978, p. 47

said will mention a respect of sameness.<sup>19</sup>

Loux takes the view that this argument does not succeed. He claims that the realist treats dyadic resemblance-claims as incomplete because resemblance is a ‘transcendental relation’ (1978: 48), one which holds between any two objects in some respect. But if this is true it shows that

‘so far from being incomplete, sentences of the form ‘*a* resembles *b*’ ... would always seem to be both complete and true’.<sup>20</sup>

If it is true that resemblance is ‘transcendental’ in Loux’s sense, then there is some point to distinguishing ‘respects’ in which things are the same. In a world where everything resembles everything else, no useful information will be conveyed by a statement that two objects resemble each other unless it is possible to specify the *respect* in which the objects are similar. Nevertheless, the nominalist will respond that we don’t *have* to do things this way. Instead of preserving our ability to describe the world by means of a resemblance-predicate, by increasing its adicity to allow us to specify the *respect* in which things are the same, we may simply accept that resemblance-claims are uninformative, because they are trivially true. In such a situation we would not have lost the ability to describe the world. The nominalist will point out that all the descriptive work worth doing can be achieved with *predicates*; consequently there is no need to preserve the informativeness of resemblance-claims.

An alternative motivation for treating resemblance-claims as ‘incomplete’ is offered by Armstrong. If we accept that resemblance depends on shared features, we can explain certain formal features of resemblance. Resemblance is symmetrical, and comes in degrees: if *a* resembles *b* to a certain extent, then *b* resembles *a* to exactly that extent; perfect resemblance is transitive, whereas imperfect resemblance is not. These facts are explained when we analyse resemblance as the sharing of one or more features, degree of resemblance in terms of the number of features shared, and perfect resemblance as resemblance in every feature. If resemblance to a certain degree is the sharing of a certain number of features, then *a* resembles *b* to degree *n* iff *a* and *b* share *n* features, in which case *b* will resemble *a*, and to exactly the same degree, for *b* shares just as many features with *a* as *a* shares with *b*. Imperfect resemblance is non-transitive because *a* may resemble *b* by sharing feature *F*<sub>1</sub>, and *b* resemble *c* by sharing feature *F*<sub>2</sub>, without there being any feature shared between *a* and *c* such that *a* resembles *c* in turn. However, perfect resemblance is

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<sup>19</sup>The idea that resemblance-claims are incomplete is also found in Bambrough 1961, p.222; however, his reasons for making the claim are unclear.

<sup>20</sup>Loux 1978, p. 48

transitive, since where *a* and *b* share all their features, and *b* and *c* share all their features, *a* and *c* will also share all their features: there will be no feature such that *a* has it and *c* does not, and *vice versa*.<sup>21</sup>

These results suggest that a theory which treats resemblance as depending on ‘respects’ in which things are the same is preferable to one which takes resemblance as a primitive dyadic relation, for a theory of the former kind can explain features of resemblance that are otherwise puzzling. However, this establishes no more than that the realist theory of ‘respects’ of sameness is superior to a nominalist theory of dyadic resemblance in one way. To establish a compelling case for realism we would need to do more than that: we would need to provide a reason why the nominalist theory should not be adopted. In what follows I suggest that the nominalist theory of dyadic resemblance is unacceptable because it has no adequate account of how qualitative sameness interacts with qualitative difference: no satisfactory explanation of the connection between similarity and *dissimilarity*.

The most recent attempt to derive a problem for the nominalist from facts about sameness and difference is Rodriguez-Pereyra’s. He asserts that the One over Many is best understood as the problem of how it is possible for two different things in some sense to be the same, the question of

‘how can different things be of the same type?’<sup>22</sup>

Rodriguez-Pereyra asserts that this problem has the same form as other “how-possible” questions, for example the problem of how knowledge is possible, given that we might be brains in a vat, or the problem of how free will is possible, given that the future is completely determined by the past. In each case, some obstacle — in Nozick’s phrase, an ‘apparent excluder’ (Nozick 1981: 10) — apparently makes it impossible that things are as we take them to be. In the One over Many, the apparent excluder is ‘the numerical difference between the particulars’ (Rodriguez-Pereyra 2000: 257), and what it apparently makes impossible is that two numerically different particulars can be qualitatively the same, or exhibit ‘sameness of type’. Rodriguez-Pereyra’s thought seems to be this: if two particulars are identical in nature or ‘have some common property’ then they are in some sense *the same*. But if the particulars are two distinct entities, then they are obviously also *different*. Yet it seems impossible for two things both to be the same and to be different. So the One over Many is the problem of how it is possible for *a* and *b* to be the same, given that they are different.

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<sup>21</sup>These results are found in Armstrong 1989a, p.102 and 1992, p. 164; they have recently been reiterated by Hossack (2007, p.39).

<sup>22</sup>Rodriguez-Pereyra 2002, p.39

It is hard to see how this constitutes a genuine problem, save for someone who fails to recognize the distinction between numerical sameness and difference, on the one hand, and qualitative sameness and difference, on the other. Why should we see even an *apparent* impossibility in two things being (numerically) different and yet (qualitatively) the same? Suppose that ‘different’ is always equivalent to ‘not the same’. Then by substitution ‘*a* and *b* are the same and yet different’ would be equivalent to ‘*a* and *b* are the same and yet not the same’ — a contradiction. But once we distinguish different senses of ‘the same’ and ‘different’, we can say that ‘numerically the same’ is equivalent only to ‘not numerically different’ while ‘qualitatively the same’ is equivalent only to ‘not qualitatively different’. This enables us to preserve the appealing principle that it is an outright contradiction to say ‘*a* and *b* are numerically the same and yet numerically different’, while there is no contradiction in saying ‘*a* and *b* are qualitatively the same but numerically different’, since ‘qualitatively the same’ is not equivalent to ‘not numerically different’, as it would have to be for us to see a contradiction in two things’ being qualitatively the same while numerically different. This objection is raised by MacBride:

‘When we say that numerically different things are qualitatively the same, the respect in which we say that they are the same is not the respect in which we say they are different. Consequently no incompatibility is generated by saying that things may in such a manner be the same yet different.’<sup>23</sup>

Once we grant the distinction between qualitative and numerical sameness and difference, the numerical difference between *a* and *b* is not even an apparent excluder of their qualitative sameness; consequently there is no need for an explanation of how it is possible that such a situation can obtain.

Rodriguez-Pereyra’s how-possible question is not the only difficulty about sameness and difference that the realist could raise. Consider the following situation:

Two balls remain on the snooker table. Both are the same size and shape, but they are different colours: one is red and the other is black.

It is clear that these two balls are numerically different. Are they qualitatively the same, or are they qualitatively different? Here the natural thing to say is that they are qualitatively the same in respect of their shape and size – they are ‘the same shape’ – but they are qualitatively different in respect of their colour – they are ‘not the same colour’. Call these balls *a* and *b*. The situation is as follows:

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<sup>23</sup>MacBride 2002, p.31



$a$  and  $b$  are numerically different, and qualitatively the same, yet qualitatively different.

A distinction between numerical and qualitative sameness can explain how it is possible for two things to be numerically different and yet qualitatively the same. But this distinction between senses of ‘the same’ cannot explain how it is possible for two things to be qualitatively the same and yet qualitatively different. Here the two objects are both the same and different in exactly the same (qualitative) sense. Being ‘qualitatively the same’ apparently excludes being ‘qualitatively different’. Yet all the time we encounter pairs or  $n$ -tuples of things which are qualitatively the same and yet qualitatively different. How is this possible?

This question can be answered if we accept the realist’s thesis that dyadic predications of resemblance are ‘incomplete’ — that qualitative sameness and difference is always sameness and difference *in some respect*. For then we can understand ‘ $a$  and  $b$  are qualitatively the same’ as ‘ $a$  and  $b$  are the same in some respect’, or more formally,

(QS)  $(\exists x)$   $a$  and  $b$  are the same in respect of  $x$ .

This analysis explains how it is possible for two things to be qualitatively the same and yet qualitatively different: when  $a$  and  $b$  are qualitatively the same, they are the same in one respect, and when  $a$  and  $b$  are qualitatively different, they are different in some *other* respect. Resemblance is now understood as a ternary relation, with the third place reserved for the respect in which the resembling things are the same; since there is no contradiction in a ternary relation holding between  $a$ ,  $b$  and one thing, while failing to hold between  $a$ ,  $b$  and a *different* thing, there is no contradiction in saying that  $a$  and  $b$  resemble each other (in one respect), yet fail to resemble another (in a different respect).

The realist analysis should not stop there. He will urge that ‘respects of sameness’ are entities shared between similar things; in other words, they are *features* or *characteristics* whose sharing makes for sameness of type. Sameness of type will be sameness of type with respect to some or another shared feature, so the realist will say that the best account of the metaphysical situation underlying (QS) will be

$(\exists x)$   $x$  is a feature or characteristic, and  $a$  has  $x$  and  $b$  has  $x$ .

Similarly, qualitative difference will be explained as the failure of qualitative sameness with respect to some feature:

$(\exists x)$   $x$  is a feature or characteristic, and  $\neg (a$  has  $x$  and  $b$  has  $x)$ .

In other words, qualitative sameness is a matter of *a* and *b* sharing some common feature, while qualitative difference is a matter of *a* and *b* failing to share some common feature.

A complication for the realist's account is provided by cases in which there is some feature which neither object has. There are two problems here. One involves the analysis of 'duplicate': one of the reasons we might find respects of sameness and shared features helpful is that we can define duplicates as objects which are the same in *every* respect (Lewis 1983: 202; Hossack 2007: 36). If the current account is allowed to stand unmodified, a consequence will be that *genuine* duplicates are not counted as such, for they will *not* share *every* feature: two green duplicates will be the same *qua* the colour green, but will not be the same *qua* the colour red. The colour red will be counted as a respect in which they are not the same; but duplicates are supposed to be the same 'in every respect'. A second, related, problem, affects the realist claim that qualitative *difference* is no more than the failure of qualitative sameness in some respect. We would not want to count two green objects as 'qualitatively different in some respect' because they fail to be the same in respect of the colour red; how could two objects be counted as 'different' in respect of a colour, when they are the *same* colour — green?

Both of these problems can be solved with one adjustment to the realist theory: that we restrict the domain of quantification so that the quantifier variable in the analysis ranges only over those features that *at least one* of the things in question has. If our domain of quantification is restricted in this way, the following equivalences will hold: (i) to say that *a* and *b* are alike in *every* respect will be equivalent to saying that every feature which *a* or *b* has is a feature which *both* have; (ii) to say that *a* and *b* are alike in *no* respect will be equivalent to saying that *no* feature which *a* or *b* has is a feature which both have; (iii) to say that *a* and *b* are 'qualitatively similar' or 'alike in some respect' will be to say that there is *some* feature which *a* or *b* has which both have; (iv) to say that *a* and *b* are 'qualitatively different' or 'different in some respect' will be to say that there is some feature which *a* or *b* has which it is not the case that both have. If *a* has some colour that *b* lacks (or *vice versa*), then *a* and *b* are qualitatively different in respect of that colour; but *a* and *b* are not to be counted as 'qualitatively different' because they fail to be the same in respect of a feature which neither has.

The proposed restriction on the domain of quantification not only enables us to make sense of the way in which we actually *do* quantify over respects of sameness; it also enables the realist to explain how qualitative difference is simply the failure of qualitative sameness with respect to some feature or characteristic, without having to accept that *a* and *b* are 'qualitatively different' in virtue of failing to share some property which neither has. For

those reasons it seems to be the right response for a realist to adopt, and subsequent discussion of sameness and difference ‘in some respect’ should be understood with this restriction to the quantifier in place.

The realist, it seems, has a satisfactory explanation of how it is possible for  $a$  and  $b$  to be at once qualitatively the same and yet qualitatively different. However, our ‘how-possible’ question becomes a pressing problem for the kind of nominalist who refuses to recognize a shared entity where there is sameness of type and claims that everything we need to say about the resemblance between  $a$  and  $b$  can be captured using a dyadic resemblance-predicate, with no mention made of *respects* in which  $a$  and  $b$  are the same. It is reasonable to suppose that qualitative *difference* is just the failure of qualitative sameness in some way. If the nominalist is willing to say that qualitative sameness may be represented by a two-place resemblance-predicate ‘... $R$ ...’, then he might also find it natural to represent ‘qualitative difference’, or dissimilarity, as the failure of that predicate to hold between two things. But it is possible for two things to be at once qualitatively the same and qualitatively different — both similar and dissimilar. If qualitative difference is just the failure of qualitative sameness, then the situation when  $a$  and  $b$  are similar and at the same time dissimilar should be

$$aRb \ \& \ \neg aRb$$

But this would make it impossible for two things to be at once qualitatively the same and qualitatively different; to claim that they would be to contradict oneself. It seems that the nominalist must either revise his account of qualitative sameness and difference to show how they are not, after all, incompatible, or accept the realist proposal which represents qualitative sameness by including an argument-place for the *respect* in which things are the same. In the next section, I argue that the only satisfactory solution is the analysis proposed by the realist. We have met other benefits of recognizing respects of sameness, but so far have lacked a decisive consideration in favour of the realist’s proposal. The current problem, I suggest, provides such a decisive consideration, for it cannot be solved adequately unless we recognize that qualitative sameness and difference are always sameness and difference *in some respect*.

### **Can the Nominalist do without Respects of Sameness?**

I have suggested that the nominalist cannot get by with a theory in which qualitative sameness is treated as a dyadic predicate holding between objects, while qualitative difference is understood merely as the failure of qualitative sameness. Such a theory leads

to contradiction as soon as we encounter objects which are the same (in one respect) but different (in another respect). However, it would be over-hasty to conclude on that account that the nominalist *must* accept that dyadic resemblance-claims are ‘incomplete’, and *must* agree to take seriously the realist’s talk of ‘respects’ in which things are the same and ‘features’ which they share. In this section I distinguish five nominalist alternatives, each of which I claim is unsatisfactory. The conclusion is that the nominalist is unable to do without respects of sameness, for there is no acceptable alternative to an account, such as that proposed by the realist, which quantifies over respects in which things are the same, or features which they have in common.

**(i) Resemblance ‘comes in degrees’**

A nominalist might try to solve our problem by appealing to the fact that ‘resemblance comes in degrees’. He will say that there is no genuine problem about our objects *a* and *b*: to say that they are qualitatively the same and yet qualitatively different is to say no more than that they resemble each other less-than-perfectly. This suggestion does little to address our difficulty, for it has nothing to say about how we should understand claims of qualitative *difference*. If resemblance ‘comes in degrees’, then any two objects that are the same in some respect will resemble each other to some degree. But this does not explain what claim we make when we say that such objects are qualitatively *different* or *dissimilar*. The nominalist cannot explain dissimilarity as the failure of partial resemblance, for then we should have to understand ‘*a* and *b* are dissimilar’ as ‘It is not the case that *a* and *b* partially resemble each other’, and that is absurd: *a* and *b* may be dissimilar in one respect although they partially resemble each other in other respects. Accepting that resemblance may be ‘imperfect’ or ‘come in degrees’ is at best a partial solution, since we still lack an account of what it is for two things to be dissimilar or exhibit ‘difference of type’; without such an account our original problem remains unsolved.

**(ii) Sameness and difference are not genuine opposites.**

A second response is to deny that qualitative sameness and difference are genuine opposites. The nominalist’s theory is only threatened with contradiction if he accepts the *prima facie* appearance that qualitative difference is simply the failure of qualitative sameness. The apparent situation is that ‘qualitatively different’ is equivalent to ‘not qualitatively the same’, in which case it is a contradiction to say of *a* and *b* that they are at once qualitatively the same and yet qualitatively different. Couldn’t the nominalist simply reject this equivalence

claim? Suppose that ‘...are the same’ and ‘...are different’ are not genuine opposites after all. Then there is no reason to suppose that we could derive a contradiction from saying that two things are qualitatively the same and yet qualitatively different. The nominalist even has a story to tell about how he came to think, quite mistakenly, that qualitative sameness and difference were incompatible: he can claim that he was led astray by the fact that the words ‘same’ and ‘different’ are used to mark identity and nonidentity, where there is a genuine incompatibility — no two things can be *numerically* different and yet numerically the same. But once we understand that talk of qualitative sameness and difference is entirely distinct from talk of *identity*, there is no longer any reason to think that qualitative sameness and difference are incompatible, and so no reason to think that qualitative sameness apparently excludes qualitative difference. So there is no reason to treat predications of qualitative sameness as ‘incomplete’, as the realist urges.

I shall argue that two problems prevent the nominalist from making such a response. One is a difficulty we have met already: if ‘qualitatively different’ is *not* understood as equivalent to ‘not qualitatively the same’, what positive account of qualitative difference does the nominalist have? So far, none has been provided, and this contrasts unfavourably with the position of the realist, who not only offers an *account* of qualitative difference, but also offers an account in accordance with the feeling that qualitative sameness and difference are two sides of the same coin, for he will say that qualitative difference is nothing other than the failure of qualitative sameness with respect to one or another feature. Indeed, it might be claimed that anyone who denies that qualitative difference is in some sense simply the failure of qualitative sameness rejects an obvious or ‘Moorean’ fact which no philosopher should deny. At any rate, it would be hard to be satisfied with an account which denied such an apparently undeniable fact about qualitative difference without offering any suggestion whatsoever about how we should understand it instead.

A second problem with this nominalist response is that, if qualitative sameness and difference are not opposites, then it should never be the case that qualitative sameness excludes qualitative difference. But this is not so. Two cases might be suggested where sameness *does* exclude qualitative difference. First, when *a* and *b* are the same in respect of some property, it is impossible that *a* and *b* should also be different in respect of that property. If *a* and *b* are qualitatively the same in both being red, they cannot also be qualitatively different in this respect. Second, when *a* and *b* are the same in *every* respect, it is impossible for them to be qualitatively different. The realist has explanations for these phenomena: qualitative sameness is a relation between two things and the respect in which they are the same, and for two things to be the same and yet different in the same one respect

would be for the two things, *per impossibile*, to at once share and not share the very same feature. The reason why there could not be two objects that resemble in *every* respect and yet are qualitatively different is that, if two objects are the same in every respect, there is no respect in which they can be different. Unless the nominalist is prepared to countenance quantification over respects, or ways, of sameness and difference, he has no explanation of the difference between cases where qualitative sameness excludes qualitative difference and cases where it does not.

The nominalist can avoid contradiction by denying that qualitative difference is the failure of qualitative sameness; but this comes at a high cost, for his theory is opaque and mysterious at points where the realist offers a clear explanation: he cannot tell us what qualitative difference *is*, and he cannot explain why — if these are not genuine opposites — there might be cases where qualitative sameness excludes qualitative difference. For those reasons, it seems that his theory is not an acceptable alternative to the realist account which quantifies over respects of sameness.

### (iii) Multiple resemblance-predicates

A third possible nominalist account of qualitative sameness is discussed by Russell (1911). He considers the position of the nominalist who wants to say that two white patches in his visual field are ‘the same colour’. In order to avoid introducing a universal which both patches share, the nominalist is required to recognize a ‘relation’ (better, a ‘relational predicate’) of ‘likeness’ that holds between the two patches. But, Russell says,

‘it is not the general relation of likeness that we require, but a more special relation, that of colour-likeness, since two patches might be exactly alike in shape or size but different in colour.’<sup>24</sup>

The idea, then, is that the nominalist introduces an extremely large vocabulary of *different* resemblance-predicates, one for each ‘respect’ of sameness that the realist claims he should recognize. By augmenting his ‘ideology’ in this way, the nominalist evades the need to increase his ontology to include entities which play the role of respects of sameness, because he can explain the facts about compatibility and incompatibility that are at issue. Qualitative sameness is compatible with qualitative difference because *a* and *b* can satisfy *one* resemblance-predicate (if *a* shape-resembles *b*), and at the same time fail to satisfy *another* resemblance-predicate (if *a* does not colour-resemble *b*). The impossibility of *a*

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<sup>24</sup>Russell 1911, p. 111

and *b* resembling and at the same time not resembling each other in the *same* respect is explained as the impossibility of *a* and *b* at once satisfying, and not satisfying, the same specific resemblance-predicate, for example if *a* and *b* were said at once to colour-resemble and not to colour-resemble one another.

Russell objects to the introduction of many different resemblance-relations because, he claims, it leads to a regress.

‘Now, *prima facie*, this relation of colour-likeness will itself be a universal... But we may apply the same analysis to colour-likeness. We may take a standard particular case of colour-likeness, and say that anything else is to be called a colour likeness if it is exactly like our standard case. It is obvious, however, that such a process leads to an endless regress: we explain the likeness of two terms as consisting in the likeness which their likeness bears to the likeness of two other terms, and such a regress is plainly vicious.’<sup>25</sup>

Russell went on to advance a similar regress against the nominalist in *The Problems of Philosophy*; a standard criticism of his presentation is that it begs the question against the nominalist by assuming the right to talk of entities — ‘likenesses’ or ‘resemblances’ — which may in turn be said to resemble each other. In fact the nominalist sees no reason to accept that any extra entity is introduced by the use of a relational predicate. If there are no entities such as ‘cases’ or ‘instances’ of likeness, there are no further resemblances of which the nominalist owes us an account.

It seems that Russell’s regress does not succeed; however, it does suggest a difficulty for the current nominalist proposal. Russell uses the regress to try to show that the nominalist has a problem accounting for the sameness of type between different situations in which two things resemble each other; I shall suggest that there is a genuine problem of that kind, for the nominalist lacks a *general* account of qualitative sameness and difference. The current proposal deals with the problem about qualitative sameness and difference by accepting that resemblance-claims *are* incomplete in a sense; however, what they need completing with is not, as the realist supposes, mention of a *respect* of sameness, but rather a specification of *which* resemblance-predicate is intended — whether colour-resemblance, shape-resemblance, or any other specific resemblance-predicate.

The nominalist produces one resemblance-predicate for objects which resemble in respect of their colour, another for objects which resemble in respect of their shape, another for objects which resemble in respect of their size, and so on for every way in which two

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<sup>25</sup>Russell 1911, pp.111-2

things might resemble one another. But this leaves us with no account of what is *common* between all the disparate situations in which one thing resembles another. All the pairs of resembling objects exhibit a similarity in point of being cases of *resemblance*; but the nominalist has no *general* account of resemblance. According to him, there are only the multitude of specific resemblance predicates: ‘...colour-resembles...’, ‘...shape-resembles...’, ‘...size-resembles...’ and so on. For two objects to resemble one another is merely for them to satisfy one or another of these specific resemblance-predicates. Why then should we recognize each of these predicates as marking the same *kind* of relationship between the objects they apply to?

The nominalist might respond that the sameness of type between the disparate resemblance-predicates is accounted for as soon as we introduce a further resemblance-predicate, ‘...resembles in respect of being a resemblance-predicate...’, for this will hold between all the resemblance-predicates and account for their sameness of type in nominalistically-acceptable fashion; however, this gives an account only of the sameness of type between the *predicates*, and what the current objection demands is an account of the sameness of type between the *objects* the resemblance-predicates apply to. There is a similarity between every pair of resembling things, no matter in what respect they resemble, but the current nominalist proposal can say nothing in general terms about what this similarity is, because it has no resemblance-predicate ‘...R...’ such that  $xRy$  for all cases in which  $x$  resembles  $y$  in some respect. By abandoning a unified resemblance-predicate in favour of many *different* resemblance predicates, it seems that the nominalist will lack the ability to say what is similar between how  $a$  stands to  $b$  and how  $a$  stands to  $c$ , where  $c$  is a thing that  $a$  resembles in a different respect. This, I suggest, is enough to make a theory of multitudinous *different* resemblance-relations untenable for the nominalist.

**(iv) Sameness of type is sameness of predicate**

An alternative nominalist response is suggested by Devitt’s account of ‘sharing a property’ in terms of shared predicates (1980: 95): to understand ‘ $a$  and  $b$  are qualitatively the same’ as true iff there is some true instance of

$$Fa \ \& \ Fb$$

and to understand ‘ $a$  and  $b$  are qualitatively different’ as true iff there is some true instance of

$$( Fa \ \& \ \neg Fb ) \vee ( \neg Fa \ \& \ Fb )$$



This would satisfy our feeling that qualitative sameness and difference should in some way be interdefinable: the nominalist can say that qualitative sameness is merely the sharing of a common predicate, while qualitative difference is failing to share a predicate which is true of one of the particulars in question. Since  $a$  and  $b$  can be the same in virtue of both being  $F$ , but different in that  $a$  is  $G$  and  $b$  isn't, there is no contradiction in saying that  $a$  and  $b$  are both qualitatively the same and yet qualitatively different. The rule that no two things could be qualitatively the same in some respect and yet qualitatively different in that respect would then follow from the truth of logic that nothing could be both  $F$  and not  $F$ , since for two things to be same and yet different in some particular respect there would be some predicate  $F$  which was true of both of them (in order for them to be qualitatively the same) and yet true of one but not the other of them (in order for them to be qualitatively different).

Several problems immediately threaten this position. One is that a statement of the truth-conditions proposed by the nominalist seems to require higher-order quantification, which may turn out to bring ontological commitment to properties in its wake. The nominalist will claim that ' $a$  and  $b$  are qualitatively the same' is true iff, for some  $F$ ,  $a$  is  $F$  and  $b$  is  $F$ . But then it looks like the claim is that the statement that  $a$  and  $b$  are qualitatively the same must be analysed as

$$(\exists F) Fa \text{ and } Fb.$$

Quantification into predicate-position is at the very least *apparently* ontologically committing; thus by accepting the analysis of sameness of type in terms of shared predicates the nominalist merely replaces the challenge posed by first-order quantification over types with the challenge posed by second-order quantification over whatever it is that predicates refer to.<sup>26</sup> To avoid this objection, the nominalist might claim that ' $Fa$  and  $Fb$ ' is schematic. The predicate-letter  $F$  is not a variable which must be bound by a quantifier; instead the nominalist has given a *schema*, every instance of which is a case in which the two things mentioned may be said to be qualitatively the same. But this too should make us suspicious, since it makes the nominalist vulnerable to the Argument from Identity. A nominalist who says that ' $Fa$  and  $Fb$ ' is a *schema* which has instances will have to say things like 'sentences  $S_1$  and  $S_2$  are instances of the same *schema*'. But if one and the same *schema* has different sentences as instances, it sounds like a *schema* is a kind of *type*, and types were precisely what the nominalist was hoping to do without. This argument is parallel to Armstrong's objection to the notion of 'same predicate' in Devitt's original proposal. In

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<sup>26</sup>See Chapter 4 for more on the ontic commitment associated with quantification into predicate-position.

Armstrong's response, just as in the current debate, it is pointed out that the nominalist 'gives a reductive analysis of types only at the cost of introducing further types' (1978a: 19), in this case linguistic *schemata* which have instances.

A further problem arises when we try to understand qualitative sameness and difference in terms of shared predicates. For if the *schemata* proposed by the nominalist are supposed to hold whatever predicate is substituted for 'F...' then they are easily falsified. Let *F* stand for the predicate 'is entirely unlike any other existing thing'. If the nominalist's proposal is taken seriously, it commits him to accepting that

(U) *a* is entirely unlike every other existing thing, and *b* is entirely unlike every other existing thing

is an instance of the *schema*

*Fa* and *Fb*

in which case it should be licit to infer from the truth of (U) that *a* and *b* are qualitatively the same. But when (U) is true, *a* and *b* are each entirely unlike every other existing thing, and so are entirely unlike each other; therefore it cannot be the case that *a* and *b* are qualitatively the same. A similar problem arises with disjunctive predicates, since a disjunctive predicate like '...is spherical or is a philosopher' can be true of two particulars without those two particulars exhibiting any qualitative sameness at all. The nominalist needs to provide some principle distinguishing which predicates make for sameness of type and which do not; but this cannot be done by distinguishing structural features of the predicates themselves, for predicates which consist of structurally simple arrangements of words may fail to make for sameness of type just as easily as structurally complex ones.<sup>27</sup> Without some means of distinguishing which predicates make for sameness of type, the attempt to produce a nominalist analysis in terms of shared predicates seems doomed to failure.

#### **(v) Resemblance to pluralities**

A final response to the problem of sameness and difference uses the machinery of Resemblance Nominalism. It might be claimed that *a* and *b* exhibiting sameness of type in respect of *F-ness* does not exclude *a* and *b* exhibiting difference of type in respect of *G-ness* because 'sameness of type in a respect' is to be construed as resemblance to the

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<sup>27</sup>The most famous example of this is of course Goodman's colour-adjective 'grue'.

plurality of *F* objects, or to the plurality of *G* objects. Here, it seems, we do not need to recognize an entity which is the ‘one’ over ‘many’ when multiple things exhibit sameness of type; what the Resemblance Nominalist will say is that, when *a* and *b* are same-typed, this is a case of *a* and *b* resembling some suitable plurality of objects. Since there are many different pluralities, there is no reason to think sameness of type excludes difference of type, as the difference of type between *a* and *b* will be difference with respect to a *different* plurality. Just as the realist resolves the problem by turning sameness of type *qua F-ness* into a relation between the objects in question and the property *F-ness*, the resemblance nominalist resolves the problem by turning sameness of type *qua F-ness* into a relation between the objects in question and the plurality of *F* objects in the world.

I suggest that this Resemblance Nominalist response fails because it is circular. The challenge which the realist poses is not merely to account for facts of sameness of type when this is understood narrowly as two things being tokens of one and the same proper type, for example when two books are tokens of the same novel; instead, the problem we face affects any kind of genuine resemblance between objects, for *a* and *b* resembling each other in one respect does not exclude *a* and *b* failing to resemble each other in another respect. The problem is to provide an analysis of resemblance which explains how this can be so. But the Resemblance Nominalist, in explaining what it is for *a* and *b* to resemble each other in some respect, appeals to the very notion of resemblance that he was challenged to analyse. *a* and *b*’s ability both to resemble and not to resemble each other cannot be explained by appealing to the resemblance between *a* and *b* and some further paradigm object(s), for then the question remains, what explains the fact that resembling these paradigm objects (in some respect) is compatible with *not* resembling them (in some other respect)? The Resemblance Nominalist takes the predicate ‘...resembles...’ as a primitive term in his theory; yet taking resemblance as a primitive predicate that holds only between the resembling objects themselves is precisely what generated a problem in the first place. If resemblance is a simple relation between resembling objects themselves, then we have done nothing to answer the original question, how is it possible for this relation to both hold and yet not hold between the same collection of things?

I have considered five different nominalist attempts to do without quantification over shared features, or respects of sameness, when explaining qualitative sameness and difference. Each of these, I suggest, is unacceptable. The only responses to our problem about sameness and difference that are likely to succeed are those which recognize sameness of type as always sameness *in some respect* — in other words, those responses which concede the right to talk of a ‘One’ over the ‘Many’ where there is genuine sameness of type.

## Conclusion

Versions of the One over Many criticized in this chapter suffer from one of two flaws: most commonly, the problem is that there is no reason why the nominalist should accept the truth of the ‘facts’ proposed by the realist, whether those ‘facts’ involve the sharing of a common entity, discourse about properties, ‘having something in common’ or identity between ‘types’. In the case of the Argument from Qualitative Sameness, there is a different problem. Here there is no reason why a nominalist must accept the proposed *analysis* or *account* of the facts of qualitative sameness — no reason to agree that facts about qualitative sameness are to be understood as involving ‘something identical’ shared between similar objects. The nominalist can respond that there is not even a *prima facie* appearance that ‘qualitative sameness’ is to be understood in terms of identity.

I suggest that the current proposal suffers from neither of these flaws. It is not threatened by a nominalist error theory, since no theorist should doubt that statements about qualitative sameness can be true; to do so would be to deny that things are capable of being similar to, and different from, each other. Nor is it vulnerable to the charge that the analysis it proposes is unmotivated. The reason why we should treat resemblance-claims as ‘incomplete’ — as always involving some *respect* in which the resembling objects are the same — is that otherwise we have no explanation of how it is possible for two things to be at once qualitatively the same and qualitatively different. If resemblance-claims are *not* understood as involving quantification over respects in which things are the same, our discourse about qualitative sameness and difference is in danger of lapsing into contradiction, for if resemblance is a dyadic predicate it must be said to hold, and at the same time not hold, between the same two things.

I have already suggested that the need to recognize quantification over respects of sameness, and features shared between similar objects, provides only a *prima facie* case for realism, for the *prima facie* situation is that this is quantification *over* a domain of existing entities, and these entities are *universals*. If it is desirable to think of the current argument as setting up a ‘Problem of Universals’, the Problem for the nominalist will be to explain how such quantifications can be true, without being quantifications over a domain of universals. The committed nominalist will want to consider two responses. One is to propose alternative semantics for quantifiers, so that quantification ‘over’ respects of sameness can express truth even if such entities do not exist. An alternative approach will be to accept the commitment to shared features, but propose some nominalistically-acceptable account of what these entities are — for example, saying that they are sets of particulars or of

exactly-resembling tropes. I address these nominalist responses in the following two chapters. The conclusion of this chapter is merely our right to a premise which ancient realists apparently took for granted: that when two things are qualitatively similar, there is something which is the *respect* in which they are similar and the *feature* which they share in common.

I have claimed that the problem of sameness and difference I propose has been overlooked by modern realists. Nevertheless, it has a venerable precursor. Zeno of Elea, famous for his paradoxes of motion, was a monist who thought that the hypothesis of plurality led to insurmountable contradictions. One of his arguments is preserved in Plato's *Parmenides*:

‘If reality is many things, it is necessary for them to be both similar and dissimilar, and this is impossible. For what is similar cannot be dissimilar, and what is dissimilar cannot be similar’<sup>28</sup>

The Greek words for ‘similar’ and ‘dissimilar’ — ὁμοιος and ἀνομοιος<sup>29</sup> — are hardly ever used to mark identity and nonidentity, so it seems that Zeno's problem concerns *qualitative* sameness and difference — the very problem which presents a genuine challenge to the nominalist. Plato is notorious for failing to give an explicit formulation of his ‘One over Many’ argument. It is clear that he was aware of the current problem of sameness and difference; we can only speculate whether he realized that universals provide a solution to it.

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<sup>28</sup>Plato, *Parmenides* 127e1-4. My translation.

<sup>29</sup>‘*homoios*’; ‘*anomoios*’.

## 2. Ontological Evasions

I have argued that statements about qualitative sameness and difference are adequately understood only when sameness and difference are treated as always sameness or difference *in some respect*. Thus it seems reasonable to treat statements of qualitative sameness as involving tacit existential quantification over *respects* in which similar things are the same. If we have an appropriate notion of logical form, we might say that the logical form of such statements involves quantification over respects of sameness which is not apparent from the ‘surface’ or ‘grammatical’ form: while the surface form of a statement like ‘*a* and *b* are qualitatively the same’ employs a dyadic predicate whose terms are *a* and *b*, it seems that the logical form of the statement would be better represented as a relation between *a* and *b* and the *respect* in which they are the same, thus:

(QS)  $(\exists x)$  *a* and *b* are qualitatively the same in respect of *x*.

As I suggested in the previous chapter, a plausible realist account will decompose the triadic predicate ‘... and ... are the same in respect of ...’ yet further, so that ‘being qualitatively the same’ in respect of some feature or characteristic will be treated as a matter of both objects *having* or *exemplifying* the feature in question. However, such a formulation is vulnerable to the nominalist objection that it prejudices a decision about the nature of the relation between similar objects and the entity (whatever it is) in respect of which they are the same; for that reason it would be better to restrict ourselves to the assertion that *any* account of the logical form of statements of qualitative sameness must leave a place for the respect in which similar things are the same.

Such a claim is not obviously unreasonable: it proposes no greater divergence between surface and logical form than is required by Davidson’s theory of events, or Russell’s theory of descriptions (Davidson 1967; Russell 1905). While adding complexity in logical structure it simplifies matters in another way, for it allows several apparently divergent English figures of speech to be represented by the same predicate at the level of logical form, for (QS) can be used to represent ‘*a* and *b* have something in common’, and a slight modification enables us to represent idioms such as ‘*a* and *b* are the same colour’, for we could understand this as

$(\exists x)$  *a* and *b* are qualitatively the same in respect of *x* & *x* is a colour.

Moreover, since we cannot deny the truth of some instances of the implicitly quantificational statement ‘*a* and *b* are qualitatively the same’, it is no longer desirable to attribute

massive error to everyday discourse which involves idioms of quantification over shared entities — for example when we say that *a* and *b* ‘have something in common’, or that they ‘share some common feature’. Since the legitimacy of such quantification is no longer in doubt, we may take these statements at face value, as true quantifications over entities shared in common between similar objects.

It is tempting to claim that we have established an ontological conclusion: since some statements of qualitative sameness and difference are true, and since such statements involve us in quantification over respects of sameness and features shared in common, there exist entities which are respects of sameness and shared features. The only remaining question would be whether the nominalist is forced to concede that these ‘respects of sameness’ are universals, as the realist claims, or whether they can be identified with another kind of entity, e.g. sets of particulars or sets of tropes. However, the realist’s argument may be challenged from another direction. The nominalist might accept the proposed analysis, but refuse to acknowledge that the quantification it incorporates is ontologically significant, i.e. he might deny that any ontological consequences follow from the truth of such quantifications, and so deny that any addition to his ontology is required. In the terminology introduced by Quine (1960: 238) the nominalist denies that the quantification in question is ‘ontologically committing’. This chapter assesses the plausibility of such a nominalist response.

In denying that this existential quantification is ontologically committing, the nominalist rejects a position which I shall call ‘objectualism’: this is the view that a bound variable of quantification ranges over a domain of existing entities, and the truth of a quantified sentence depends on whether the predicate governing this variable is true of entities in the domain.<sup>30</sup> An existentially quantified sentence will be true iff the predicate is true of at least one entity in the domain; a universally quantified sentence is counted as true in the absence of counter-examples, i.e. iff there is no entity in the domain to which the predicate fails to apply.

Against this view the nominalist may provide one or another ‘non-objectualist’ account. One such is ‘substitutionalism’, which treats variables of quantification as ranging over *expressions* rather than entities, and the truth of a quantification as depending on the truth of ‘substitution-instances’ in which the variable is replaced by an expression in the language; alternatively the nominalist might adopt the view of Prior (1971: 36) that a given

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<sup>30</sup> For the purposes of this discussion, I shall take ‘object’ and ‘entity’ to be equivalent terms: anything and everything that exists is an entity and also an object. A distinction between two different kinds of thing, ‘concepts’ and ‘objects’, will not be needed until the discussion of Frege in Chapter 4.

quantification might be true either in virtue of a true ‘substitution-instance’ or in virtue of the predicate being satisfied by a nameless entity. Other options include the account proposed by Russell (1905), that a quantification ‘ $(\exists x)x \text{ is } F$ ’ should be understood in terms of a propositional function ‘ $x \text{ is } F$ ’ being ‘sometimes true’, and the ‘Neutralist’ position that no formal account of quantifier truth-conditions is necessary or possible.

My discussion falls into four parts. First, it is necessary to arrive at a satisfactory account of the aim and nature of ontic commitment itself, since it is not clear that everyone understands the notion in the same way. Here I distinguish two radically different conceptions of ontic commitment, and argue that only one of them is of great importance to debates about ontology. Second, I distinguish two different kinds of Quinean ‘paraphrase’ that might be employed to evade ontic commitment. Once we understand this distinction it becomes apparent that the nominalist cannot use paraphrase to evade the ontic commitment involved in quantification over respects of sameness and shared features. Third, I respond to arguments in favour of non-objectualism. Finally, I present a critical discussion of rival non-objectualist accounts. Here it is claimed that the varieties of non-objectualism share three flaws in common: one is that they relieve the first-order quantifier of ontic commitment only if supplemented with a Positive Free Logic according to which atomic sentences containing non-referring singular terms can express truth; another that they do not succeed in freeing us from commitment to entities — types — which are *prima facie* to be construed as universals; the third is that they are extensionally inadequate, incorrectly assigning truth or falsity to statements whose truth-value is not in doubt.

I should mention three limits on the scope of the discussion. First, I shall proceed on the provisional assumption that the ontological question is the question of what *exists*: that there is no lower grade of ‘being’ or ‘subsistence’ which we might attribute to universals while denying that they exist. The opposing view, that our ontology might include things that do not exist, will only become relevant when I discuss the claims made by the Neutralist. Second, since the quantification involved in the realist’s analysis of sameness and difference is first-order existential quantification, or ‘quantification into subject position’, my main concern will be the ontic<sup>31</sup> commitment of the first-order quantifier. Consideration of the ontic significance of quantification onto predicate- and adjective- position will be postponed until the discussion of predicate-reference in Chapter 4.

Third, I shall consider arguments for and against non-objectualism only in the specific case of quantification over shared features and respects of sameness. This is not merely

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<sup>31</sup>See Quine 1960, p.120 for his preference for ‘ontic’ rather than ‘ontological’ commitment. The variation is merely stylistic.



because it would be a huge task to assess the plausibility of non-objectualist accounts with regard to *every* use of the quantifier. Anything said against non-objectualism in the specific case of quantification over respects of sameness will count against the general hypothesis that *all* quantification is non-objectual; moreover, the non-objectualist proposal is often presented as an invitation to use non-objectual quantifiers in understanding one or another particular area of discourse, rather than as a thesis about the nature of all quantification (Kripke 1976: 377; Haack 1978: 42). In that case, our question will be whether non-objectualism is plausible for quantifications of the kind to which the realist appeals. Even if were established that non-objectual quantifiers are needed to understand *some* fragments of English discourse, it would remain an open question whether they should be employed to understand cases where we say that two objects 'have something in common' or are the same 'in some respect', where they are needed if the nominalist is to dismiss the appearance of ontological commitment to universals.

## ‘Implicit’ and ‘Explicit’ Commitment

The realist alleges, and a certain kind of nominalist denies, that discourse about qualitative sameness is ontologically committed to entities which are the *respects* in which similar things are the same, or the *features* that they share in common. Without a working account of ‘ontological commitment’, the suspicion will remain that participants in the debate are talking past one another. In this section I distinguish two rival accounts of ontic commitment, and argue that only one of them is relevant to the matter at hand.

Quine’s favoured account of ontic commitment is that it is a matter of ‘what a theory says there is’ (Quine 1969a: 93; 1980: 15). This can be understood by analogy with ‘what a person says there is’. We might find out ‘what a person says there is’ by rounding up the totality of existential claims made by that person, whether unprompted or in response to questioning. If we pursue the analogy, we could discern ‘what a theory says there is’ by similar means: by assembling all of the existential claims made within the theory; more precisely (since inert theories cannot make existence-claims) by rounding up all the sentences within the theory which would serve to make existence-claims when uttered by someone asserting the theory. The entities we claim to exist when we utter those sentences would then constitute the ontological commitments of the theory.

Ontological commitments of that kind might be described as ‘explicit commitments’, since such a commitment is only incurred by means of some explicitly existential claim incorporated in the theory. Such an account of ontic commitment may seem unsatisfactory, for it isolates only those entities that a theory explicitly claims to exist, and this may not be an accurate measure of the ontology that is *required* for the truth of the theory. For that reason, we may introduce a second notion, that of ‘implicit commitment’. The question of a theory’s *implicit* commitments is the question of what entities or kinds of entity are *required* for the truth of the theory; of how much a theory *costs* us in the coin of the ontology that has to be there if the theory is true. We might describe the implicit commitments of a theory as its ‘ontic preconditions’: they are what there has to be, given that the theory is true.<sup>32</sup> In what follows I argue that discussion of ontic commitment

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<sup>32</sup>This distinction is found in Hodes (1990), who uses the terms ‘thin’ and ‘thick’ ontological commitment. I disagree with Hodes when he claims that we can incur ‘thin’ (explicit) ontological commitment without thereby acquiring ‘thick’ (implicit) commitment as well. Similar distinctions within the notion of ontic commitment are found already in Chihara (1968); however he does not recognize that this diversity is the result of any deep disagreement about what we want a criterion of commitment *for*, merely commenting that the notion of ontic commitment is so imprecise as to be unfit for purpose. The terminology of ‘implicit’ and ‘explicit’ commitments is also found in Michael (2008), although it is not clear that we understand the distinction in the same way: Michael restricts a theory’s ‘implicit commitments’ to the logical consequences

has been hindered by a lack of clarity about the distinction between explicit and implicit commitment; then I substantiate my claim that *implicit* commitment is the notion of interest when we investigate ontological questions.

Although the purpose of the current discussion is not Quinean exegesis, we should notice that both of our two notions of ontic commitment can be read into Quine. His widespread use of the phrase ‘what a theory says there is’ suggests that to investigate commitment is to enquire into existential claims made within the theory — i.e., to investigate the theory’s *explicit* commitments:

‘We look to bound variables in connection with ontology not in order to know what there is, but in order to know what a given remark or doctrine... says there is.’<sup>33</sup>

‘we have moved now to the question of checking not on existence, but on imputations of existence: on what a theory says exists.’<sup>34</sup>

Elsewhere Quine’s presentation makes sense only on the supposition that what is at issue is *implicit* rather than *explicit* commitment. He introduces the notion of ontological commitment to clarify a dispute between himself and the imaginary philosophy ‘McX’ over statements like ‘Pegasus does not exist’. Here the accusation is that ‘the denial of Pegasus cannot be coherently maintained’ (Quine 1980: 2); that in denying the existence of Pegasus, Quine ‘presupposes’ (1980: 9) the existence of Pegasus; that if Pegasus did not exist, the sentence ‘Pegasus does not exist’ would be ‘nonsense’ (1980: 2).

McX’s challenge to Quine cannot be understood as a claim about *explicit* commitment, for ‘Pegasus does not exist’ would be explicitly committed to Pegasus only if the sentence (paradoxically) served to *say that* Pegasus exists. Instead, the challenge to Quine rests on a background semantic assumption, the assumption that no sentence can be meaningful which contains a non-referring singular term (Moseley 1983: 198). The claim is that Pegasus is a precondition of the meaningfulness of ‘Pegasus does not exist’; if a sentence must be meaningful to express truth, Pegasus may then be counted a precondition of the truth of that sentence. The allegation, then, is that Pegasus is an *implicit* commitment of ‘Pegasus does not exist’. To understand the debate between Quine and McX, it is necessary to treat it as a disagreement about whether Pegasus is an *implicit* commitment, or ontic precondition, of ‘Pegasus does not exist’. One way to deny McX’s allegation would be to construe the

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of statements of the theory, and on his preferred way of understanding ‘logical consequence’ the logical consequences of a theory fail to encompass everything that there must be, given that the theory is true.

<sup>33</sup>Quine 1980, p.15

<sup>34</sup>Quine 1969a, p.93

logical form of the sentence such that the name ‘Pegasus’ gives way to a bound variable of existential quantification (Russell 1918: 250; Quine 1980: 6, 1986: 25); another would be to reject the background semantic assumption that meaningful sentences cannot contain non-referring singular terms (Moseley 1983: 200; Sainsbury 2005).

Commentators have disagreed about how best to elucidate the claim that a theory is ontologically committed to an entity or kind of entity; we cannot understand their disagreements without employing the distinction between implicit and explicit commitment. Scheffler and Chomsky (1958: 82) denounce Quine’s procedure for discerning ontological commitment as purposeless: whereas Quine recommends that we ascribe ontological commitment to a theory on the basis of the entities needed as values of variables of first-order quantification when the theory has been translated into formal ‘canonical notation’, they suggest that the same result could be achieved more simply, by collecting the theory’s existential statements ‘in all their original, untranslated variety’ (1958: 81). Here it is clear that they are thinking of ontological commitment as *explicit* commitment, as seeking to do no more than isolate the existential claims or assertions incorporated in the theory. Indeed, they go so far as to ask rhetorically, ‘how far is the description of what a *theory* says from what a *man* says?’ (1958:79).

Parsons (1970) assesses various formal accounts of ontological commitment relative to an ‘intuitive meaning’ of the term which is

‘given roughly by “what a person or theory says there is”’.<sup>35</sup>

He recognizes it as a problem with his preferred formal account that it is in tension with this ‘intuitive meaning’, insofar as it is a consequence of his preferred account that the sentence ‘There was an author of the *Cratylus*’ is ontologically committed to the class of Athenian males. The reason this is problematic, given Parson’s ‘intuitive meaning’ of ontological commitment, is presumably that anyone uttering such a sentence does not count as *saying that* there was a class of Athenian males; but of course such considerations are relevant to assessing an account of ontological commitment only if we are thinking of ontological commitment as *explicit* commitment.

To make sense of accounts such as Scheffler and Chomsky’s, and Parsons’, we need to recognize that they are discussing *explicit* commitment. Other accounts attempt to give an account of *implicit* commitment; rather than seeing these accounts as opposed to the first kind of explanation, we should understand that they are concerned with a different

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<sup>35</sup>Parsons 1970, p.70

notion, namely the notion of an ‘ontic precondition’ for the truth of a theory. One such account is Devitt’s:

‘The key idea is that a person is committed to the existence of those things that must exist for the sentences he accepts to be true.’<sup>36</sup>

This account is endorsed by Oliver (1996: 60), who translates it from the material to the formal mode: sentence  $S$  is ontologically committed to  $\phi$ s iff  $S$  entails some explicitly existential sentence such as ‘There are  $\phi$ s’ or ‘ $\phi$ s exist’. The connection with the modal notion of things that *must* exist in order for the sentence to be true is maintained by means of the stipulation that entailment is to be interpreted in terms of necessity: sentence  $S$  entails ‘There are  $\phi$ s’ iff it is not possible for the former to be true and the latter false. A similar account is found in Jackson (1989: 192): A theory  $T$  is ontologically committed to  $\phi$ s iff  $T$  entails that there are (exist)  $\phi$ s. It is Jackson’s account of ontological commitment that I shall adopt in subsequent discussion.

Once we recognize the distinction between explicit and implicit commitment, we can establish that implicit commitment is the proper concern for an ontologist. The reason is simply that there might be cases in which a theory has more implicit commitments than explicit commitments. In such a situation, the ontologist who paid attention only to explicit commitments would end up with a smaller ontology than he *ought* to have, and in the limiting case where the theory is true, a smaller ontology than there actually is. To show that a theory’s implicit commitments may outstrip its explicit commitments, we need only provide cases where a theory fails to incorporate any sentence used to make an existential claim about  $\phi$ s but where the theory cannot be true *unless*  $\phi$ s exist. In such cases the  $\phi$ s will be reckoned among the implicit commitments of the theory (for they are among the ontic preconditions of its truth), yet should not be counted among the explicit commitments of the theory (for the theory does not explicitly ‘say that’ they exist). Once we realize that what there has to be in order for a sentence to be true depends partly on semantic rules about the referential function of different kinds of words, such cases are not hard to generate.

My examples are as follows: first, the case in which the theory fails to contain any sentences which make ontological claims about  $\phi$ s but does say of some named objects that they are  $\phi$ s. On some not unreasonable views of the semantics of the situation,<sup>37</sup> the theory cannot be true *unless* there are  $\phi$ s, yet the theory does not contain any sentence like

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<sup>36</sup>Devitt 1980, p.435

<sup>37</sup>Specifically, that sentences containing names are not existential quantifications in disguise, and that there can be no truth expressed by a sentence of the form ‘ $a$  is a  $\phi$ ’ where the name  $a$  fails to refer.

‘There are  $\phi$ s’ which could count as an ontological claim about  $\phi$ s. So  $\phi$ s are among the implicit, but not the explicit, commitments of the theory. Second is the case in which every meaningful predicate refers to a universal. Here every subject-predicate sentence bears implicit commitment to universals, since no such sentence could even be meaningful, let alone true, unless there existed a universal to which the predicate referred. In this situation *every* theory is implicitly committed to universals, even if no theory explicitly states that universals exist.

The third example is provided by Davidson’s theory of action sentences. He has urged that such sentences should be understood as involving tacit existential quantification over events.<sup>38</sup> The analysis is justified by its explanation of how action sentences modified by adverbs or adverbial phrases, e.g. ‘Jones buttered the toast slowly at midnight’, entail their unmodified counterparts — ‘Jones buttered the toast’. Once the adverb is understood as describing the *event* over which we quantify, no mystery remains as to how the unmodified sentence is entailed by the adverbially modified one. If Davidson is right — and if the quantifier is ‘objectual’, requiring the existence of an entity meeting the associated condition — then any theory containing an action sentence like ‘Jones buttered the toast’ is implicitly committed to events, whether or not the adherents of the theory believe in events, or are even aware of Davidson’s theory. But we should not want to say that our description of Jones is *explicitly* committed to events, for it stretches credulity to say that the sentence ‘Jones buttered the toast’ is ever used to make an existential claim regarding events; to do that we would have to use sentences like ‘Events exist’ or ‘There are events’.

It seems that *implicit* commitment — the question of a theory’s ontic preconditions — is the topic of interest to an ontologist, whether he is involved in the current task, of determining whether some already-accepted theory or statement requires that we expand our ontology to include entities of a certain kind, or whether the issue at hand is deciding between rival theories, on the basis of which is committed to more entities or kinds of entity. In each case, the ontologist needs to know the *actual ontological cost* of the theory in question, and there is no guarantee that investigation of a theory’s explicit commitments alone will provide the right answer. A theory may incur an ontological cost even though it does not wear that cost on its sleeve in the form of an explicit existence-claim. It seems that questions about ontological commitment are to be settled by investigating implicit commitment.

This conclusion may be challenged. Call a theory ‘complete’ if its explicit commitments are the same as its implicit commitments. Such a theory would enable us to read off its

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<sup>38</sup>Davidson 1967; 1977

implicit commitments simply by attending to the existential claims it incorporates, for it will incorporate an existential claim for each of the entities, or kinds of entity, among its ontic preconditions. If it is possible to impose some restriction on the theories whose ontic commitment we consider, such that we only assess the ontic commitment of ‘complete’ theories, then there is no need to distinguish between implicit and explicit commitment, for in a complete theory they are the same. Unfortunately, it does not seem that there is any reliable way of specifying such a restriction on theories. One suggestion is given by Quine (1960: 242): he claims that reformulating our theory in a ‘canonical notation’ of first-order logic will make all the hidden commitments of a theory ‘explicit’. But a theory formulated in canonical notation will be complete only if certain semantic assumptions are correct, most importantly that all names are removed in favour of bound variables of quantification governed by uniquely identifying predicates, and that predicates do not refer to universals. A notation whose predicates refer could not guarantee the completeness of a theory stated in it, for the theory might require referents for its predicates as a precondition of its truth, without explicitly stating that such entities exist. Therefore we can be sure of the completeness of a theory in canonical notation only *after* we have investigated the implicit commitments of the various kinds of expression it employs.

A second suggestion for making sure that we deal only with ‘complete’ theories is to restrict our attention to theories that are closed under some appropriate notion of logical consequence (Michael 2008: 47). If it is guaranteed that the theory contains every existence-statement that is a consequence of a statement in the theory, then hopefully the theory will incorporate an existential claim regarding every one of its ontic preconditions. It is possible, as Jackson suggests (1989: 192), that this approach is proposed by Quine, when he says

‘The ontology of a theory is a question of what the assertions say *or imply* that there is’<sup>39</sup>

Some caution is required with this approach. Depending on how we construe the notion of logical consequence, a theory might be closed under logical consequence and nevertheless fail to be ‘complete’ in the sense under consideration, for the theory might have ontic preconditions whose existence is not a ‘logical consequence’ of any statement contained in it. This will especially be so if the logical consequence relation is treated as formal implication — as depending on the logical structure of the sentences in question and the patterns of inference into which these sentences enter purely in virtue of their structure.

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<sup>39</sup>Quine 1951, p.14. My italics.

This is the approach recommended by Quine (1986: 48); however we should not expect it to capture all the ontic preconditions of a theory. A persuasive example is given by Jackson (1989: 193): the implicit commitments of a theory containing the sentence ‘There are some people who are taller than some other people’ should include people who are shorter than some other people, for such people would have to exist, given that the theory is true. Yet if to be ontologically committed to  $\phi$ s is to formally imply ‘ $\phi$ s exist’, the theory cannot be counted as bearing any such commitment, since ‘There are some people who are taller than some other people’ does not formally imply ‘People who are shorter than some other people exist’.

These problems suggest that we should not expect to discern the ontic preconditions of a theory simply by settling on some criterion of ‘completeness’ and then investigating the existential claims made in the course of asserting a ‘complete’ theory. For that reason it is desirable to investigate the ontic preconditions of a theory *directly*, by investigating the semantic question of which parts of speech — quantifiers, predicates, names and so on — must be associated with entities if their sentences are to express truth. Such a conclusion has two consequences for the debate between nominalist and realist over ontic commitment. First, since ontic commitment may be associated with kinds of expression, such as predicates, which are not part of the machinery of existential assertion, it cannot be assumed against the realist that predicates themselves lack ontic commitment. Second, it should not be assumed that the correct way to discern the ontic commitment of idioms of quantification is to form a view about whether such idioms are among the machinery of existential assertion. Which expressions must be associated with an entity, or class of entities, in a theory of truth for a language is not the same question as which expressions involve us in existential claim or assertion. The former, rather than the latter question is what we should consider when investigating ontic commitment. Confusion about this point is encouraged by the use of ‘commit’ in everyday speech to ascribe a psychological attitude. Someone’s discourse may be ‘ontologically committed’ to  $\phi$ s in the sense of  $\phi$ s being among its ontic preconditions, without it being appropriate to describe the speaker as ‘committed to the existence of  $\phi$ s’ in the sense that they do, or are prepared to, assert the existence of the entities in question. Ontic commitment, unlike other senses of ‘commitment’, does not require that we are aware of the commitment we have undertaken.



## Paraphrase and Retraction

Nominalists who seek to reduce their ontological commitments are familiar with Quine's injunction to 'paraphrase or retract' (1969a: 100) those sentences that apparently bear commitment to universals. To retract a sentence is to accept that it is untrue;<sup>40</sup> since the theorist need admit only those entities which are the ontic commitments of sentences he takes to express *truth*, retracted sentences are irrelevant to ontology. To offer a paraphrase is to *replace* the original sentence with a different sentence which serves the theorist's purpose just as well; success for the nominalist will consist in providing a replacement which neither is nor appears to be committed to universals.

The disjunction 'paraphrase or retract' is not exclusive: only when a paraphrase is introduced as a *translation* or *analysis* of the target sentence must the paraphraser stand by the truth of the target sentence, for an adequate translation or analysis will be truth-conditionally equivalent to its target sentence. Yet a paraphrase need not be a translation, or even preserve truth-conditions, for what is said by a paraphrase may be different from what was said in the original sentence — for example when the original sentence is genuinely committed to  $\phi$ s but the theorist realizes that his communicative or descriptive goals can be met by means of a different sentence which bears no such commitment to  $\phi$ s. In such cases, the theorist provides a paraphrase but is under no obligation to stand by the truth of the target sentence; instead he will retract it, claiming that the untrue target sentence is at best a misleading *façon de parler*. In this situation, the theorist takes *both* of the options offered by Quine: he both retracts the original sentence *and* provides a paraphrase which serves his purposes just as well. When used in this way, paraphrase is not to be understood as a means of reducing the ontological commitments of our original theory, but rather as the process of finding a replacement with fewer ontic commitments which does the job just as well.<sup>41</sup>

We may distinguish 'paraphrase as analysis' from 'paraphrase as replacement'. In the former, the paraphraser claims that the target sentence expresses truth, and proposes a translation or analysis of that target sentence. The paraphrase preserves the ontological commitments of the target sentence, but may, in providing an analysis, reveal that some apparent commitments were merely apparent, and not genuine. In the latter — 'paraphrase

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<sup>40</sup>I do not intend to suggest that *sentences* are bearers of truth or falsity; strictly speaking sentences should be said to *express* truths rather than to *be true*. Nevertheless, I shall slip into the less fastidious usage where it seems unavoidable.

<sup>41</sup>Something like this distinction between two ways of understanding paraphrase is proposed by Jackson (1980: 306ff); however Jackson makes the strange assumption that only one of the two accounts can be correct.

as replacement’ — no such claims are made: the target sentence is abandoned as unworthy of the theorist’s continued allegiance, and all that is required of the paraphrase is that it serve the theorist’s purposes just as well as the discredited target sentence.

Paraphrase as Analysis	Paraphrase as Replacement
Target sentence maintained	Target sentence disowned
Paraphrase is equivalent to target sentence	Paraphrase not equivalent to target sentence
Ontological commitments preserved	Ontological commitments changed

The distinction between these two kinds of paraphrase enables us to answer those who doubt that paraphrase helps us ascertain the genuine ontic commitments of our theory. Since they claim that problems with paraphrase show it is impossible to establish an ontology by investigating ontic commitment, it is important that the realist can respond to them. Moreover, a proper understanding of how paraphrase is possible reveals that there is no prospect of the nominalist using paraphrase to evade commitment to universals in the current dispute.

One commonly-raised issue is how paraphrase can *reduce* ontic commitment when the paraphrase is presented as a translation or analysis of the target sentence. If two sentences are alike in meaning then surely they should also be alike in their ontological commitments; thus paraphrasing from one to the other will not reduce ontic commitment (Alston 1957; Jackson 1980; Glock 2002). Understood in one way, this issue is trivially resolved by recognizing the distinction between the *genuine* and *apparent* commitments of a sentence. Paraphrase by analysis is not an operation which reduces the *genuine* ontological commitments of a sentence; rather it is a means of coming to see that the *apparent* ontological commitments of the target sentence are merely apparent. *Of course* someone who proposes an analysis accepts that *analysans* and *analysandum* agree in their ontic commitments — for otherwise it could not be claimed that the *analysans* revealed something about the genuine commitments of the target sentence.

A more sophisticated problem threatens. Given that the same *genuine* commitments can be shared by sentences with different *apparent* commitments, what way do we have of knowing that we should treat the proposed analysis rather than the target sentence as the more accurate guide to the actual ontic commitments involved? Suppose someone proposed that we analyse

- (1) Socrates has wisdom

as

(2) Socrates is wise.

What reason could be given for thinking that the apparent commitments of the second sentence (one thing — Socrates) are a better guide than the apparent commitments of the first sentence (two things — Socrates and wisdom) in determining the *genuine* ontic commitment which the two sentences allegedly share? Without a reason to prefer one paraphrase over another, we have no way of deciding what our ontic commitments actually are.

We cannot solve the problem by appealing to a principle of parsimony, automatically preferring the paraphrase with the fewest apparent commitments, for sentences may have fewer apparent commitments than genuine commitments just as easily as they may have more apparent commitments than genuine commitments. For that reason we cannot adopt the explanation proposed by Alston, that

‘It is the seductive grammatical family likenesses of sentences like (1) which render them objectionable... the point of translating (1) into (2) lies in the fact that once anyone sees that what he says when he uses (1) can he just as well said by using (2), the power of the grammatical lure will be broken.’<sup>42</sup>

Alston’s point is this: the availability of a paraphrase (2) with fewer apparent commitments than the target sentence (1) shows that the extra apparent commitments of (1) were merely quirks of grammar rather than signs of *genuine* commitment. But this response only works if there could not be a sentence whose apparent commitments are fewer than its genuine commitments, for otherwise there would be no reason to suppose that the *extra* apparent commitments are merely a misleading ‘grammatical lure’. There is no reason to think such sentences impossible; therefore we are not entitled to assume that the sentence with fewest apparent commitments is the best guide to genuine commitments. A striking example of such a misleading sentence is given by Searle (1969: 110). Let ‘*K*’ stand for the conjunction of all the claims of current science. Then define a predicate ‘*F(x)*’ as ‘*x* = this pen & *K*’. Then any scientist with a pen in front of them may paraphrase the whole of current science by means of the one sentence ‘*F*(this pen)’. Clearly we cannot conclude from the availability of a paraphrase with so few apparent commitments that the *genuine* commitments of science are similarly limited to this pen alone.

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<sup>42</sup> Alston 1958, p.16. Alston’s example sentences (1) and (2) are different from mine, but his point applies equally in the current case.

An alternative response is suggested by the approach to ontic commitment developed in the preceding section. There we found that the ontic commitments of a statement are its *implicit* commitments; therefore the question of a statement's *genuine* commitments is settled by determining what the ontic preconditions of the truth of that statement really are; a sentence misleads us as to its ontological commitment insofar as it seems to entail that  $\phi$ s exist although it does not actually entail that  $\phi$ s exist. The paraphrase which is the best guide to ontic commitment will be the one whose structure best reveals the logical relations in which the original sentence stands. But (on at least one plausible notion of logical form)<sup>43</sup> the aim of an account of the logical form of a sentence is to represent the content of that sentence in such a way as to make it apparent what patterns of entailment the sentence enters into. If we are interested, as we should be, in implicit commitment then the paraphrase that gives the best account of ontic commitment will be the paraphrase which best represents the actual logical form of the sentence.

What consequences does this account of paraphrase have for the nominalist? When faced with the realist's problem sentences, two kinds of paraphrase are available. He can propose a paraphrase either as an *analysis* of the original sentence that more accurately represents its genuine commitments, or as a *replacement* for that sentence. I shall argue that neither kind of paraphrase can succeed. Employing paraphrase as replacement involves us in retracting the target statement — accepting that it is untrue. Since the 'targets' in this case are any and every statement of qualitative sameness, the nominalist cannot propose a paraphrase of this kind unless he is prepared to deny all statements of this kind; but no-one should be willing to deny the 'Moorean fact' that different things exhibit similarity or qualitative sameness.

The prospects for paraphrase as analysis seem scarcely better. In the case of the sentence to which the realist appeals, ' $(\exists x)a$  and  $b$  are the same in respect of  $x$ ', an analytic paraphrase with fewer apparent commitments is trivially available, for we can paraphrase back into the original sentence, ' $a$  and  $b$  are qualitatively the same'. However, there is good reason to think that the sentence involving quantification over respects of sameness is the *better* guide to the logical relations in which statements of qualitative sameness and difference stand, for it enables us to explain how qualitative sameness and difference are compatible with each other. According to the current proposal for discerning genuine ontological commitments, the sentence which is the better guide to logical relations is thereby the better guide to genuine ontological commitments. But then the quantified sentence should be treated as a better guide to genuine ontic commitments than the paraphrase which lacks

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<sup>43</sup>See Neale 1993 for discussion of this, and other, conceptions.

explicitly quantificational structure. This suggests that the quantification over respects of sameness indicates *genuine* rather than merely apparent commitment. Indeed, since any analytic paraphrase which omitted explicit quantification over respects of sameness would be a *worse* guide to genuine commitments, we may conclude that no paraphrase by analysis is likely to persuade us that the commitment to respects of sameness is merely apparent and not genuine. Paraphrase does not enable the nominalist to avoid commitment to shared features and respects of sameness.

## The Case against Objectualism

The objectualist holds that an existential quantification  $(\exists x)Fx$  is true iff there exists some  $F$  entity in the domain of quantification. It is not unreasonable to hold that objectualism is the default position, since it may seem obvious that when we talk about ‘something’, we talk about something that *exists*; moreover, the quantifier notation itself is often introduced and explained in terms of explicitly existential figures of speech, so that  $(\exists x)Fx$  is explained as ‘There exists an  $x$  such that  $Fx$ ’, or as ‘There are  $F$ s’. The nominalist who wants to deny commitment to the quantifier needs to give us good reason to reject this natural way of thinking about quantification. In this section I consider and respond to two arguments against objectualism. One argument attacks the connection between quantification and the speech-act of asserting existence, or ‘making an ontological claim’; another is the argument that ‘prior ontological considerations’ should persuade us that the truth of an existential quantification does not require the existence of an appropriate object.

### 1. Quantification and Existence

Quine sometimes takes investigation into ontological commitment to be a matter of determining what existential claims or assertions are incorporated into a theory; it is also clear that he believes this question about existential claims — the question of ‘explicit’ ontic commitment — is adequately answered by isolating the existentially quantified sentences of that theory. He even says that ‘existence is what existential quantification expresses’ (1969a: 97). Therefore it is not unreasonable to attribute to Quine the following thesis about the existential quantifier:

(Q) Every assertion of a sentence of the form  $(\exists x)Fx$  is an act of asserting that there exists an entity satisfying the predicate  $F$ ...

Thesis (Q) says, in effect, that an utterance of a sentence gets to be an existence-claim in virtue of the sentence having a specific kind of logical form — one involving an un-negated existential quantifier.<sup>44</sup> In this section, I consider whether the nominalist could deny that quantification is a locus of ontic commitment by rejecting (Q); my response is that rejecting thesis (Q) is a necessary, but not a sufficient, condition of denying ontic commitment to the quantifier.

Why might someone doubt the correctness of (Q)? Two reasons present themselves. One is the phenomenon of hidden quantification revealed by analysis, for example in Russell’s

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<sup>44</sup>Something like Thesis (Q) is explicitly adopted by David Lewis: see Lewis 1990, pp.24-5.

theory of descriptions or Davidson's theory of events. The other is the use of quantifiers to formalize English expressions of generality such as 'some', 'something', 'someone' and so on. In each case, the challenge is the same. How can someone be counted as *asserting* the existence of an entity by using such sentences, if these sentences can be used by people with no interest in expressing ontological conclusions or even awareness of the quantificational structure underlying what they say?

Consider Russell's theory of descriptions. Here we are told that an expression such as 'the King of France' is not a logical subject-expression, but rather an 'incomplete symbol' (Russell 1918: 241) — a grammatical unit corresponding to no one thing at the propositional level. Instead, the description phrase reveals something about the logical structure of the sentence as a whole, namely that it contains an existential quantifier governing the predicate '...is a King of France', coupled with a clause requiring that this description is satisfied by one thing at most. One of the criticisms levelled at this theory by Strawson is that Russell, in attributing quantificational structure to sentences involving 'the', makes each un-negated use of such a sentence into a case of 'existential assertion' (Strawson 1971: 14), when clearly it is not.

'To use the word "the" in this way is then to imply... that the existential conditions described by Russell are fulfilled. But to use "the" in this way is not to state that those conditions are fulfilled.'<sup>45</sup>

We could imagine a similar complaint levelled at Davidson's treatment of action-sentences: in saying that Jones buttered the toast, we may say something that cannot be true *unless* there exists an individual event that fits the bill; nevertheless it would be absurd to take anyone who made such a statement to have made an existential *assertion* concerning this event. Genuine existential assertions are made by people when they answer questions about ontology, the objection goes; who are you to tell us that we assert the existence of things like events without knowing it? Yet these observations need not impugn the correctness of Davidson's or Russell's analysis, for it would be possible to maintain the analysis, and grant the force of the criticism, by rejecting thesis (Q). It is only because thesis (Q) commits us to construing every sentence with an existentially quantified form as a means of making an existence-claim that we found the quantificational structure incongruous in Davidson's and Russell's proposals, and if we reject this assumption about the quantifier then we no longer need regard a definite description as involving us in assertion of existence *even if* it has the quantificational structure that Russell credits it

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<sup>45</sup>Strawson 1971, p.14

with.

The second reason for doubting thesis (Q) is the apparent gulf between this account of the quantifier in terms of existential assertion, and the purpose for which quantifier notation was originally introduced — namely, to give a better account of the logic of statements of generality using the terms ‘some’ or ‘all’. Consider sentences like

Someone stole my bicycle!

And then I go and spoil it all by saying something stupid like ‘I love you’.

Someday my prince will come.

These are *obviously* quantificational in structure; yet I take it that it would be a revision of our everyday notion of existential claim to say that an utterance of any of them counts as claiming that some entity exists. Again, there is a tension between the range of sentences to which we attribute quantificational structure, and the rather smaller class of sentences which we pre-theoretically regard as suited to making existential claims. If we abandon thesis (Q), the tension vanishes, for we are no longer constrained to regard all such quantified sentences as making existential assertions.

It is tempting to think that, if we show that thesis (Q) is false, we have thereby shown something important about where ontological commitment occurs in everyday speech. In recent papers both Jody Azzouni (2007) and Thomas Hofweber (2004) have argued that the first-order existential quantifier is not ontologically committing by appealing to cases in which a quantified sentence clearly does not count as making a move in an ontological debate, and therefore does not count as making an existence claim of the kind required by thesis (Q). Hofweber appeals to the fact that existentially quantified sentences like ‘Something is more fun than being an accountant, namely being a philosopher’ follow trivially from ‘metaphysically innocent’ (2004: 262) sentences such as ‘Being a philosopher is more fun than being an accountant.’ He claims that this inference is only valid so long as the existentially quantified sentence is understood in such a way that it is also ‘metaphysically innocent’. But what is it to be ‘metaphysically innocent’? It cannot be that ‘being a philosopher is more fun than being an accountant’ counts as metaphysically innocent on the grounds that no entity, *being a philosopher*, is required to exist for them to be true. To claim this would be to beg the question against any realist who thought that ‘being a philosopher’ names a universal. Instead the argument must be reconstructed as follows: quantifications follow from sentences that are ‘innocent’ in that they are not used to express ontological conclusions; therefore quantified sentences are likewise innocent, and are not explicitly committing as thesis (Q) claims. Azzouni’s strategy is different: he



argues that quantified sentences do not make existential claims because the very same quantified idioms are used by people whose personal ontologies may differ; such people do not take themselves to be engaging in ontological debate by using these idioms, therefore uses of these idioms cannot be cases of existential assertion.

In both cases, what is attacked is thesis (Q), since what is argued for is the irrelevance of quantifier phrases to existential *claim* or *assertion*. Both Hofweber and Azzouni make a persuasive case that there are uses of quantified sentences which can only be understood on the assumption that these sentences are *not* being used to make an existential claim, or participate in an ontological debate. If they are right, then it should not be claimed that quantifications of this kind involve us in *explicit* ontological commitment, for an utterance of such a quantified sentence is not a case of existential assertion — not a case of ‘saying that’ something exists.

However, it is a further question whether the rejection of thesis (Q) deprives the quantifier of *implicit* commitment. Whether a sentence is *explicitly* committed to  $\phi$ s depends on whether it is used to assert the existence of  $\phi$ s; whether a sentence is *implicitly* committed depends on whether  $\phi$ s are among the ontic preconditions of the truth of that sentence. If a sentence cannot be true unless there are  $\phi$ s, it is implicitly committed to  $\phi$ s regardless of whether an utterance of the sentence should be understood as claiming that  $\phi$ s exist. We have already seen cases in which implicit commitments could outstrip explicit commitments; could we respond to the current challenge by asserting that quantification itself is an example of implicit commitment outstripping explicit commitment: that existential quantification is implicitly, but not explicitly committing?

More needs to be said to forestall the impression that such a strategy is incoherent. The objectualist says that the truth of a quantification depends on the existence of an appropriate entity; therefore he claims that the existence of such an entity is an ontic precondition of the truth of an existentially quantified sentence. Objectualism seems best described as the view that quantification is *implicitly* committing. The debate over thesis (Q), however, is a debate about whether such a sentence should be counted as making an existential claim purely in virtue of its quantified form. It is possible to adopt other views of how existential assertions are made, and it is not obvious that any of these alternatives is incompatible with the ‘objectualist’ view that the truth of quantification *depends* on the existence of an appropriate object. A sentence (even a quantified sentence) may *depend* on the existence of some entity without counting as *asserting* the existence of that entity.

If we are convinced that thesis (Q) should be rejected, then, we abandon the view that an ‘existential assertion’ is so in virtue of its quantificational structure. Some alternative

proposal is needed. One is that the burden of existential assertion is to be borne by a specially designated existence-predicate; another is that ‘making an existential claim’ does not depend solely on the form of words used, but also on features of the context and circumstances of debate. Such a view is suggested by the fact that sentences using ‘exists’ may be used carelessly in circumstances where ontology is not at issue, for example when a literature student is accused of ‘writing as though Anna Karenina, Tristram Shandy, and Mrs. Dalloway simply didn’t exist’.<sup>46</sup> To adopt one of these rival accounts, of course, is to abandon the explanation of the quantifier as a device for making existential claims, for this function is no longer assigned to it. A better explanation is that the quantifier is a device of generality, such that  $(\exists x)Fx$  serves to tell us that something is  $F$ ; yet better would be — as the name ‘quantifier’ suggests — that it is a device for counting, such that  $(\exists x)Fx$  tells us that *at least one thing* is  $F$ .

It seems that the current suggestion — that  $(\exists x)Fx$  tells us that *at least one thing* is  $F$  — provides a coherent account of how the existential quantifier could *require* the existence of an appropriate entity in the domain of quantification, and hence be *implicitly* committing, without counting as a means of making existential *assertion*, and hence being *explicitly* committing. It is plausible to suppose that, if I tell you that at least one person is at the bar, I have no more indulged in existential assertion than I would by telling you that *John* is at the bar. Nevertheless, in each case the truth of what I say *depends* on the existence of an entity satisfying the appropriate condition, and so in each case we may say that what I say bears implicit commitment even if it does not bear explicit commitment.

Objections to this revisionary account of existential quantification will come from two directions; fortunately for the realist, both responses have the consequence that the nominalist’s original argument has no force. One is a refusal to countenance the rejection of Thesis (Q). Habituated to read  $(\exists x)Fx$  as ‘there exists an  $x$  such that  $Fx$ ’, the opponent may contend that it is impossible, nonsensical even, to question the link between quantification and existential assertion. Someone who holds such a view is no opponent of objectualism: his argument is merely that the examples which the nominalist proposes are insufficient to motivate the rejection of thesis (Q), and if thesis (Q) is maintained then it cannot be doubted that the existential quantifier is implicitly committing, for how could a quantification *say that*  $F$ s exist and yet be true although  $F$ s do not in fact exist?

A second objection is that little sense can be made of the notion of ‘making an existential claim’ which I have been employing rather freely to characterize the debate about explicit commitment. To some ears, there is nothing specially ‘existential’ about ‘There exists

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<sup>46</sup>See Van Inwagen 2000, p.239 for this, and other, examples.

at least one person in the bar' which is not shared with 'At least one person is in the bar'. It may be said that *both* are 'existential' in the sense that they can only be true if an appropriate entity exists; beyond that there is no special act of 'making an existential claim' or 'participating in an ontological debate' such that we can participate in such an act by means of the former but not by the latter sentence. Again, this objection militates in favour of the objectualist. Arguments such as those offered by Hofweber and Azzouni *depend* on the acceptance that we can isolate some sentences, or utterances of sentences, as 'making an existential claim' or 'participating in an ontological debate'. Their argument is that, when we distinguish such sentences from 'ontologically neutral' sentences which are not used to participate in ontic controversy, we find that many quantified sentences do *not* involve us in making an existential claim. But if there is no sense to be made of a distinction between 'existential' and 'innocent' sentences, then there is no way for Hofweber and Azzouni to argue that quantified sentences fall on the wrong side of the division, for there is no division there in the first place. The only sensible question we could ask in such a circumstance would be the *semantic* question of whether the truth of an existential quantification depends on the existence of an appropriate entity, since it would not even be possible to raise the question of which forms of speech have the special function of making an existential claim.

The first argument against objectualism was that many existentially quantified sentences do not serve to 'make an existential assertion'; therefore the quantifier should not be regarded as ontologically committing. I propose that this claim cannot be sustained, for it is possible to advance a coherent account of the quantifier according to which it is not, after all, a means of existential *assertion*, yet nevertheless is *implicitly* committing. Such a view of the quantifier faces objections; however, these objections, if successful, would force us to reject the non-objectualist's argument in any case. We may conclude that the first argument against objectualism is unsuccessful.

## 2. The Argument from Ontology

The other argument against objectualism has a wide constituency.<sup>47</sup> This argument offers sentences which quantify over ontological undesirables, but which apparently express truths. It is argued that the sentences express truth, but the entities do not exist; therefore the quantification employed cannot be ontologically committing. Such a strategy reverses the realist's argument: the realist argues that his quantifications are true, and are ontolog-

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<sup>47</sup>See for example Barcan Marcus 1972, p.242; Gottlieb 1980, p.54; Jackson 1980, p.304; Glock 2002 p.248; Azzouni 2004, p.62ff.

ically committed to  $\phi$ s, therefore  $\phi$ s exist, while the nominalist argues in response that  $\phi$ s do not exist, and these quantifications are true, therefore these quantifications are not committed to  $\phi$ s.

The nominalist may well feel this is the appropriate response in the case of quantification over respects of sameness. If the realist's analysis is correct, we have to accept that some quantifications over respects of sameness express truth, for every claim of similarity between objects involves us in quantification over such entities. The question, then, is whether we have any right to be sure that the entities we apparently quantify over do not exist. Barcan Marcus suggests that such entities should be excluded because 'on prior ontological considerations we don't want them showing up in our domain' (1972: 242). It is clear that this argument hinges on the notion of 'prior ontological considerations'. If we have no prior reason for believing that entities of a certain kind do not exist, and no prior reason to think that the semantics for quantifiers are non-objectual, then the proper response is simply to admit these entities to our ontology. Proponents of this argument need to give some *reason* for rejecting entities of the kind in question.

Is there any justification for finding universals so unpalatable that we revise quantifier semantics to evade ontic commitment to them? Barcan Marcus suggests that universals are ontologically undesirable because they are not 'in principle confrontable, encounterable, dubbable by an act of ostention' (1978: 123). Yet even if it were obvious that we should impose such a restriction on the entities we are prepared to recognize, it is not clear that universals fail to pass it. It does seem that many universals are in fact encountered in perception — for example when we notice two things exhibiting the same colour. Moreover, universals are introduced not merely in the course of saying what it is for an object to have the attributes it has, but also to give an account of types or kinds, entities which are not only encounterable in experience but also 'dubbable by an act of ostention' as Barcan Marcus demands. I can decide to call this car Herbie, but I can equally well declare that I shall call this *kind* of car the VW Beetle. Likewise, it is possible to give a name to a particular *type* of hairstyle by declaring that it is the style shared by *this* footballer and *this* television presenter. Since the universals proposed by the realist include examples which are both encounterable and ostendable, the rejection of unencounterable entities can at best reduce the number of universals recognized; it cannot show that universals must be abandoned.

Is there any other 'prior ontological consideration' which might reveal universals to be ontological undesirables? One is encapsulated in Quine's slogan 'no entity without identity' (1969a: 23), the principle that it is undesirable to admit entities to our ontology when we

have no idea about ‘the circumstances under which... [they] may be said to be the same or different.’<sup>48</sup> Quine has been criticized on the grounds that he makes the following demand: that we recognize a kind *K* of entity only if it is possible to state identity conditions for *K*s which apply to all and only things of kind *K* — in other words, to give an account of ‘being the same *K* as’ which every *K* bears to itself and which no non-*K* entity bears to itself (or to anything else). This demand is rejected because it is unreasonable to expect that, given entities of a kind *K*, there must be necessary and sufficient conditions for one *K* being the same *K* as another *K* — conditions which are applicable to all and only *K*s — for it cannot be assumed that every (or any) kind of thing has associated with it distinctive identity-conditions applying to all and only things of that kind (Strawson 1976: 22; Jubien 1996: 347).

However, Quine’s critics may have attributed a view that he does not endorse. Quine’s objection to things of the kind *universal* is not that there is no way to give distinctive identity-conditions specifically tailored to *universals* — that the realist can offer no account of a relation ‘identity-for-universals’ which all and only universals stand in. Instead, he finds realism objectionable because we have no criterion of identity whatsoever which might be extended to cover things of kind *universal*. It is nowhere alleged that this criterion, in addition to applying to universals, must *fail* to apply to things which are not universals. Quine’s own comments support this reading. After all, the problem with universals (Quine’s ‘attributes’) is that

‘The positing of attributes is accompanied by no clue as to the circumstances under which attributes may be said to be the same or different.’<sup>49</sup>

It is not that we have failed to give identity conditions applying to all and only universals; rather it is that none of the ways we have of specifying identity-conditions happens to work when applied to universals. This first criticism of Quine’s demand is based on a mistaken assessment of his position.

How then should we understand Quine’s challenge to the realist? It may take an epistemological form, as the principle ‘no entity without the ability to tell whether *this* is the same entity as *that*’. Here we are led to question both the correctness of the principle and whether it can succeed in excluding universals from our ontology. The re-identification of kinds or types is a skill that all of us have to some degree, where the types in question are models of car or hairstyles; where the identificatory work is more complex (matching exact

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<sup>48</sup> Quine 1969a, p.19; see also Quine 1960 pp.200-9

<sup>49</sup> Quine 1969a, p.19

shades of colour or species of frog) we defer to experts whose ability is more developed. The epistemological criterion does not exclude such universals; but we might wonder whether it deserves our adherence anyway, for there are cases when we lack such an ability to decide identity-questions even with regard to spatiotemporal objects. No amount of inspection of the apple on my desk could enable me to determine *conclusively* that it was the same one I encountered yesterday rather than an exactly similar one which has replaced it overnight.<sup>50</sup> The epistemological principle is at odds even with a nominalist ontology of spatio-temporal objects; therefore nominalist and realist alike should agree not to appeal to it.

A better formulation will be methodological: that we should admit to our ontology only entities for which criteria of identity can be given — never mind whether we have the ability to conclusively determine that those criteria have been met. To give such a criterion will be to specify some (complex) dyadic predicate ‘...R...’ such that, when ‘a’ is a name of some object, ‘aRb’ is true iff ‘b’ refers to the same object as does ‘a’. One objection to this form of the principle, due to Strawson, is that we habitually recognize as entities things for which no criteria of identity are available, for example

smells, feels, timbres, ways of walking, manners of speech, literary styles,  
architectural styles or hair-styles.<sup>51</sup>

However, these are all examples of attributes or types — in other words, universals — and hence just the sort of thing which Quine claims *ought* to be rejected on the grounds that they lack identity-conditions. Quine is perfectly aware that the ‘community’s ontology’ includes things which lack identity-conditions; but it is for just this reason that he thinks it deserves revision.<sup>52</sup> The most Strawson’s objection can achieve is to remind us how much we are giving up when we accept the demand for identity-conditions; but it cannot be assumed, prior to settling the nominalism-realism debate, that the identity principle is false merely because it excludes universals such as those Strawson lists.

A better version of the objection is that we lack identity-criteria even for everyday material objects such as tables and apples (Jubien 1996: 345). We accept such objects in our ontology, and are surely right to do so; yet it does not seem possible to give a ‘criterion’ which must be satisfied in order for there to be one apple rather than two. Quine’s view is that we should

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<sup>50</sup>This point is due to Jubien (1996, p.345)

<sup>51</sup>Strawson 1976, p.22

<sup>52</sup>See Quine 1969a, p.15ff

‘understand a physical object... as the aggregate material content of any portion of space-time, however ragged and discontinuous.’<sup>53</sup>

This suggests a straightforward criterion of identity for spatio-temporal objects: objects *a* and *b* are one and the same iff they occupy exactly the same region of space-time. Such an account will be congenial only to those who accept a ‘perdurantist’ (Lewis 1986: 202) account of persistence through time, according to which only *part* of an object is present at any given time; the object itself is spread out through time just as it is spread out through space. If perdurance is rejected, and objects are taken to be wholly present at each time at which they exist, the problem about identity-conditions for physical objects recurs, for then a given object (if it moves) will be wholly present in *different* regions of space at different times, in which case the question arises, what are the criteria according to which the material wholly present in *this* spatial region at time  $t_1$  counts as the same object as the material wholly present in *that* spatial region at time  $t_2$ ?

Moreover, even the perdurantist’s position might seem unstable. His account of the identity-conditions of spatio-temporal objects was: *a* and *b* are identical iff they occupy the *same* space-time region. But to say this is merely to explain identity of object in terms of identity of space-time region. Without an account of the identity-conditions of space-time regions, no satisfactory account of the identity of objects has been given at all, for (according to the demand for identity-conditions) we cannot claim to understand claims of identity as they apply to a certain class of thing, until we have supplied identity-conditions for such things. We may conclude that neither perdurantist nor anyone else can supply identity-conditions for physical objects which meet Quine’s challenge; surely we should reject the challenge sooner than exclude physical objects from our ontology.

The plausibility of the Argument from Ontology depends on whether the nominalist can supply ‘prior ontological considerations’ powerful enough to encourage non-objectualism about quantification over universals. However, it does not seem that universals can be declared *entia non grata* by appealing either to their remoteness from perception and ostension, or to the supposed need for a principle of identity that can be extended to cover them. The first attempt fails because it is plausible to consider universals perceptible and ostensible; the latter because the demand for identity-conditions has not been met even in the case of physical objects, and so neither nominalist nor realist should accept that demand if they want to maintain any ontology at all.

To conclude the argument of this section. Our original question was whether there is any

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<sup>53</sup>Quine 1976, p.258; see also 1960, pp.170-1.

good reason to reject objectualism, and deny that the existential quantifier is ontologically committing. I have considered two arguments which might support this nominalist proposal. The first, that there is good reason to doubt the connection between quantification and *assertions* of existence, loses its force when we distinguish between explicit and implicit commitment, for then it becomes apparent that this is an argument about *explicit* commitment, when what is at issue is *implicit* commitment. Second, the nominalist claimed that 'prior ontological considerations' militate in favour of a non-objectual reading, for we regularly quantify over entities which we would prefer to exclude from our ontology. It is not possible to survey all of the possible 'prior considerations' which might make universals undesirable constituents of our ontology; however I have argued that two of the most commonly-raised objections to universals have no force when properly understood.



## Non-objectualist Accounts and their Shortcomings

I have alleged that arguments against objectualism are inconclusive. This is not all that the realist can say in defence of his ontology, for three problems threaten the attempt to maintain nominalism by appealing to non-objectualism. First, such accounts enable the nominalist to evade ontic commitment to respects of sameness only if he also adopts the view of Positive Free Logic, that truth can be expressed by atomic sentences containing non-referring singular terms; second, statements of non-objectual truth-conditions are ontologically committed to types, and types are precisely what the nominalist needs to be able to do without. The final charge is that the non-objectualist semantics are extensionally inadequate: they do not give the right truth-values for the quantified sentences at issue.

Before raising these problems for non-objectualism, I should consider a position that promises to block all three objections against the nominalist. This is the suggestion that the nominalist might remain 'neutral' about the semantics of the existential quantifier, refusing to give any formal account of the necessary and sufficient conditions for the truth of quantified statements, but instead contenting himself with the observation that ' $(\exists x)x$  is  $F$ ' and 'something is  $F$ ' are equivalent. Certain remarks of Prior's (1971: 36) suggest this position; if legitimate, it would obviate the necessity for the nominalist to come up with a *rival* theory to objectualism.

Neutralism, if it is to be relevant to the controversy between nominalist and realist, must be more than the refusal to enter a debate about the conditions under which a quantification will be true. If a neutralist is simply someone who does not participate in the argument about quantifier truth-conditions, then he has no opinions that we are obliged to take seriously. Neutralism, then, must incorporate at least one positive claim about the circumstances under which an existential quantification is true: this is the claim that such quantification can be true even if nothing exists which satisfies the associated condition; that  $(\exists x)Fx$  does not entail the existence of an  $F$  entity.

I shall argue that neutralism is unacceptable because, on the best way of making the proposal intelligible, it simply collapses into realism. The neutralist accepts quantification apparently 'over' universals, but denies that universals 'exist'. This position is maintained at the cost of denying that a quantified sentence entails the corresponding claim using the English word 'exists'. There are two ways of understanding the position. One is that 'exists' is being used in its normal sense, while the quantifiers function — we know not how — without committing us to a domain of entities. Another is that the quantifiers are functioning objectually, and ranging over a domain of things, although the neutralist

is reluctant to say that these things ‘exist’. In that case, all that separates the neutralist’s proposal from realism is a certain restriction on the use of the words ‘exist’ and ‘entity’. It seems the latter interpretation makes more sense as a way of understanding neutralism. Such an account explains why the neutralist is willing to indulge in discourse that — according to the realist — is *about* universals, for example when he allows the truth of ‘*a* and *b* share some common feature’. Why should such discourse be desirable or necessary if it is not talk *about* things in the world? The case for this interpretation is strengthened if the neutralist takes idioms beginning ‘there is / are...’ to express his ‘neutral’ quantifier. When we encounter a theorist who accepts that *there are* universals, but denies that they ‘exist’, surely we cannot but understand them as recognizing universals in their ontology.<sup>54</sup> After all, even some *realists* would deny that universals ‘exist’, saying instead with Russell that they ‘subsist’ or ‘have being’ (Russell 1912: 57).

It seems, then, that neutralism is in danger of collapsing into realism, for all that separates the two positions is the neutralist’s reluctance to use the words ‘exist’ and ‘entity’ in connection with universals. Instead of interpreting the neutralist as disagreeing with us in ontology, but agreeing with us in our use of ‘exists’, surely it is better to understand him as agreeing with us in ontology, but disagreeing with us about the right use of the words ‘exist’ and ‘entity’. A similar suggestion is made by Lewis (1990), against Meinongians who, like the neutralist, are willing to quantify over more things than they say ‘exist’. In such circumstances, we can interpret them either as ‘noneists’ or ‘allists’. Allists have an ontology containing everything they quantify over, and a fastidious approach to the word ‘exists’, while noneists have an ontology containing only the things they say ‘exist’ and (by our lights) an approach to quantification that is hard to make sense of. Here the suggestion is that it is much more plausible to construe the neutralist, who is happy to quantify over universals but denies that they ‘exist’, as an ‘Allist’, for then we can make sense of why he acquiesces in discussion of universals rather than repudiating it. Our debate with the neutralist is not ontological, but rather a question of which of the things there are we should say ‘exist’.<sup>55</sup>

Comparison with substitutionalism reveals why this problem is especially pressing for the neutralist. Substitutionalists provide a clear statement of alternative truth-conditions for quantification. Insofar as these truth-conditions are intelligible, they reveal how the

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<sup>54</sup>See Kripke 1976, p.379 for this suggestion about ‘there are’.

<sup>55</sup>Quine (1980, p.3) raises a related complaint against those who distinguish subsistence from existence: to agree with your opponent about what *exists* yet also recognize a domain of things that ‘subsist’ is merely ‘preserving an illusion of ontological agreement’ although in fact your ontology far exceeds that of your opponent.

substitutionalist's employment of quantification differs from the realist's, and make it apparent what the truth of such a quantification depends on. For that reason there is no room to question whether the substitutionalist should be counted a 'noneist' or an 'allist': he is obviously a 'noneist', for his truth-conditions explain *how* a quantified statement can be true in the absence of entities over which we quantify. In contrast, the neutralist, who refuses to give a formal account of truth-conditions for quantifiers, can do nothing to dispel the suspicion that, instead of disagreeing with the realist about ontology, he has a different view about the circumstances under which we might apply the word 'exists'. I suggest that, unless the neutralist is prepared to say more about truth-conditions for quantifiers than simply alleging that existential quantification does not entail the corresponding 'existence'-claim, there is no reason to take him as expressing serious disagreement with the realist's project. We may turn directly to the problems facing attempts to specify a non-objectualist *account* of quantifier semantics.

### 1. Non-objectualism and Positive Free Logic

I have suggested that the nominalist should not expect to evade ontic commitment to features shared between objects, and respects in which objects are the same, by remaining 'neutral' about the semantics of quantifiers; the reason is that it is necessary to give alternative truth-conditions for

(QS)  $(\exists x) a$  and  $b$  are qualitatively the same in respect of  $x$

in order to avoid the suspicion that the disagreement is not ontological, but rather about the correct application of the words 'exist' and 'entity'. Once an alternative account is given, there are three objections a realist might raise. The first is that any such account allows us to evade ontological commitment to shared features and respects of sameness only if we adopt a Positive Free Logic, according to which there can be true atomic sentences containing a non-referring singular term. We may distinguish three different non-objectualist accounts; I shall explain the current objection in the course of introducing the accounts themselves.

#### Substitutionalism

A paradigm 'substitutional' account is given by Barcan Marcus, who uses the phrases 'E-quantifier' and 'U-quantifier' to avoid the incongruity of talking about *existential* quantification while denying that it is to be understood in terms of existence:

‘A sentence with a U-quantifier followed by an open sentence is true iff the open sentence is true on all substitutions of names for the variable bound by the quantifier. An E-quantified sentence is true iff it is true on one such substitution.<sup>56</sup> We will call the results of such a replacement a substitution-instance of the quantified sentence.’<sup>57</sup>

How could this account enable us to reduce the ontological commitment of a quantified sentence such as (QS):  $(\exists x) a$  and  $b$  are qualitatively the same in respect of  $x$ ? The suggestion would be that (QS) is true, not in virtue of an entity meeting the indicated condition, but rather in virtue of some true substitution-instance where the variable  $x$  is replaced by an appropriate expression in the language:

(QS\*)  $a$  and  $b$  are qualitatively the same in respect of  $c$

To deny that (QS) is ontologically committing, it is necessary to hold that it could express a truth although no entity existed to which the variable corresponds. On the substitutionist account, this would require the possibility of a true substitution-instance (QS\*) in which the subject-expression ‘ $c$ ’ refers to no entity. But the *prima facie* position is that subject-predicate sentences of this kind, where one subject-expression lacks a referent, should either be counted as truth-valueless or false. On the one hand, it might be counted as failing to express something with a determinate truth-value — as Strawson puts it in the case of empty descriptions, ‘the question of whether his statement was true or false simply *did not arise*.’<sup>58</sup> Alternatively, we might adopt the view of Negative Free Logic, that an atomic sentence containing a non-referring subject-expression is false.<sup>59</sup> Neither of these options enables us to deny commitment (QS), for on neither account can (QS\*) be counted as expressing a *truth*. For that to be possible, we should have to adopt the view of Positive Free Logic, that there can be *true* atomic sentences in which a subject-expression lacks a referent. This is at best counterintuitive; can the other two non-objectualist accounts of the quantifier avoid such a consequence?

### **Prior’s account of the quantifier**

Our second non-objectualist theory is the account proposed by Prior (1971). Prior suggests that the availability of a true substitution-instance is *sufficient* for the truth of an existentially

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<sup>56</sup>Clearly, by ‘one’ Barcan Marcus intends ‘one or more’

<sup>57</sup>Marcus 1972, p.243

<sup>58</sup>Strawson 1950, p.12. This view is already present in Aristotle, *Categories* 13b.

<sup>59</sup>Of course, the *negation* of such a sentence will then be counted *true*. See Sainsbury 2005, pp.64-75 for a lucid account of such a free logic.

quantified sentence, for example ‘Something is red-haired’; however it is not *necessary* for truth, since

‘its truth may be due to the red-hairedness of some object for which our language has no name or which non-one is in a position to point to while saying “*This* is red-haired”.’<sup>60</sup>

Prior’s suggestion is that an existentially quantified sentence may be true *either* in virtue of a true substitution-instance *or* in virtue of some appropriate object; again, if we want to maintain that ‘ $(\exists x) a$  and  $b$  are the same in respect of  $x$ ’ can be true in the absence of such an appropriate object, it must be in virtue of a true substitution-instance in which the expression which replaces the variable lacks a referent. Ontic commitment to respects of sameness is avoided only if we adopt Positive Free Logic.

There is some evidence that Prior himself would not have accepted that first-order quantification could lack ontological commitment in this way. He claims that ‘Something is red-haired’ is true if a ‘specification’ (substitution-instance) of it is true,

‘meaning by a “specification” of it any statement in which the indefinite “something” is replaced by a specific name of an object or person, such as ‘Peter’, or by a demonstrative “this” accompanied by a pointing gesture.’<sup>61</sup>

If the expression substituted for the variable ‘something’ must be a name *of an object or person*, then there can be no substitution-instance in which the substituend expression fails to refer, and again we are returned to the view that a first-order quantification requires the existence of an appropriate object, just as the objectualist claims. Prior’s account enables us to avoid ontological commitment in the current case only if we are willing to accept the possibility of true substitution-instances containing non-referring subject-expressions.

### **The Russell-Orenstein Account**

The third non-objectualist theory might be called the ‘Russell-Orenstein account’, as it originates with Russell (1905: 42; 1918: 232) and is defended by Orenstein (1978). In *On Denoting* Russell introduces the idea of a ‘propositional function’ as a bearer of truth and falsity:

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<sup>60</sup> Prior 1971, p.36

<sup>61</sup> Prior 1971, pp.35-6.

‘My theory, briefly, is as follows. I take the notion of the *variable* as fundamental; I use “ $C(x)$ ” to mean a proposition (more exactly a propositional function) in which  $x$  is a constituent, where  $x$ , the variable, is essentially and wholly undetermined. Then we can consider the two notions “ $C(x)$  is always true and “ $C(x)$  is sometimes true. Then *everything* and *nothing* and *something*... are to be interpreted as follows:

$C(\text{everything})$  means ‘ $C(x)$  is always true’

$C(\text{nothing})$  means “‘ $C(x)$  is false’ is always true’

$C(\text{something})$  means ‘It is false that “‘ $C(x)$  is false’ is is always true’ ”<sup>62</sup>

In a footnote Russell says that he will also shorten the account of ‘ $C(\text{something})$ ’ to ‘ $C(x)$  is sometimes true’. Again, if this strategy is to relieve the existential quantifier of ontic commitment, we must accept that among the occasions in virtue of which the ‘propositional function’ is ‘sometimes true’ there may be cases where it is true although the undetermined constituent  $x$  stands for no entity.

In fact, there is good reason to believe Russell thought that his quantifier *did* require the existence of an appropriate entity. In 1905 he had not yet given up the account of propositions as complexes of *entities*, for he makes a distinction between a proposition and its ‘verbal expression’ (1905: 43), and talks of ‘the difficulties which seem unavoidable if we regard denoting phrases as standing for genuine constituents of the propositions in whose verbal expression they occur’ (1905: 45), suggesting that here a proposition is to be thought of as a complex of entities rather than an arrangement of words, as he went on to claim in the *Lectures on Logical Atomism*.<sup>63</sup> Yet if propositions are complexes of entities — particular and universal — and propositional functions are propositions with an undetermined constituent, then for a propositional function to be ‘sometimes true’ is for it to be true on some *determination* or fixing of that undetermined constituent, and every such fixing of the constituent will be a case of fixing it *as* one or another entity. Within Russell’s early account of propositions as truth-bearers, there is simply no room for the idea that there could be a true instance of ‘ $x$  is  $F$ ’ in which  $x$  stood for no object at all, because then there would be no proposition to function as truth-bearer.

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<sup>62</sup>Russell 1905, p.42

<sup>63</sup>Russell 1918, pp.192-3. Linsky (2003, p.373) disagrees with this account of Russell’s ontology on the grounds that Russell must have rejected propositions from his ontology as soon as he arrive at the ‘multiple-relation’ theory of belief. Linsky then has great trouble explaining Russell’s willingness to speak of and quantify over propositions in the *Principia* — enough, I think, to show that Linsky’s account must be false.

Instead, it is necessary to construe the ‘propositional function’ as something like an open sentence, such that the ‘undetermined constituent’ ranges over *expressions* rather than objects. But here too it is necessary to accept that there can be truths containing non-referring subject-expressions, for otherwise there will be no occasion on which the quantification is true in the absence of an entity to which the substituent expression refers.

I suggest that the first problem for someone who wants to deny ontic commitment to our first-order quantifications over respects of sameness and shared features, is to explain why we should believe that the quantification could be true in virtue of an ‘instance’ or ‘occasion’ on which the variable of quantification is replaced by a non-referring subject-expression, yet the resulting sentence is counted true. The *prima facie* situation is that there are no such occasions which yield truth, for the resulting sentence should be counted either false or meaningless. Only the proponent of a Positive Free Logic can follow the nominalist even this far.

I have claimed that it is counter-intuitive to accept true subject-predicate sentences containing non-referring subject-expressions. It seems that a nominalist who adopts such a view faces another, more serious problem: if truth can be expressed by sentences in which the subject-expression fails to refer, we shall no longer be able to explain the truth of such sentences in terms of the predicate ‘being true of’ some object to which the subject-expression refers (Dummett 1981: 526; Bealer 2006: 230), nor in terms of the referent of the subject-expression ‘satisfying’ the predicate. The loss of such an account should not be a trivial matter for the nominalist: a large part of the plausibility of nominalism depends on its ability to give an account of truth of this kind, in which reference is assigned only to subject-expressions, rather than relying on a realist account in which reference is ascribed to subjects and predicates both (MacBride 2006: 427). In order to maintain that quantification over respects of sameness is ontologically uncommitting, the nominalist must accept Positive Free Logic; but to do this is to abandon hope of an account of truth in terms of satisfaction, or in terms of predicates being ‘true of’ objects. It may be that this alone is enough to make such a nominalist approach unviable.

## **2. Commitment to Types**

A second problem for the nominalist is that none of the proposed statements of quantifier truth-conditions is intelligible unless we understand it as ontologically committed to *types* of entity; and of course, once the nominalist recognizes the existence of types, he is obliged to give some account of what kind of thing they are, since the *prima facie* situation is that

types are universals; at any rate, the nominalist cannot claim to get by with an ontology of concrete particulars alone. This is precisely the objection attributed to Armstrong in connection with the Argument from Identity (Chapter 1): to avoid the obligation of having to give an account of what types *are*, if they are not universals, the nominalist must be capable of stating his theory in terms that do not presuppose the existence of types. Since any statement of non-objectualism presupposes the existence of types, the nominalist cannot hope to evade troublesome ontological commitment by such means.

The objection that *substitutional* quantification is ontologically committed to types is present already in Parsons (1971: 237), and is plausible because statements of substitutionalist truth-conditions appear to quantify over substitutable expression-*types*; what is quantified over cannot be expression-tokens, for then the truth of a substitutional quantification would depend on there being a physical *token* of the required substitution-instance. Quantification over expression-types is apparent in accounts given by both Quine and Kripke:

‘An existential substitutional quantification is counted as true if and only if *there is an expression* which, when substituted for the variable, makes the open sentence after the quantifier come out true.’<sup>64</sup>

‘ $(\Sigma x_i)\phi$  is true iff *there is a term  $t$*  such that  $\phi$ ’ is true where  $\phi$ ’ comes from  $\phi$  by replacing all the free occurrences of  $x_i$  by  $t$ .’<sup>65</sup>

Given the presence of the quantifier-expression ‘there is’ on the right of the biconditional, it would be reasonable to represent the condition on the truth of such a substitutional quantification using an objectual quantifier:

$(\exists x)x$  is an expression whose substitution for the variable yields truth.

But then the truth of a substitutional quantifier depends on the existence of an appropriate expression-type, and types are what substitutionalism was introduced to try to do without.

Van Inwagen attempts to extend this argument to show that ‘neither I nor anyone else understands substitutional quantification’ (1981: 285). His reason is that the ‘friends of substitutional quantification’ (1981: 284) deny that the correct account of substitutionalism is in terms of objectual quantification over expressions in the language, but have no alternative account to offer. It does not seem that this alternative, stronger accusation

<sup>64</sup>Quine 1969a, p.104. My italics in both quotes.

<sup>65</sup>Kripke 1976, p.330



can be maintained: although Orenstein, whom Van Inwagen names as his target, denies that substitutionalism should be explained by means of objectual quantification over expressions (1978: 34), this is because he prefers the alternative Russellian account already considered. Moreover, at least one prominent ‘friend of substitutionalism’ is prepared to explain it using objectual quantification over expressions, for Kripke goes to some lengths to emphasize that this is precisely his view:

‘[Metalanguage]  $M$  is to contain referential quantifiers ranging over arbitrary (well or ill-formed) strings of indecomposable (atomic) symbols of [object-language]  $L$ ; such an ontology of expressions is part and parcel of the project of defining the class of ‘true’ expressions of  $L$  ... [the biconditional] straightforwardly gives the truth-conditions of  $(\Sigma x_i)\phi$  in terms of the existence of a certain term.’<sup>66</sup>

Later, he asserts that

‘it is clear that every substitutional formula is equivalent to one involving referential quantifiers over expressions.’<sup>67</sup>

It seems, then, that nominalists who appeal to substitutional treatments of the quantifier evade one problematic commitment (to respects of sameness) only at the cost of incurring another (to expression-types). Can this problem be avoided by denying that the quantification involved in the account of truth-conditions is objectual? Dunn and Belnap suggest that the quantifiers employed on the right of the biconditional might *also* be considered substitutional rather than objectual (1968: 184). However, there are two reasons why such a suggestion is unsatisfactory: one, that the account of substitutionalism would be circular and hence uninformative, for it would be an account that was intelligible only to those who *already* understand substitutional truth-conditions (Parsons 1971: 237); another, that (as Kripke points out) a metalanguage  $M$  could not be regarded as giving the truth-conditions for sentences in the object-language  $L$  *unless*  $M$  purported to refer to or quantify over expressions in  $L$  — indeed, if this were not the case it would be hard to see what would justify the claim that statements in  $M$  were capable of being *about* the language  $L$  at all (Kripke 1976: 341).

If substitutionalism faces such a problem, can either of the two remaining non-objectualist accounts do better? I shall argue that they cannot. Prior’s account quantifies over

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<sup>66</sup>Kripke 1976, p.341

<sup>67</sup>Kripke 1976, p.353

‘substitution-instances’, and these entities are *prima facie* to be understood as *types* rather than *tokens*; therefore he too owes us an account of what these entities are. It might seem that the Russell-Orenstein account can be stated in a way that avoids troublesome commitment, for it can be given as

‘something is  $F$ ’ is true iff it is sometimes true that  $x$  is  $F$

or

‘something is  $F$ ’ is true iff sometimes,  $x$  is  $F$ .

Since this account does not involve quantification, it may seem that we have provided an account of the quantifier which genuinely frees it from ontological commitment. However, a problem remains. The challenge was to explain non-objectual truth-conditions in a metalanguage which we already understand; otherwise we could follow Van Inwagen and condemn non-objectualism as an unintelligible doctrine. Yet a sentence ‘sometimes,  $x$  is  $F$ ’, or ‘it is sometimes true that  $x$  is  $F$ ’ is not a sentence of English which we antecedently understand, prior to an account of how we understand the expression ‘ $x$ ’. We may understand ‘ $x$ ’ either as a variable which is capable of standing for any of a range of objects or as what Quine would call a ‘schematic letter’ (1986: 49) which may be exchanged for appropriate expressions in the language; but until it has been specified *how* we are to understand ‘ $x$ ’ in ‘ $x$  is  $F$ ’ we do not yet know even whether these truth-conditions are genuinely non-objectual. If the truth-conditions are taken to mean that some occasions on which we assign an entity to the variable  $x$  result in truth, then the quantifier turns out to be ontologically committing after all, for every occasion where the open sentence is assessed for truth or falsity will be one where the variable takes an existing thing as its value.

To avoid misunderstanding, the proponent of the Russell-Orenstein account must stipulate that we understand the expression ‘ $x$ ’ as replaced on different ‘occasions’ by *expressions* of the language, rather than as standing for entities. But this will return us to the old problem of objectual quantification over expression-types, for it will not be possible to explain the way in which the expression ‘ $x$ ’ functions as an ‘undetermined constituent’ without quantifying over expression-types in the course of expressing the view that  $x$  stands in for expressions rather than objects. We may conclude that none of the three non-objectualist strategies — substitutionalism, Prior’s semantics, and the Russell-Orenstein account — can be stated in a manner consistent with the nominalist’s refusal to recognize the existence of types.

### 3. Extensional Inadequacy

Non-objectualism is implausible as an account of quantification over shared features and respects of sameness both because it requires us to accept there could be *true* sentences of the form ‘*a* and *b* are the same in respect of *c*’ in which the name ‘*c*’ lacks a referent, and because the ontological commitment to shared features and respects of sameness is avoided only at the cost of commitment to other entities which are *prima facie* universals — expression-*types*. I shall suggest that a final demerit of non-objectualism is that it is extensionally inadequate: it fails to assign truth and falsity correctly to the quantifications under consideration. One widely-raised problem is the lack of sufficient substituent expressions (Quine 1969a: 105-6; 1986: 93; Teichmann 1992: 8; Haack 1978: 51; Azzouni 2004: 72); another is the claim that non-objectualism cannot make sense of ‘count’ quantifiers like ‘at least three’ (Lewis 1985). Both of these problems apply in the case of quantification over shared features and respects of sameness.

The first problem is that there are not *enough* substitutable expressions to guarantee the truth of quantification over shared features, if this is understood non-objectually. The nominalist will propose that the realist’s analysis

(QS)  $(\exists x)$  *a* and *b* are qualitatively the same in respect of *x*

is to be counted true because of the availability of some substitutable expression — in this case, a property-designator such as ‘wit’ or ‘charm’.<sup>68</sup> As we have seen, there is a problem about how it could be true that *a* and *b* are the same in respect of *wit*, although the subject-expression ‘wit’ names no existing thing. But even if it is granted that this is possible, another problem arises: the language simply does not have enough property-designators to serve as candidates for substitution, for not every respect in which two things might be qualitatively the same is correlated with an English property-designator. For example, two objects might have some property in common which science has not yet isolated and named. In such a circumstance, we would not want the fact of their sameness of type to depend on the availability of a substitutable expression.

Substitutionalism is afflicted by such a problem, for it treats a quantification as depending on the availability of a substitutable expression; it is also damaging to the Russell-Orenstein account, for the advocate of such a position counts an existential quantification like (QS) as true iff the associated ‘propositional function’ is ‘sometimes true’, and we have already seen that this enables the nominalist to evade ontic commitment only if the occasions

<sup>68</sup>For a suggestion of this kind see Schiffer 1987, p.234

on which the undetermined constituent of the propositional function is replaced, not by an *object*, but by an appropriate expression of the language. The Russell-Orenstein account, then, makes statements of sameness of type depend on the availability of a substitutable expression, and is equally vulnerable to the objection from the paucity of available expressions.

Two possible strategies remain for the non-objectualist. One is to adopt Prior's account, according to which the presence of a substitutable expression is merely a sufficient, and not a necessary, condition of the truth of the quantifier; the other is to expand the class of 'available' expressions to include names which the language does not presently have but *could* include, as recommended by Sellars (1980: 8) and Gottleib (1980: 48). I shall argue that neither is satisfactory. In Prior's case, the problem is that he explains the truth of quantifications for which no substitutable expression is available as depending on the existence of some nameless *object* satisfying the indicated condition (1971: 36); in the current situation that would require us to accept the existence of a nameless object to serve as the respect in which two similar things are the same, and respects of sameness are precisely what the nominalist was hoping to do without. The only way to make Prior's account consistent with the rejection of such entities would be to say (as he does not) that a quantification like (QS) could be true although there is *neither* an available substitution-instance *nor* any object meeting the indicated condition. In that case, it would be an utter mystery *when* such a quantification should count as true, and when false. Nevertheless it is clear that some instances of (QS) are true and others false, and the challenge to the nominalist is to say how this can be so if the truth of (QS) does not depend, as the realist alleges, on the existence of an appropriate entity.

What of Sellars' and Gottleib's suggestion, that we expand the class of 'available' substituent expressions to include terms which the language doesn't have, but *could* have? Here the problem is that of 'false positives' — the problem that quantifications over respects of sameness like (QS) would then *all* be counted as true, for even if we currently lack a substitutable expression that two dissimilar objects *a* and *b* may be said to have in common, the language *could* have had one, in which case this expression will be in the class of available expressions and the quantification will count as true. Suppose we follow Schiffer's proposal (1987: 234) of introducing a substitutable property-designator corresponding to every predicate, so from any predicate '*...is F*' we derive an expression 'the property of *F*-ness' which two *a* and *b* will count as 'being the same in respect of just in case they are both *F*'. It is a familiar point that sharing a predicate is not sufficient for sameness of type, so not every expression 'the property of *F*-ness' generated in this

way will mark a genuine sameness of type. If (QS) will be counted as true on the basis of some substitutable expression ‘the property of *F*-ness’ that *a* and *b* share just in case both are *F*, we are in no better state than the Predicate Nominalist in the previous chapter, who wanted to analyze sameness of type as sameness of *predicate*. The problem is that expanding the class of substitutable expressions to include all expressions of the form ‘the property of *F*-ness’ makes (QS), and consequently statements of sameness of type, come out true whenever there is some (actual or possible) shared predicate from which such an expression can be derived, and this fails to restrict (QS) to cases where there is genuine similarity or resemblance between the objects in question, as the analysis requires.<sup>69</sup>

It seems, then, that neither Prior’s nor Sellars’ proposals enable the non-objectualist to evade problems about an insufficiency of substitutable expressions. A second, related problem for extensional correctness is given by the phenomenon of ‘nonstandard’ or ‘counting’ quantifiers. H. A. Lewis argues that we should concern ourselves with more quantifier-expressions in English than simple those introduced by ‘some’ and ‘all’:

‘Not only do we have ‘all’ and ‘some’, but also ‘most’, ‘a few’, ‘one’, ‘several’ and many other expressions that should be counted as quantifiers by any definition that was not artificially restrictive.’<sup>70</sup>

Among these quantifiers we find many that require very exact quantities, for example the ‘mini-triple quantifier’ in

(G) ‘At least three grains of sand are pure silica.’<sup>71</sup>

It is a familiar point that the substitutional interpretation mistakenly counts this as false if there are three or more grains of sand which are made of pure silica, but fewer than three *named* grains of sand which are pure silica. Lewis suggests another problem for accounts which give truth in terms of substitution-instances: since such accounts seem committed to counting substitution-instances rather than objects, they will count *three* substitution-instances, and treat (G) as true, even in the case where there is only *one* grain of sand which is pure silica, but that grain of sand has three names.

A similar problem can be proposed for quantifications involving shared features and respects of sameness, for count quantifiers are used here too, and it is implausible to suggest that what is counted might be substitution-instances rather than entities. When we say that two objects

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<sup>69</sup>See Tomberlin 1990 for the suggestion that Schiffer’s proposal will lead to trivially true ‘false positives’.

<sup>70</sup>Lewis 1985, p.447

<sup>71</sup>Lewis 1985, p.448

‘have three things in common’; or

‘are the same in two respects’

it cannot be claimed that what is counted are substitution-instances. For example, we ought not to agree that two animals are the same in two respects because there are two true substitution-instances, ‘*a* and *b* are the same in respect of *being bipedal*’ and ‘*a* and *b* are the same in respect of *being two-legged*’. The realist can explain why we do not agree in counting two respects of sameness here: it is because both substituted expressions name *the same* feature. But talk of non-identity and identity between features makes sense only on the assumption that such things as features exist. The substitutionalist has no comparable explanation of why being bipedal and being two-legged do not count as two respects of sameness for the purposes of quantifier semantics.

Can this problem be solved by either of the two alternatives to substitutionalism? It seems that it cannot. Prior’s account differs from substitutionalism by making the availability of a substitution-instance sufficient but not necessary for the truth of an existential quantification; therefore it is plausible that Priorian semantics for a count quantifier would make the availability of the correct number of substitution-instances a sufficient but not a necessary condition of their truth. In that case, he too will miscount respects of sameness when there are two or more substitution-instances mentioning the same respect of sameness. Likewise, the Russell-Orenstein account, although it will count occasions of substitution rather than substitutable expressions, seems committed to counting too many ‘occasions’ when the expressions substituted on different occasions are different names of the same respect of sameness.

I have suggested two ways in which non-objectual accounts provide the wrong truth-conditions for quantifications over shared features and respects of sameness. One is that we may lack appropriate substitutable expressions; another that non-objectualist accounts can make no sense of our practice of counting shared features and respects of sameness. These problems suggest that none of the non-objectualist accounts under consideration is a satisfactory account of our quantification over respects in which things are the same.

## Conclusion

The task of this chapter has been to respond to the suggestion that the quantificational truths advanced as a basis for realism are not ontologically committing. We found that the nominalist could not evade commitment to respects of sameness by paraphrase of either kind — either as replacement or analysis. The only prospect for the nominalist

was to embrace a ‘non-objectual’ view of the first-order quantifier, according to which quantification apparently over shared features and respects of sameness can express truth although there exists no entity meeting the associated condition. However, the arguments for non-objectualism are weak, and the realist’s counter-arguments are persuasive. Non-objectualists about quantification over respects of sameness must be willing to embrace a Positive Free Logic which allows truth to be expressed by sentences containing non-referring subject-expressions. Even if that is accepted, there remain the problems that each non-objectual account is itself ontologically committed to expression-types, which seem to be universals, and that non-objectualism is extensionally inadequate, for we can correctly assign truth and falsity to quantifications over respects of sameness only on the supposition that their truth depends on the existence of an appropriate entity. It seems, then, that non-objectualism is untenable with regard to the quantifications on which the realist makes his case. Such quantifications are ontologically committing after all; therefore we may conclude that respects of sameness and shared features exist. The only task left for the realist is to show that these shared features are a *sui generis* metaphysical kind — universals — rather than being reducible to some other kind of entity. That is the business of the next chapter.

### 3. Doing without Universals?

If he has been convinced by the argument up to this point, the nominalist will no longer deny the ontological significance of idioms involving quantification over ‘features objects have in common’ or ‘respects in which things are the same’, for he will accept that the idioms in question are indispensable, and that the quantification they employ is quantification *over* a domain of existing entities. However, to make such a concession is not yet to capitulate to realism about universals. The realist urges us to accept *two* claims: first, that such entities as respects of sameness and shared features exist; and second, that these entities constitute a *sui generis* metaphysical category — that of *universals*. A nominalist might accept the former, *ontological* claim, that entities exist over which we quantify, while rejecting the latter, *descriptive* claim, that such entities are examples of the *sui generis* metaphysical kind, *universal*. In such a circumstance, the nominalist is under obligation to give an account of what respects of sameness are, if not universals. The thesis of this chapter is that the nominalist has no satisfactory alternative account of these entities; if no such account can be given, the realist theory of universals is the only remaining acceptable hypothesis.

Our current position might be described like this: we have good reason to believe that there exist entities which ‘play the property-role’ (Oliver 1996: 11), but we have not yet established that these entities must be universals. Caution should be exercised in the use of the word ‘property’. It might seem a conveniently neutral term to pick out the entities, whatever they are, that we speak of when we describe the features that objects share; nevertheless, we should be aware of the disadvantages of this way of talking. First, talk of ‘properties’ may allow us to lose sight of what role, precisely, these entities are needed to play. The entities of which the nominalist owes us an account are those entities whose sharing makes for sameness of type between the objects which share them; but it is not to be assumed that everyone will understand the term ‘property’ as picking out an entity whose sharing is constitutive of similarity in this way. One example is the practice, widespread in formal semantics and elsewhere, of associating a ‘property’ with any collection of things that can be specified, whether or not they exhibit sameness of type. Indeed, the word ‘property’ may be used unreflectively as a term for the *set* containing these individuals.<sup>72</sup> Another example is Lewis’ distinction between ‘natural’ and ‘unnatural’ properties: while ‘natural’ properties are the ones ‘whose sharing makes for resemblance’ (1983: 192), the ‘unnatural’

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<sup>72</sup>This way of talking has a distinguished philosophical pedigree; for example Goodman’s ‘Seven Strictures on Similarity’ (1970: 443ff.) contains an argument about the number of properties two things must share which only goes through on the assumption that ‘properties’ are sets.



properties may be shared by objects which have nothing in common with each other. The current challenge to the nominalist is to give an account of ‘natural’ properties, for these are the ones needed in the analysis of sameness of type.<sup>73</sup> In Lewis’ idiolect, to demand an account of ‘properties’, without the stipulation that we mean *natural* properties, is to misstate the challenge facing the nominalist.

Second, ‘property’ contrasts with ‘relation’. Informally, a property combines with only one object in each of its instances, while a relation must occur standing between — ‘relating’ — two or more objects. The realist’s challenge is to account for relations and properties both, as respects of sameness may be relations just as easily as they are properties — for example, when two *pairs* of things (*a, b*) and (*c, d*) are said to be the same in some respect on account of *a* and *b* standing in the same relation in which *c* and *d* also stand.

A final disadvantage to talk of ‘properties’ is that it allows us to forget that there are more cases of sameness of type than are comfortably described by means of property-talk. For example, two physical books might exhibit sameness of type if both are copies of *War and Peace*. Although the realist will describe this sameness of type as a case of two individual books instantiating the same universal, it is not clear that a pre-theoretical description of the situation would mention a *property* which both books have, the property of *being War and Peace*. The worry is that a metaphysician who concentrates only on *properties* will fail to attend to such cases of sameness of type in favour of the more straightforward cases where ‘property’ marks what we would be happy to describe as a ‘quality’ or ‘attribute’ of the thing in question, for example *redness* or *triangularity*. For these reasons it would be preferable not to formulate the challenge to the nominalist as that of accounting for ‘properties’, save for the fact that no convenient locution exists to replace the term. In the discussion that follows, ‘property’ is used for brevity’s sake alone; it is not intended that we should ignore the need to account for *every* case in which two things might be said to share a ‘common feature’ or ‘be the same in some respect’.

The debate between nominalism and realism is sometimes presented as the attempt to adjudicate between realism and a range of competing ‘nominalisms’ (Armstrong 1978a; 1989a). If we grant the realist his ‘ontological’ claim, that there exist entities that play the role of shared features and respects of sameness, then which of these nominalisms

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<sup>73</sup>Lewis later suggests (1986: 61) that naturalness may come in degrees, so there may be properties that are neither wholly natural nor wholly unnatural. From the current perspective, whether the nominalist owes us an account of the ‘less-than-perfectly natural properties’ as well as the ‘perfectly natural’ ones will depend on whether ‘less-than-perfectly natural’ properties are such that sharing them makes for sameness of type. Some, at least, will not, for some are ‘disjunctive’ (Lewis 196: 86), and consequently could be shared between objects which exhibit no sameness of type.

have been excluded? We can no longer be Ostrich Nominalists, insofar as the Ostrich Nominalist refuses to accept the ontological claim that things which exhibit sameness of type thereby have something in common. It might seem that Predicate Nominalism is better placed, for the Predicate Nominalist can *accept* that two *F* things have something in common — namely, the predicate ‘...is *F*’. This fails for reasons familiar from the preceding chapters. Sharing a predicate in common is neither necessary nor sufficient for sameness of type, since there can be samenesses of type for which we have no descriptive predicates, and descriptive predicates which can be shared in the absence of sameness of type. For that reason, Predicate Nominalism does not seem capable of providing an account of what we quantify over when we quantify over respects in which things are the same.

Certain flavours of Resemblance and Trope Nominalism are also ruled out by acceptance of the realist’s ‘ontological’ claim. The Resemblance Nominalist accounts for the sameness of type between two *F* objects in terms of resemblance between those objects and other *F* objects, or in terms of resemblance between those objects and a paradigm *F* object. This theory may take resemblance as a primitive, and fail to recognize entities which are the *respects* in which things resemble each other; if it does it will be incompatible with the analysis proposed in Chapter 1, that sameness of type is always sameness *in some respect*. A Resemblance Nominalist who rejects this analysis cannot explain why our ascriptions of qualitative sameness and difference interact as they do; for that reason a theory of this kind should be rejected. For similar reasons we should reject the naïve trope theory which recognizes only ‘particularized properties’ such as the *F*-ness of *a* and the *F*-ness of *b*. If *a*’s *F*-ness trope is a distinct entity from *b*’s *F*-ness trope, then strictly speaking *a* and *b* do not share any common entity. If the trope theorist is to take advantage of our proposed analysis of sameness of type, he must supplement his ontology of particularized properties with a theory of the entities which are *shared* between objects exhibiting sameness of type. The task for the nominalist is to perform what Quine described as an ‘explication’ (1960: 258) of respects of sameness, shared features, and types — i.e. to say what these entities are if they do not constitute a *sui generis* ontological category of universals. This will be a matter of, in Quine’s words,

‘systematically fixing upon some suitable already recognized object[s]... with which to identify [them].’<sup>74</sup>

The most plausible candidates for this task are *sets*. The nominalist will say that, when *a* and *b* have something in common, what they have in common is a set. But which set? One

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<sup>74</sup>Quine 1960, p.258

problem for the set-theorist will be to decide what kind of entities these sets are composed of. He might accept a traditional ‘Class Nominalist’ account, and identify the property of *F*-ness with the set of all *F* things. Alternatively, if he finds tropes attractive, he might say that the property of *F*-ness is the set of *F*-ness tropes. On this view, for *a* and *b* to have something in common is for there to be some set containing a trope belonging to *a* and a trope belonging to *b*: *a* and *b* have this set ‘in common’ in the sense that each contributes a trope to it. The choice between sets of particulars and sets of tropes constitutes one major axis of decision for the nominalist. Where it is important not to prejudice the decision, I shall talk of objects as being ‘associated’ with a set; this should be taken to mean that the object in question either is a member of that set or contributes a trope to it.

A second axis of decision is how the nominalist proposes to distinguish those ‘natural’ sets which make for sameness of type from the ‘unnatural’ sets which do not. Such a distinction is necessary because of the role sets have to play in the analysis of qualitative sameness. The nominalist proposes to enlist sets rather than universals to serve as the entities we quantify over when we quantify over *respects* in which objects are the same. But objects may share membership of many sets while failing to exhibit qualitative sameness in *any* respect, since there is a set corresponding to *every* collection of things, no matter how dissimilar those things are. In a world containing *n* objects, there will be  $2^n$  of what we might call ‘first-order’ sets — non-empty sets which do not have other sets as members. Any given object will be a member of half these sets, so in a world containing *n* objects, each object will be a member of  $2^{n-1}$  sets; moreover any two things will share membership of  $2^{n-2}$  sets, or a quarter of all first-order sets.<sup>75</sup> But it should not be a consequence of our theory of ‘respects of sameness’ that every two objects must be the same in as many respects as any other two objects, or that it is impossible that there be two objects which are the same in no respect at all — especially if we are giving an account of ‘degrees of resemblance’ in terms of the *number* of respects in which things are the same, as Rodriguez-Pereyra suggests (2002: 67), for then necessarily every two things will resemble each other to precisely the same degree as every other two things. To avoid his analysis being falsified by results of this kind, it is desirable for the nominalist to make a distinction between ‘natural’ sets, which are to be counted as respects of sameness, and ‘unnatural’ sets, shared membership of which is irrelevant to the question of sameness of type.

The same problem afflicts those who would account for respects of sameness in terms of sets of *tropes* which belong to the objects in question. Not every set of tropes can be identified as a ‘respect of sameness’, for two entirely dissimilar objects may each contribute

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<sup>75</sup>See Goodman 1970, p.443 for this result.

a trope to such a set without thereby exhibiting sameness of type. We should not say that *a* and *b* exhibit sameness of type merely because there is a set containing *a*'s *F*-ness trope and *b*'s *G*-ness trope, for in such a situation *a* and *b* may in fact be entirely dissimilar. Again, the trope theorist must give some account of what distinguishes 'natural' sets of tropes, which make for sameness of type between the objects which contribute tropes to them, from 'unnatural' sets of tropes which are irrelevant to the question of sameness of type.

An account of what differentiates natural from unnatural sets may proceed in one of three directions. David Lewis briefly considers that idea that natural sets are those which correspond to genuine universals: the universals could serve to 'pick out' the relevant natural sets, then 'retire... and leave their jobs to the natural properties [sets]' (1983: 192). To accept this theory would be to capitulate to realism, since it requires that we accept universals as well as sets; for that reason it requires no response from the realist. A second strategy, proposed by Anthony Quinton (1957), is that naturalness be taken as primitive, or more precisely, that the predicate '...is a natural property' should be admitted to the theory although it is claimed not to permit of further analysis. Theories of this kind may be called 'Primitive Naturalness Views'. A final option is to present a theory of naturalness in terms of resemblance: those sets which are 'natural' are those whose members meet some criterion of resemblance. The 'Resemblance Class Nominalist' will say that the natural sets are those whose members resemble each other, or resemble some suitable paradigm object, to a particular degree — perhaps simply resemble each other more than they resemble anything which isn't a member of that set.<sup>76</sup> The 'Resemblance Trope Nominalist' will say that the natural sets are those containing tropes which resemble each other to a sufficient degree. A plausible account is that a natural set is one containing *exactly* resembling tropes — the idea is that my mug shares a common feature with yours when the colour-trope *the redness of my mug* is an exact match for the colour-trope *the redness of your mug* (Williams 1953: 117; Armstrong 1989a: 122).

Contrasted with realism about shared features and respects of sameness (the view that these entities are universals) are four nominalist theories of shared features as sets. The distinction between them is given by the answers the nominalist gives to the two questions, what kind of thing do the sets have as members; and what is it that marks the natural sets off from the unnatural? We could tabulate the options:

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<sup>76</sup>Beyond this rough characterization, the resemblance nominalist's proposal will become rather more complex. See Rodriguez-Pereyra 2002, p.56ff for detail, and below, section on Resemblance Class Nominalism, for criticism.

**'Primitive Naturalness' views**

<b>Primitive Class Nominalism</b> Properties are sets of particulars	<b>Primitive Trope Nominalism</b> Properties are sets of tropes
<b>Resemblance Class Nominalism</b> Properties are sets of resembling particulars	<b>Resemblance Trope Nominalism</b> Properties are sets of resembling tropes

**'Resemblance' views**

In this chapter I argue that none of the four theories is a satisfactory alternative to realism about universals. First, I consider problems which threaten *any* attempt to replace universals with sets; then I explain difficulties faced by the two 'primitive naturalness' views, Primitive Class Nominalism and Primitive Trope Nominalism; finally I suggest reasons why Resemblance Class Nominalism and Resemblance Trope Nominalism are unsatisfactory.

## General Problems for Properties as Sets

Some objections apply to *any* attempt to employ sets to do the work of universals in discourse about sameness of type. One is that it is doubtful whether an ontology of sets in fact *preferable* to a world of universals. The replacement of universals by sets requires some modification to the theory embodied in everyday discourse about shared features and respects of sameness, for we do not usually treat objects as *members* of the features they exhibit, nor do we consider characteristics or qualities of objects to *contain* the objects that exemplify them, as sets contain their members. If there is no good reason to prefer sets to universals, surely it is better not to embark on such a revision of everyday discourse about properties. Second is the ‘Modal Objection’ that sets cannot do the work of properties because a given property could have been instantiated by different objects, while a set could not have had members other than the ones it has. Finally, there is the ‘Coextension Problem’: the problem that there may be two properties where there is only one set, for example when all the same things exhibit the properties *being renate* and *being cordate* (having a kidney and having a heart).

### (i) Are Sets preferable to Universals?

There are two ways in which a realist might give expression to a suspicion that sets are every bit as ‘ontologically disreputable’ as universals. One is to suggest that, instead of giving an account of universals in terms of sets, we should reverse the direction of reduction and give an account of sets in terms of universals. The ontological economy is the same in either direction, for the end result is one ontological category where before there were two; the realist’s claim is that we should prefer this one category to be universals rather than sets. An alternative strategy is to argue that sets, insofar as they are intelligible, are entities so similar to universals that the ontological cost of recognizing universals in addition to sets is negligible; the proposed reduction of universals to sets confers so little benefit that it is hardly worth doing at all. I shall suggest that, while the first strategy cannot succeed, the second presents a real challenge for the nominalist.

The first of these strategies, the reduction of sets to universals, has a vociferous defender in John Bigelow. The proposal is that membership of a set by some objects should be understood as the instantiation of some universal by all and only the ‘members’ of that ‘set’. This universal will be an essential property of each of the members of the set (Bigelow 1990, 1993); quantification *apparently* over sets will be reconstrued as quantification over these universals. Such a suggestion enables the realist to take advantage of an economy of

ontological categories without abandoning universals, by rejecting sets instead.

Bigelow's suggestion will not be congenial to a realist who wants universals to serve as respects in which things are the same, and features which similar objects have in common. In order for them to play such a role, universals must be *sparse*: we can afford to recognize a shared universal only where there is genuine sameness of type between objects. If we agree to provide a universal corresponding to each set, as Bigelow suggests, there will be a universal shared by every collection of objects, for any collection of objects will make up a set, regardless of whether these objects exhibit sameness of type. Bigelow appears to take a relaxed view about the abundance of his universals, and is happy to accept that the universal corresponding to each a set is 'something [the set's members] share in common' (1993: 83). But in that case it would no longer be possible to give an account of sameness of type as the sharing of a common universal, for even collections which exhibit no sameness of type will share a universal in common; worse, we would know *a priori* that such a collection shared a common universal, for we know *a priori* that they make up a set. It is not possible to adopt Bigelow's suggestion while retaining the analysis of sameness of type in terms of shared universals.

If there is little prospect of removing sets from our ontology in favour of universals, the realist can adopt a second approach: it may be argued that an ontologist who accepts sets has already made so many concessions that he has nothing to lose by accepting universals as well. The argument is a form of *tu quoque*: it is pointed out that all of the features of a theory of universals to which the nominalist might object are also present in a theory of sets; therefore the benefit of eliminating talk about universals in favour of property-discourse is illusory.

First, universals are criticized because they violate the 'laws of thinghood' (Quinton 1957: 44) in their capacity either to be wholly present in many different places simultaneously, if 'aristotelian', or to lack spatiotemporal location altogether, if 'platonic'. Yet similar criticisms may be levelled at sets themselves. Where is a set composed of some spatiotemporal objects? If sets are 'platonic', lacking any location whatsoever, then they are every bit as strange as platonic *universals* which lack location; alternatively if sets are 'aristotelian', located where their members are, then it seems that we have entities which, like aristotelian universals, are wholly present in different locations at once. It would be strange, at least, to say that *part* of a set is located with each of the set's members, for a set is commonly supposed to be one indivisible thing to which its members bear the set-membership

relation, rather than a whole that has parts.<sup>77</sup> On the question of location, then, it seems that universals are no more contentious than sets.

A similar charge can be made with regard to epistemology, for the epistemology of sets may be claimed to be in no better shape than that of universals. Realists are sometimes accused of presenting a theory for which no adequate epistemology can be given, on the grounds that universals are 'causally inert'. Precisely the same accusation can be levelled at the set theorist, for any working set theory must recognize 'pure' sets constructed by recursive operations on the empty set  $\emptyset$ , for example  $\{\emptyset\}$ ,  $\{\{\emptyset\}\}$ , and so on. Such sets, which have no spatiotemporal objects as members, exist necessarily, and as such may be claimed to be isolated from the causal order of things. Indeed, the realist's epistemology can be claimed to be in *better* shape than that of the set theorist's, for the realist who believes in universals as features which objects share, or respects in which they are the same, can claim that we are aware of these features of objects in perception. No comparable claim can be made for an ontology of 'pure' sets.

Finally, realism may be accused of dealing in mysteries because it takes the relation of *instantiation* as a theoretical primitive which cannot be explained in other, more basic terms. But precisely the same problem arises with the set-membership relation itself, for this too must be taken as a primitive in the nominalist's theory. Worse still, although it can plausibly be claimed that the notion of *instantiation* is already present in common-sense discourse about objects 'having' features, qualities, or characteristics, there is no easy way to explain the set-membership relation in terms of everyday notions of 'collections' or 'combinations', for an account in terms of 'combinations' does not enable us to understand why there should be a difference between an object *a* and the object's singleton set  $\{a\}$ , as set theory demands (Lewis 1991: 30).

This *tu quoque* suggests that the nominalist who accepts sets has nothing to lose by accepting universals as well, for once he has overcome the reluctance to admit entities which violate the normal constraints on location, which are epistemologically controversial, and whose relationship with particulars must be taken as a theoretical primitive, there remains scant reason why he might object to an ontology of universals. The only benefit to which the nominalist can appeal is the ontic economy of getting by with one category of thing (sets) where before he had two (sets and universals). But it is not clear to what extent an economy of kinds of entity can justify the revision of common sense that is required when we replace universals with sets. Perhaps mere economy is no longer enough to allow

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<sup>77</sup>However, see Lewis 1991 for an audacious attempt to reduce the axioms of set theory by means of the part-whole relation and the axioms of mereology.



the nominalist's set-theoretic proposal to count as an obvious improvement on discourse about universals.

## (ii) The Modal Objection

The Modal Objection is as follows: properties cannot be sets because sets and properties differ in their modal characteristics. The property *F*-ness could have been instantiated by different things, or by nothing at all, but the set of *F* things could not have had different members, because the identity of a set is determined by its members; therefore the property *F*-ness cannot be identical with the set of *F* things (Wolterstorff 1970: 180; Armstrong 1989a: 27).

Responses to this objection come in two forms. First, it might be alleged that the problem arises only if properties are to be reduced to sets of *particulars*, and does not appear if we use sets of *tropes* instead (Armstrong 1989a: 128-9). Second, it may be claimed that the problem is solved by embracing Modal Realism, the view that this world is one of countless existing 'possible worlds' of the same kind as our own.<sup>78</sup> If the property of being *F* is a set containing all the actual *and possible F* things spread across worlds, then the set does not change its members depending on which of these many worlds happens to be the actual one. It may be granted that Modal Realism solves the current problem; however, it is not clear that the promise of an ontology free from universals is sufficient to motivate the astonishing hypothesis of Modal Realism, nor is it likely that the massive quantitative increase in ontology that it requires could be justified by other theoretical benefits that might accrue elsewhere (Melia 1992). For those reasons, the theorist should not appeal to Modal Realism to save his theory of properties-as-sets.

What of the claim that this problem does not arise for properties-as-sets-of-*tropes*? It does not seem that this can be maintained. The identity of any set depends on it having the members it does. If one of those members did not exist, neither would the set. So we need only imagine a counterfactual situation in which the set of *F*-ness tropes would not have existed, yet the property *F*-ness *would* have existed, in order to make the theory of sets-as-tropes vulnerable to the Modal Objection; if the modal characteristics of the property *F*-ness differ from those of the set of *F*-ness tropes, the two are not the same. Here is one such situation. Suppose there are only two things *a* and *b*, and each has an *F*-ness trope. Then the property *F*-ness is to be identified with the set of the two tropes {the *F*-ness of *a*, the *F*-ness of *b*}. But *b* could have failed to exist. In that counterfactual

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<sup>78</sup>The idea originates with David Lewis (1986, p.51), but has recently been endorsed by Rodriguez-Pereyra (2002, p.99)

situation, the trope, the *F*-ness of *b*, would not exist, and so nor would the set {the *F*-ness of *a*, the *F*-ness of *b*}. Yet the property *F*-ness clearly would still exist, even if only one thing was *F*. It seems that neither adopting Modal Realism nor appealing to trope theory is an appropriate response to the Modal Objection.

### (iii) The Coextension Problem

The third difficulty is the Coextension Problem: the problem that two properties may be different yet instantiated by the very same things. The identity-conditions for sets are extensional: if all and only *F*s are *G*s, then the set of *F*s is identical with the set of *G*s. Yet we should not conclude on that account that *F*-ness is the same property as *G*-ness — a position that seems obligatory if we accept the reduction of universals to sets.

The problem is solvable for *contingent* coextension using the resources of Modal Realism, for if it might not have been the case that all and only *F*s are *G*s, then (says the Modal Realist) there are some possible individuals which are *F*s but not *G*s or *vice versa*. If the properties of *F*-ness and *G*-ness are identified with sets of actual and possible *F*s and *G*s respectively, then they will fail to be coextensive and the claim of non-identity between *F*-ness and *G*-ness may be upheld (Lewis 1986: 51). This is only a partial solution so long as we believe that there are distinct properties that are necessarily coextensive; such properties will be coextensive in every possible world, and so will be associated with one and the same set of *possibilia*. Alternatively, the problem may be solved by the trope theorist, for even if all and only *F*s are *G*s, each of these objects will have a distinct *F*-ness trope and a distinct *G*-ness trope. If the properties *F*-ness and *G*-ness are sets of *F*-ness tropes and of *G*-ness tropes respectively, then each set will have different members (one set containing *F*-ness tropes, the other containing *G*-ness tropes), and so the claim of non-identity between the two coextensive properties may be upheld (Campbell 1981: 134).

The trope theorist, then, has a way out. Can the traditional Class Nominalist avoid this problem without adopting the desperate excesses of Modal Realism? At first glance it seems he can. Set theory provides the resources to generate a large number of *different* sets from precisely the same starting elements:  $\{a, b, c\}$ ,  $\{\{a, b, c\}\}$ ,  $\{\{a\}, \{b\}, \{c\}\}$ ,  $\{\{a\}, b, c\}$ , and so on. Why not, given two coextensive properties, identify one of the properties with the set of objects that have the property —  $\{a, b, c\}$  — while the other property is identified with some other set formed from the same starting elements — for example,  $\{\{a, b, c\}\}$  or  $\{\{a\}, \{b\}, \{c\}\}$ ?

Such a solution has parallels in other devices that the Class Nominalist might adopt. One

is the treatment of relations. A relation, say *being to the left of*, is accounted for as a set of ordered pairs  $\{ \langle a, b \rangle, \langle c, d \rangle, \langle e, f \rangle \dots \}$  such that the first member of each ordered pair is to the left of the second member of the pair. But then some account must be given of ordered pairs themselves. An ordered pair  $\langle a, b \rangle$  cannot simply be the set  $\{a, b\}$ , since sets do not impose any order on their members. The problem is solved by construing the ordered pair  $\langle a, b \rangle$  as a set-theoretic construction, either the original Wiener device of  $\{\{a\}, \{b, \emptyset\}\}$  or the so-called ‘Wiener-Kuratowski’ device  $\{\{a\}, \{a, b\}\}$  (Quine 1960: 258-9). In these accounts, relations are identified with ‘higher-order’ sets — sets of sets — rather than with sets of objects.

Another parallel is the theory of ‘structured’ sets developed by David Lewis to give a set-theoretic account of necessarily coextensive properties and propositions (1986: 56). Here the suggestion is that, if we need to make a distinction between necessarily coextensive properties such as triangularity and trilaterality, we should identify them with ordered pairs (‘structured’ properties), set-theoretic constructions from ‘unstructured’ properties which are construed simply as sets of objects. Since there may be more than one ‘structured’ property generated from each collection of objects, there are enough structured properties to assign a different set to each of the necessarily coextensive properties. Again, the problematic properties are identified with higher-order sets rather than sets of objects.

It is a pressing question for the Class Nominalist whether it is legitimate to identify properties and relations with higher-order sets in this way. If it is, then the Coextension Problem no longer threatens, but if such devices are not acceptable, then it is not possible for the Class Nominalist to give an adequate account even of relations, for no unstructured set will be capable of preserving the ordering of the pairs standing in a given relation. The problem with identifying properties and relations with higher-order sets is that the identification is extremely arbitrary. It seems up to us whether we account for ordered pairs using the Wiener-Kuratowski device  $\{\{a\}, \{a, b\}\}$  or Wiener’s suggestion  $\{\{a\}, \{b, \emptyset\}\}$ ; likewise, in the suggested response to the Coextension Problem, it is up to us *which* of the two coextensive properties is identified with the set  $\{a, b, c\}$ , and which with the set  $\{\{a, b, c\}\}$ . Such freedom of choice may seem incompatible with the suggestion that properties and relations really are sets. This is the attitude taken by Armstrong, who concludes that

‘Although these classes may be fitted to *represent* a relation between *a* and *b*, yet they do not seem to *be* that relation.’<sup>79</sup>

The thought is this: if we are choosing a means to *represent* relations in set-theoretical

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<sup>79</sup> Armstrong 1989a, p.32

terms, then it does not matter that the precise manner in which that representation is accomplished is left up to us. But if the claim is that relations really *are* the set-theoretical constructs proposed, how could it be that it is a matter for free choice which of the possible accounts is correct? We just do not get to determine the nature of reality on a whim. Such an objection is exactly parallel to that raised by Benacerraf in opposition to a set-theoretic reduction of numbers: since it is possible to effect such a reduction in several ways, and the choice is entirely up to us, it cannot be claimed that the reduction reveals what the disputed entities are *really like*, but at most provides a way of modelling the real situation (Benacerraf 1965).

Can Armstrong's objection be answered on behalf of the Class Nominalist? It might be thought that we impose too heavy a burden on a theory of properties by demanding that there be nothing arbitrary in the decision about *which* sets are employed. The Class Nominalist may respond that the possibility of *many* different theories which use sets rather than universals does not affect his main point, that we *can*, and therefore *should*, recast our theories in such a way as to do without universals. Just such a position is adopted by Quine in *Word and Object*: arbitrary choice of this kind is to be tolerated because we are replacing our ordinary common-sense theory with a new, different, account, which accomplishes '[the] same purposes through other channels' (1960: 260). We do not attempt to show what a kind of entity is 'really' like; instead, we suggest other candidates which might serve our purposes equally well.

Such a response cannot be sustained, for two reasons. One depends on considerations about the wider role these sets are intended to play; the other questions the Class Nominalist's ability to give a consistent account of 'having a property' in terms of set membership, if some properties are construed as 'structured'. First, the 'property-role' for which sets are candidates is such that sharing a property makes for similarity or 'sameness of type'; an important feature of this role is that whether or not some objects share a common property is *not* something that is up to us. Properties, whatever they are, should be features of objective reality, not dependent on the whim of the observer. There is a danger that a Class Nominalist account makes the sharing of properties dependent on our whim in just this way. If it is down to us *which* sets we identify as the natural properties and relations, then in an important sense it is *up to us* whether two objects share a property, for we can rob an object of a property simply by changing which set we identify as the property in question. Allowing an arbitrary element into the choice of set-theoretic reduction has the consequence that whether two objects exhibit sameness of type may depend on the whim of the theorist, and this is incompatible with the role properties were introduced to play:

that of grounding the objective similarities between objects.

Secondly, the adoption of arbitrary devices to deal with recalcitrant properties in terms of higher-order sets risks making the Class Nominalist account of 'having a property' unworkable. The original proposal was that '*a* has the property *F*' should be reconstrued as '*a* is a member of the set which is the property *F*'. But in the case of 'structured' properties which are higher-order sets, this simple translation rule cannot be employed. In some cases, having such a property will be a matter of being a member of the set that is the property; in other cases it will be being a member of a member of that set; in other cases, where ordered pairs are involved, to have the property may be to be a member of some set containing the object itself plus the empty set, which in turn is a member of some set which is a member of the property itself. The original appeal of Class Nominalism was the easy way in which a realist theory could be translated into one which does without universals. The procedure was simple: for 'property', read 'set', and for 'has the property...' read 'is a member of the set ...'. Once structured properties are admitted, such a straightforward translation cannot be achieved; indeed it is doubtful whether *any* systematic account of having a property, and hence of sameness of type, can be given. Unless the Class Nominalist can give a systematic explanation of how to reconstrue property-talk in set-theoretic terms, it seems that the legitimacy of the entire Class Nominalist project is in doubt.

Three problems threaten any attempt to send sets to do the work of universals. The first is that sets and universals are so similar that there may be no gain in ontic economy achieved by repudiating universals while recognizing sets. Another is the 'Modal Objection', that sets and properties differ in their modal characteristics. This can only be avoided by endorsing the unpalatable hypothesis of Modal Realism. Third is the problem of coextensive properties. Although this problem can be avoided by the Trope theorist, the Class Nominalist's attempt to solve it reveals an even greater difficulty: Class Nominalist accounts must appeal to 'structured properties' which are higher-order sets. This introduces an unacceptably arbitrary element into the choice of set-theoretic reduction; further it prevents the Class Nominalist from giving a systematic account of 'having a property' in terms of set membership. These problems are serious enough by themselves; combined with the difficulties I shall outline for each of the nominalist's four specific proposals, they strongly suggest that the attempt to account for shared features and respects of sameness in terms of sets is a hopeless endeavour.

## Problems for Primitive Naturalness Views

We have seen that only ‘natural’ sets can be candidates for role of shared features or respects of sameness; if *every* set were a shared feature then any collection of objects whatsoever would count as sharing some feature, and thereby as exhibiting sameness of type. Primitive Naturalness Views are those which claim the naturalness of some sets (and the unnaturalness of others) as a primitive notion; more precisely they claim that the predicate ‘...is a natural property’ does not permit of analysis or elucidation in other terms. We must grant each theory the right to have some primitive predicates; to demand that every predicate be definable in terms of other predicates would only result in circular definitions, for no theory can be stated without employing *some* predicates (Lewis 1983: 199).

One question for proponent of a Primitive Naturalness View is whether we can understand such a predicate as ‘...is a natural property’ sufficiently well to treat it as a primitive. If it is legitimate to treat naturalness as a primitive notion, one which receives no *explicit* definition within a theory, this can only be because it is a notion which we already understand in some way. We could not introduce a predicate ‘*F*...’ as a theoretical primitive *unless* it could be meaningfully employed in the absence of explicit definition; the result is that the nominalist’s right to appeal to naturalness as a primitive depends on whether we have some pre-theoretic understanding of what it is for a property to be ‘natural’.

It may be doubted whether we do in fact have such an understanding. Naturalness is introduced as a property of *sets*; but most (perhaps all) of what we know about sets is derived from the axioms of set theory; how then could it be claimed that we have some intuitive understanding of the difference between natural and unnatural sets, such that the predicate ‘...is a natural property’ could be introduced to our theory without explicit definition? One way to explain the notion is to say that natural sets are those which make for sameness of type between their members. But then it seems that it is not naturalness, *per se*, but rather *sameness of type*, that is being introduced as a primitive, for the former notion is being explained in terms of the latter. As we saw in Chapter 1, it is undesirable to take sameness of type as a primitive, unanalysed notion, for then we lack an understanding of the logical interaction between sameness of type and difference of type.

Beyond these worries about the acceptability of primitive naturalness itself, two difficulties afflict views that treat naturalness as a primitive. One is the need for the nominalist, consistently with the analysis of sameness of type he has now accepted, to recognize an entity — the property of *naturalness* — to which the predicate ‘...is a natural property’

corresponds. This property, I shall argue, threatens to violate the axioms of set theory; therefore it seems that the nominalist needs at least one property that is not reducible to a set. The other is the question how similarities between objects themselves can be determined by a feature of some set with which the objects are associated. How can a property of one thing — a set — be responsible for the similarities exhibited by objects which are members of it? I examine these difficulties in connection with each of our two Primitive Naturalness Views in turn.

**Primitive Class Nominalism:  
Properties as sets of particulars**

Someone might think that taking the predicate ‘...is a natural property’ as a primitive excuses the nominalist from recognizing an entity, the property of *naturalness*, to which that predicate corresponds. This would be a mistake. If we are to take advantage of the proposed analysis of sameness of type in terms of entities shared in common between same-typed objects, then it is necessary to recognize a shared entity wherever there is genuine sameness of type. Since the naturalness predicate marks a genuine respect in which sets may be qualitatively the same, the nominalist should introduce an entity to serve as the respect in which the natural properties are the same. The need to recognize a naturalness property generates a serious problem for Primitive Class Nominalism.

It is important not to misunderstand the position. It is not claimed that a naturalness property should be introduced because every predicate must be accompanied by a property to serve as its ontological correlate, for it cannot be assumed against the nominalist that predicates function to introduce properties; nor should a naturalness property be introduced as a truthmaker for the naturalness of the sets, since a naturalness property does not fulfil the ‘truthmaking’ role of ensuring, merely by existing, that the natural properties are natural. Instead, the reason for recognizing a property of naturalness is that we recognize a respect of sameness or shared feature everywhere there is objective sameness of type, and the natural properties exhibit objective sameness of type *qua* being natural. The sets which are identified with the natural properties *having mass m* and *having charge c* have something in common with each other which they fail to share with unnatural sets containing random collections of objects. The nominalist has already conceded that, where there is sameness of type, there is an entity shared between the same-typed individuals; moreover, it seems impossible to deny that all the natural properties exhibit a particularly important kind of sameness of type, namely, that each of them makes for resemblance between the individuals that exhibit it. Therefore, the nominalist is compelled to recognize

an entity which is the respect in which all the natural properties are qualitatively the same. Just as he recognizes an entity which is the respect in which any other collection of similar things are the same, consistency demands that he accept the higher-order property of *naturalness*.

Why is this a problem for the Class Nominalist? He claims that properties are sets containing the individuals that exhibit the feature in question. So the property of *naturalness* is itself a set containing all and only the things that exhibit that property. Since naturalness is exhibited only by *sets*, the naturalness property — call it *NP* for short — will be a set whose members are the natural sets. But *NP*, since it was introduced to account for the sameness of type between natural sets, must also be natural. In other words, *NP*, the naturalness property, is self-instantiating. Since *NP* is the set of natural sets, and *NP* is itself a natural set, *NP* will be such that  $NP \in NP$  — a result which is forbidden by the Axiom of Foundation in standard ZFC set theory.<sup>80</sup> If properties are sets, they must be sets based on a different axiom-set from that provided by ZFC.

It might be claimed that the violation of ZFC does not render Primitive Class Nominalism untenable. Axiomatic set theories have been developed which reject the Axiom of Foundation, and so permit the formation of self-membered sets like *NP*. It has been suggested that the Axiom of Foundation itself derives from an outdated conception of a set as ‘an empty box  $\{\}$  that can be used for collecting together some things’ (Barwise 1984: 21). Instead we should adopt a view in which ‘sets arise from independently given structured situations by dropping the structure’ (1984: 21). If a set is thought of as a container of things, it is hard to see how we could imagine a container which contains itself. There is considerably less difficulty in imagining a self-instantiating set as a kind of model of, or abstraction from, a self-involving situation. Sets which are not bound by the Axiom of Foundation are known as hypersets, and it is to these the Primitive Class Nominalist will turn.

This appeal to hypersets should not be permitted, for two reasons. One is that conceiving of sets as ‘abstractions’ derived from structured situations, rather than entities which contain their members, is one more concession towards the view that sets are no different from universals. Once we abandon the idea of a set ‘containing’ its members, there seems even less reason to think that sets might be a radically different kind of entity from universals, in which case there is correspondingly less impetus to abandon realism in favour of a theory of properties as sets. Second, the theorist leaves a hostage to fortune in making his theory

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<sup>80</sup>The axiom states that any non-empty set *A* must contain an element *B* that is disjoint from *A* — in other words that *A* and *B* must have no members in common. But suppose that *A* is the set  $\{B\}$ , which is a set by the Axiom of Pairing, and that  $B \in B$ . Then the intersection of *A* and *B* will be *B* and not  $\emptyset$  as the Axiom of Foundation demands.



depend on the correctness of *one* of many competing kinds of axiomatization, namely the kind that lacks the Axiom of Foundation. It does not seem possible to accept that the plurality of incompatible axiomatizations may all be equally correct: for the set nominalist who does not want an ontology containing many different kinds of set, it is surely necessary to hold that there is one true axiomatization to be found, and that the others describe the nature of sets incorrectly to a greater or lesser degree. But then how can he be sure that the ‘correct’ account of sets will be compatible with the existence of hypersets? The nominalist can only claim an advantage in ontological economy if he reduces universals to a kind of thing we need in our ontology anyway — in this case, the sets required by mathematics, semantics and other formal disciplines. There is no guarantee that *these* sets will turn out to permit the violation of the Axiom of Foundation.

Two other possible Class Nominalist responses seem unsatisfactory. One is to say that properties are not sets, whose behaviour is constrained by the axioms of ZFC, but rather *classes*. (I understand the distinction between sets and classes in this way: sets must obey the axioms of set theory, but classes need not. A class is any collection whose members all satisfy a given condition. All sets are classes, but not all classes are sets.) The disadvantage with this approach is that, in abandoning the strictly axiomatized region of set theory, and entering the jungle of *classes*, we find ourselves dealing with a kind of entity that can no longer be claimed indispensable for other disciplines. An account of properties as sets offers ontological economy because sets are needed already in fields such as mathematics and formal semantics; but the need for *sets* does not justify the admission of the unruly mass of classes, some of which may even violate the law of non-contradiction. For example, the ‘Russell set’  $\{x : x \notin x\}$  is sometimes claimed to be a ‘class’ although it is not a genuine set.

Another approach would be to claim that properties are not sets, but rather *aggregates* of the things which exemplify them. But this fails as an account of properties and relations in general, for membership of an aggregate (e.g. a heap) is transitive whereas set-membership is not. For that reason, any number of sets can be built up out of the same initial elements, whereas only one aggregate is possible which includes all the same elements. But then the theory of properties-as-aggregates lacks the resources to make the necessary distinctions between  $\{a, b, c\}$ ,  $\{\{a\}, \{a, b\}\}$ ,  $\{\{b\}, \{b, c\}\}$  and so on, which are needed to supply sets to replace relational universals, and to solve the Coextension Problem.

The need to recognize a naturalness property *NP* is a serious problem for the Primitive Class Nominalist, for it presents him with a dilemma. On the one hand, if *NP* is *not* identified with the set of all natural properties, then the nominalist is left with one property

which he cannot reduce, and which presumably must be admitted to be a genuine universal. On the other, if *NP* is the set of natural properties, the nominalist can no longer offer the ontic economy which would accrue if properties could be reduced to the sets which obey the axioms of ZFC, and which are employed in other formal disciplines. The need to recognize a property of naturalness presents a compelling objection to the Primitive Class Nominalist.

A second difficulty for Primitive Naturalness Views is the question of how the naturalness of a *set* can be expected to make a difference to the sameness of type exhibited by its members. How does the naturalness of one thing (the set) result in the sameness or resemblance exhibited between *different* things (its members)? A set is an entity distinct from its members; how then can a fact about its members, namely the sameness of type they do or don't exhibit, depend on a fact about the set itself, i.e. whether the set has the naturalness property *NP*?

A metaphor makes the problem clearer. Suppose we think of sets in the traditional fashion as boxes which contain their members. Some but not all of these boxes will contain objects that all resemble each other in some respect. The Primitive Naturalness theorist seems committed to saying that whether or not the contents of a given box all resemble each other in this fashion depends on something about the box — namely, whether the box is 'natural' or not. But then why should any feature or characteristic of the *box* make a difference to the characteristics of the things inside it? The box is merely a container. Likewise, we might think, a *set* is merely a container. Why should a feature of the set make a difference to the similarity exhibited by the things it contains?

This problem has affinities with Lewis' so-called 'mighty biceps' objection. Lewis rejects Armstrong's account of laws of nature as higher-order necessitation-relations between universals because he cannot see any reason why such a necessitation-relation should mandate a necessary connection, other than the fact that the relation has been named 'necessitation' by Armstrong. Of course, merely calling something a necessitation-relation does not make it capable of entering into necessary relations,

'any more than one can have mighty biceps just by being called  
"Armstrong"' .<sup>81</sup>

The analogy is this: in our case we wonder why some sets are natural properties, ones which make for sameness of type between their members, and we are told it is because these sets are 'natural'. But merely naming these sets 'natural properties' does nothing to

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<sup>81</sup>Lewis 1983, p.214

explain why this is so; and we cannot get by without an explanation, because *prima facie* there is no reason at all to suppose that a feature of a set — its naturalness — could in any way affect the resemblances between the objects it contains as members.

Armstrong also raises difficulties of this kind against Class Nominalism. In particular, he asks why we should expect membership of a set to *determine*, rather than *be determined by*, the features of an object (1978a: 36; 1989a: 27), and why membership of a set should be expected to make a difference to the causal powers of things, insofar as those causal powers depend on properties of the object, and those properties are determined by the object's membership of particular sets (1989a: 28). In light of the current discussion, we can see that the focus of these criticisms should be adjusted: the question is not whether membership of a set determines, or is determined by, features of the object itself, for the sets exist whenever their members do, regardless of the features exhibited by their members, and so nothing about the features of an object can be said to depend on its *membership* of one or another set. The question is rather how, and whether, the *naturalness* of one of these sets can make a difference to its members. In the absence of any response from the Class Nominalist, it may seem that the direction of explanation should be reversed: instead of saying that objects can be described as sharing features in common because the set to which they belong is natural, we should say that a group of objects constitute a natural class precisely because they all exhibit some feature in common. In this picture, like the suggestion of Lewis' considered earlier, natural classes are determined by features of objects, and these features are universals.

We may conclude that Primitive Class Nominalism is unacceptable for two reasons. One is that it has no satisfactory account of the higher-order naturalness property *NP*; the other that we lack an explanation of how resemblance between objects could be determined by a characteristic of a *set* that contains them, namely the 'naturalness' of that set. It remains to be seen whether a Primitive Naturalness account can fare better if respects of sameness and shared features are construed as sets of tropes.

### **Primitive Trope Nominalism: Properties as Sets of Tropes**

The Primitive Trope Nominalist will say that two things *a* and *b* 'have something in common' with each other because there is a natural set containing a trope belonging to *a* and a trope belonging to *b*. The limitation to natural sets is necessary because not every set of tropes makes for sameness of type: for example, the set containing my redness and

your sphericity does not make for qualitative sameness between us. Here I shall argue that the Primitive Trope Nominalist can escape one of the problems raised for the Primitive Class Nominalist — the need to recognize a naturalness property *NP* — although trope theory has no good answer to the other — the problem of how a characteristic of a *set* can make a difference to the resemblances between its members. For that reason Primitive Trope Nominalism is unsatisfactory.

The trope theorist says that properties are sets of tropes. For reasons familiar already, it is necessary to recognize an objective distinction between natural and unnatural sets of tropes, and hence to recognize the higher-level naturalness property, *NP*, which is the respect in which all natural properties are the same. However, this does not lead to paradox as it did for the Primitive Class Nominalist. Consistency with his own theory requires that the trope theorist treats *NP* as a set of naturalness tropes, one contributed by each natural property. Since *NP* is itself a natural property, it must contribute a naturalness trope of its own. Here the Primitive Class Nominalist fell into difficulty because accepting that *NP* is a natural property has the result that *NP* is a self-membered set; no such problem afflicts the trope theorist, who can appeal to the distinction between tropes and the sets that contain them. To say that *NP* is self-instantiating is not to say that *NP* is a set containing itself, for the trope theorist says that *NP* contains tropes contributed by the natural properties, rather than containing the natural properties themselves. So *NP* — like every other natural property — contributes a naturalness trope to the set of naturalness tropes. The consequence is that *NP* at once *contains* and *contributes* one and the same trope, but this is not an obviously impossible situation, for there does not seem to be any reason why, given an ontology of sets and tropes, there cannot be a set *NP* and a trope  $t_{NP}$  such that  $t_{NP} \in NP$  although  $t_{NP}$  is instantiated by *NP*. For that reason it seems that Primitive Trope Nominalism can escape the first of our objections.

The second objection is genuinely problematic. We are told that objects resemble each other as a result of contributing a trope each to a ‘natural’ set; it is supposedly the naturalness of the set that makes the difference between the objects’ resembling and not resembling each other, for when the tropes belong to an unnatural set there need not be any resemblance between the objects that contribute them. Again, the question arises, *how* does a feature of a set — its ‘naturalness’ — translate into resemblance between the objects that contribute tropes to that set? Indeed, the problem is even worse for the trope theorist than it was for the Primitive Class Nominalist. If a property is a set of objects, then the naturalness of the set has to translate into resemblance between its members. But if a property is a set of *tropes*, then the naturalness of a set must not only translate into some feature of the

tropes themselves, but must then be transmitted *again* to the objects themselves so that the objects resemble each other. There are, as it were, two degrees of separation between the naturalness of the set and the resemblance of the objects, for this 'naturalness' (whatever it is) must make a difference to objects which are not even contained within the set, but merely contribute a trope to it.

It may be replied that the trope theorist can reduce the strangeness of the position. Suppose that the resemblance between objects is held to depend on exact resemblance between one or another of their tropes. Then all the naturalness of a set must do is make it the case that the *tropes* it contains exactly resemble each other. The resemblance of the objects that contribute those tropes would then need no further explanation. Again, it strikes us that this may get things the wrong way round. Surely the naturalness of a set of tropes is a *consequence* of the resemblance between the tropes it contains. Just as in the case of Primitive Class Nominalist, to suppose otherwise is to propose a situation where a feature of a box or container — the naturalness of the set — must be supposed to determine characteristics of its contents — the resemblance between the tropes it contains. How could this be so? If this line of thought is persuasive, the nominalist should abandon his attempt to treat the naturalness of a set as primitive, even when the sets in question are construed as sets of tropes.

## Problems for Resemblance Views

The difficulties considered so far suggest that there is no satisfactory set-theoretic account of properties which takes naturalness as a primitive. The alternative is to attempt to explain the naturalness of a set in terms of resemblance between its members. ‘Natural’ sets — those which play the role of *respects* in which things are the same — will be sets of objects or tropes, such that their members resemble each other to a sufficient degree. Two problems threaten accounts of this kind. One is the suggestion that explaining the ‘naturalness’ of properties in terms of resemblance is incompatible with the analysis proposed in Chapter 1, according to which resemblance or ‘sameness of type’ is always to be understood as resemblance in respect of some natural property. To adopt both the current account of naturalness in terms of resemblance and the account of resemblance in terms of the sharing of natural properties is to indulge in viciously circular explanation. A second difficulty is that it is not even *possible* to give an account of naturalness solely in terms of resemblance, for accounts of naturalness in terms of resemblance will mistakenly count ‘imperfect communities’ as natural properties, and wrongly reject genuinely natural properties when they happen to be ‘companion’ sets. These two problems are fatal to Resemblance Class Nominalism, the view that natural properties are sets of resembling *particulars*. Resemblance Trope Nominalism, which treats properties as sets of resembling *tropes*, can escape the Companionship and Imperfect Community difficulties. However, I shall argue that it is guilty of the charge that it cannot take resemblance between tropes as a primitive *and* enjoy the benefits of the analysis of sameness and difference for which properties were introduced in the first place.

### **Resemblance Class Nominalism: Properties as Sets of Resembling Particulars**

In Chapter 1, I urged that we should not adopt a traditional Resemblance Nominalism, which gives an account of sameness of type employing only particulars themselves and the predicate ‘...resembles...’. Such a theory would be unsatisfactory because, if we represent resemblance or ‘qualitative sameness’ with a dyadic predicate, we lack a satisfactory explanation of the logical interaction of qualitative sameness and difference; this explanation is achieved only if we allow that qualitative sameness and difference are always sameness and difference *in some respect*.

It might seem that the same objection could be levelled at a sophisticated Resemblance Nominalism that recognizes properties as sets constructed from mutually resembling

objects.<sup>82</sup> Here the case is not so clear. Such a Resemblance Nominalist *agrees* that there are entities that play the role of respects of sameness, or features shared in common between objects; he may also agree that the quantificational idioms associated with sameness of type — when we say that similar things ‘have something in common’ — should be treated as quantification over a domain of entities; he even acquiesces in the analysis proposed in Chapter 1, according to which sameness of type, resemblance, and similarity are always sameness *in some respect*. The additional claim made by the Resemblance Class Nominalist is that the entities in question are not universals, but sets: ‘natural’ sets whose naturalness depends on the resemblance between their members. The charge against such a theorist will be that his account of resemblance is circular: that he gives an account of resemblance in terms of natural properties, but then gives an account of the naturalness of those properties in terms of resemblance. Can such a theory be sustained?

It does not seem that the Resemblance Class Nominalist can avoid the accusation of circularity by dropping his account of resemblance as the sharing of some or another natural property. To do so would leave him vulnerable to the same objection I raised against the traditional Resemblance Nominalist, that he has no satisfactory explanation of qualitative sameness and difference. Nor is it feasible to abandon the explanation of naturalness in terms of resemblance, for doing so would require that he treat naturalness as a primitive notion, with all the difficulties that entails. There is one further option for the Resemblance Class Nominalist: he can claim that the accusation of circularity depends on conflating two separate requirements: one, that the nominalist agree that resemblance or ‘sameness of type’ is always sameness *in some respect*; the other, that the nominalist give a non-trivial account of resemblance itself. The former is the requirement that we treat resemblance as a three-termed relation, with the third term reserved for the *respect* in which the qualitatively similar things are the same; the latter is the demand for an analysis of resemblance in terms that do not presuppose the notion. While the former requirement is a consequence of the account proposed in Chapter 1, it may seem that the latter demand cannot be sustained, since it is unreasonable to deny the Resemblance Nominalist the admission of resemblance as a primitive; to do so would be to rule out Resemblance Nominalism from the start. It may be claimed that the charge of circularity depends on mistakenly advancing the second of the two requirements, when only the first is justified by the argument up to this point.

I shall suggest that this defence on behalf of the Resemblance Class Nominalist cannot

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<sup>82</sup>Such a view is considered by Armstrong (1989a: 31ff) and defended at length by Rodriguez-Pereyra (2002).

succeed. The problem is that, in accepting that resemblance is a three-termed relation, with one term being the property that is shared, the nominalist also accepts that resemblance *depends on* the sharing of such a property. The explanation for the resemblance between two things is given by saying which natural property they share. But then the Resemblance Class Nominalist *also* makes the naturalness of a set depend on the resemblances that hold between the members of that set. So the Resemblance Class Nominalist is committed to saying *both* that resemblance depends on shared natural properties *and* that the sharing of a natural property depends on resemblance. Just as Euthyphro was infuriated to find that he could not hold both that (i) what is holy is so because it is approved by the Gods and that (ii) what is approved by the Gods is so because it is holy,<sup>83</sup> so the Resemblance Class Nominalist cannot hold both that (i) *a* and *b* resemble each other because they share a natural property and that (ii) *a* and *b* share a natural property because they (and the other members of the set in question) resemble each other. Resemblance Nominalists take the notion of resemblance as fundamental; but as soon as we accept the argument of Chapter 1 we must regard resemblance not as fundamental, but as depending on the sharing of natural properties or ‘respects of sameness’. It seems that the objection of circularity should be allowed to stand.

Our first objection, then, presents a genuine difficulty for Resemblance Class Nominalism. A second objection is that it is not possible to give necessary and sufficient conditions for the naturalness of a property in terms of resemblance alone. The previous argument suggested that giving an account of naturalness in terms of resemblance involves the Resemblance Class Nominalist in vicious circularity; the current problem is that no such account of naturalness in terms of resemblance can in fact be given. Two problems of this kind were proposed by Nelson Goodman — the so-called ‘Companionship’ and ‘Imperfect Community’ problems.<sup>84</sup> These difficulties are well-known, and widely believed to be fatal to Resemblance Class Nominalism. However, Rodriguez-Pereyra (2002) has recently argued that they are soluble. The task at hand, then, is to consider whether this new solution succeeds. I shall argue that, although his approach solves the Imperfect Community difficulty, Rodriguez-Pereyra becomes involved in a vicious circularity while dealing with the Companionship difficulty; the accusation is that his account of naturalness in terms of resemblance depends on a prior understanding of what it is to be a natural property.

The *Companionship* difficulty arises when we realize that the account of naturalness in terms of resemblance must be supplemented by a ‘maximality condition’. The resemblance

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<sup>83</sup>Plato, *Euthyphro* 10e-11a

<sup>84</sup>Goodman 1966, p.160ff



nominalist cannot say that just any set of mutually resembling things constitutes a natural property, for it would be undesirable to count the set containing all but one of the *F* objects as natural simply on the grounds that all its members resemble each other *qua* being *F*. For that reason, it is necessary to stipulate that natural properties shall be ‘maximal’ sets of resembling objects, where a set is understood as ‘maximal’ iff nothing outside the set resembles all the members of that set. Under this definition, the set of all but one of the *F*s does not count as maximal, and therefore is not a natural property, because one thing outside that set — the sole excluded *F* object — resembles every member of the set.

Unfortunately, the maximality condition which is needed to rule out sets which contain all but one of the things exhibiting sameness of type in a certain respect *also* mistakenly excludes genuinely natural sets from counting as natural properties. The Companionship difficulty arises in any situation where the things exhibiting one respect of sameness — call them the *F*s — are a subset of the things exhibiting another respect of sameness — call them the *G*s. In this case, it turns out that the set of the *F*s is not maximal, and so is not a natural property. Each member of the set of *F*s is also a *G*, and being a *G* makes for resemblance, so each member of the set of *F*s resembles each of the *G*s which lie outside the set; therefore the set of *F*s is not a maximal set, and so does not count as a natural property. This shows that the proposed criterion of resemblance is not *necessary* for naturalness, as the set of *F*s constitutes a natural property but is not counted as one by the resemblance nominalist’s criterion. The problem becomes pressing when we realize that it arises for all cases when natural determinate properties have natural determinables. All scarlet things resemble each other; but they also resemble all red things. So the class of scarlet things is not maximal, and so is falsely counted unnatural by the resemblance criterion.

The *Imperfect Community* difficulty shows that mutual resemblance is not *sufficient* for naturalness, since we can construct classes of individuals such that each resembles another in some respect, but there is no respect of sameness that they all share. Such a set should not be counted as a natural property, for there is no one feature or respect of sameness shared between them, yet the criterion of resemblance *does* count it as natural. In the case where there are only three individuals *a*, *b*, and *c*, we can construct a set of this kind using only three respects of sameness. Let *a* and *b* resemble each other by being *F*, *b* and *c* resemble each other by being *G*, and *a* and *c* resemble each other by being *H*. Then each individual resembles each other individual, yet there is no one thing they have in common. To generalize the case to larger communities of individuals, we only need as many respects of sameness as there are individuals, since the problem arises just in case each individual in the set lacks some property that the other individuals share.

These difficulties combine to show that an account of the naturalness of sets in terms of resemblance between the individuals which make up those sets will fail to pick out the correct sets as the natural ones. It will wrongly select imperfect communities such as those imagined by Goodman, and unfairly reject determinate properties merely because their extension is a subset of the extension of some determinable. How might the nominalist respond to these charges? The most prominent defender of a resemblance account of naturalness has been Rodriguez-Pereyra. In the remainder of this section I set out his responses to Goodman's difficulties, and argue that they do not ultimately succeed.

Rodriguez-Pereyra responds to the two difficulties as follows. In the case of the Companion-ship difficulty, he claims that the problem can be solved by reformulating the Maximality requirement in terms of two other notions: *degree* and *perfection*. A 'perfect' community is a set such that there is some property shared by all of its members (2002: 170), while a perfect community of *degree*  $n$  is one whose members all resemble each other to at least degree  $n$  (2002: 181). The degree to which two particulars resemble each other is given by the number of properties they have in common; therefore the degree of a community may be identified with the number of properties such that each of these properties is exemplified by every member of that community. The Maximality requirement can then be stated using the following definition of maximality: A perfect community  $\alpha$  is *maximal* iff  $\alpha$  is a perfect community of degree  $d$  and it is not the case that  $\alpha$  is a subset of a perfect community of degree  $d$  (2002: 182). This emendation of the Maximality condition does not unfairly prevent the set of  $F$ s from counting as a natural property when the set of  $F$ s is a subset of the set of  $G$ s, for the set of  $F$ s has (at least) two properties shared between all its members, which are at once  $F$ s and  $G$ s, while the set of  $G$ s contains some members that are not  $F$ s, and so there is only one property shared between all the members of this set. In Rodriguez-Pereyra's account of *degree*, this means that the  $F$ s form a set of degree 2, while the  $G$ s form a subset of degree 1, since they only have *one* property shared between all members of the set. Since the set of  $F$ s is a community of a different degree from the set of the  $G$ s, the set of  $F$ s is maximal under the new definition; it is not a subset of a perfect community of the *same* degree.

The Imperfect Community difficulty is solved trivially, as a result of Rodriguez-Pereyra's definition of *perfection*. A *perfect* community is one such that there is some property which all its members share. If we stipulate that *perfection* is a necessary condition for naturalness, then no imperfect communities will be wrongly counted as natural properties, since an imperfect community is one where there is no property shared between all its members, and such a community does not count as 'perfect' under Rodriguez-Pereyra's definition.

But a problem threatens Rodriguez-Pereyra's project: the worry that his definitions are circular. The resemblance nominalist's task is to formulate an account of naturalness in terms of resemblance, to evade the difficulties associated with taking naturalness as a theoretical primitive. But if the definition of naturalness in terms of resemblance appeals to the notion of (natural) properties in order to get the correct results for the criterion of resemblance, it seems that naturalness is *presupposed* rather than explained by the theory.

Rodriguez-Pereyra does not, of course, take the notion of an object's having a natural property to be primitive; if he did he wouldn't have done anything to face the challenge to say what properties are without taking them to be *sui generis* universals. A perfect community, he says, is one whose members all share at least one property in common, but what it is for a community to 'share a property' is in fact this: a resemblance-relation holds, not only between each two things in the class, but also between each two *pairs* of things in the class, and between each two pairs of pairs in the class, and between each two pairs of pairs in the class, and so on up to infinity (2002: 170). Imperfect communities do not 'share a property', in Rodriguez-Pereyra's sense. Consider the community where  $a$  and  $b$  are  $F$ ,  $b$  and  $c$  are  $G$ , and  $a$  and  $c$  are  $H$ . Although it is true that each two members of this community resemble each other, it is *not* true that any two *pairs* of members of the community resemble each other. For example, it is not true that the pairs  $\{a, b\}$  and  $\{b, c\}$  resemble each other, because one is a pair of things which are both  $F$ , the other is a pair of things only one of which is  $F$  (2002: 166).

Let it be granted that this solves the Imperfect Community difficulty in a non-circular fashion, without making prior appeal to the notion of a natural property to ground the structure. However, there remains a problem of circularity in Rodriguez-Pereyra's account of naturalness in terms of resemblance. Although the account of 'perfection' in terms of resemblance ensures that there will be no imperfect communities which are wrongly identified with natural properties, the Resemblance Nominalist cannot claim to have given an account of natural properties *until* he has solved the Companionship problem as well, for without modification the Maximality condition will unfairly rule out some genuinely natural sets from counting as natural properties. Rodriguez-Pereyra's modification to the Maximality condition requires that we know what it is for a community to be a perfect community of degree  $d$ . Since the degree of a perfect community depends on how many natural properties are shared by all its members, this requires that we know what it is for a community to share one or more natural properties in common; but we cannot know what it is for this to be the case until we know what it is for a set to be a natural property, and we cannot know what it is for a set to be a natural property until we know how to

specify the Maximality condition so as not to rule out of consideration ‘companion’ sets. This long-winded charge of circularity can be put more simply: the proposed response to the Companionship difficulty relies on a prior understanding of what it is to be a natural property at the same time as it attempts to give an *account* of what it is to be a natural property. For this reason, we may conclude that Rodriguez-Pereyra fails in his attempt to give an account of naturalness in terms that do not presuppose the notion, and the Companionship difficulty continues to present an insuperable obstacle to the replacement of *sui generis* universals with sets of particulars unified by resemblance between their members.

### **Resemblance Trope Nominalism: Properties as Sets of Exactly Resembling Tropes**

The fourth and final option for the nominalist is to construe natural properties as sets, not of resembling objects, but of resembling tropes. A *natural* property, on this account, will be a set of tropes each of which exactly resembles every other trope in the set; thus a natural property will be an ‘equivalence class’ under the symmetrical, transitive and reflexive relation of exact resemblance.<sup>85</sup> This account avoids the Companionship and Imperfect Community difficulties as follows. No imperfect community is counted as a natural property, as when the community is imperfect there is no kind of trope such that all the members of the community have a trope of that kind — i.e. there is no *F* such that all members of the community share an *F*-ness trope. Therefore, on the assumption that only tropes of the same kind can exactly resemble each other, there is no set of exactly resembling tropes to which every member of the community contributes a trope, and hence no property, as we would expect. The Companionship difficulty is solved with comparable ease. Since *F*-ness tropes are distinct entities from *G*-ness tropes, the set of *F*-ness tropes will be a different set from the set of the *G*-ness tropes, for these sets will have different members. Therefore *F*-ness is counted as a different property from *G*-ness even if all *F*s are *G*s. The distinctness of the sets is guaranteed by the distinctness of the tropes that are their members.<sup>86</sup>

We may draw the conclusion that the Resemblance Trope Nominalist can avoid one of the difficulties that threatened Resemblance Class Nominalism, for he can give an account of naturalness in terms of resemblance between tropes which is not susceptible to Goodman’s problems. Can he also evade the charge of circularity? Again it might be urged that the

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<sup>85</sup>See Armstrong 1989a, p. 41

<sup>86</sup>For these results see Campbell 1981, p.134

nominalist is involved in viciously circular explanation, for he explains the naturalness of a given set as depending on resemblance, yet is committed (by the account of sameness of type for which properties were needed in the first place) to the view that resemblance between objects depends on the naturalness of the 'properties' (sets) with which they are associated. We might allege that the Resemblance Trope Nominalist, no less than his Class Nominalist counterpart, makes resemblance depend on shared features, but at the same time makes sharing a feature depend on resemblance. However, the analogy with the situation of the Resemblance Class Nominalist goes no further, for in the current theory features shared between objects are said to depend on resemblance between *tropes* of those objects. It may seem that this provides the means to escape the charge of circularity; I shall argue that this appearance is illusory, and Resemblance Trope Nominalism is genuinely guilty of the charge.

The Resemblance Trope Nominalist does not commit the *obvious* circularity of making resemblance between objects depend on the naturalness of a set of which they are members, while also making the naturalness of a set depend on the resemblance between its members. Instead, resemblance between objects is explained in terms of the naturalness of a set to which they contribute a trope, and since the naturalness of a set depends on the resemblance between the tropes that are its members, it can be seen that the resemblance between objects depends ultimately on the resemblance between their tropes.

Nevertheless, a difficulty remains for the theory. The nominalist recognized respects of sameness in the first place to solve a problem about resemblance or similarity. We were originally confronted with the *explanandum*, how is it that sameness of type and difference of type between objects are compatible with each other? The Resemblance Trope Nominalist suggested that this may be answered by accepting the realist proposal that resemblance between *objects* is always resemblance in respect of some shared feature, characteristic or property, but denying that 'shared features' are universals; instead he claimed that they are sets of exactly resembling tropes. By making resemblance depend on the sharing of properties, but giving an account of properties as sets of tropes, he intended to take advantage of the analysis proposed by the realist without having to accept an ontology of universals. But surely this trope-theoretic account of resemblance merely postpones the problem, rather than solving it. The same *explanandum* we encountered in the case of resemblance between *objects* recurs with regard to 'sameness of type' or resemblance between tropes, for the current theory takes resemblance between *tropes* as a primitive, even if it acquiesces in the realist analysis of resemblance between *objects* as always 'resemblance in some respect'. Our original question arises again: how is it possible

for two tropes  $t_1$  and  $t_2$  to resemble one another (in one respect) and yet fail to resemble one another (in another respect)? The very same *explanandum* recurs for higher-order resemblances between tropes that the introduction of ‘properties’ (sets of tropes) was intended to explain. Therefore it seems that the Resemblance Trope Nominalist’s account of ‘properties’ does not give the explanation of qualitative sameness and difference that properties were introduced to provide.

The trope theorist will respond that there is a crucial disanalogy between the resemblance that holds between objects and the resemblance that holds between tropes. Tropes, we are told, exhibit *perfect* or *exact* resemblance to each other (Armstrong 1989a: 121). In that case there will be no need to account for the possibility that two tropes may resemble each other in some respect, yet fail to resemble one another in a different respect, for there will be no such thing as two tropes that resemble each other partially — any two tropes will resemble each other ‘exactly’ or not at all. If this is right, the trope theorist can claim to have solved the problem, by making the problematic notion of partial resemblance, or ‘resemblance in some respect’ depend on the entirely unproblematic notion of ‘exact’ resemblance, of which no explanation is required.

However, it is doubtful that we can make sense of the idea that the resemblance between tropes is ‘perfect’ in such a way that they can never be said to be the same (in one respect) and yet different (in another). It may be that two tropes *of the same kind* can be said to exactly resemble each other; nevertheless they will *partially* resemble tropes of *different* kinds, for example in the case where an *F*-ness trope is dissimilar to a *G*-ness trope in point of its *F*-ness, although both tropes are similar insofar as both are of the same metaphysical kind, namely *tropes*. So the problem recurs: challenged to account for partial similarity between objects, the Resemblance Trope Nominalist posits sets of tropes unified by resemblance to serve as *respects* in which things are the same, yet the resemblance between the *tropes* themselves may also be partial, in which case our original *explanandum* remains: how is it possible for two tropes to resemble one another (in one respect) and not resemble each other (in another respect)? The theorist cannot afford to recognize *respects* in which tropes resemble each other, for consistency will demand that these respects are treated as sets of higher-order tropes which are instantiated by the resembling first-order tropes, and an infinite regress threatens when we realize that the resemblance between higher-order tropes will in turn require us to recognize *respects* in which those tropes can be the same or different. But if resemblance between tropes is taken as primitive, the theorist will encounter precisely the same logical difficulty as we found with resemblance between objects themselves, and which we were attempting to explain by positing properties. We

may conclude that the Resemblance Trope Nominalist succeeds only in transferring the problem of explaining partial similarity, from partially similar objects, to partially similar tropes. Therefore such a theorist cannot claim the benefits of an analysis of the kind proposed in Chapter 1.

## Conclusion

If the theorist is to explain what shared features and respects of sameness are without invoking an ontology of universals, he needs to give an account of these ‘properties’ in terms of *sets*, either of tropes or of objects themselves. Three problems threaten *any* such endeavour: these are the question whether universals are any less reputable than sets, the Modal Objection and the Coextension Problem. In addition, there are good reasons why the nominalist should find each of the four flavours of set theory distasteful. Views that take naturalness as a primitive must address the problems (i) that some account is required of the property *NP*, *being a natural property*, in set-theoretic terms; and (ii) that we lack a good explanation of how a property of a *set* — its naturalness — could be responsible for the resemblances between the things it contains. Both of these are genuine problems for Primitive Class Nominalism, while Primitive Trope Nominalism can answer the first, but not the second, and for that reason should not be accepted. Turning to views that account for naturalness in terms of resemblance, we encounter two different problems. One is the suggestion that an account of naturalness in terms of resemblance would give incorrect necessary and sufficient conditions for being a natural property; the other that the theorist is involved in vicious circularity if he agrees to analyse resemblance as the sharing of a natural property, but then attempts to explain naturalness in terms of resemblance. Again, both difficulties are damaging to the Resemblance Class Nominalist, while the Resemblance Trope Nominalist is affected only by the second. Nevertheless, this in itself is enough to make Resemblance Trope Nominalism unacceptable as an alternative to realism. It has already been observed that adoption of any of the four nominalist options under consideration would involve us in some revision of ordinary discourse about sameness of type, for when we talk about the ‘features’, ‘characteristics’ or ‘properties’ shared between objects we do not consider these properties to *contain* the objects themselves or their tropes. Moreover, there are good reasons to think that each of the theories is unacceptable as an account of the entities that play the role of features objects share and respects in which they are the same. In the absence of any other suggestion about how to explain the nature of shared features and respects of sameness, it is reasonable to conclude that these entities are universals after all.

## 4. Predicates and Universals

We might distinguish two questions concerning the hypothesis that universals exist. One is, is there any good reason to believe it? The preceding chapters have suggested that this can be answered in the affirmative: universals are needed to serve as the respects in which things are qualitatively the same: the *features* they have in common or the *characteristics* they share. The second question is, is there any good reason to believe that universals do *not* exist? I shall suggest that two pressing difficulties threaten the theory of universals: first, the problems associated with the thesis that predicates refer to universals, which (I shall argue) is an inescapable corollary of realism itself; and second, the family of difficulties subsumed under the name ‘Bradley’s regress’. This chapter deals with the problems of predication, while chapters 5 and 6 outline a realist response to Bradley and his progeny.

The view that predicates refer to universals might seem a default position for the realist. Since antiquity a common approach has been to frame the question of universals as the question whether we should recognize a kind of entity that is introduced or picked out by predicates or general terms. Thus Aristotle defines a universal as ‘that which is of such a nature as to be predicated of many things’.<sup>87</sup> For Abelard, writing around the start of the 12th century, the question of universals is the question of what, if anything, is designated by a ‘universal word’<sup>88</sup> (general term); moreover, the question at issue is how to explain ‘the ability of universal words to refer to things in general’. In the modern debate, Frege introduces his ‘concepts’ specifically to serve as the referents of predicates (1892a: 45), while the tradition survives in the attitude of Strawson, Wiggins and Dummett, that the best reason for introducing universals to our ontology is to explain facts about the logical behaviour of predicates — whether this is to give an account of the subject-predicate distinction (Strawson 1959: 172), to explain quantification into adjective position (Wiggins 1984: 315), or to explain how the elements of a sentence unite to form a judgeable content (Dummett 1981: 246, 256).

Apart from the weight of tradition, there are three good reasons, given that we believe in universals, to accept the further thesis that predicates refer to them. One is the need to make sense of English idioms of higher-order quantification; another to account for the apparent equivalence in content between sentences employing a dyadic predicate (‘...is wise’) and those expressing a relation between an object and one of its characteristics (‘...has wisdom’); third is that a subject-predicate sentence must be viewed as introducing

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<sup>87</sup> Aristotle, *De Interpretatione* 17a38. My translation.

<sup>88</sup> Abelard 1992, p.30. So too following quote.



a universal in order to explain how it succeeds in describing a world composed of both particulars and universals. I discuss these reasons in more detail in a subsequent section. However, the view that predicates refer ('predicate-reference') faces serious objections: the lack of sufficient universals to serve as referents for *every* predicate; Frege's problem about 'the concept *horse*'; and the problem of accounting for 'the unity of the proposition'. This situation constitutes an antimony for the realist: it seems that realism about universals entails predicate-reference, and it seems that predicate-reference is untenable. If the realist cannot overcome the difficulties associated with predicate-reference, he may have no option other than to abandon realism itself. The aim of this chapter is to show how the realist may deal with these problems; the conclusion is that we may agree that predicates refer to universals without fear that our theory is untenable.

## What is Predicate-Reference?

It is important to be clear precisely what we claim when we assign referential function to predicates. Wary of over-committing themselves, realists have generally avoided saying outright that predicates ‘refer’ to universals, instead selecting a convenient euphemism. Thus Strawson says that predicates ‘introduce’ (1959: 168) or ‘specify’ (1974b: 17) universals; Armstrong prefers to say that universals ‘satisfy’ (1978b: 59) predicates; while Wright replaces reference with a different relation, ‘ascription’ (1998: 87). Attempts have been made to sanitize Frege’s project by offering translations of *Bedeutung* (‘reference’) which do not require that a predicate which has *Bedeutung* is thereby related to some extra-linguistic entity (Tugendhat 1970); on the English side of the translation, Dummett (1981: 245) has suggested that ‘reference’ as a technical term in Frege’s philosophy may be construed as ‘semantic role’ rather than by analogy with the ‘name-bearer relation’. The one lesson we might draw from all this manoeuvring is that confusion will attend any claim that predicates refer unless more is said about what is intended by such an assertion. Our first task, then, is to clear up precisely what we accept if we accept predicate-reference.

First, and most importantly, the reference we attribute to predicates should be construed as a relation between a predicate and a distinct, existing entity — a universal. Dummett vacillates over whether a construal of ‘reference as semantic role’ requires us to treat reference as essentially involving a relation to a non-linguistic entity, or whether a predicate can count as having ‘reference’ in this sense merely by making a contribution to the truth-conditions of sentences in which it occurs.<sup>89</sup> I take it that the philosophically interesting versions of predicate-reference theory are those according to which reference is a relation between a predicate and an entity. We can all agree – even nominalists – that predicates play a certain role in semantics; reference must be conceived as more than merely making a semantic contribution if the doctrine that predicates have reference is to be controversial at all.

One element of Dummett’s presentation deserves emphasis. It is surely correct to insist, as he does, that reference should be a *semantic* relation, in the sense that the truth or falsity of the content expressed should depend, at least in part, on which entity is referred to. If predicates refer to universals, then *which* universal is referred to should play a role in

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<sup>89</sup>For example, see Dummett 1981, p.523: ‘If we assume no more about reference than that it constitutes the semantic role of an expression, then we have as yet no entitlement to assume... that the semantic role of each expression can be specified by the association to it of an *entity* of one kind or another’; yet elsewhere he talks as though ‘reference’ and ‘having a referent’ are one and the same, even when reference is construed as semantic role. See Dummett 1981, pp.245-6, and below.

determining whether what the sentence says is true or false. For example, it should be part of the explanation of the truth of 'a is red' that the predicate '...is red' refers to the universal it does and not to some other universal, such as the universal referred to by the predicate '...is blue'. Reference to universals should not be a mere epiphenomenon from the point of view of truth-conditional semantics, for when we predicate a universal we represent it as *combined* with the particular in question. The accuracy of our representation, and hence the truth of our statement, depends on *which* universal or universals we represent as combined in this way; hence the referent of the predicate must play a role in determining the truth of our statement.

The current position, then, is this: reference is a relation to an entity, such that the entity referred to plays a role in determining the truth-conditions of sentences in which the referring expression occurs. To say that a predicate ' $\phi$ ...' has reference is to claim that the predicate is related to a universal, and that whether  $\phi$  is true of a given object will depend in part on *which* universal it is related to in this way. We might sum this up by saying that predicate-reference is the theory that predicates bear a semantic relation to universals; however the current proposal should be differentiated from two alternative, less plausible, theories. One is the theory that a universal is the 'meaning' or 'sense' of a predicate rather than its reference.<sup>90</sup> The current thesis, that the reference of a predicate plays a role in determining whether the predicate applies in particular cases, does not commit us to identifying the referent of the predicate as its 'sense' or 'meaning', for referring terms can apparently have the same reference but different meanings. Just as we might want to say that the names 'Hesperus' and 'Phosphorus' have the same referent but different meanings, it may be desirable to treat the predicates '...is water' and '...is H<sub>2</sub>O' as referring to the same universal yet differing in meaning. Moreover, the thesis that universals are *meanings* of predicates suggests the view that *all* meaningful predicates refer to universals; as we shall see later, this is not something the realist will want to grant.

A second alternative to the current theory goes beyond the modest claim that the entity referred to by the predicate 'plays a role' in determining truth-conditions, to state that the semantic contribution of a predicate is *exhausted* by its referring to the universal which it does. Such a theory should not appeal to the realist. I argue later that it would make it impossible to solve the problem of the Unity of the Proposition; in any case the idea that the semantic contribution of a referring expression *must* be exhausted by its having the referent that it does is shown to be false by the phenomenon of inflected languages in which

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<sup>90</sup>See Frege 1892b for the original distinction between sense and reference, and Quine 1980, p.11 for the accusation that realists want universals to be the *meanings* of predicates.

even *names* make a greater semantic contribution than merely picking out their referent, as their inflectional ending indicates which argument-place their referent occupies in the predicate. Neither of these two alternative proposals — that universals are the *meanings* of predicates, and that the identification of a universal as referent exhausts the semantic contribution of the predicate — should be preferred to the account of predicate-reference outlined above.

## **Predicate- or Adjective-reference?**

So far it has been assumed that the realist will want to ascribe reference to predicates such as ‘...is red’ rather than to proper parts of predicates such as the adjective ‘red’. This should not be taken for granted. When we consider whether reference is to be attributed to predicates or to adjectives, logic and English grammar pull us in different directions. The gain in simplicity achieved by excluding the copula from representations of predicate logic leads us to represent ‘...is red’ by means of a single predicate-letter, and this notation encourages the idea that it is the complete predicate that refers to the universal. Why should it be necessary to divide the predicate into a referential part (‘red’) and a non-referential part (‘is’), if our best representation of logical form marks no such internal complexity? On the other hand, we observe that idiomatic higher-order quantification in English is achieved by replacing only part of the predicate, leaving the copula untouched, for example in:

Socrates is *something* that Milo is not — namely, wise.

She is *everything* that I look for in a woman

Here the quantificational variables ‘something’ and ‘everything’ replace adjectives; argument from analogy with the case of first-order quantification suggests that the adjectives replaced by these variables are complete referring expressions — for when we generalize using first-order quantification, for example in ‘someone stole my bicycle’, the expression ‘someone’ occupies a place usually filled by a complete referring expression. Likewise we might expect that when ‘something’ is used in higher-order quantification it fills a place usually reserved for a complete referring expression; since only part of the predicate is replaced, with the copula left intact, it is tempting to conclude that it is the adjective, and not the whole predicate, that functions as a referring expression. The copula then will have either a different function, or no function at all.

The arguments from everyday English are far from conclusive. We describe the process of generalizing from ‘Socrates is wise’ to ‘Socrates is something’ as a procedure of replacing an adjective ‘wise’ with an expression of generality, but we could equally well describe it as a procedure of replacing a whole predicate ‘...is wise’ with a generalized predicate ‘...is something’. Indeed, when we generalize from a predicate containing only an intransitive verb, we replace the verb with just such a generalized predicate: thus from ‘Theaetetus flies’ we can form ‘Theaetetus does something’, where the verb ‘does’ is clearly part of the expression of generality.<sup>91</sup> If it is the whole predicate that is replaced by a device of generality, then it should be the whole predicate that we take to be the bearer of reference. Moreover, although English doesn’t regularly present clear cases where an entire predicate is replaced by a device of generality, there would be nothing unintelligible were the language to be extended to allow this. We would perfectly well understand the kind of generality involved were we to be told that ‘Socrates somethings’, just as we understand the admixture of formal logic and standard English in ‘Socrates  $\phi$ s’; perhaps the only reason custom insists that we choose either ‘Socrates *does* something’ or ‘Socrates *is* something’ is the precision gained by specifying whether Socrates is acting in a certain way or merely exhibiting a certain characteristic.

The evidence from English idioms of quantification can be interpreted in a fashion consistent with the thesis that predicates refer. Thus far it seems preferable to adopt the view suggested by predicate logic: that it is the predicate as a whole that refers to a universal. A different argument might be advanced against this view. The semantic role of the predicate cannot be *exhausted* by its referring to the universal to which it does refer, for the predicate ‘...is wise’ must do more than merely pick out the property of wisdom; otherwise we should have no explanation how ‘Socrates is wise’ says something about Socrates yet ‘Socrates wisdom’ does not. Therefore, it is claimed, the predicate should be divided into two parts: one part – ‘wise’ – which refers to a universal, and another, the copula, which indicates that particular and universal are combined (Wiggins 1984: 318). Then, of course, it will not be the predicate as a whole that refers to the universal, but merely the general term or adjective remaining when the copula is removed. I shall postpone discussion of the need to distinguish different semantic roles played by the predicate. For now, we need only notice that treating the predicate as having a dual role, both picking out a universal and indicating propositional combination, cannot force us to divide each predicate into two parts, one to fulfil each of these different semantic functions. For many predicates, no such division into two parts is possible, for the predicate consists of only one word. A venerable example would be ‘...flies’ in ‘Theaetetus flies’.

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<sup>91</sup> A similar point about ‘does’ is made by MacBride 2006, p.440

Wiggins provides the material for a response to this problem, by suggesting that a one-word predicate could be decomposed into a referring verb-stem, and its inflectional endings, with the verb-stem performing the function of referring to the universal, while the inflectional ending ‘-s’, ‘-es’, ‘-ed’ and so on are assigned the task of indicating propositional composition (1984: 318). However, we should not adopt this proposal, because it is not always possible to separate a finite verb into two distinct parts of which one is clearly a stem and the other the inflectional ending. This is obvious as soon as we notice the phenomenon of *ablaut*, in which inflectional variation is marked by a change in the stem vowel and not by any ending. Thus ‘David writes the letter’ may become ‘David *wrote* the letter’. In the latter sentence, how should we separate the predicate ‘wrote’ into a referring and a non-referring part? Is it to be claimed that the referring expression is the stem *minus* its vowel, so that what refers to the predicate is the string ‘wr—te’? The proposal shades off into absurdity, but was never very plausible in the first place, for even in ‘flies’ we notice that the infinitive, uninflected form ‘fly’ loses a letter before the inflectional stem is added. If we take seriously the idea that it is the *unchanging* part of the predicate that bears referential function, we are forced to conclude that the referential part is simply the two consonants ‘fl—’. The hypothesis that every predicate can be divided artificially into a referring part and a part that ‘indicates propositional combination’ cannot be sustained.

It seems, then, that there is no good argument to prevent us from following the course suggested by predicate logic, and ascribing reference to the complete predicate rather than to a part of it. However, we might adopt a conciliatory approach to those who feel that ‘wise’ in ‘...is wise’ should be allowed to refer to a universal *wisdom*. An unquestioned assumption has been that the choice is exclusive: either we conclude that predicates refer (and adjectives do not) *or* we decide that adjectives refer while whole predicates do not. Yet this assumption is unjustified, and may indeed be false. No-one should deny that the reference of a complex expression depends on the reference of its parts: the reference of ‘the capital of France’ is partially determined by the reference of its constituent expression, ‘France’. Sometimes, indeed, the reference of a complex expression will be no more than the reference of its parts. Thus ‘John and Sarah’, a compound expression, refers both to John and to Sarah, simply because it has proper parts, the names ‘John’ and ‘Sarah’, which themselves refer to John and Sarah. The analogical lesson to be drawn is this: reference may be preserved when simple referring expressions are combined into compound expressions. Applying the analogy to predicates like ‘...is wise’, why should we not hold that *both* the predicate, and the adjective it contains, refer to the universal *wisdom*? We may ascribe reference to the predicate ‘...is wise’ precisely because it is a compound

formed of a referring expression 'wise' and the copula, and it is not unreasonable to say that a compound expression with a part referring to an entity  $x$  might itself refer to that entity  $x$ . From this perspective, it is possible to appreciate the merits of both predicate- and adjective-reference; however we may say that the proponents of each view are mistaken insofar as they believe their opposing views to be incompatible. In fact, there is no obstacle to ascribing reference to predicates and adjectives both.

## Why Predicate-Reference?

The thesis that predicates refer to universals may not be compelling in its own right; however, once it has been accepted that universals exist, there are good reasons for the realist to accept that predicates refer to them. In this section I distinguish three such reasons. A consequence of the discussion is that there is no prospect of denying that predicates refer to universals while remaining a realist. The only option is to engage with the problems of predicate-reference and attempt to solve them.

### 1. Higher-order Quantification

It is a familiar claim that accepting the legitimacy of quantification into predicate- or adjective-position would require us to accept that predicates refer. We may take arguments offered by Wiggins and Dummett as examples of such an approach. Wiggins claims that we should accept predicate reference because of the ‘need to find something intelligible in second level quantification’ (1984: 315); his thought is that such quantification is intelligible only if we take ourselves to ‘quantify over what one- and two-place predicates stand for’ (1984: 315) — i.e. entities of a certain kind. Dummett’s argument is that higher-order quantification must be explained in a manner ‘analogous’ (1981: 224) to first-order quantification: since the explanation of first-order quantification requires us to posit a domain of *entities* over which the variables range, higher-order variables of quantification must likewise be understood by appealing to a domain of entities over which the quantified variable ranges (Dummett 1981: 521, 224). Here I argue that Wiggins’ and Dummett’s arguments fail to motivate their conclusion in favour of predicate-reference; however, some of the arguments advanced in Chapter 2 with regard to the first-order quantifier may be adapted to higher-order quantification, providing good reason to believe that predicates refer.

Wiggins and Dummett both appeal to the idea that higher-order quantification can be explained only if the variable of quantification is understood to range over a domain of entities. Their arguments invite an obvious response: why is it not possible to render higher-order quantification ‘intelligible’ by means of a substitutionalist account, one which sees the truth of a higher-order quantification ‘*a* is something’ as depending on the truth of an appropriate substitution-instance, ‘*a* is *F*’? Under such a treatment, we could not be sure that the quantification was ontologically committing unless we were *already* sure that the substituend predicate-expressions refer; therefore, we could not *derive* the conclusion that predicates refer from considerations about truth-conditions for quantification.



It will not do to reject substitutionalism for higher-order quantifiers merely on the grounds that substitutionalism is unacceptable in the first-order case, as I argued in Chapter 2. The opponent of predicate-reference claims that there is a huge difference in the semantic operation of predicates and names, such that only the latter kind of expression is capable of referring to entities. Why then should he not claim a similar disparity between the function of quantified variables taking the place of these expressions, with those which replace referring expressions ranging over entities, while those which replace non-referring expressions going proxy for the expressions themselves? It cannot be assumed from the outset that the semantic role of predicates and subject-expressions is sufficiently similar that the behaviour of predicate-variables must be ‘analogous’ to that of subject-variables.

Dummett offers a different response to the substitutionalist:

‘The advocates of substitutional quantification share with Quine the assumption that ontological commitment is required only by ‘ontic’ quantification, and therefore regard substitutional quantification as a means of achieving liberation from ontic commitment. Both are wrong, because it is not quantification which in the first place requires the ascription of reference: reference must be ascribed to any expressions which function as significant units of sentences of a language, if we are to be able to frame a semantic account of that language.’<sup>92</sup>

The thought is this: even on a substitutional account, we must assign reference to any expression substituted for a variable of quantification in a substitution-instance, in order to account for the truth-conditions of those substitution-instances; therefore even ‘substitutional’ quantification is ontologically committing, since a variable of substitutional quantification stands in for a class of expressions to which reference must be ascribed in any case. The variable of quantification imparts ontological commitment derivatively, as a result of standing in for expressions which refer, and which are themselves ontologically committing.

Within Dummett’s own scheme, this argument is unconvincing. As we have seen, he makes a distinction between reference ‘as semantic role’ and reference ‘by analogy with the name-bearer relation’; if reference is construed as the former, it is hard to see why it should be automatically considered as a *relation* between a predicate and a non-linguistic entity, rather than being something that any expression may have, provided it makes a contribution to the truth-conditions of the sentences in which it occurs. Certainly,

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<sup>92</sup>Dummett 1981, p.528

Dummett's argument justifies us only in supposing that it is 'reference as semantic role' that we should ascribe to predicates, since his reason for ascribing 'reference' to predicates is merely that they make a contribution to truth-conditions. But then why think that the 'reference' we ascribe to predicates must involve a domain of entities that serve as their referents? Dummett is aware that it is a 'further thesis' that the semantic role of an expression will always involve 'the association of some appropriate entity with it' (1981: 524). For that reason it is doubtful whether Dummett's own reflections justify us in introducing *entities* to serve as the referents of predicates; moreover we cannot expect the opponent of predicate-reference to concede that predicates must be associated with a domain of entities merely because they have 'reference' in the sense of making a contribution to truth-conditions. But if the predicates that occur in substitution-instances for higher-order quantification can have 'reference' without standing in a relation to entities — universals — there is no reason to suppose that this quantification is ontologically committing on a substitutional interpretation, as Dummett claims.

Dummett's and Wiggins' appeals to higher-order quantification fail to motivate the view that predicates have entities as their referents. However, we may defend such a view by adapting arguments deployed in favour of objectualism about the *first-order* quantifier in Chapter 2. One is the argument from the paucity of substitutable expressions: that substitutionalism wrongly counts a sentence as false when what makes the quantified sentence true is some entity which is the referent of no expression in the language. This problem also arises for quantification into predicate-position, as Strawson observes:

'Tom does whatever William does; however William acts, Tom acts thus too. William's behaviour might be indescribable without therefore being inimitable.'<sup>93</sup>

The same point applies to quantification into adjective-position: Tom might be something that William is not, although the language has no adjective capable of describing precisely what this is. Here 'something' cannot be said to stand in for an expression, for there is, by hypothesis, no adjective in the language which might replace it. Hence it must stand in for an entity — the universal which Tom has but William lacks. This problem cannot be evaded by adapting Prior's strategy for the higher-order case, and saying that the presence of a substitutable predicate-expression is a sufficient but not a necessary condition of the truth of the quantifier. Prior suggests that quantifications which lack true substitution-instances may nevertheless be true in the first-order case because the predicate is true of

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<sup>93</sup>Strawson 1974a, p.66

some nameless object (1971: 36); but no comparable explanation may be offered for the truth of quantifications into predicate-position which lack substitution-instances, unless we are prepared to say that such quantifications may nevertheless be true because of the presence of some *entity* to which no predicate refers. To make such a move would be to concede a domain of entities to which predicates refer in the normal course of affairs, and so requires that we capitulate to predicate-reference after all.

A second reason for treating higher-order quantification as ontologically committing is that such quantifiers interact with the machinery of counting in just the same way as in the first-order case, and of course we can make no sense of a ‘count’ quantifier unless we can specify *what* is being counted. Just as in the first-order case we can say that ‘Socrates and Plato have *two* things in common’, we may employ higher-order count-quantifiers, for example to say

‘Socrates and Plato are *two things* that Aristotle is not’

Here, the verb ‘are’ is clearly functioning as copula rather than identity-predicate, and so the expression of generality ‘two things’ must be taken to occupy adjective-position. But then what are we counting when we employ such a higher-order quantification? Just as in the first-order case, it would be absurd to claim that we were counting substitutable expressions, for we would not count the sentence as expressing truth in virtue of the fact that Socrates and Plato, but not Aristotle, were both *bipedal* and *two-legged*. Since ‘bipedal’ and ‘two-legged’ mark the *same* shared feature, it is not the case that Socrates and Plato are *two* things that Aristotle is not — they are only *one* thing that he is not, but this one thing may be described in different ways. Therefore, what we count when we indulge in higher-order count-quantification must be not the substitutable expressions, but what those expressions refer to — the universals themselves. Although Dummett and Wiggins’ arguments gave us no reason to treat higher-order quantification as ontologically committing, these reflections on the extension adequacy of non-objectual accounts of the quantifier justify the conclusion that quantification into predicate- and adjective-position requires the existence of appropriate entities over which the variable of quantification ranges.

Would it be feasible to accept that *quantification* into predicate-position is ontologically committing, and yet deny that reference should be ascribed to predicates themselves? This seems a desperate strategy for the opponent of predicate-reference. The problem is that, in conceding the ontological commitment of higher-order quantification, we accept that a predicate (or adjective) can be replaced by a variable of quantification which ranges

over a domain of entities. In so doing, we accept that an expression (the variable itself) can stand in the position of a predicate and at the same time *stand for* one or another entity in the domain. It would be a mystery how this could happen unless the expressions that the predicate-variable replaces could *also* stand for entities of the same kind as those over which this variable ranges. For that reason, ascribing ontological commitment to higher-order quantification requires that we also accept the thesis of predicate-reference.

## 2. Equivalence

The argument from equivalence has been repeatedly emphasized by Strawson (1974a: 81; 1974b: 27). He considers pairs of sentences such as

Socrates has wisdom  
Socrates is wise  
and  
Socrates exemplifies bravery  
Socrates is brave.

The claim is that, given that the realist recognizes the first of these pairs as introducing a universal, he should recognize the second as doing the same, for is it not the case that the two sentences convey the same information about the world? As Strawson puts it,

‘The theory of ‘commitment’ by noun, but not by adjective or verb, is as absolutely implausible as any philosophical view could be.’<sup>94</sup>

In other words, if we recognize that ‘Socrates has wisdom’ mentions two things – a particular and a universal – then we should likewise recognize the predicative form ‘Socrates is wise’ as mentioning a universal by means of the predicate ‘...is wise’.

It is not possible to assay the response that we accept the equivalence of ontic commitment between the nominalized and predicative sentences, but deny that *either* bears ontic commitment to a universal, for the previous chapters gave us good reason to believe both that universals exist, and that we are capable of quantifying over them using the first-order quantifier; how then could we deny that it is possible to refer to universals by means of subject-expressions such as ‘wisdom’ or (for the scientifically-minded) ‘mass’, ‘spin’, ‘charge’ and so on? It would be an absurd position to accept that bound variables of

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<sup>94</sup>Strawson 1974a, p.83; 1974b, p.27

first-order quantification could stand in for universals, but deny that universals could be referred to by means of subject-expressions themselves.

A more serious objection to the argument from equivalence is proposed by Loux (2002, p.37) and Teichmann (1989, p.156). It is that we generate a vicious infinite regress if we recognize that the content of '*a* is *F*' is given more perspicuously by '*a* has the property *F*'. The second of these sentences employs a predicate '*...has the property F*', and so, according to the current argument, should be taken as equivalent to a sentence containing a property-designator corresponding to the predicate, i.e. as equivalent to the sentence '*a* has the property of having the property *F*'. But this too involves a predicate '*...has the property of having the property F*', and so is equivalent to a yet more complicated sentence '*a* has the property of having the property of having the property *F*.' The problem, roughly speaking, is that each of these reformulations is a more accurate representation of the fact of the matter than the formulation that precedes it, for at each iteration a universal which had been introduced *implicitly* by means of a predicate is now introduced *explicitly* by the property-designator beginning '*the property of...*'. But then, in Teichmann's words, we are led to '*imputing an infinitely complex logical form*' (1989: 157) to apparently simple predications, and so the theory that predicates and corresponding nominalizations co-refer collapses into absurdity.

The complaint is about the '*logical form*' we end up imputing to the sentence '*a* is *F*'. But here it is hard to see what the problem is. Suppose that the logical form is held to be infinitely complex because each iteration of the regress mentions a greater number of entities, and we can see that the limit of the process will have a logical form that mentions an infinite number of entities. In this case, the realist can respond that no reason has been given why we should suppose that the more complicated locutions further along the regress mention more entities than do the original pair, '*a* is *F*' and '*a* has the property *F*'. The realist's original point was that these two sentences could differ in surface form and yet mention the same number of entities – two. Why then should he agree that *more* than two entities are mentioned in any iterated sentence '*a* has the property of having the property of ... having *F*'? A better account would be that a syntactically complex property-designator '*the property of having the property of ... having F*' designates only one universal, and this is exactly the same universal as we might designate by means of the less loquacious '*the property of F*' or even by '*F*-ness'.

To take an alternative formulation, perhaps the point of mentioning logical form is not to raise a problem about the need for an infinite number of entities, but instead to complain about the extra *complexity* of logical form in iterated forms of the regress. Again, the

complaint seems unjustified. Take any stage in the regress — ‘*a* has the property of having the property of ... having *F*’. What kind of logical form should we attribute to it? Surely the correct description of the logical form here is that it is given by the schema  $xRy$  where  $x$  stands for the object *a*,  $y$  stands for the property-designator ‘the property of having the property of ... having *F*’, and  $R$  for the relational predicate ‘...has...’. As the complexity of the iterations increases, the length of the property-designator ‘the property of having... the property *F*’ will increase, but it would be a mistake to think that the logical form of the sentence is thereby more complex. Again, we find nothing objectionable. The correct response to this regress is to allow that we may indulge in ever more long-winded descriptions of the situation described by ‘*a* is *F*’, but deny that this extra length of description corresponds to any increased complexity in what is said, either in terms of the number of entities mentioned or in terms of the logical form of what is said. We may conclude that there is no good objection to recognizing an equivalence in content between ‘*a* is *F*’ and ‘*a* has *F*-ness’; therefore the argument from equivalence provides a second cogent reason to believe that predicates refer to universals.

### 3. Describing the World

The realist claims that the world is composed of two kinds of entity in combination – particulars and universals.<sup>95</sup> Whereas the nominalist may recognize only particulars, the realist recognizes not only particulars but also their properties and the relations they stand in. But the mere recognition of these extra entities does not qualify one for realism in the usual sense, for an essential claim of realism is that this world of particulars and universals is *arranged* in a certain way: particulars *instantiate* universals, and these universals may in turn instantiate other universals. If our description of these patterns of instantiation is correct, then what we say is true. So a natural position for the realist to adopt is that a subject-predicate sentence represents the world of particulars and universals as arranged in a certain way, and truth is achieved iff the world is indeed arranged just as we represent it to be. But this poses a problem for the realist who denies that predicates refer to universals: if the predicate in a declarative sentence *Fa* fails to pick out, or refer to a universal which *a* is said to combine with, how can this sentence *Fa* hope to describe the structure of the world as it is? The sentence mentions only one kind of entity – a particular – but the fundamental structure of the world is given by the arrangement of *two* kinds of entity,

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<sup>95</sup>Nothing I say in this section depends on recognizing particulars in addition to universals. The same argument would apply, *mutatis mutandis*, to theories according to which the world is composed only of universals in combination.

particular and universal. How does a sentence succeed in conveying information about the world if it mentions the particular on its own without mentioning one or more universals with which it combines?

This argument is sometimes generalized to attack nominalist views which refuse to correlate predicates with any non-linguistic entities. The challenge is then to explain in what sense the predicate is 'informative' if this informativeness does not consist in the identification of some universal which the predicate picks out (Armstrong 1980: 105). We should not adopt such an argument, for the simple reason that even the realist should not adopt a general principle that any expression that makes an 'informative' contribution to the truth-conditions of a sentence must be associated with an entity to which it refers. To do so would be to commit us to entities which are referents for expression such as the truth-functional connectives 'and', 'or' and so on, and this would inflate our ontology beyond anything the realist should find plausible.

Notwithstanding the failure to generalize this argument as a criticism of nominalism, it is clear that it presents another good reason for the *realist* to accept that predicates refer, for surely we must accept that subject-predicate sentences are capable of describing a combination of particular and universal, and to be able to do so it must be capable of picking out a universal in some way, in which case the natural supposition is that the sentence picks out a universal by incorporating a predicate that *refers* to that universal. To summarize: there are three compelling reasons, given that universals exist, to credit predicates with referring to them. One is the phenomenon of quantification into predicate- and adjective-position; another the equivalence of content between sentences mentioning a universal and those employing the corresponding predicate. A final reason is the need to explain how a subject-predicate sentence can succeed in describing the combination of particulars and universals. I now turn to the difficulties associated with the thesis that predicates refer to universals. It is of great importance that the realist should have a solution to these problems, for otherwise his position will be untenable: finding that belief in the existence of universals compels him to accept that predicates have reference, and that predicate-reference is incoherent, it might seem that his only option is to abandon realism itself.

## Problems for Predicate-Reference

### 1. The Problem of Sparseness

A first problem for predicate-reference is that not *every* predicate can serve to ascribe a corresponding universal. Such a ‘Problem of Sparseness’ afflicts the current theory because universals were introduced in the first place to serve as ‘respects’ in which similar things are the same; yet there are many predicates which mark no similarity between the things to which they apply. Indeed, much of the argument in favour of realism depended on the fact that sharing a predicate is not sufficient for resemblance in some respect; this was the reason why Predicate Nominalism failed, and is among the reasons why a substitutional treatment of quantification into predicate-position is unsatisfactory. It seems, then, that the predicate-reference theorist must recognize many predicates which refer to no universal.

How much a problem of sparseness afflicts a theory of predicate-reference depends on how abundantly we stock our ontology with universals, and this will depend in turn on what theoretical purpose the universals were originally introduced to serve. In this respect, the introduction of universals to make sense of quantification over ways or respects in which things are similar puts us at an advantage. Other ‘sparse’ theories of universals have been proposed according to which there are far fewer universals to serve as referents than our current account suggests. One is a ‘scientific’ theory that accepts only those universals that are required for the truth of a scientific theory; another a ‘minimal’ theory that accepts only those universals that are needed at the fundamental ontological level on which everything else supervenes. The two diverge because it cannot be guaranteed that science restricts its enquiry to the fundamental level: a ‘scientific’ ontology might include universals required for the truth of biology or geology although these disciplines do not operate at a level that could be called ‘fundamental’ (Schaffer 2004: 94-5).

Neither account presents a genuine threat to a theory of universals as respects of similarity, for these rival criteria derive from *different* reasons to accept universals in the first place. This is especially clear in the case of the ‘minimal’ ontology: here universals are introduced to ‘ground’ truths about the world — i.e. to provide the ontological footing on which higher-level truths supervene. But to play this role is not to function as *respects* in which higher-level things are the same, for two higher-level things might genuinely be the same in some respect without there being some one universal at the fundamental level which they share.

Similarly, the ‘scientific’ ontology fails to recognize those universals which are required to



serve as the respects of sameness that we describe in everyday discourse. It will be noticed that literary critics classify styles, meters, 'tropes' and so on shared between literary works; those with an interest in fashion notice relevant similarities between hairstyles; we all know what we want when we want the same model of car as the one seen in the advert. We constantly attempt to classify the things around us, and we debate the propriety of such classifications largely on the basis of whether they succeed in capturing genuine similarity between the things so classified. It seems that the universals required as 'respects of sameness' far outstrip those that are required by the totality of scientific discourse. To avoid such a conclusion it would be necessary either to attribute massive error to all non-scientific discourse about qualitative sameness, or to expand our understanding of 'scientific' to the point where it encompasses our total theory of the world.

The purpose for which we introduced universals is only served when we recognize a universal behind *every* genuine similarity. Our proposed analysis makes resemblance depend on the sharing of a common universal; therefore it can no more be achieved by sparse ontologies which are restricted to 'scientific' or 'fundamental' universals than it could by abundant ontologies which recognize universals even where there is no genuine similarity. If we doubt that two things share a common universal, we may for that reason doubt whether they really do resemble one another. What we cannot do, without abandoning the analysis that was our reason for accepting universals in the first place, is to accept that two things genuinely resemble each other although they share no universal in common.

To conclude that a given predicate refers to a universal, then, it is necessary only to establish that it picks out a genuine respect in which things are qualitatively the same. Thus there is no obstacle to the view that even 'non-scientific' predicates such as '... is wise' refer to universals. However, this is not enough to avoid problems about sparseness. Given that there are predicates which do not make for sameness of type, how are we to uphold the claim that predicates refer to universals? In particular, given that the contribution a predicate makes to the truth-conditions of sentences in which it occurs is partly determined by *which* universal it picks out, how are we to make sense of the contribution made to truth-conditions by predicates which pick out no one universal? We may decompose the problem into two different challenges for the realist. One is how to deal with predicates which apply in virtue of not one but one of *many* universals, for example when the predicate '...is grue' may apply in virtue of the universal *green* instantiated at one time or the universal *blue* instantiated at another; the other is what to say about predicates which correspond to no universal at all.

The first of these problems may be solved if we are willing to adapt some remarks made by

David Armstrong. Armstrong notices that the ‘sparse’ realist cannot accept that predicates and universals stand in one–one correlation to each other. Instead,

‘Given a predicate, there may be none, one, or many universals in virtue of which it applies.’<sup>96</sup>

In particular, some predicates may apply in virtue of one or another of a class of universals; for example, ‘...has mass’ applies to a particular in virtue of one or another specific mass-universal, while ‘...is a game’ applies in virtue of one or another of a loosely-allied family of universals, and ‘...is either a raven or a writing-desk’ applies in virtue of some universal in a class which has no internal unity whatsoever (Armstrong 1978b: 17). In this scheme, the correspondence between predicates and universals may be one–one, but it may also be one–many. (Of course, it may also be many–one, for different predicates may pick out the same feature and so correspond to the same universal.)

The adaptation I wish to suggest to Armstrong’s account is a simple one: that instead of talking of predicates ‘applying in virtue of’ or ‘corresponding’ (1978b: 17, 13) to universals, we should say that Armstrong’s relation of correspondence, or applying-in-virtue-of, is simply the reference-relation by another name. To see why the strategy is plausible, consider the phrase ‘apply in virtue of’. When a predicate *applies* to an object in this sense, it is said to be ‘true of’ the object; indeed to say that the predicate ‘F...’ applies to an object named by ‘a’ is to say no more than that the sentence ‘Fa’ expresses a truth. So ‘application’ is a *semantic* concept, in the sense that it is closely linked with the conception of truth itself. (In fact, ‘application’ in this sense is no more than the converse of Tarski’s relation of ‘satisfaction’: objects *satisfy* predicates, while predicates *apply to* objects.) It seems that ‘applying in virtue of’ does not differ in any significant respect from ‘referring to’; thus it is plausible that any theorist happy to say that predicates ‘apply in virtue of’ universals should also be happy to say that predicates *refer* to universals.

One difficulty with this solution is that we would normally think that reference should be a relation between an expression and *one* entity. If we are to have predicates which correspond to whole families of universals, then surely the correspondence in question cannot be reference, unless it is possible to conceive of reference as a one–many relation. Worse, not all the entities referred to by such a predicate need be instantiated in order for the statement in question to yield truth, for the predicate ‘...has mass’ applies to an object when that object instantiates one or another specific mass-universal, for example the universal of *having mass of 5kg*. The predicate cannot refer ‘conjunctively’ to the whole

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<sup>96</sup> Armstrong 1978b, p.9

set of different mass universals, for not all of them need be instantiated for the predicate to apply; indeed, only one of them *can* be instantiated by an object at a given time. Instead, we must be happy to allow that reference to the class of mass universals is ‘disjunctive’ or ‘indiscriminate’, such that the predicate ‘...has mass’ applies when one of the family of mass-universals is instantiated. It will be objected that it is highly implausible to think of reference as a one–many relation of this kind.

It is possible to say three things in response. One is to point out that there is nothing in our characterization of the reference-relation that requires that it be one–one. Earlier we suggested that a predicate should be said to ‘refer’ to a universal iff *which* universal is referred to plays a role in determining the truth or falsity of occurrences of that predicate. Even when a predicate refers to several universals in this new, ‘disjunctive’, fashion, it will be the case that *each* of the family of universals so referred to meets the condition on being a referent, for the application-conditions of the predicate ‘...has mass’ will be determined by *which* universals are members of that family. Since each member of the family determines one way in which the predicate could apply, each mass-universal plays a role in determining the predicate’s contribution to truth-conditions.

Second, the defender of predicate-reference might point out that there is no reason to suppose a disjunctive reference-relation of this kind impossible even in the case of paradigmatic referring expressions, namely *names*. There is no *logical* reason why we could not introduce a name ‘*c*’, stipulated to refer disjunctively to *a* and *b*, such that a predication *Fc* is counted true iff *Fa* or *Fb*. Indeed, it is possible that we *already* have names that refer ‘conjunctively’ to more than one thing, such that a predication involving the name is true iff both of its referents satisfy the predicate. Examples would include ‘Billary’ and ‘Brangelina’. It is true that Brangelina is here iff Brad is here and Angelina is here; why couldn’t the couple also have a name such that predications involving that name are true if Brad *or* Angelina satisfies the predicate? Such a name would exhibit an exact analogue of the ‘disjunctive’ reference suggested for predicates, for the suggestion is that a disjunctively referring predicate is true of an object iff the object instantiates one or another of the universals to which the predicate refers.<sup>97</sup>

<sup>97</sup>Strawson (1974b, p.5) has an argument to the effect that there could not be disjunctive names of the kind I suggest are a genuine possibility. He claims that, once we have allowed conjunctive and disjunctive predicates such as ‘(F or G)...’, to admit disjunctive names would force us to recognize the following set of equivalences:

- (1)  $(Fa \text{ and } Ga) \text{ or } (Fb \text{ and } Gb) \leftrightarrow$
- (2)  $(F \text{ and } G)a \text{ or } (F \text{ and } G)b \leftrightarrow$
- (3)  $(F \text{ and } G)(a \text{ or } b) \leftrightarrow$
- (4)  $F(a \text{ or } b) \text{ and } G(a \text{ or } b) \leftrightarrow$
- (5)  $(Fa \text{ or } Fb) \text{ and } (Ga \text{ or } Gb).$

Finally, if some doubt remains about the appropriateness of ‘reference’ as a term for a relation between predicates and universals that may be one–many, we need not cavil too long over our right to use the word, for what we are claiming can be made perfectly precise in terms that do not presuppose a pre-theoretical notion of ‘reference’. We could understand our claim for the one–one case along these lines:

‘*F*..’ refers to universal  $U_1 \leftrightarrow$  ‘*F*..’ is true of  $a$  iff  $a$  instantiates  $U_1$ .

In the case where the reference relation is disjunctive and one–many, the account will be this:

‘*F*..’ refers disjunctively to universals  $U_1, U_2, U_3, \dots, U_n \leftrightarrow$  ‘*F*..’ is true of  $a$  iff  $a$  instantiates one or more of  $U_1, \dots, U_n$ .

In the case of predicates like ‘is a game’, the account may be modified to specify some minimum number of features that a procedure must exhibit to count as a game. If our opponent continues to complain about extending the term ‘reference’ to cases in which the relation in question is one–many, then the term can simply be abandoned. What is really at stake is the thesis that the semantic contribution of a predicate consists in a relation to some universal or universals, such that *whether* a given sentence is true depends in part on whether the particulars mentioned do indeed instantiate the universal or universals picked out by the predicate. Indeed, when the thesis is put in this form, it is hard to see how any realist could deny it, for even the realist who recognizes only fundamental scientific properties should accept that predications concerning the non-fundamental features of objects at the macro level must depend on the *fundamental* properties of those objects in some systematic way. It seems that the part of the Problem of Sparseness is solved by allowing that predicates which mark no genuine similarity between all their instances may refer ‘disjunctively’ to a family of universals.

A further objection threatens. What are we to make of predicates to which *no* universal corresponds? If we expect that *names* which lack a referent cannot play a role in meaningful sentences, why then should we expect a *predicate* to be meaningful although it refers to no

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Since (5) is not in fact equivalent to (1), it is claimed that disjunctive names are impossible without violating logic. However, the argument does not go through, since we should not accept the equivalence he proposes between (3) and (4): whereas (4) will be true in the circumstance where  $Fa$  and  $Gb$ , (3) will not, for the conjunctive predicate ( $F$  and  $G$ ) will be true neither of  $a$  (which is merely  $F$ ), nor of  $b$  (which is merely  $G$ ); consequently the predicate ( $F$  and  $G$ ) should not be counted as applying to the disjunctive name ( $a$  or  $b$ ). If (3) and (4) are not equivalent it is not possible to derive the intended *reductio* by showing that (5) is equivalent to (1).

universal whatsoever? An example given by Armstrong is the predicate ‘...is bewitched’ (1978b: 17). Since there is no such thing as witchcraft, there is no universal picked out by this predicate; yet we perfectly well understand what would be claimed of someone if we said that they were (literally) bewitched.

Two options are open to the predicate-reference theorist at this point. First, it will be noticed that the problem is much more serious for an ontology, like Armstrong’s, which admits only universals which are actually instantiated at one time or another. If uninstantiated universals are rejected, then predicates are denied a referent merely by failing to apply to any objects. It is not clear that the realist *should* deny the existence of uninstantiated universals. It would not be unreasonable to recognize universals that are respects in which things *could* be the same, as well as accepting the features that objects *actually* exhibit; in any case it is not clear that there is any good reason to deny uninstantiated universals. One reason given by Armstrong is that

‘Once you have uninstantiated universals you need somewhere special to put them, a “Platonic heaven” as philosophers often say.’<sup>98</sup>

Certainly we can agree that uninstantiated universals must be ‘platonic’, lacking spatiotemporal location, for where else in the world could a universal be located except where its instances are? However, it will not do to suggest, as Armstrong does, that the acceptance of unlocated universals requires us to accept

‘two realms: the realm of universals and the realm of particulars’.<sup>99</sup>

There is a difference between holding that universals lack spatiotemporal location, and claiming that universals are located in a separate realm, a ‘Platonic heaven’; certainly the latter does not follow automatically from the former, and uninstantiated universals are not to be rejected on the grounds that they require us to believe in ‘two realms’ of reality.

A second argument against uninstantiated universals is that, if universals can exist without being instantiated, and universals exist independently of particulars, then the instantiation of universals by particulars must be treated as a genuine relation — a thing in its own right — which leads to a vicious infinite regress (Armstrong 1978a: 66). It is not possible to say a great deal here without pre-empting the discussion of Bradley’s Regress in subsequent chapters. The relevant conclusions of that discussion are, first, that we are *not* excused giving an account of the relation that holds between universals and particulars merely

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<sup>98</sup> Armstrong 1989a, p.76

<sup>99</sup> Armstrong 1989a, p.76

by adopting an ‘aristotelian’ theory according to which universals are located at their instances and cannot exist uninstantiated; second, that a satisfactory solution to Bradley’s regress can only be achieved by treating the relation between universals and particulars as genuine. If that is true, then this second argument against uninstantiated universals does not succeed.

If we accept uninstantiated universals — ways in which things *could* be the same — then there is no problem about a predicate such as ‘...is a witch’: although there have never been genuine witches, there could have been such, in which case the uninstantiated universal *being a witch* would have been instantiated. However, there may be other reasons why uninstantiated universals are unacceptable; alternatively a problem may remain if there are meaningful predicates which correspond to *no* respect in which things *could* be the same and so cannot be correlated even with an uninstantiated universal. For those reasons we should explore a second option for the predicate-reference theorist, which is to adapt the Negative Free Logic (NFL) which has been developed for *names*, to enable it to apply to predicates.

A Negative Free Logic for names allows that sentences containing non-referring names are capable of truth or falsity; however the truth-value assigned to such a sentence is determined by the rule that any simple or ‘atomic’ sentence containing a non-referring singular term shall be false. The truth-values of molecular sentences containing non-referring singular terms are then determined according to the usual rules for truth-functional connectives (Sainsbury 2005: 64). Such a theory is appealing because of the solution it offers to the problem of negative existentials: ‘Pegasus does not exist’ is counted as true because it is understood as the negation of the *false* atomic sentence ‘Pegasus exists’ (Sainsbury 2005: 196).

A similar proposal can be advanced in the case of non-referring *predicates*. Let every atomic sentence containing a predicate which lacks a referent be counted false, and let the truth-values of molecular sentences containing non-referring predicates be determined by the usual operation of truth-functional connectives. Thus ‘Boris is a Heffalump’ is automatically false, in virtue of the absence of a universal *being a Heffalump*; ‘Boris is not a Heffalump’ is true, because it is the negation of ‘Boris is a Heffalump’.

One problem with this proposal is that it breaks the link between the *meaningfulness* of a predicate and its having a referent, for we allow that predicates can be meaningfully used which have no referent. What account could we give of the meaning of a predicate, if the meaning of a predicate does not consist in the successful identification of a universal as its referent? To develop an adequate theory of meaning for predicates would be an end

in itself; here I can do no more than suggest two directions such an account might take. One is to say that a predicate may be meaningful in virtue of being associated with some body of information which in normal cases does, and in deviant cases does not, succeed in determining which universal is referred to. Another would be to adopt the suggestion offered by Sainsbury, that understanding the meaning of a name is a matter of ‘immersion in a name-using practice’ (2005: 93); we might make a similar claim regarding immersion in a ‘predicate-using practice’. Indeed, there might be reason to hope that a fully-worked out theory could synthesize the best of each suggestion, for it may be that we are able to engage *in* a predicate-using practice only in virtue of some body of information which enables our use of the predicate to coincide with that of our neighbour. As long as these options are not thought implausible, and as long as we retain the distinction between the *reference* of predicates and their sense or meaning, there will be no obstacle to allowing meaningful predicates which refer to no universal.

A second objection to adopting NFL for predicates is that, in attributing referential function to predicates, we make referring something that is integral to an expression functioning *as* a predicate; something that no expression could fail to do yet still function predicatively. How then could there be predicates which *fail* to refer yet make a contribution to truth-conditions? Here again we may adopt a response proposed by Sainsbury in the case of names, that to credit an expression with a characteristic role is not to exclude the possibility of some expressions of that kind failing to perform it:

‘A pencil is for writing, a heart for pumping blood. A broken pencil is still a pencil, even though it cannot be used to write; a malformed heart is still a heart, even though it cannot pump blood. A referring expression which fails in its function of introducing an object is still a referring expression.’<sup>100</sup>

We may claim that we *aim* to use predicates to refer to universals; that the truth of a given predication depends on the instantiation of the universal(s) to which the predicate refers; that a predicate which picks out no universal whatsoever is never truly applied. Nothing in this position is threatened by the phenomenon of predicates which fail to pick out a *single* universal, or which pick out no universal at all. Therefore we may conclude that the absence of sufficient universals for each predicate to stand in a one–one relation to its referent does not generate a problem for the thesis that predicates refer. The Problem of Sparseness does not present an insuperable obstacle to predicate-reference.

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<sup>100</sup>Sainsbury 2005, p.93

## 2. Co-referentiality, and the concept *horse*.

Attempts to claim that predicates refer encounter what is known as the ‘problem of the concept *horse*’. This problem arises when it is alleged that expressions which refer to ‘predicative’ entities — entities which are the referents of predicates — can appear in meaningful sentences only when those expressions occupy a place appropriate to a predicate. The difficulty was first proposed by Frege, with regard to the ‘concepts’ to which he believed predicates refer. His claim is that

‘The behaviour of the concept is essentially predicative, even where something is being said about it; consequently it can be replaced there only by another concept, never by an object. Thus what is being said concerning a concept does not suit an object.’<sup>101</sup>

Frege’s view is clear: we cannot say the same things about concepts as we can about objects, and *vice versa* — it is ‘impossible, senseless to do so’ (1892a: 50). The instance of this rule which particularly troubled Frege is the case in which we try to fill a place suitable for a subject-expression (an expression referring to a Fregean ‘object’) with an expression which refers to a concept. This, Frege says, cannot meaningfully be done.

I shall postpone consideration of Frege’s reasons for adopting this position until we have established how it threatens the coherence of predicate-reference. If Frege is right, we face two related difficulties. One is that the entities that serve as referents for predicates cannot, after all, be universals, if universals are what we talk about when we quantify over or name respects of sameness or shared features. Quantification over respects of sameness is first-order, and generalizes over entities whose names could occupy the same position in a sentence as does the bound variable of quantification. But according to Frege, no name of the referent of a predicate could take subject-position in combination with a first-level predicate; therefore talk about respects of sameness (universals) is not talk about the referents of predicates.

Suppose that this is granted, and two categories of entity are admitted to the ontology, to serve as referents for predicates and as values for first-order quantification over respects of sameness respectively.<sup>102</sup> Nevertheless, the ‘concept *horse*’ problem has teeth, for a second difficulty threatens the realist: namely, that it is impossible coherently to say what it is

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<sup>101</sup>Frege 1892a, p.50.

<sup>102</sup>Wiggins (1984, pp.320-1) proposes such a two-category view, according to which ‘concepts’ are the referents of predicates, whereas in first-order talk of ‘properties’, properties are taken to be ‘the nominalizations of various explanatory premisses... that subsume things of the relevant kind under Fregean concepts’.



that a given predicate refers to, or even to state the general doctrine that predicates refer (Dummett 1981: 212). There is no way to complete the sentence ‘the predicate “ $\xi$  is a horse” refers to ...’ with an expression capable of referring to the referent of the quoted predicate, since — according to Frege — *no* expression incapable of occupying predicate-position in a sentence could refer to the referent of a predicate. Likewise, there is no way to complete the general claim ‘every predicate refers to a ...’ in such a way that the general term (‘concept’, ‘universal’, ‘entity’?) with which it is completed is capable of describing the referent of a predicate rather than the referent of a subject-expression. Such a description would take the form ‘... is a concept’, and according to the current problem, any such description could only apply to the referents of subject-expressions, not to the referents of predicates. Once the consequences of the denial that predicate- and subject-expressions can co-refer are set out in this way, it is possible to see why a solution proposed by Dummett is unsatisfactory. He suggests that claims about the reference of predicates can be expressed coherently using the resources of idiomatic second-order quantification in English (the kind of quantification found in ‘Plato is something which Socrates is not’), plus the relational expression ‘stands for’. Then the referent of a given predicate may be specified in the following manner:

‘A philosopher is what “ $\xi$  is a philosopher” stands for.’<sup>103</sup>

This suggestion is of dubious propriety. The expression ‘a philosopher’ is not by itself capable of functioning as a predicate, so according to Frege’s stipulation it is incapable of referring to the same entity as a predicate refers to, and we cannot accept that ‘a philosopher’ correctly specifies the referent of the predicate ‘ $\xi$  is a philosopher’ as Dummett suggests.<sup>104</sup> If it were permissible to give the referent of ‘ $\xi$  is a philosopher’ using an expression which itself cannot function as a predicate, why can’t we simply say that ‘ $\xi$  is a philosopher’ stands for the universal *being a philosopher*? According to the strictures laid down by Frege, the referent of a predicate could only be specified using an expression capable of functioning as predicate, in which case Dummett would be constrained to employ the incoherent ‘is a philosopher is what “ $\xi$  is a philosopher” stands for’ to specify the referent of ‘ $\xi$  is a philosopher’.

We know that the problem of the concept *horse* is a serious one, if it forces us to conclude that predicates cannot co-refer with other parts of language which lack predicative function. But why should we accept the premise from which the problem derives, that it is impossible

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<sup>103</sup>Dummett 1981, p.217

<sup>104</sup>This objection is raised in different forms by Wiggins (1984, p.132), Wright (1998, pp.80-1), and MacBride (2006, p.462).

for predicative and non-predicative expressions to refer to the same entities? A natural position for the realist to adopt is that predicative and non-predicative expressions *do* co-refer: for example, the predicate ‘...is wise’ and the name ‘wisdom’ apparently refer to one and the same universal. Two reasons are commonly advanced as the basis for Frege’s denial of coreferentiality between predicative and non-predicative expressions. One appeals to a ‘Reference Principle’ according to which any two co-referring expressions can be interchanged within a non-intensional context without change in truth-value. Since any attempt to replace a predicate with a non-predicate destroys even the ability of the string of words to express a thought, truth-value is not preserved on such a substitution and we are forced to conclude that the predicate and non-predicate do not corefer. A second reason appeals to considerations of the ‘unity of the proposition’. It is claimed that the referents of predicates must be a special kind of entity to explain how it is possible for them to combine with the referents of subject expressions to make a unified content. Further, it is claimed that the unique nature of these predicative or ‘incomplete’ entities precludes reference to them by any means save by the predicates themselves. In this section I consider whether either reason is capable of justifying Frege’s denial of coreferentiality; discussion of the wider significance of problems about propositional unity, and how these affect the thesis of predicate-reference itself, will be postponed until the following section. It seems misguided to attempt to settle on one of these interpretations of Frege at the expense of the other. There is little gain in arguing about whether *the* reason for Frege’s problem was his adherence to the Reference Principle or concerns about the ability of concepts to combine with objects, for it is undeniable that he argues for a radical difference in function between concepts and objects, and their corresponding linguistic expressions, *both* on the basis of the Reference Principle, *and* because he needs to show how concepts and objects are capable of combination. This cannot be attributed to a change in his position over time, as both arguments are already present in *On Concept and Object*. Thus

‘the words ‘the concept *square root of 4*’ have an essentially different behaviour, as regards possible substitutions, from the words ‘square root of 4’ in our original [predicative] sentence; i.e. the meaning [*Bedeutung*] of the phrases is essentially different.’<sup>105</sup>

and

‘Not all the parts of a thought can be complete; at least one must be ‘unsaturated’,

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<sup>105</sup>Frege 1892a, p.50

or predicative; otherwise they would not hold together.’<sup>106</sup>

Frege was moved by both considerations at once in denying coreferentiality between predicative and non-predicative expressions; it is unwise to expend effort attempting to promote one at the expense of the other. In what follows, the priority will be to answer, is *either* reason a good one? The realist must have an answer to each if he is to make a theory of predicate-reference workable.

### **The Reference Principle**

The Reference Principle may be stated as follows:

If expressions ‘*a*’ and ‘*b*’ refer to the same entity, then replacing ‘*a*’ with ‘*b*’ does not affect the truth-value of a sentence in which ‘*a*’ occurs in a non-intensional context.

The principle derives its initial plausibility from the behaviour of names: in a non-intensional context, co-referring names may be swapped freely *salva veritate*. For example, so long as ‘Hesperus’ and ‘Phosphorus’ co-refer, if it is true that Hesperus is rising, then it cannot fail also to be true that Phosphorus is rising. (This is not the case in intensional contexts — within the scope of intensional phrases such as ‘believes that’ and modal operators.) But something goes badly wrong when the Reference Principle is applied to predicates, for the effect of substituting the predicate ‘...is red’ with its supposedly co-referring property-designators ‘redness’ or ‘the property of being red’ is to generate nonsense, not another true sentence. From ‘This lampshade is red’ we get ‘This lampshade redness’, and from ‘Red is a colour’, we get ‘Is red is a colour’. If the Reference Principle applies to predicate-expressions as well as names, then predicates and property-designators cannot co-refer. The Reference Principle, coupled with the thesis that predicates have referents, leads us straight to the ‘concept *horse*’ problem.

Attempts have been made to undermine the Reference Principle itself by giving cases where it fails to apply even to uncontroversially co-referring terms in uncontroversially extensional contexts in English (Oliver 2005: 182 ff; MacBride 2006: 467ff). Oliver points out that the principle fails for pairs of co-referring names and definite descriptions: ‘The reference of “Crispin”’ fails to substitute *salva veritate* with ‘Crispin’, for ‘Clever Crispin solved Frege’s paradox’ may be true, but ‘Clever the reference of ‘Crispin’ solved Frege’s paradox’ is ungrammatical; the substitution cannot be made even *salva congruitate*.

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<sup>106</sup>Frege 1892a, p.54

I shall suggest that the Reference Principle should never have been appealing in the first place to anyone with an eye on languages other than English. The Principle is accorded the status of an *a priori* truth which applies to any human language; it could not be treated merely as an ideal standard to which formal languages aspire, for then there would be no justification for supposing the Principle to apply to English itself (Oliver 2005: 182). The failure of the Principle for even one natural language fatally undermines the status of the Principle, and the Reference Principle in fact fails for any inflectional language where the endings of nouns, and not the word order, determine the noun's status as subject or object of the sentence. I make the point using Latin, but it could be made equally well with any other genuinely inflectional language, for example Ancient Greek, Sanskrit, Old Norse, modern Polish or modern Russian. The general phenomenon which undermines the Reference Principle for inflectional languages is that changing the inflection of a noun can alter or undermine the truth-value of the sentence, although different inflected forms of the same noun obviously do not have different referents. For example,

(L<sub>1</sub>) Caesar ambulat

yields a straightforward sentence, 'Caesar is walking', but if we chance the inflected ending of the noun,

(L<sub>2</sub>) Caesarem ambulat

the sentence becomes nonsense. The inflected ending of 'Caesarem' marks that Caesar occurs in the sentence as the object rather than subject of the verb, but the verb is intransitive (takes no object), and so we cannot make sense of the sentence. Yet 'Caesarem' and 'Caesar' obviously co-refer.<sup>107</sup>

It might seem that the correct response to a 'concept *horse*' problem founded on the Reference Principle is to reject the Principle altogether. However, this cannot be a wholly satisfactory response to the problem, for the Principle *does* succeed in stating an important truth about the behaviour of *some* uninflected referring expressions in *some* languages – for example, about the substitutivity *salva veritate* of co-referring names like 'Hesperus'

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<sup>107</sup>It has been objected, against this example, that this level of inflectional flexibility is only a feature of the written, highly stylized literary language, and not of everyday 'natural' spoken Latin. However, the nature of spoken Latin is in fact reasonably well understood both from preserved texts in the vernacular (shopping lists, letters home from uneducated soldiers & c.) and in comparative evidence from later Romance languages, and it is clear that the inflectional variability was present in the vernacular as well (Herman 2000). Even if Latin were disallowed because of an excess of scepticism about the evidence of ancient texts, the example could be made in any modern inflected language, and so the point would still stand.

and 'Phosphorus' in English intensional contexts. Instead of rejecting the Reference Principle outright, we need some account of the circumstances in which it is to be suspended. One suggestion is that the Principle should be suspended for expressions which are 'multifunctional', where the information they convey is not restricted to the identification of an entity as their referent (MacBride 2006: 470). This suggestion has the merit of explaining why we should find counterexamples to the Principle in Latin, where nouns may be ascribed the functions both of indicating a referent *and* of conveying information about how they are to be combined with the predicate, by means of the case marking incorporated in their inflectional ending. Moreover, it justifies the suspension of the Principle for predicates, for a predicate may be attributed the distinct functions of (i) referring to a universal; (ii) indicating the 'propositional combination' of its referent with the referents of the subject-expression(s).

There are clear counter-examples to the Reference Principle both in English and in other natural languages. Moreover, one plausible explanation of why it fails in such cases would also require us to suspend the Principle for 'multifunctional' predicate-expressions. For this reason, we may conclude that the failure of substitutivity exhibited by predicates and property-designators is not to be assessed under the Reference Principle, and hence the Principle gives us no reason to deny that predicates and property-designators co-refer. If the 'concept *horse*' problem is a genuine one for the realist, it does not derive its legitimacy from the Reference Principle.

### **Incompleteness and the Unity of the Proposition**

The second pressure which might justify Frege's denial of co-referentiality between predicative and non-predicative expressions is the need to give some account of the 'unity of the proposition'. The suggestion is this: in order to explain how concepts and objects can combine to form a unified content, it is necessary to treat the concepts to which predicates refer as 'incomplete' or 'unsaturated' (*'ungesättigt'* in Frege's own German: 1891: 31). Further, it is claimed that the incompleteness of concepts prevents us from referring to them, save by means of expressions that are likewise 'incomplete' – i.e. predicates, which are 'incomplete' in the sense of having gaps or 'argument-places' that have to be filled by one or more lower-level expression. Thus it is possible to arrive at a 'concept *horse*' problem without any appeal to the Reference Principle, for if concepts are incomplete, and incomplete entities may be referred to only by predicates, no 'complete' expression which is not itself a predicate may refer to a concept.

Two questions confront us. One is whether it is true that the incompleteness of a concept requires the incompleteness of any expression capable of referring to it. If *concepts* themselves may be incomplete yet *concept-expressions* may be either incomplete or complete, then there is no reason to deny that the referents of predicates may also be referred to by complete expressions such as names. I shall urge that this first question be answered in the negative: the concept may be ‘incomplete’, but that does not mean that the expressions referring to it must be so too. The other question is whether any genuine worry about propositional unity threatens the realist in the first place. Discussion of this question will be postponed to the following section, since it has relevance for the coherence of predicate-reference itself, not merely for the denial of co-referentiality between subject-expressions and predicate-expressions.

In Frege’s mature theory, every (first-level) concept combines with every object; the result of the combination is a truth-value: True if the concept and object are combined in actuality, False if they are not (1891: 30). Although the object Socrates combines with the concepts introduced by ‘ $\xi$  is a philosopher’ and ‘ $\xi$  is an ostrich’, only the first of these combinations results in truth. Since Frege is a realist about relations, and since the combination between an object and a concept seems to be a relation, we may anticipate a regress of relations along the lines of that presented by Bradley, where each new relation introduced must in turn have another relation combining it and the other objects with which it combines.<sup>108</sup> To escape this threatening regress, Frege suggests that we suppose the concept to be ‘incomplete’ or ‘unsaturated’ or ‘in need of completion’; this is to explain how it is possible for concept and object to combine without the need for any intervening relation. We may put to one side the question of whether treating concepts as ‘incomplete’ does anything to explain their ability to combine directly with objects (for detailed discussion see Chapter 6); here our question is whether the incompleteness of concepts entails that we can only refer to them by means of ‘incomplete’ expressions, i.e. predicates.

The *prima facie* position must surely be that the incompleteness of a concept in no way entails incompleteness in the expressions that refer to it. In other cases we would think it ridiculous to attribute properties of referents to the expressions which refer to them. Merely because Socrates is wise, it does not follow that the name ‘Socrates’ is wise. Likewise, it should not follow immediately from the fact that a concept is incomplete that any expression referring to it is *also* incomplete. This conclusion is reinforced when we notice that the ‘incompleteness’ of a concept is a different feature from the ‘incompleteness’ of a *concept-expression*: the latter consists in the expression’s having ‘gaps’ or argument-places

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<sup>108</sup>See Currie 1984, pp.332-3 for excerpts from Frege’s correspondence supporting this interpretation.

which may be filled by other expressions to produce a sentence, while the incompleteness of concepts is explained either as their inability to exist outside of combination, or their ability to combine directly with objects without an intervening relation (Currie 1984: 337). To derive a ‘concept *horse*’ problem from the incompleteness of *concepts*, some argument is necessary. I shall consider two such arguments: one given by Dummett, the other extrapolated from some remarks of Frege’s.

Dummett’s argument is that the nature of incomplete expressions — in particular, their incompleteness — is explained by the fact that they can only be referred to by expressions which are themselves ‘incomplete’ — i.e. predicates. If we abandon the restriction that leads to the ‘concept *horse*’ problem, and allow *complete* expressions to refer to incomplete entities, we lose the resources to draw the distinction between complete and incomplete entities which is allegedly essential to understanding how they can combine:

‘A relation... is *explained* as being that for which a relational expression stands, and hence if we allow that an expression of a different kind can stand for a relation, the whole explanation of what a relation is falls to the ground.’<sup>109</sup>

‘an entity is incomplete if it can be introduced in the first place *only* as the referent of an incomplete expression. We cannot explain what a function is except by indicating what functional expressions are and then saying that by a ‘function’ is meant whatever can be the referent of such an expression.’<sup>110</sup>

Dummett’s argument can be challenged. One problem is that there *are* other ways to explain the incompleteness of Fregean *concepts* without employing an analogy with the incompleteness of concept-*expressions*. For example, we might explain the incompleteness of a concept as its inability to exist uncombined, or its ability to combine directly without a mediating entity to function as combiner. If we have an explanation of how concepts and relations differ from objects which does not appeal to facts about what kinds of expression can refer to them, there is no need to follow Dummett’s supposition that only one kind of expression can refer to incomplete entities.

A second problem is this: even if we *must* mention the kinds of expression which can refer to a concept in the course of explaining what a concept is, it is hard to see why we should adopt Dummett’s proposed explanation,

**D1:** A concept is an entity that can *only* be referred to by an incomplete expression,

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<sup>109</sup>Dummett 1955, pp.106-7

<sup>110</sup>Dummett 1981, p.254

rather than the weaker explanation,

**D2:** A concept is an entity that *can* be referred to by an incomplete expression (but may be referred to by expressions of other kinds as well).

The latter is a close relative to a traditional criterion of universalhood, for we might attempt to state the distinction between particulars and universals by noticing that universals *can*, but particulars *cannot*, be the referents of predicates.<sup>111</sup> It would be a further step to suggest that this account be augmented with the restriction that universals *cannot* also be the referents of subject-expressions. If nothing is done to block the alternative explanation of concepthood **D2**, Dummett's argument fails, for there is no reason to suppose that concepthood must be explained by the restrictive **D1** rather than the lenient **D2**. In that case, there will be no reason to suppose that an 'incomplete' concept cannot be the referent of a 'complete' subject-expression.

A different argument for the view that only incomplete expressions can refer to incomplete entities is suggested by some remarks of Frege's:

'The behaviour of the concept is essentially predicative, even where something is being said about it; consequently it can be replaced there only by another concept, never by an object. Thus what is being said concerning a concept does not suit an object.'<sup>112</sup>

This cryptic passage can be understood along the following lines: the incomplete or 'predicative' nature of concepts means that their behaviour is tightly circumscribed, such that a first-level concept can never be the argument of an object or another first-level concept, but must have an object as its argument, whereas a *n*th-level concept may have as its argument any concept of level *n* - 1 or below. To represent concepts combining in impossible ways is to produce nonsense; just such a case of nonsense is when we attempt to represent a concept by means of a non-predicative expression such as a property-designator 'redness'. To do so would be to represent the concept *red* as falling under another first-level concept, and therefore to represent it as combining in an impossible way. The reason for the denial that non-predicative expressions could refer to concepts would be this: to attempt reference to a concept by means of a non-predicative expression would, *ipso facto*, be to represent it as capable of combinations it is nonsense to suppose it can form; therefore merely to attempt to refer to a concept by an expression that is not a predicate is

<sup>111</sup>See MacBride 2005, p.597ff for critical discussion of this account of universalhood.

<sup>112</sup>Frege 1892a, p.50



to engage in a nonsensical practice. In short, the reason for holding that only incomplete expressions can refer to incomplete entities is that only incomplete expressions are capable of representing the logical behaviour of incomplete entities without gross impropriety.

This is a more compelling reason for denying that subject-expressions can refer to concepts; however it too is ultimately unsatisfactory. The problem is that it rules out, quite without justification, the possibility that a first-order sentence of the form  $Fa$  might be used to represent a first-order concept falling under a higher-order concept. For example, when we say 'red is a colour', we represent a universal 'red' falling under a higher-order concept ' $\xi$  is a colour', but we do so without employing any linguistic resources that would not also be appropriate to describing objects falling under first-order concepts. English does not have the resources to distinguish between different levels of predicates; therefore it cannot be taken for granted that the predicate ' $F$ ' in a subject-predicate sentence ' $Fa$ ' stands for a first- and not a second-level concept. But of course if the predicate ' $F$ ' stands for a second-level concept, then there need not be any logical impropriety about using a subject-expression ' $a$ ' to stand for a first-level concept, for to do so is not necessarily to represent that concept as combining in an illegitimate way; it may be that what we do is to represent a first-level concept  $a$  as combining — perfectly legitimately — with a second-level concept to which ' $F$ ' refers.

To sum up: we have reason to believe that we suggest a logical impossibility by using a complete expression to refer to a concept only if we are sure that the sentence we use represents an impossible situation, and we can be sure of this only if we are sure that the predicate which we attempt to apply to the concept refers to an entity (another first-level concept) with which the concept cannot combine. If the predicate ' $F$ ' in the sentence ' $Fa$ ' refers to a *second*-level concept, then the situation we describe in the case where ' $a$ ' stands for a first-level concept is not an impossible one; therefore appeal to the restricted logical behaviour of concepts cannot justify a claim that there is something improper about using a 'complete' expression — name or definite description — to refer to a concept.

The question at issue has been whether there is any good reason to accept the Fregean denial that predicate-expressions and subject-expressions can refer to the same entities. Two considerations suggested such a view. One, the Reference Principle, does not motivate the Fregean denial of coreferentiality because there is good reason to refuse to apply the Principle to 'multifunctional' predicate-expressions. A second consideration was the thought that 'incomplete' entities could be the referents only of expressions that are similarly 'incomplete'. Such a view was justified neither by Dummett's claim that the explanation of what an incomplete entity *is* collapses if complete expressions can refer to

incomplete entities, nor by the argument I have attributed to Frege, that to represent a concept by means of a 'complete' expression is to represent it as behaving in a nonsensical way. Consequently, I suggest, there is no reason to accept Frege's denial of coreferentiality between predicate-expressions and property-designators, and therefore no reason to be concerned about a problem of the 'concept *horse*'.

### 3. The Unity of the Proposition

The third difficulty for predicate-reference is a concern about our ability to account for the 'unity of the proposition'. As we have seen, the need to solve some such difficulty was one of the factors that led Frege to deny coreferentiality between predicative and non-predicative expressions; our conclusion was that considerations about the ability of Fregean 'concepts' and 'objects' to combine do not license any such restriction on the behaviour of the predicate-expressions and subject-expressions that stand for them. However, this is not to say that there is no genuine problem about propositional unity for the realist to solve. It would be an enormous task to complete a historical survey of the problem, and the variety of responses that have been proposed. My aim here is merely to achieve clarity about what the problem is, and thereby suggest what form a satisfactory solution would take. To some extent my treatment is deflationary, for I shall argue that a proper understanding of the problem leaves a solution looking much less difficult to achieve.

A common first step in expressing a concern about propositional unity is to ask the question, what makes the difference between a sentence and a 'mere list' (Dummett 1981: 256; Wiggins 1984: 324; Gaskin 1995: 162)? Lists are not capable of being used to *say that* something is the case, while sentences clearly are capable of being used to assert a content in this way. Care should be exercised about what is meant by 'list' in this formulation. It cannot be that we are challenged to say what makes the difference between a sentence and a mere list of the *expressions* that are constituents of that sentence, for this permits of a trivial answer: in order to list the expressions contained in a sentence it is necessary to enclose them in quote marks, and separate them with commas or semicolons thus:

'Socrates', 'is wise'.

To put the question in this way is to ask, what is the difference between *using* an expression (to refer to something else), and talking *about*, or *mentioning* the expression itself? The problem, and its answer, seems trivial: when we list the expressions in a sentence, we

enclose the expressions in quote marks. This serves to indicate that we are referring to the expressions themselves rather than to the *referents* of those expressions.<sup>113</sup>

Instead, we should understand the challenge as follows: what makes the difference between a sentence and a list of the *referents* of the expressions contained in that sentence? If we list the particular Socrates and the universal *wisdom*, we have not yet said *that* Socrates is wise. What accounts for the peculiar ability of a sentence to be used to assert such a content, when merely enumerating the entities involved does not involve us in assertion at all? When the problem is posed in this fashion, it becomes clear why worries about unity should be the special concern of the predicate-reference theorist, for in attributing referential function to predicates as well as particulars he treats each subject-predicate sentence as a referring expression followed by another referring expression, in which case it seems that there is nothing to distinguish a sentence, capable of asserting a content, from a mere sequence of referring expressions that cannot.

Setting the problem out in that way has consequences for its scope, and for the range of responses that would count as possible solutions. In particular, we should accept the following constraints: first, that — since the problem is supposed to arise *within* a theory of predicate-reference — it should not be presented in such a way that even the *nominalist* cannot solve it. The relevance of concern about propositional unity to the current enquiry is that it might show predicate-reference to be untenable; if the same question about propositional unity arises even when it is denied that predicates have reference, the argument loses its place in the dialectic. It cannot count against predicate-reference, or even realism, that it faces an insoluble problem of propositional unity if every other theory of predication finds the problem equally insoluble.

A second constraint on the problem is that (despite its misleading name), our concern about ‘the unity of the proposition’ is neither best explained as a problem about the unity of some composite entity — the proposition — nor is it to be solved by rejecting propositions, nor by introducing propositions but showing them somehow to be unified. If we take the problem at face value, as a concern about ‘the unity of the proposition’, then it might seem that it can be solved simply by repudiating the entities — propositions — whose unity is in question. This approach is recommended by Palmer (1988); however it seems fair to say that the repudiation of propositions does not solve the problem. Even if propositions are denied, it is still legitimate to ask for an account of the difference between a sentence, which expresses a content, and a mere list of referents, which does not. The distinction between strings of words which express a content and those which do not must still be

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<sup>113</sup>Frege 1892c, p. 58 and Russell 1905, p.48 make similar points.

made, even if it is denied that ‘expressing a content’ is a matter of the sentence being related to an entity — a proposition — that is identified as the content so expressed.

An alternative approach sees propositions — unified complexes of particular and universal — as the *solution* to the problem, rather than its cause. Thus Gaskin suggests that the unity of the proposition is ‘prior’ to, and hence explains, the ability of sentences to express a unified content:

‘What *determines* whether a purported declarative sentence has an acceptable syntax and is sententially unified is just whether it is a member of a class of synonymous sentences expressive of some given unified proposition.’<sup>114</sup>

Gaskin’s idea is that, if we can explain the unity of a kind of entity — propositions — we shall thereby explain what makes the difference between content-expressing and non-content-expressing strings of words: a content-expressing string will be so in virtue of its bearing a relation to some or another proposition.

It does not seem likely that Gaskin’s approach can solve the problem. The difficulty is this: let it be granted that entities — propositions — exist, and that propositions genuinely unify their constituents. Clearly then we would want to say that any sentence expressing a content bears a relation to a proposition, while any non-content-expressing string of words does not bear such a relation. But the original question occurs in a different guise: why is it that *these* (content-expressing) strings bear such a relation to a proposition, while *those* (non-content-expressing) strings do not? Why is it that ‘Socrates is wise’ is related to a proposition, while ‘Socrates wisdom’ is not? It cannot be that a sentence gets to be related to a proposition merely by containing expressions which refer to the constituents of that proposition, for then ‘Socrates wisdom’ would have just as good a claim to refer to a proposition as does ‘Socrates is wise’. Even if propositions are introduced, and their unity is conceded, there remains a problem about why certain strings of words bear a relation to a proposition and others do not. For that reason it seems that looking to propositional *entities* to provide the solution to our difficulty is to look for the answer in the wrong place.

These reflections on attempts to provide an ‘ontological’ solution to the problem suggest a further constraint on our presentation. It is that we should take care to distinguish a concern about ‘the unity of the proposition’ from the family of difficulties known as ‘Bradley’s regress’, which I consider in subsequent chapters. Two considerations suggest such a distinction. One is that Bradley poses a problem about the ability of particulars and universals to combine with each other, whereas the current difficulty can be raised, as we

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<sup>114</sup>Gaskin 2008, p.26

have seen, even in circumstances where there is no suggestion that the difference between a sentence and a ‘mere list’ should be explained in terms of the unity of non-linguistic *entities* such as particulars and universals.

A second reason for distinguishing Bradley’s Regress from the Unity of the Proposition is that Bradley was concerned to attack the ability of particular and universal to combine ‘in fact’ (1911: 74) — the capacity of a particular *have* a property, or *stand in* a relation — whereas a problem about propositional unity is a problem about the ability of a string of words to *say that* particular and universal are so combined. The current problem is not that of how particulars and universals are capable of actually instantiating one another; it is a problem about their ability to be *represented* as combined in instantiation. This is a common theme in discussions from Frege on: Frege is concerned to explain how concept and object can ‘hold together’ as constituents of a ‘thought’ which may be true or false (1892a: 54), while Russell notes that the problem of unity that he hopes to solve by means of the multiple-relation theory of belief is one about unity or ‘synthesis’ *in thought* rather than in fact:

‘Suppose we wish to understand “A and B are similar”. It is essential that our thought should... “unite” or “synthesize” the two terms and the relation; but we cannot *actually* “unite” them, since either A and B are similar, in which case they are already united, or they are dissimilar, in which case no amount of thinking can force them to become united.’<sup>115</sup>

The problem of propositional unity is one about our ability to express a unified content when we think, or assert, that the world is a certain way; it is not the familiar Bradleyan problem of how it is possible for universals and particulars to combine with each other in reality.

It seems, then, that a concern about propositional unity can be answered neither by appealing to the unity of propositional entities, nor by assimilating it to Bradley’s regress. How then could it be solved? An alternative direction our solution might take has been suggested by material earlier in this chapter. Can we not give an account of the difference between a sentence and a ‘mere list’ by attributing some special feature to the predicate? Predicates are found in content-expressing sentences, but are not found in lists unless enclosed in quote marks, in which case they function to refer to themselves rather than to their referents. For that reason it seems plausible to suggest that it is the predicate that is responsible for the ability of a sentence to express a content. To take this approach is to

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<sup>115</sup>Russell 1992, p.116

admit that predicates are ‘multifunctional’ (MacBride 2006: 470), and that in addition to the function of picking out a universal as their referent, they ‘indicate propositional combination’ (Strawson 1974b: 24; 1959: 149). But what is it to ‘indicate propositional combination’? Strawson describes this as the role of showing

‘that we have a truth-or-falsity-yielding combination ... , i.e. a combination which yields truth if the particular exemplifies the concept — or, if the concept applies to the particular — and falsity if not. A truth-or-falsity-yielding combination we call a propositional combination.’<sup>116</sup>

To say that the predicate in a sentence ‘indicates propositional combination’ is therefore to credit the predicate with the extra role (over and above referring to a universal) of indicating *that* there is a content to be assessed. If such a suggestion is legitimate, it enables us to answer the question of what makes the difference between a sentence and a list: a *list* containing only subject-expressions succeeds in identifying a succession of entities; but since it lacks a predicate it lacks an expression capable of fulfilling the additional function of indicating *that* these entities are combined in a content. We may, as it were, explain the difference between a sentence and a list in this way: both sentences and lists are formed from a succession of referring expressions; however one of the referring expressions in a *sentence* has an additional function, that of indicating *that* the entities referred to are combined. This is how a sentence succeeds in expressing a content that is a candidate for truth or falsity, whereas a list does not.

It might be claimed that such a proposal lacks sufficient content to count as a genuine solution. To attribute an extra function to predicates ‘explains nothing until the relevant functions are identified, and without begging the question’ (Gibson 2004: viii). However, it is not so clear that the predicate-reference theorist *must* give an account of the extra function that he attributes to predicates, in such a way that it does not rely on a prior understanding of predication. To set the bar for an acceptable solution to that height would make the problem one that no *nominalist* theory of predication is capable of answering, and that would suggest that the problem is a perfectly general one, which has no bearing on the acceptability of predicate-reference. Instead, I propose that we should understand the dialectic in this way: the predicate-reference theorist suggests that predicates, as well as subject-expressions, should be counted in the category of referring expressions. But a consequence of this is that *both* content-expressing sentences *and* non-content-expressing ‘mere lists’ may turn out to be no more than strings of referring expressions. Consequently

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<sup>116</sup>Strawson 1974b, p.17

a problem arises about how the predicate-reference theorist is able to distinguish, within these strings of referring expressions, those which are, and are not, capable of expressing a unified content. But *this* worry vanishes when we abandon the view that led to it — namely, that both sentences and mere lists are strings of expressions that do *no more than* refer to their referents. Of course, to accept that predicates are ‘multifunctional’ is to reject such a view, for then predicates will not *merely* refer to their referents, and it can no longer be claimed that a sentence is a string of expressions that have *merely* referential function.

There are three serious objections to a solution of this kind. First, that it is illegitimate to assign a function to a predicate that cannot be explained in terms of a connection with some entity; another that ascribing reference to predicates prevents us from assigning any function to them beyond that of picking out the entity that is their referent; finally it might be claimed that a predicate cannot indicate ‘propositional combination’ because predicates are used in sentences which do not make assertions, e.g. questions. I shall argue that each can be overcome.

The first of our problems is the suggestion that it is not permissible for the defender of predicate-reference to assign a special function to a predicate (that of indicating propositional combination) that does not consist in a relation to some non-linguistic entity. If ‘indicating propositional combination’ is to refer to some entity — call it the propositional-combination relation *PC* — then we would face anew the question of what distinguishes a sentence from a ‘mere list’ of the universal(s) and particular(s) involved in that sentence, plus the relation *PC* itself. It is for this reason that Strawson rejects the view that ‘indicating propositional combination’ should be understood as merely specifying a further concept of ‘exemplification’ (1974b: 18).

Why should it be claimed that it is not permissible for a realist to assign an extra function to a predicate that does not consist in reference to some extra *entity*? It cannot be because there is something intrinsically unacceptable about expressions which play a role in determining the content expressed, yet are not correlated with an entity as their referent. The *opponents* of predicate-reference, after all, want to say that the predicate plays a role in determining the content expressed by a sentence, although they claim that predicates lack referents altogether. If the opponent of predicate-reference allows that predicates can be ‘informative’ without referring to any entity, how can he claim that predicate-reference theorist goes wrong in recognizing that part of the contribution made by a predicate does not consist in the identification of an entity, but merely in ‘indicating propositional combination’?

A different reason for thinking that ‘indicating propositional combination’ can only be understood in terms of some extra *entity* would be if the realist were committed to a general

claim that the informativeness of expressions is always to be explained in terms of the entities to which they refer. Then there would be something strange about recognizing one case of informativeness without reference, where propositional combination is indicated without any entity being picked out, while maintaining that informativeness *in general* does require reference. But the realist is committed to no such general claim. Predicate-reference is *not* a view adopted because of adherence to a general principle that the informativeness of any expression must consist in its reference to an entity; if it were we should have to attribute reference not only to predicates but also to the logical constants by means of which we form molecular from atomic sentences, for these too play a role in determining the content expressed by such a sentence. Predicate-reference does not commit us to the view that *any* contribution made to the content expressed by a sentence must be ‘cashed out’ as a relation to an entity; therefore there is nothing inappropriate in the current proposal that *part* of the contribution made by a predicate consists in a non-referential function of indicating propositional combination rather than in reference to an entity.

A second objection to our account of predicates as indicators of propositional combination is that, in ascribing reference to an expression, we forswear the right to assign any semantic function to it above and beyond the identification of the entity which is its referent. This thought may be motivated in two ways. One is that, for our paradigm class of referring expressions — names — the contribution they make to the content that is assessed for truth or falsity genuinely does seem to be exhausted by the fact that the name refers to what it does. This is the reason that predicate-reference is rejected by those who can see no way to understand the thesis that predicates refer to universals other than as the thesis that predicates are *names* of universals (Quine 1986: 67; Haack 1978: 53); if predicates are *names* of universals, then the contribution made by the predicate can consist *solely* in having the referent which it has, and there is no room to attribute an additional function to the predicate that enables us to distinguish sentences from mere lists of entities. An alternative reason to assert that predicates, if they refer, can have no function in addition to picking out their referent is given by the identification between the reference of an expression and its ‘semantic role’ (Dummett 1981: 245; 1993: 24). Then we should have to say that to identify an entity as the referent of a predicate is to identify that entity as the semantic role of the predicate. In that case the job of specifying the contribution made by a predicate is completed as soon as we identify a universal as its referent; there is no scope to assign an *extra* function of indicating propositional combination.

It seems that this objection cannot stand, whether the opponent of predicate-reference appeals to analogy with names or to theoretical considerations about reference as semantic



value. One problem is that the idea even of a *name* as having only one function — that of referring to its referent — is based on an over-hasty generalization from English and other modern European languages in which names lack inflection. As we have seen, in any language in which names carry inflectional endings to mark cases the contribution of a name will *not* be restricted to the fact that it refers to what it does. In addition, the inflectional variation of the name will mark whether the name occurs as subject or object, i.e. it will mark which argument-place it occupies in a relational predicate. Given that even a *name* in such languages is able to play a role in determining the content expressed that goes beyond merely the role of referring to the entity that it does refer to, we may conclude that even understanding predicates as *names* of universals does not prevent us from attributing to a predicate an extra contribution beyond that of simply referring to the entity to which it refers.

Moreover, there is no need to reject the view that predicates ‘indicate propositional combination’ *even if* we accept the restriction that a predicate can make no semantic contribution beyond merely picking out its referent. This is because it need not be claimed that indicating propositional combination is a *semantic* function in the sense in which we talk of a ‘semantic role’ that ought to be exhausted by a predicate having the reference that it does. Consider the account of ‘semantic value’ given by Dummett:

‘The semantic value of an expression is that feature of it that goes to determine the truth of any sentence in which it occurs’<sup>117</sup>

It seems that the function of indicating propositional combination should fall *outside* the concerns of a theory of semantic value as Dummett presents it. It is desirable to distinguish those factors which play a role in determining the truth or falsity of a content expressed (the ‘semantic value’) from factors which indicate *that* there is a content to be assessed in the first place. But to indicate propositional combination is simply to indicate *that* the expressions in question are to be assessed as presenting a content; it is not a function that plays a role in determining the truth or falsity *of* that content, and so is not a matter of the predicate making an extra ‘contribution’ to the truth-conditions of the content so expressed. Even if we believed that the *semantic* contribution of the predicate is exhausted by its referring to what it does, there is no reason not to ascribe the predicate an *extra* function of indicating that there is a content suitable for assessment in the first place. We may dismiss this second objection to our proposed solution to the ‘unity of the proposition’.

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<sup>117</sup>Dummett 1993, p.24

One final objection remains. It is that not every use of a predicate is a case of our asserting a proposition, for as well as ‘Socrates is wise’ we have ‘Is Socrates wise?’ and ‘Who is wise?’ (Strawson 1959: 150; Teichmann 1989: 153). But if a sentence does not assert a proposition, how can its predicate be said to ‘indicate propositional combination’? This complaint can be dealt with by the realist. He might say: sentences are capable of expressing a range of different *attitudes* to a propositional content. The function of indicating propositional combination is one of indicating *that* particulars and universals are combined in this content; nevertheless this function does not extend to ensuring that the attitude taken to the content must be one of *assertion*, for it might be denial, or a questioning attitude. For example, if we say ‘Socrates is not wise’, it is not so much that the predicate *fails* to indicate a propositional combination of Socrates and wisdom; rather, the predicate indicates a combination *Socrates’ being wise*, and the operator ‘not’ indicates that this content is said *not* to be the case. For that reason it is not enough to generate a problem to complain that there must be ‘propositional combination’ even in circumstances when the content in question is *denied* rather than *asserted*. By distinguishing between a propositional content, and the attitude taken to that content, it seems that the last objection to the view that predicates ‘indicate propositional combination’ can be overcome.

## Conclusion

The challenge of distinguishing between a sentence and a list, if both are construed as strings of referring entities, may be answered adequately by crediting the predicate with an additional function — indicating propositional combination — which is lacking in the corresponding nominalized forms (‘wisdom’, ‘yellow’ *etc.*). Nor was there any reason to reject predicate-reference because of a ‘problem of the concept *horse*’, for we found no reason to acquiesce in the Fregean denial of coreferentiality between predicates and their corresponding nominalizations. Our first problem for predicate-reference, that we lacked sufficient universals to serve as the referents for every predicate, was solved by allowing the reference-relation to be sometimes one-many rather than always one-one, and by accepting that the *meaningfulness* of a predicate need not depend on the existence of a referent, so there can be meaningful (false) predications to which no universal corresponds. There is, then, no compelling reason for the realist to deny that predicates refer to universals. This is fortunate, for the first part of this chapter presented arguments to the effect that realism about universals *requires* us to accept predicate-reference. If predicate-reference

had turned out to be untenable, that would have serious consequences for realism itself. For that reason, we may count overcoming the difficulties associated with predicate-reference as an important achievement in the defence of realism itself.

## 5. Bradley's Regress: I

### Three regress arguments

The infinite regress described by Bradley (1897) occupies a special place among criticisms of the realist project, because it threatens to reveal a problem with the central thesis of realism itself. The realist claims that reality is composed of universals — properties and relations — in combination with particulars; Bradley responds that his regress shows an ontology of distinct entities combined in this way is untenable:

‘The arrangement of given facts into relations and qualities may be necessary in practice, but it is theoretically unintelligible. The reality, so characterized, is not true reality, but is appearance.’<sup>118</sup>

In particular, Bradley suggests that there is something incoherent about supposing universals and particulars to *combine* with each other. If this combination is ‘unintelligible’ then the nature of reality *cannot* be as the realist claims it to be; at best the realist’s theory gives an account of the way things appear, not the way they really are.

I shall argue that debate about Bradley’s regress encompasses *three* structurally similar regress arguments, which I shall refer to as the ‘Sameness’, ‘Difference-Maker’ and ‘Relatedness’ arguments. In this chapter I claim that the Sameness and Difference-Maker regresses may be solved by the realist; the following chapter addresses the Relatedness regress. In the course of my discussion I shall distinguish and criticize various ‘canonical’ responses which are overwhelmingly popular among realists. A subsidiary thesis of this and the following chapter is that faith in the canonical responses is misplaced.

All three regress arguments share a common structure. When two entities *A* and *B* are related, it is claimed that the realist must introduce a relation *C* which holds between the original two terms. But then the realist must introduce a further relation *D* which links *C* to *A* and *B*. Bradley takes up the story:

‘[His opponent says] “There is a relation *C*, in which *A* and *B* stand; and it appears with both of them”. But here again we have made no progress. The relation *C* has been admitted different from *A* and *B*... Something, however, seems to be said of this relation *C*, and said again of *A* and *B*. And this something is not to be the ascription [identification] of one to the other. If

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<sup>118</sup>Bradley 1987, p. 25

so, it would appear to be another relation, *D*, in which *C*, on one side, and, on the other side, *A* and *B* stand. But such a makeshift leads at once to the infinite process. The new relation *D* can be predicated [identified] in no way of *C*, or of *A* and *B*, and hence we must have recourse to a fresh relation *E*, which comes between *D* and whatever we had before.<sup>119</sup>

Any new relation *C* which is introduced between *A* and *B* must itself be related to *A* and *B* by a further relation *D*; but this new relation *D* must be related to *A*, *B* and *C* by a new relation *E*. We might represent the stages of the regress in this way:

*A* \_\_\_\_ related by *C* to \_\_\_\_ *B*  
*C* \_\_\_\_ related by *D* to \_\_\_\_ *A*, *B*  
*D* \_\_\_\_ related by *E* to \_\_\_\_ *A*, *B*, *C*  
*E* \_\_\_\_ related by *F* to \_\_\_\_ *A*, *B*, *C*, *D*  
 ...

Bradley's target is the 'relational way of thought — any one that moves by the machinery of terms and relations' (1897: 33). His own examples are of cases where two 'qualities' (monadic universals) are said to stand as terms of some dyadic universal, or 'relation'; however, his presentation is consistent with cases where two particulars (*A* and *B*) are said to stand in a relational universal (*C*). Moreover, it can be applied to cases where one particular is said to instantiate a monadic universal, for the combination of universal (*A*) and particular (*B*) is a kind of relatedness, and *prima facie* the realist must account for this by saying that there is some relation (*C*) in which they stand.

We can construct a Bradleyan argument for the case where particular *a* instantiates the monadic universal *F* in this way. If *a* has the property *F-ness*, then in some sense *a* and *F* are combined — in Bradley's phrase, they 'co-exist in a certain way' (1897: 20). We might use the phrase 'instantial combination' for the kind of combination which is exhibited by things and the universal they instantiate. When *a* has the property *F*, then *a* and *F* are instantially combined. But in that case some *relation* is required to join *a* and *F* together in instantial combination — a relation of *instantiation* (*I* for short). But if *instantiation* is a relation in which *a* and *F* stand, then *instantiation* is a universal which is instantially combined with *a* and *F*. Therefore, it is claimed, some further relation *I\** is required to join *a*, *F* and *I* in instantial combination. The regress looms:

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<sup>119</sup>Bradley 1897, p.21

$a$  \_\_\_\_ related by  $I$  to \_\_\_\_  $F$   
 $I$  \_\_\_\_ related by  $I^*$  to \_\_\_\_  $a, F$   
 $I^*$  \_\_\_\_ related by  $I^{**}$  to \_\_\_\_  $a, F, I$   
 $I^{**}$  \_\_\_\_ related by  $I^{***}$  to \_\_\_\_  $a, F, I, I^*$   
 ...

When the problem is set out in this way, it can be seen that it is merely a special case of the regress described by Bradley. We can construct a Bradleyan regress for the case where  $a$  and  $F$  are related by instantiation, just as we can for the case where  $a$  and  $b$  stand in the relational universal  $R$ , and for Bradley's original example where two universals  $F$  and  $G$  stand in a higher-order relation  $R$ .

The structure of the regress is clear, whether the original *relata*  $A$  and  $B$  are taken to be two particulars standing in a relation, or particular and monadic universal in instantial combination. However, this account offers no more than an outline of the problem. Two questions in particular remain unanswered: what motivates the introduction of a new relation at each fresh stage of the regress; and why should we believe that the regress is vicious? Bradley's own comments do not permit of uncontroversial interpretation, and in the absence of agreement about precisely what problem the regress raises, there is no settled consensus about how we should respond to it. One common realist strategy is to deny that there is a genuine relation of instantiation which holds between particulars and universals, perhaps suggesting that what holds between particulars and universals is a 'non-relational tie' (Strawson 1959: 168), 'unmediated linker' (Loux 2002: 41) or 'nexus' (Bergmann 1964: 197); another to say that universals, like Fregean concepts, are 'unsaturated' and so naturally suited to combine with particulars (Armstrong 1997: 29); alternatively it might be conceded that an infinite number of relations is required, but denied that the regress is vicious (Russell 1903: 99). Another response is to introduce further entities — facts or states of affairs — which somehow ensure that a relation combines with its terms (Armstrong 1997: 118). Recently, Armstrong has proposed that the problem be avoided by saying that 'instantiation is a matter of non-mereological partial identity' (Armstrong 2004b: 139) between particular and universal.

The current proposal is that the availability of such different responses to the regress — which, in the case of Armstrong and Russell, may be advanced at different times by the same author — is symptomatic of the fact that 'Bradley's regress' is not *one* argument against realism, but rather a family of structurally similar arguments. The fact that the same process is appealed to each time — producing a new relation whose terms are the entities mentioned in the previous iteration — gives the misleading impression that the

same argument is discussed in every presentation. In fact, three very different regress arguments can be discerned which make use of the same process, but differ in their account of why each new relation must be introduced and why the relation is vicious.

One, discussed by Ryle (1939), Forrest (1984) and Armstrong (1974, 1978a), suggests that the need for a new relation at each stage is a consequence of the realist's commitment to a shared universal wherever there is qualitative sameness or 'sameness of type'. The situation in which  $a$  has  $F$  is similar to other instantiation-situations, e.g. where  $b$  has  $G$ , so the realist is committed to a universal  $I$  which accounts for this sameness of type and which both situations share. The same reasoning is applied to show that a further universal  $I^*$  must be introduced to account for the sameness of type between the new situations  $I(a,F)$  and  $I(b,G)$ ,<sup>120</sup> and an infinite process gets going when it is realized that again we have two new situations —  $I^*(a,F,I)$  and  $I^*(b,G,I)$  — which exhibit a sameness of type which requires analysis. This version of the regress is said to be vicious on the grounds that it reveals an inadequacy in the realist's account of qualitative sameness as the sharing of a common universal: every time a shared universal is proposed to 'account for' some qualitative sameness, a new qualitative sameness is introduced of which the realist owes us an account. I shall call it the 'Sameness' argument.

A different version of the regress is suggested by a challenge Bradley raises during the controversy with Russell in *Mind* of 1909-11. He asks

'What is the difference between a relation which relates in fact and one which does not so relate?'<sup>121</sup>

The relevance of the regress to Bradley's challenge is this: the regress is used to block one possible account of what makes the difference in the situation where a relation does, rather than does not, relate its terms. The realist might suggest that what makes the difference is the presence of a further relation  $I$  in which  $a$ ,  $R$  and  $b$  stand only when the relation  $R$  genuinely relates. A regress follows because the question recurs: what makes the difference in the situation where  $I$  does, rather than does not, relate its terms  $a$ ,  $b$ , and  $R$ ? For consistency's sake, the realist seems committed to a further relation  $I^*$  which makes the difference between a situation where  $I$  relates its terms and one where it does not, and then a further relation  $I^{**}$  to make the difference between a situation where  $I^*$  relates its terms and one where it does not.

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<sup>120</sup>I shall use a notation in which every capital letter stands for a universal and every lower-case letter stands for a particular. Where there is the possibility of doubt, brackets are mark off *relata* from relation. Thus  $I(a,F)$  is to be read as ' $a$  and  $F$  instantiate  $I$ '.

<sup>121</sup>Bradley 1911, p.74

Here the regress is vicious because it shows that the realist has no account of what makes the difference between situations where relations relate, and situations where they do not, and this raises the suspicion that nothing ever gets related in fact. An account of what makes the difference between  $aRb$  and  $\text{not-}(aRb)$  is an account of ‘in virtue of what’ (Campbell 1990: 29; Meinertsen 2007: 2)  $a$  and  $b$  stand in the relation  $R$  — in Bergmann’s phrase, an account of how such a relational truth is ‘grounded ontologically’.<sup>122</sup> Other authors talk of providing the ‘ultimate ontological basis’ (Cameron 2008a: 4) or the ‘unifier’ (Vallicella 2002: 19) of  $a$ ,  $R$  and  $b$ . I shall refer to this version of the regress as the ‘Difference-Maker’ argument.

A third version of the regress argument, which I shall suggest is closest to that intended by Bradley in *Appearance and Reality* (1897), aims to show neither that the realist has no satisfactory account of sameness of type, nor that the realist lacks an ontological ground or basis for truths about the combination of particulars and universals. Instead the claim is that the regress derives merely from the fact that instantial combination is a kind of *relatedness*, and from the realist’s commitment to the view that ‘relatedness requires a relation’ — that every case of relatedness is one where the related entities stand as terms to some further entity, a relation. Here the suggestion is that the combination between universal and particular is impossible, for each relation introduced in the course of the regress functions only as a term of some further relation, and never plays the role of actually *relating* some terms. I call this version of the regress the ‘Relatedness’ argument.

It seems reasonable to treat different ‘canonical’ realist responses as appropriate to different versions of the regress. In particular, the Fregean doctrine that universals are ‘incomplete’ entities, naturally suited to combine directly with particulars, promises to block the Relatedness regress by showing how particular and universal can combine directly without the need for even one intervening instantiation-relation, let alone an infinite hierarchy of relations. But a doctrine of the incompleteness of universals has little prospect of answering the other two regress arguments, for saying that universals are incomplete does not account for the sameness of type between different instantiation-situations, nor does it answer the question, what makes the difference between a situation where a relation  $R$  relates  $a$  and  $b$  and a situation where it does not? The incompleteness of a relation  $R$  at most guarantees that it combines with some terms or other, but it has nothing to say about what is different in the world between situations where  $R$  relates  $a$  and  $b$  and situations where it relates some other things instead (MacBride 2005: 606-7).

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<sup>122</sup>Bergmann 1964, p.196; see also Vallicella 2002, p.11



One realist response clearly *should* be understood as directed towards the Difference-Maker argument. This is the introduction of states of affairs. The states-of-affairs theorist claims to have an answer to Bradley's question, what makes the difference between *aRb* and not-*aRb*? According to such a theorist, the difference is that an entity exists in the former situation which does not exist in the latter — namely, the state of affairs *a's having R to b*. It is claimed that a regress is avoided: the Difference-Maker is not a relation, but rather a state of affairs which guarantees by its very existence that *aRb*. It seems that different realist responses to the regress are appropriate to different ways of formulating Bradley's regress. A first point, then, in favour of distinguishing between these different forms of regress argument is that it explains why the solutions proposed by realists are so diverse.

## Bradley

It is important not to ignore the differences between Bradley and his recent exponents. I shall suggest two significant points of comparison. First, Bradley's arguments are directed against an ontology of 'relation and quality' (1897: 25) in which a given particular — say a lump of sugar — is conceived as a bundle of its qualities in relation. Thus

'Sugar is, of course, not the mere plurality of its different adjectives; but why should it be more than its properties in relation? When 'white', 'hard', 'sweet' and the rest co-exist in a certain way, that is surely the secret of the thing.'<sup>123</sup>

This explains why Bradley presents his regress as an argument that qualities cannot be related to each other, rather than attacking a realist ontology in which universals and particulars combine in instantiation. His motivation for neglecting to include a factor of bare particularity in his regress is partly epistemological — that 'we can discover' (1897: 19) no such thing — and partly an assumption that any factor of particularity would have to function as a unifier of its different qualities. For the lump of sugar, 'its reality lies somehow in its unity' (1897: 19), so any element which constitutes the particularity of the lump of sugar must do so by relating its qualities. For Bradley, it seems, any unifying factor of particularity must itself be a relation. Thus Bradley's argument is an attack not merely on the ability of relations to combine with their terms, but also on the ability of properties to combine with each other to make up an object. This second problem is the bundle-theoretic analogue of our problem about the ability of a particular and a universal

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<sup>123</sup>Bradley 1897, p.20

to combine instantially. Were Bradley to encounter an opponent with an ontology of universals *and* particulars, he would no doubt raise our problem as well.

A second difference is that Bradley does not merely argue that an ontology of properties and relations is incoherent: he attempts to demonstrate the truth of monism, the view that in reality there is only one thing, the 'Absolute' (1897: 144). He argues that our division of the world into discrete things is a mistake, an illegitimate attempt to infer the structure of reality from the structure of appearance. Thus Bradley thinks that nominalist and realist alike are mistaken in their ontology. The nominalist should be wary of defending Bradley's argument, for if it succeeds on Bradley's own terms it will be disastrous for (pluralist) nominalist and realist alike.

Fortunately for the nominalist, he need not accept the reasoning behind Bradley's claim that the regress entails monism. Bradley's view is that the division of the world into distinct things requires that we accept the reality of relations, for two things, in order to be distinct, must themselves stand in the relation of *difference*. If there is no such relation between the two, then they are distinct only in appearance and not in fact:

'if there are no differences, then there are no qualities, since all must fall into one. But, if there is any difference, then that implies a relation.'<sup>124</sup>

This argument should not convince the nominalist, for he will reject Bradley's assumption that '*a* and *b* are different' can be true only if there exists a relation — *difference* — which holds between *a* and *b*. The nominalist disputes this point, claiming that '*a* and *b* are different' is committed to the existence only of the *relata a* and *b*. The nominalist can, consistently with his own position, claim that numerically distinct particulars *are different*, while denying that there is a relation of *difference*. Consequently there is no reason to accept Bradley's claim that his argument establishes monism as well as threatening realism.

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<sup>124</sup>Bradley 1897, p.25

## The Sameness Regress

Bradley offers no explicit reason why an extra entity — a new relation — must be introduced at each stage of the regress, apart from insisting that a relational predicate must refer to a relation if it is meaningful:

‘Without a relation it has no meaning; it is a mere word, and not a thought.’<sup>125</sup>

Subsequent presentations have offered alternative reasons why a new relation is required at each iteration of the regress. One is proposed by Gilbert Ryle (1939), who suggests that new relations are needed to account for the connection between ‘instances’ of the instantiation-relation and the relation itself:

‘Take the two propositions “this is square” and “that is circular”. We have here two different cases of something exemplifying something else. We have two different instances of the relation being-an-instance of. What is the relation between them and that of which they are instances? It will have to be exemplification Number 2. The exemplification of  $P$  by  $S$  will be an instance of exemplification, and its being in that relation to exemplification will be an instance of a second-order exemplification, and that of a third, and so on *ad infinitum*.’<sup>126</sup>

Ryle suggests that, once we have an exemplification-relation  $I$ , the relation between  $I$  and its ‘instances’ in different situations will be ‘instances’ of a new relation  $I^*$ , and the relations between  $I^*$  and its instances will in turn be instances of a further relation  $I^{**}$ , and so on without end. Each ‘instance’ of instantiation is a token of a higher type, but the instances of the relation between a type and its tokens are in turn tokens of some further type.

Ryle’s regress may be answered by the realist. Ryle relies on the premise that the ‘instances’ of the instantiation-relation  $I$  are numerically different between two different situations where a particular instantiates a universal — call them  $I(a,F)$  and  $I(b,G)$ . Only if *instantiation* as found in  $I(a,F)$  is numerically different from *instantiation* as found in  $I(b,G)$  can it make sense to claim that the instantiation-relations involved are tokens of some higher type. If the very *same* relation of instantiation is present in  $I(a,F)$  and  $I(b,G)$ , then we have one relation, not two ‘instances’ of a relation which must be counted as tokens of a higher type. But the central thesis of the realist is that relations are universals, literally identical

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<sup>125</sup>Bradley 1897, p.25. See also p.17

<sup>126</sup>Ryle 1939, p.138

in every situation in which they occur. So the realist can block Ryle's regress by claiming that there is only one instantiation-relation, and it is present in both situations  $I(a,F)$  and  $I(b,G)$ . There are no entities which are 'instances' of this relation distinct from the relation itself, and so no need to posit a higher type of which these 'instances' are tokens, in which case there is no need to posit a further relation which holds between the tokens and the type itself.

Armstrong (1974) offers a repair to this regress. For him, it is the *situations*  $Fa$  and  $Gb$  which are numerically distinct but are both tokens of a common type:

'Participation is a type of situation. So the question must arise what account we give of this type. Consistency demands that we say all the participation-situations participate in the Form of participation. This introduces further participation-situations. If this second-order participation is a different type of relation from first-order participation, then there will be an infinite regress of different Forms of participation.'<sup>127</sup>

Armstrong's regress proceeds as follows: take any two situations in which a particular and universal stand in the relation of *instantiation*:  $I(a,F)$  and  $I(b,G)$ . These situations exhibit a similarity or 'sameness of type': they are instances of the same *type* of situation. But, as Armstrong points out in a later presentation of the same argument,

'this is the very sort of situation which the theory of Forms finds unintelligible and insists on explaining by means of a Form.'<sup>128</sup>

We must introduce a further instantiation-relation  $I^*$  to serve as the type of which these situations are tokens. But being a token of a type is a situation in which the token 'participates in' the type, so there must be further 'participation-situations'  $I^*(a, F, I)$  and  $I^*(b, G, F)$ . These two situations *also* exhibit sameness of type, and must in turn be tokens of some further type  $I^{**}$ . So on without end. Armstrong alleges that arguments of this kind (which he describes as 'Relation regresses' (1978a:70)) are vicious because they show that the theorist has no satisfactory 'reductive account of all type-notions' (1978a: 21). The sameness of type between two instantiation-situations is accounted for by their sharing a common universal  $I^*$ , but then we have two *new* situations whose sameness of type must be accounted for as the sharing of another common universal  $I^{**}$ , which in turn gives rise to two new situations which also exhibit sameness of type and so require the realist to reapply his analysis.

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<sup>127</sup> Armstrong 1974, p.197; see Crane and Farkas 2004, p.220 for a similar account.

<sup>128</sup> Armstrong 1978a, p.70

Can this regress be blocked? Both Armstrong and Ryle believe it can, provided we deny that instantiation is a genuine relation. Ryle takes his regress to show only that ‘there is no such relation as being-an-instance-of’ (1939: 138), while Armstrong presents his version as an objection only to a ‘platonic’ realism which takes the instantiation-relation to be an entity in its own right (1974: 196). I shall argue that this is a mistake. Armstrong suggests that the realist’s obligation to explain the sameness of type between two instantiation-situations  $Fa$  and  $Gb$  arises only if he supposes that each situation contains a common constituent  $I$ . In fact, it is reasonable to think that there is a sameness of type of which the realist owes us an account even if we do not start from the assumption that the situations have some entity in common. The situations  $Fa$  and  $Gb$  are similar insofar as each is a situation in which a particular instantiates a property — a case in which a particular and universal are instantially combined. Therefore, it seems that there is a similarity of which the realist owes us an account even if we do not begin with the assumption that both situations involve a further entity, the relation *instantiation*. The realist is committed to positing a shared universal wherever there is objective similarity or sameness of type; it turns the procedure on its head to say that the realist is committed to recognizing sameness of type only where he *already* recognizes a shared entity. For that reason it seems that Armstrong’s version of the regress threatens even the realist who refuses to recognize an instantiation-relation.

As stated, the regress challenges the realist to account for the a similarity between the ‘situations’  $Fa$  and  $Gb$ . These ‘situations’ seem to be something like ‘states of affairs’ or ‘facts’ — entities whose constituents are the particulars and universals they involve. Can the regress be blocked by rejecting such entities? If there are no states of affairs — no ‘situations’ or ‘cases’ of  $a$ ’s being  $F$  — then there are no two entities, the situations  $a$ ’s *being F* and  $b$ ’s *being G*, which may be said to be similar, and so no need for the realist to posit an extra universal to account for their sameness of type. This line of thought is initially plausible, but collapses on inspection. Let us grant that there are no states of affairs, or ‘situations’ or ‘cases’ of  $a$ ’s being  $F$ . Nevertheless, pairs or n-tuples of things can exhibit similarities: Mr and Mrs Smith can be similar to Mr and Mrs Jones in that both are married, even though there is no composite object composed of the two Smiths or the two Joneses. Thus, it seems, there is a sameness of type between the two Smiths and the two Joneses of which we owe an account, even if we deny that there are composite objects formed from each pair.

A similar claim can be made with regard to the four entities  $a$  and  $F$ , and  $b$  and  $G$ . When it is the case that  $Fa$  and  $Gb$ ,  $a$  and  $F$  are similar to  $b$  and  $G$ , insofar as each pair of objects is instantially combined. This similarity holds regardless of whether we accept that there are

additional entities — states of affairs — which are composed of these particular/universal pairs. If the realist is committed to accounting for all similarities in terms of shared universals, he should posit a universal to account for the similarity between the two entities  $a$  and  $F$ , and the two entities  $b$  and  $G$ , when  $(Fa \ \& \ Gb)$  is the case, even if it is denied that  $a$  and  $F$  together make up some composite object. We may conclude that the Sameness regress cannot be blocked by denying ‘situations’ or ‘states of affairs’, for the realist is obliged to account for the sameness of type when  $Fa$  and  $Gb$  even if the particulars and universals are not constituents of composite ‘situations’ or ‘states of affairs’.

Armstrong claims that this kind of ‘relation regress’ is vicious because it shows the inadequacy of some proposed eliminative analysis of sameness of type (1978a: 21). But why should he level this accusation against the platonic *realist*? The realist is not required to offer an *eliminative* analysis of types, for his claim is that types exist, and they are universals. Armstrong’s initial formulation of his challenge to the nominalist makes this clear: realists are under no obligation to provide an eliminative analysis of statements about types, since they

‘hold that the apparent situation is the real situation’<sup>129</sup>

Other alternatives to realism are criticized because they attempt to do without types, although type-notions recur in their analysis. But an accusation of this kind cannot be made against the realist, for his is *not* trying to do without types in his analysis. If types exist, what harm is there in proposing a theory which mentions them?

The realist’s response, then, is this: at most, the regress demonstrates that, according to his theory, there is an infinite hierarchy of different universals, one to correspond to the sameness of type noticed at each new stage of the regress; but demonstrating that there are infinitely many different samenesses of type to which the realist analysis must be applied does not show that the analysis is in some way inadequate. The dialectical position would be different if the regress showed that the realist was endlessly deferring his responsibility to give an account of the original sameness of type between the cases where  $Fa$  and  $Gb$ ; then it could be claimed with some justice that he had no satisfactory account of this sameness of type, for the original *explanandum* would recur at each fresh stage of the regress. But this is not the accusation against the realist. Instead, the process is this: the realist says that the sameness of type between  $Fa$  and  $Gb$  consists in their sharing a common universal  $I$ ; but then a *new* sameness of type is noticed, between the higher-order situations  $I(a,F)$  and  $I(b,G)$ . When the realist says that this sameness of type consists in

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<sup>129</sup> Armstrong 1978a, p.12

their sharing a common universal  $I^*$ , it is replied that there is now some *new* sameness of type between the third-order situations  $I^*(a,F, I)$  and  $I^*(b,G, I)$ . But then the regress does not demonstrate that there is any one sameness of type for which the realist has no account; instead it shows that there are very many *different* samenesses of type to which the realist must be prepared to apply his analysis. The realist can reply: “I knew that already! There is no way to show that an account is inadequate simply by pointing out the number of different cases in which it must be applied.” For that reason, I suggest, the Sameness argument does not generate a vicious regress for the realist.

An alternative allegation is that the regress is ‘viciously uneconomical’ (Armstrong 1989a: 108) even if it is not strictly speaking vicious, since it requires that we recognize a distinct universal  $I$  to account for the similarity between  $Fa$  and  $Gb$ , another universal  $I^*$  for the similarity between  $I(a,F)$  and  $I(b,G)$ , another universal  $I^{**}$  for the similarity between  $I^*(a,F,I)$  and  $I^*(b,G,I)$  and so on into an infinite process. Even this claim can be challenged. Suppose we follow the stages of the regress. It is claimed that there is an objective similarity between the situations  $Fa$  and  $Gb$ ; the realist is obliged to account for this sameness of type by introducing a shared universal — call this  $I$ . Next, it is claimed that there are two new situations (or collections of entities) which themselves exhibit sameness of type —  $I(a,F)$  and  $I(b,G)$  — and that the realist must introduce a *further* universal to account for this sameness of type. Here it seems the realist can stop the regress, for he claim that the sameness of type between two situations is *already* accounted for when we accept that they involve a common universal, namely  $I$  itself. What accounts for the sameness of type between the situations *Socrates’ being wise* and *Plato’s being wise* is that both situations share a common universal: both involve the universal *wisdom*. Likewise, we might expect that the sameness of type between  $I(a,F)$  and  $I(b,G)$  has *already* been accounted for when we recognize that both situations involve the same universal, the relation  $I$ . If the sameness of type between the two situations can be accounted for by their having a universal in common, there is no reason to suppose that further universals must be introduced as Armstrong supposes.

This realist response suggests two conclusions: first, that the Sameness argument does not motivate a genuine regress, for we can account for the sameness of type between two instantiation-situations by appealing to the fact that they involve a common universal  $I$ , without introducing *further* universals  $I^*$ ,  $I^{**}$ , ... which both situations instantiate; second, that recognizing a genuine relation of instantiation,  $I$ , is not, as Armstrong thinks, the mistake which gets this version of the regress started in the first place. Instead it is the means by which we account for the sameness of type between different situations in

which particulars and universals are instantially combined. All such situations are similar insofar as they involve a common universal — the relation *instantiation*. Indeed, there is good reason to think that the sameness of type in question should be accounted for by the positing of only *one* universal found in every such case. The original reason for recognizing a sameness of type between *Fa* and *Gb* was that each is a case of instancial combination. But then what *Fa* and *Gb* have in common with each other, they will also have in common with every other case of particulars and universals combining with each other, and indeed with any case of universals combining with each other in higher-order instantiation. If the analysis of sameness of type compels us to recognize a universal shared between *Fa* and *Gb*, it will be *one* universal shared between these situations and every other case of instantiation.

I have argued that the possibility of an infinite regress of distinct samenesses of type does not reveal the realist's account of sameness of type to be inadequate, and that the sameness of type between different instantiation-situations is accounted for as soon as we realize that each such situation involves the *same* instantiation-relation; moreover, there is good reason to suppose that only *one* such universal is required to account for the sameness of type between every instantiation-situation. Thus we may conclude that the Sameness regress is not a genuine threat to realism.



## The Difference-Maker Argument

Although Bradley in *Appearance and Reality* concentrates his attack on the ‘theoretical intelligibility’ of the combination between relations and their terms, his exchange with Russell in *Mind* of 1909-11 changes the focus of the criticism slightly. Here, he states his ‘difficulty as to “unities”’ as the question,

‘Is there anything... in a unity besides its “constituents”, i.e. the terms and the relation, and, if there is anything more, in what does this “more” consist?... What is the difference between a relation which relates in fact and one which does not so relate?’<sup>130</sup>

The connection between the two halves of the quotation is this: Bradley expects that the difference between a relating relation and a relation which fails to relate is to be provided by some unifying factor which is a distinct entity from the terms and the relation. The relevance of his earlier regress argument is to show that this unifying factor cannot be a relation, for if it is a relation it must be unified or ‘instantially combined’ with the things it unifies, and so there must be a *third* relation doing the work of unifying this second relation with the original relation and its terms, and a *fourth* relation doing the work of unifying the third relation with the other entities, and so on *ad infinitum*.

In this section I advance three major claims about the Difference-Maker argument. First, that it depends on acceptance of the Strong Truthmaker Principle:

(STM) If  $p$ , some  $x$  exists such that  $x$ ’s existence necessitates that  $p$ .<sup>131</sup>

Call an entity a ‘necessitating truthmaker’ for  $p$  if that entity meets the condition laid down by (STM) — if it necessitates, by its existence, that  $p$  is the case. I shall claim that the demand for a difference-maker is simply the demand for a necessitating truthmaker; indeed, once this is made explicit, we can see that the regress itself functions as no more than a piece of theatre, for we do not need to use the regress to show that relations cannot serve as difference-makers in the sense intended by Bradley. My second major claim is that there are reasons to think that the demand for a difference-maker cannot be met by the realist even if he introduces composite ‘facts’ or ‘states of affairs’, while the third is that the correct response to the Difference-Maker argument is to drop the Strong Truthmaker Principle (STM). Once (STM) is abandoned, this argument no longer threatens the realist project.

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<sup>130</sup> Bradley 1911, p.74

<sup>131</sup> See Fox 1987, p.189; also Armstrong 2004a, p.5

## Difference-Makers are Necessitating Truthmakers

It becomes apparent that the Difference-Maker argument is a demand for necessitating truthmakers for truths about instantiation when we consider why two *prima facie* acceptable responses do not solve the problem to Bradley's satisfaction. One approach is proposed by Russell:

‘A complex differs from the mere aggregate of its constituents, since... the relation which is one of its constituents enters into it as an actually relating relation, and not merely as one member of an aggregate.’<sup>132</sup>

This account is of a piece with the distinction Russell made in *The Principles of Mathematics* between ‘a relation in itself and a relation actually relating’ (1903: 49). We may interpret Russell's position in this way: ‘what makes the difference’ when  $aRb$  is that the relation  $R$  actually relates its terms; when it is not the case that  $aRb$ , the relation  $R$  does not actually relate the terms.

Bradley, writing in response to Russell's suggestion about ‘actually relating relations’, claims that

‘my difficulty as to “unities” remains.’<sup>133</sup>

Any adequate account of the demand for a difference-maker should explain *why* the demand is not met to Bradley's satisfaction simply by saying that what is different between  $aRb$  and  $\text{not-}aRb$  is that the relation ‘actually relates’ in the former but not the latter case. Here is one suggestion: what Bradley finds objectionable is that Russell's response is merely *descriptive* — it tells us *how* the world is different, but without identifying some *entity* that makes the difference. This interpretation also explains why Bradley is interested in finding something ‘more’ in a complex besides the relation and its terms: according to the current proposal, what Bradley wants to find is some *entity* which can be identified as making the difference between  $aRb$  and  $\text{not-}aRb$ .

If Bradley's demand is for a difference-making *entity*, we might suggest an alternative, trivial solution. If an entity is needed to make the difference, why shouldn't it be the relation itself? Indeed, an alternative reading of Russell suggests that he wants to credit relations themselves with the difference-making power that Bradley demands, and we may also ascribe a view of this kind to Bergmann (1964: 197), who accepts the need for something

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<sup>132</sup>Russell 1910, p.374

<sup>133</sup>Bradley 1911, p. 74

to serve as an ontological ground of relational truths, but claims that his ‘fundamental tie or nexus’ is an entity capable of serving as just such an ontological ground.

In the exchange with Russell, Bradley does not discuss the possibility that the relation itself could be credited with ‘making the difference’ between  $aRb$  and not- $aRb$ ; we must suppose that he is aware of the possibility but thinks it unacceptable for some reason. One possible diagnosis is that Bradley believes whatever makes the difference must be something external to, or distinct from, the things it brings together in combination: that since  $R$  is among the things that are to be combined, it cannot be that  $R$  is also the thing that makes the difference when they *are* so combined. However, it is not clear why a difference-maker for a relational fact *must* be something external to that fact; if nothing further is said to justify this supposition then we will have no reason to deny that it is the relation  $R$  that ‘makes the difference’ when  $aRb$ .

A different suggestion has gained wide currency among interpreters of Bradley. It starts from the idea that a difference-making entity must be one that plays a certain role, namely, the role of (in some sense) ensuring or bringing it about that the relation and *relata* are combined as they are. Moreover, the difference-making entity is supposed to ensure this combination merely by its presence in the world. When the demand for a difference-maker is understood in this way, it is clear why the relation  $R$  cannot be the difference-making entity for  $aRb$ , for  $R$  could have been present although  $aRb$  was not the case — for example in a situation in which  $cRd$  instead. This problem is not solved by claiming that relations are intrinsically made for relating — that there are no relations which do not relate — as Read claims (2005: 319), because the demand for a difference-maker is not merely a request to say what grounds the relatedness, rather than the unrelatedness, of a given relation: it requires more than that, that we say what grounds the combination of a relation  $R$  with the very things it does relate. What makes the difference between the case when  $R$  relates  $a$  and  $b$  and the case when it relates  $c$  and  $d$  instead? It cannot be the mere presence of the relation itself, even if there are no relations which do not relate, because the relation  $R$  could have been present relating  $c$  and  $d$  without  $aRb$  being the case.

If we accept this diagnosis of Bradley’s thought, it is a small step to accepting that his challenge to the realist is merely the demand for a necessitating truthmaker in disguise. It may also be agreed that the ‘regress’ is an inessential feature of the argument as we are reconstructing it, serving merely as a piece of theatre. Each claim may be established swiftly. First, the demand for a difference-maker is the demand for an entity that can ‘ensure’ (MacBride 2005: 603) that the relation relates the terms that it does; the difference-making entity is supposed to be *responsible* for the particulars and relations combining as they are.

But an entity that ensures by its mere presence or existence that  $aRb$  is an entity whose existence *necessitates* that  $aRb$ , and an entity that is responsible for the truth of  $aRb$  is an entity *in virtue of which*  $aRb$ . I submit that Bradley's demand for a difference-maker is merely the demand for a truthmaker in disguise. In particular, it is the demand for a *necessitating* truthmaker — one that satisfies the Strong Truthmaker Principle:

(STM) if  $p$ , some  $x$  exists such that  $x$ 's existence necessitates that  $p$ .

Bradley does not have to apply this principle to *every* truth in order to get his result, for he extends the demand for a difference-maker only to relational truths such as the truth that  $aRb$ . So the Difference-Maker argument does not depend on our acceptance of 'Truthmaker Maximalism' (Armstrong 2004a: 5), the doctrine that all truths have truthmakers. We need only accept that truthmakers are needed for relational truths such as the truth that  $aRb$ . Nevertheless, no weaker formulation than (STM) will capture the nature of the difference-making entities that Bradley requires, for his challenge is to identify the entity such that its mere presence in the world necessitates that  $aRb$  is the case. If Bradley required anything less than a necessitating truthmaker, it would be a mystery why he could not accept a solution on the lines of those offered by Russell and Bergmann.

It seems that Russell was aware of this feature of Bradley's argument. He observes,

'This opinion seems to rest upon some law of sufficient reason, some desire to show that every truth is "necessary"... it appears to me that fundamental truths are merely true in fact, and that the search for a "sufficient reason" is mistaken.'<sup>134</sup>

The principle of sufficient reason, roughly, is the principle that every contingent truth must have an explanation (Pruss 2006: 10); these explanations need not be *causal*, but they must involve showing how the truth in question is necessitated by some prior feature of the world. In adverting to the Principle of Sufficient Reason here Russell demonstrates his awareness of the kind of thinking behind Bradley's argument: the thought that there must be something underlying the truth that  $aRb$ , and furthermore, something that *necessitates* that  $aRb$ .

My second claim, that the regress in this argument functions merely as a piece of theatre, seems reasonable because, once we make explicit the constraints on an acceptable difference-maker, we immediately realize that *no* relation is capable of satisfying them.

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<sup>134</sup>Russell 1910, p. 374

Bradley's demand is for an entity which, by its mere existence, necessitates that  $aRb$ . But any relation — as long as it could have existed although instantially combined with different *relata* from those it actually relates — is completely unfit to play that role, since the existence of such a relation does not *necessitate* that any specific relational truth is the case. A regress develops when we propose a relation  $I$  to ensure that  $a$ ,  $R$  and  $b$  are combined, by relating them to each other, but then notice that  $I$  by itself is not sufficient to *ensure* that it relates the things it does, and propose a further relation  $I^*$  to ensure that  $I$  combines with its *relata*. Again, it is noticed that  $I^*$  is not sufficient to ensure that it relates the things it does, so a further relation  $I^{**}$  is proposed, and so on in an infinite process. However, once we understand what an entity would have to do in order to be a difference-maker for a relational truth, it is immediately apparent that no contingently instantiated relation could play that role, in which case there is no reason even to embark on the regress in the first place. We do not need to play along with the charade of vainly proposing relation after relation as a candidate to 'make the difference', if we already understand that *no* relation could do the requisite difference-making work.

The Difference-Maker 'regress' shares a striking feature with many other regress arguments in logic and mathematics. Often it is the case that the premises used to generate a regress can be shown to entail a contradiction; the regress is merely a more vivid way of showing that the initial position is absurd. Examples include Plato's 'Third Man' argument in the *Parmenides* (Vlastos 1954), and the mathematical proof that  $\sqrt{2}$  is irrational (Nolan 2001: 524). Likewise, we might think, the 'regress' here is merely a vivid way of demonstrating to the realist that he has accepted an inconsistent set of premises: to enter the regress, the realist must suppose that the demand for necessitating truthmakers is legitimate, and that relations are suitable candidates to play that truthmaking role, but to find each relation an unsatisfactory truthmaker he must be aware that contingently instantiated relations of the kind he proposes are completely incapable of playing such a role.

I have argued that the Difference-Maker argument is best understood as an argument about necessitating truthmakers; given this interpretation, it becomes apparent that the 'regress' part of the argument is mere theatre and is not integral to Bradley's challenge. In the next section I suggest that the demand for a difference-maker is not one the realist can meet; the final part of this chapter will argue that the correct response to the Difference-Maker argument is simply to drop the Strong Truthmaker Principle on which it depends. As we have seen, this is a response with which we could expect Russell to have some sympathy.

## States of Affairs

The canonical response to this version of Bradley's regress is a strategy proposed by David Armstrong, that the truthmaker for a relational truth of this kind is a *sui generis* entity — a 'fact' or 'state of affairs' containing  $a$ ,  $R$  and  $b$  as constituents — which by its very existence necessitates that  $aRb$  (Armstrong 1997: 118; 2004a: 49). The state of affairs is the extra entity which makes the difference between a case where a relation does relate in fact, and a case where the relation does not relate. When the state of affairs  $aRb$  exists, it is true that  $aRb$ , and when the state of affairs does not exist, it is not. This suggestion fits Bradley's expectation perfectly: not only have we said what is the difference between  $R$  relating and  $R$  not relating, we have also explained this difference in terms of the existence of an extra entity which ensures by its presence in the world that  $R$  actually relates  $a$  and  $b$ . Since this entity *successfully* ensures that  $aRb$  is the case, there is no need to introduce further entities to do difference-making work, and so no need to embark on a regress. Armstrong's account is presaged by Wolterstorff (1970, p.101), who talks of 'cases of a relation' rather than 'states of affairs', and is already present in Russell's appeal to 'complexes' whose constituents are  $a$ ,  $R$ , and  $b$  (Russell 1910: 374).

It might seem that introducing states of affairs to serve as truthmakers is an adequate response to the demand for a Difference-Maker. However, we should be suspicious of the idea that merely introducing an extra composite *entity* presents an automatic solution to the problem. Bradley describes his demand for a Difference-Maker as 'my difficulty as to "unities"' (1911: 74); since he goes on to talk of a 'unity' as having 'constituents' it is clear that his problem arises in *response* to an ontology of Russellian 'complexes'. Bradley supposes that he can raise a genuine challenge even against a theorist who recognizes composite entities such as states of affairs. I shall suggest that he is right: if the demand for a truthmaker, or 'Difference-Maker' is accepted, then there are truths about states of affairs for which no adequate necessitating truthmaker can be supplied. My suggestion is this: when we realize the features that a state of affairs must have in order to count as a necessitating truthmaker, it is no longer plausible to accept that there is no *further* truthmaker for facts about how the state of affairs stands to its constituents. If relational truths stand in need of truthmakers, then surely truthmakers are *also* needed for truths about the relation between a state of affairs and its constituents.

What, precisely, *is* the relation between a state of affairs and its constituents? A state of affairs [ $aRb$ ] is a 'complex' whose constituents are particulars  $a$  and  $b$ , and relational universal  $R$ . A first point is that the existence of the complex must not be guaranteed by

the existence of its constituents: if the complex  $[aRb]$  exists in every circumstance that  $a$   $R$  and  $b$  exist individually, then  $[aRb]$  is not a necessitating truthmaker, for there may be circumstances in which  $a$ ,  $R$  and  $b$  exist although  $a$  and  $b$  do not stand in the relation  $R$ . This stipulation serves to distinguish states of affairs from entities such as sets and mereological fusions or aggregates, which are commonly thought to exist automatically, so long as their constituents do. Instead, the state of affairs  $aRb$  must be thought of as existing only when  $a$  and  $b$  actually stand in the relation  $R$ .

We may distinguish two other features of states of affairs. First, states of affairs have their members essentially: the state of affairs  $[aRb]$  could not have existed with different members, e.g.  $c$ ,  $R^*$  and  $d$ , for then it could have existed although  $a$  and  $b$  did not stand in  $R$  and it is not a necessitating truthmaker for that truth. Second, a given state of affairs must impose an order on its constituents: the state of affairs  $[aRb]$  could not exist although it unified the same constituents in a *different* arrangement, for then it could have existed when it was the case that  $bRa$  and  $\neg aRb$ , and is not a necessitating truthmaker for the truth that  $aRb$ .

Armstrong recognizes that there must be a special kind of relation between a state of affairs and its constituents:

‘States of affairs hold their constituents together in a non-mereological form of composition, a form of composition which even allows the possibility of having different states of affairs with identical constituents.’<sup>135</sup>

In fact, this description understates what is required of ‘non-mereological composition’: the theorist must be willing not only to claim that there can be different states of affairs with the same constituents, but also that the identity of a state of affairs depends on preserving the arrangement of its constituents. As I have argued, if a state of affairs  $[aRb]$  could have existed with the same constituents arranged in a different way, it is not a necessitating truthmaker for the truth that  $aRb$ .

One criticism of Armstrong’s states of affairs is notorious: it is David Lewis’ allegation that

‘unmereological composition... to my mind is a contradiction in terms’;<sup>136</sup>

Lewis could make such a charge without fear of inconsistency with his ontology of sets because, according to him, even the apparently ‘unmereological’ composition by which a

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<sup>135</sup> Armstrong 1997, p.118

<sup>136</sup> Lewis 1992, p.213

set is ‘composed’ of its members can be explained in terms of the axioms of mereology: sets can be treated as mereological fusions of the singleton sets of their members (Lewis 1991). We might not share Lewis’ confidence that *every* way in which a composite entity is ‘composed’ of its constituents must be reducible to the calculus of parts and wholes; nevertheless I shall argue that there is something incoherent about the states of affairs under consideration, namely that the truthmaker theorist lacks adequate necessitating truthmakers for relational truths about the relation between a state of affairs and its constituents.

The first stage in making such a case is to suggest that a state of affairs is a *distinct* entity from its constituents, and indeed from any sum or whole of which its constituents are parts. This seems reasonable, for we have observed that the existence of the constituents, and even the existence of the constituents in combination, does not guarantee the existence of the state of affairs: it could be that  $a$ ,  $R$  and  $b$  exist, and that they are combined (if  $bRa$  is the case), although there is no state of affairs [ $aRb$ ]. It might be thought that the thesis that a state of affairs is a distinct entity rather than a sum or whole of its constituents depends on some Lewisian prohibition on sums of objects being made up in unmereological ways; if the Lewisian prohibition is dropped we might simply say that the state of affairs is *not* a distinct entity from its constituents, and in fact is simply some collection or ‘sum’ of these constituents, albeit a sum made up according to unmereological rules that allow for some ordering of parts. To make such a suggestion would be to misunderstand the reason for saying that a state of affairs is a distinct entity, rather than a sum of which its constituents are parts, which is that its existence is not *guaranteed* by the existence of its constituents, as, for example, the existence of a given set follows automatically from the existence of its members. Such an argument is employed by Vallicella:

‘A fact is not a whole of parts in that the existence of the parts does not entail the existence of the whole.’<sup>137</sup>

Even if we allow entities —  $\phi$ s — which we identify with ‘unmereological sums’ of their constituents, we ought to do so only if the existence of the constituents guarantees the existence of the  $\phi$ s; otherwise how could it be claimed that the  $\phi$ s are identical with *any* sum or collection of their constituents, whether mereologically constituted or not?

We should expect Armstrong to have some sympathy with this point, for he is the inventor of the doctrine of the ‘ontological free lunch’, that

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<sup>137</sup> Vallicella 2002, p.21



‘What supervenes is no addition of being.’<sup>138</sup>

The idea is that, if the existence of some alleged extra entity *a* supervenes on the existence of an entity *b*, then *a* ‘is not really... an extra entity’ (1997: 13). For that reason, mereological wholes (whose existence supervenes on that of their parts) are not really additional entities (1997: 13). The current proposal simply turns Armstrong’s principle around, to claim that

What does not supervene *is* an addition of being.

In other words, since the existence of a state of affairs is not guaranteed by the existence of its constituents, a state of affairs ought to be considered a distinct entity over and above its constituents. I submit that this converse of Armstrong’s principle is at least as plausible as the doctrine of the Ontological Free Lunch itself. If that is correct, we must think of a state of affairs as an entity distinct from its constituents that is related in certain ways to them; these relational truths will be truths of the very kind for which Bradley suggests truth-makers must be provided.

The second step in setting up a problem for the theorist of states-of-affairs-as-truthmakers is to point out that, since the state of affairs is a distinct entity from its constituents, those constituents stand in a relation to it — a relation which we might call ‘state-membership’ — and that there are various truths about the relation between a state of affairs and its members for which, *prima facie* at least, truthmakers must be provided. One is that state-membership guarantees the *unity* or *combination* of the members, for *a R* and *b*’s membership of the state of affairs [*aRb*] guarantees that they are instantially combined with each other; another is that state-membership imposes an *order* on the constituents, for the consequence (for *a*) of being a state-member of [*aRb*] is different from the consequence of *a*’s being a state-member of [*bRa*]. Finally, there is the simple fact that a given state of affairs has the members that it does, rather than having some other entities as its members.

It might seem that the states-of-affairs theorist has a straightforward answer to the demand for truthmakers for such truths: it is that they have a truthmaker, namely the state of affairs [*aRb*] itself. Since [*aRb*] *could not* have existed unless it had the constituents that it does, unified in the order that they are, all these truths are necessitated by the existence of [*aRb*] itself; therefore [*aRb*] is their truthmaker. But this, I suggest, only *postpones* the problem of providing truthmakers, for now we need to know, what is the truthmaker for the modal truth that [*aRb*] could not have failed to have the constituents that it does, in the order that they are in? Three factors make it plausible to think that such a truthmaker

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<sup>138</sup> Armstrong 1997, p. 12

may legitimately be demanded in this case. First, since the state of affairs  $[aRb]$  is a distinct entity from its constituents, there is no logical impossibility in its having had different constituents from those it has; it is not as if we can guarantee that the state of affairs has its members essentially simply because it is called ' $[aRb]$ '. If the impossibility of a state of affairs changing its members is not a *logical* impossibility, then we should expect its truth to be made true by something in the world.

Second, the modal truth that a state of affairs could not have had different members is a *relational* truth: it is the truth that, necessarily, *these very things*  $a$ ,  $R$  and  $b$  stand in the state-membership relation to the state of affairs  $[aRb]$ . If the state-of-affairs theorist has a consistent treatment of relational truths, then the truthmaker for *any* relational truth will be a state of affairs whose constituents are the relation itself and the things which stand in it. In that case, the truthmaker for the truth that  $[aRb]$  has the members it does will be a higher-order state of affairs whose constituents are the state-of-affairs  $[aRb]$ , the state-membership relation, and  $[aRb]$ 's constituents. But then there will be an even higher-order relational truth that the second-order state of affairs has the members it does, and we should recognize a third-order state of affairs guaranteeing the membership of the second-order state of affairs. A regress can be resurrected at this point.

Third is an *ad hominem* point against Armstrong. He attacks theories that fail to provide adequate truthmakers for modal truths (2004a: 1-3). Why is it not equally blameworthy to propose the modal truth that  $[aRb]$  could not have failed to have the members that it does, without providing a truthmaker for that truth? It does not seem enough to say that the state of affairs itself makes it true that it could not have had other members than its actual members: how could one entity (the state of affairs) guarantee that it could be related only to *these* entities (its constituents) rather than any others? The necessity involved is at best mysterious.

Such considerations suggest we should not expect states of affairs to meet Bradley's challenge, for there are truths about the relation between a state of affairs and its constituents for which no satisfactory necessitating truthmaker can be given. Could any other kind of entity provide adequate truthmakers? In Chapter 1, I suggested that the nominalist could claim that the demand for truthmakers is adequately met either by an ontology of states of affairs which have only particulars, rather than particulars and universals, as constituents, or by positing tropes to serve as truthmakers. Now that universals have been introduced for another reason — to play the role of respects in which things are the same — it does not seem that either account of truthmaking entities should appeal to the realist. He ought not recognize states of affairs which lack universals as constituents, because any given truth,

according to the realist, is a truth *about* the combination of universals and particulars. How could such truths be grounded in entities composed of particulars alone?

The second suggestion is more plausible, since there is nothing obviously incoherent about an ontology of both universals and tropes (Armstrong 1989a: 17): tropes would be instantiated by particulars, and universals by tropes, with the universals accounting for the sameness of type between the tropes and (indirectly) for the sameness of type between the particulars themselves. However, it would be better not to introduce two categories of entity — universals and tropes — if it is possible to get by with universals alone. For that reason we should not accept such an ontology unless we are sure that the demand for necessitating truthmakers is legitimate. In the next section I argue that the truthmaker theorist has given us no good reason to accept that demand.

## Doing without Necessitating Truthmakers

The current version of Bradley's Regress depends on our willingness to apply the Strong Truthmaker Principle (STM) to relational truths such as the truth that  $aRb$ . No weaker truthmaker principle is sufficient to motivate a problem for the realist, for the demand for a 'difference-maker' is the demand for a necessitating truthmaker, an entity that guarantees, by its mere existence, that  $aRb$ . I shall argue that there is no compelling reason to accept (STM) in the case of relational truths of the kind at issue. If (STM) is rejected, there is no reason for the realist to acquiesce in the demand for an *entity* — a necessitating truthmaker — which guarantees that  $aRb$  is the case; consequently rejecting (STM) enables the realist to defeat this version of Bradley's regress.

No-one should deny that *some* true propositions have necessitating truthmakers. Socrates is clearly a suitable necessitating truthmaker for 'Socrates exists', for the existence of Socrates necessitates the truth of 'Socrates exists'. He is also a suitable necessitating truthmaker for essential truths about himself. For example, if Socrates could not have failed to be human, then Socrates himself necessitates, and hence is a suitable truthmaker for, 'Socrates is human'. He could also be said in these circumstances to be a candidate necessitating truthmaker for 'someone is human'. Moreover, any existing thing is a candidate necessitating truthmaker for any necessary truth: if there is no possible world in which  $p$  fails to be true, then there is no possible world in which a given entity exists and  $p$  fails to be true, so that entity may be said to 'necessitate' the truth of  $p$ .<sup>139</sup> What is at stake, then, is not whether the world contains entities that play the role of necessitating truthmakers, but whether the principle that truths have necessitating truthmakers should be extended to cover truths about the instantial combinations formed by universals and particulars, schematically truths such as  $Fa$  and  $aRb$ . (I put to one side the question of whether we must also be able to supply truthmakers for negative, general and modal truths.)<sup>140</sup>

In what follows I distinguish three kinds of argument in favour of extending the requirement for necessitating truthmakers to truths of this kind. First, that the Strong Truthmaker Principle (STM) should be adopted because it enables us to arrive at a fair assessment of what there is; second, that (STM) follows from 'commonsense realist' intuitions about the 'groundedness' of truth. Finally it might be claimed that (STM) enables us to 'catch cheaters' who propose unacceptable metaphysical theories. I argue in each case that the argument does not succeed in motivating the Strong Truthmaker Principle;

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<sup>139</sup>These observations are made by Lewis 1992, p.216.

<sup>140</sup>See Armstrong 2004a, Parsons 2006, Rodriguez-Pereyra 2006 and Mumford 2007 for recent discussion.

therefore the realist has no reason to accept the demand for necessitating truthmakers and consequently need not fear the Difference-Maker regress. We can reply to Bradley along the lines proposed by Russell: what is different in the world when ' $aRb$ ' is true is simply that  $aRb$ .

## 1. Truthmakers and Ontic Decision

A first argument in favour of (STM) is that investigating the entities required as necessitating truthmakers for our theory is the best way of reaching a decision about what there is; that we should accept that truths have necessitating truthmakers *because* ontological enquiry is best understood as the search for the truthmakers for statements in our theory. Whereas Quine associates ontological commitment with bound variables of quantification, the truthmaker theorist may claim that the ontological commitments of a theory do not go beyond the entities needed as *truthmakers* for that theory. This view of ontological commitment is endorsed by Cameron:

‘What are the ontological commitments of a theory? For Quine, it is those things that must be said to lie within the domain of the quantifiers if the sentences of the theory are to be true. I am a truthmaker theorist: I hold that the ontological commitments of a theory are just those things that must exist to make true the sentences of that theory.’<sup>141</sup>

The same thought is suggested by some remarks of Armstrong’s:

‘To postulate certain truthmakers for certain truths is to admit those truthmakers to one’s ontology. The complete range of truthmakers admitted constitutes a metaphysics.’<sup>142</sup>

Similar views may be found expressed by Melia (2005: 74) and Heil (2003: 9). In each case, the suggestion is that we ought to accept an ontology containing *only* those entities which are needed as truthmakers for our truths.

Two criticisms of this position arise immediately. One is that mere adherence to the Strong Truthmaker Principle does not justify an ontology restricted to truthmaking entities alone. (STM) is a principle about what there is — necessitating truthmakers — rather than about what there is *not*. How do we get from a principle that truths have necessitating truthmakers

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<sup>141</sup>Cameron 2008b, p. 4

<sup>142</sup>Armstrong 2004, p. 23

to the view that our ontology should include nothing other than the entities needed as truthmakers? The truthmaker theorist might reply that the existence of a necessitating truthmaker for  $p$  is *sufficient* for the truth of  $p$ ; therefore we can be sure that nothing else is required for  $p$ 's truth. The inference can be challenged, however. Suppose there are entities whose existence is a precondition of the existence of the truthmaking entity itself, but which are not themselves sufficient for truth. These entities will be *required* for the truth in question, but are not truthmakers for it. For example, suppose that the truthmaker is the state of affairs [ $aRb$ ]. This state of affairs could not have existed unless its constituents  $a$  and  $b$  existed; but  $a$  and  $b$  are not truthmakers for the truth that  $aRb$ ; hence the current proposal seems committed, quite mistakenly, to excluding  $a$  and  $b$  from the list of the ontic commitments of ' $aRb$ '.

A second criticism is that restricting our ontology to truthmaking entities alone leaves us with no way of doing systematic semantics, for semantics depends on our ability to associate certain kinds of expression with entities to serve as their referents. The current proposal would require that we admit *only* the entity needed as a truthmaker for ' $aRb$ '; but any systematic semantics requires more than that, for it requires in addition the non-truthmaking entities  $a$  and  $b$  to serve as referents for the expressions ' $a$ ' and ' $b$ '. It will be objected that truthmaker theorists *do* accept non-truthmaking objects and properties in addition to truthmaking states of affairs; for example, Armstrong suggests that an ontology based on the truthmaker principle will *also* accept non-truthmaking objects and properties on the basis of the role they play in truths such as ' $aRb$ ' and ' $Fa$ ' (2004: 24). However, such additions to his ontology leaves the theorist vulnerable to a charge of inconsistency with the stated commitment to an ontology of truthmakers *alone* (Shaffer 2008: 13). It is not clear *why* the truthmaker ontologist thinks that we *should* accept properties and relations on the basis of such truths, for which they are not truthmakers.

These objections suggest that acceptance of (STM) licenses only that we augment our ontology to include truthmakers in addition to the ontological commitments we recognize already, not that we restrict our ontology to truthmaking entities alone. The question at issue, however, is whether there is independent reason to think that such an ontology is preferable; whether the ontological revision associated with (STM) constitutes a reason to accept the Principle itself. Such an argument has been advanced by Armstrong. He is a long-standing critic of Quine's association of ontic commitment with variables of first-order quantification alone, on the grounds that this fails to recognize the significance of the predicate.

'Quine gives the predicate what has been said to be the privilege of the harlot:

power without responsibility. The predicate is informative, it makes a vital contribution to telling us what is the case, the world is different if it is different, yet ontologically it is supposed not to commit us. Nice work: if you can get it.<sup>143</sup>

In more recent work on truthmakers, this criticism of Quine is suggested as a reason why metaphysics is better construed as an enquiry about truthmakers:

‘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 of propositions but also on their *predicates*... when we look to truthmakers for truths, subject and predicate start as equals, and we can consider the ontological implications of both in an unbiased way.’<sup>144</sup>

One problem with Armstrong’s argument is already familiar: if we restrict our ontological enquiry to the search for *truthmakers* for truths, then the ‘truthmaker’ account of the ontology associated with a truth such as ‘*aRb*’ is no fairer than the Quinean account. Since the truthmaker account recognizes only one entity — the fact or ‘state of affairs’ that makes it true that *aRb* — it cannot be claimed to do any more justice to predicates than does the Quinean account. Neither recognizes an entity which is picked out by the predicate alone.

An obvious response can be made. A truthmaker ontology is fairer to predicates because it recognizes entities — states of affairs — which have universals as constituents. The ontological significance of predicates is recognized indirectly, in virtue of the fact that universals are needed as constituents of truthmaking entities. However, this response can be met. I argued in Chapter 1 that we should not rule out the possibility of ‘nominalistically acceptable’ accounts of truthmaking entities, either as states of affairs whose only constituents are particulars, or as tropes. If we have no reason to believe that universals are needed as constituents of the truthmaking entities, there is no reason to believe that our account of truthmakers will require universals, and no ground for the suggestion that a truthmaker ontology will ‘do justice to predicates’.

Indeed, Armstrong’s position faces a more serious objection, this time from the nominalist. Truthmaker ontology is supposed to be preferable because it does justice to the predicate; but what justifies the assumption that a ‘fair’ assessment of the ontic significance of the predicate will associate it with a universal? The nominalist claims that the ‘fair’ assessment of the predicate is one which treats it as non-referential, associated with *no* entity or at

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<sup>143</sup>Armstrong 1980, p.105

<sup>144</sup>Armstrong 2004, pp.23-4

best associated with the particulars it is true of, as in medieval nominalist theories of *suppositio*. The nominalist who does not already accept a ‘truthmaker ontology’ will complain that he lacks a reason to believe that the realist’s ascription of ontic significance to the predicate is warranted. Indeed, there may be no non-circular way for Armstrong to establish that the predicate should have ontic significance. The distinctive claim of the truthmaker ontologist is that ontological questions are to be decided by investigating truthmakers; since the correctness of this approach to ontology is urged on the basis of its ‘correct’ assessment of the ontic significance of predicates, the ontic significance of predicates cannot at the same time be urged on the basis that predicates play some role in truthmaking (e.g. as constituents of states of affairs). If truthmaker ontology is to be justified because it gives us ‘fair’ ontic decisions, the claim that these decisions are fair must not be justified solely by appeal to truthmaking considerations, on pain of circularity. But any other means of establishing the ontic significance of predicates will be inconsistent with the truthmaker ontologist’s claim that *all* ontological decisions should be taken on the basis of truthmaking considerations. Thus it seems that the claim that a truthmaker ontology is preferable because it is ‘fair’ to predicates cannot be sustained.

We may conclude that the first proposed reason to accept (STM), that a ‘truthmaker ontology’ is a better account of our ontic commitments, and in particular of the ontic significance of predicates, is unsatisfactory in two ways. First, it is not clear that the introduction of entities to serve as truthmakers will have the consequence that the ontological significance of predicates is recognized, for universals might not be needed even as constituents of the truthmaking entities. Second, it is not clear that the truthmaker theorist can give a reason why we *should* ascribe ontic significance to predicates that does not beg the question against the nominalist who believes that predicates have no ontic significance. For those reasons it seems that the first proposed justification for (STM) may be counted a failure.

## **2. Truth is Grounded**

An second argument advanced in favour of the Strong Truthmaker Principle (STM) is that it follows from an intuition we have already, namely that truth is *grounded* in reality. This is what lies behind another of Armstrong’s attempts to justify the principle:

‘My hope is that philosophers of realist inclinations will immediately be attracted to the idea that a truth, any truth, should depend for its truth on



something ‘outside’ it, in virtue of which it is true.’<sup>145</sup>

A similar thought is offered by Rodriguez-Pereyra:

‘The root of the idea of truthmakers is the very plausible and compelling idea that the truth of a proposition is a function of, or is determined by, reality.’<sup>146</sup>

I shall argue that there is no prospect of deriving (STM) from innocent-sounding intuitions about reality without begging the question against the opponent of truthmakers by starting from some formulation that he rejects precisely *because* of its overt commitment to necessitating truthmakers. In making such a claim I am in agreement with a growing consensus (Dodd 2000, 2002; Hornsby 2005; Liggins 2008). The problem is that (STM), with its insistence that truth should be grounded in an *entity* that is responsible for the truth of a proposition merely by existing, introduces extra commitments which cannot be derived from the initial statement of the intuition; in other words, (STM) is too strong a formulation to encapsulate an intuition about realism that all should be expected to share.

First, what is the general shape of the truthmaker theorist’s argument? It starts from some compelling statement about the way in which truths are answerable to the world. Then it is claimed that this statement, properly understood, entails (STM). Before considering which statements are candidates for the initial intuition, I shall suggest some conditions on a successful demonstration from the truthmaker theorist. One condition is suggested by Hornsby: an elucidation only supports (STM) if it ‘exhibits ontology’ (2005: 36). What this means is that our initial sentence must plausibly be interpreted as connecting truth with the existence of some *entity* in the world. (STM) is a principle about the existence of truthmaking *entities*; unless our original intuition brings in some entity in the world, it cannot justify such a principle.

Two other conditions also seem reasonable. Since (STM) is vindicated only if we have reason to introduce *necessitating* truthmakers — entities whose mere existence entails the truth of the given proposition — we can add the condition that the form of ‘groundedness intuition’ to which we appeal must support a picture of the truthmaking relation as necessitation. This suggests that certain kinds of correspondence intuition will not provide a satisfactory motivation for (STM), since it will not be enough simply to say that true propositions ‘correspond to facts’: the mere presence of a correspondence between a truth and a fact-entity does nothing to motivate the view that this correspondence is guaranteed to hold simply by the existence of the fact-entity in question, as (STM) requires.

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<sup>145</sup>Armstrong 2004a, p.7

<sup>146</sup>Rodriguez-Pereyra 2005, p.20

A third condition is this: the statements which are advanced as elucidating the ‘groundedness intuition’ must be plausible as statements of ‘commonsense realism’ (Liggins 2008: 183). They should express sentiments that are common to anyone who believes that truth depends on mind-independent reality. Otherwise, the truthmaker theorist can no longer claim to be deriving his principle from common sense. We could describe a statement which fails to meet this condition as ‘theory-laden’: rather than expressing a view which is common to all realists, it imports prior theoretical considerations that not all commonsense realists share. Again, the view that a true proposition ‘corresponds to a fact’ is ruled out, since it is ‘theory-laden’ in the sense that it expresses a correspondence theory of truth that not all commonsense realists share: one could be a commonsense realist, believing that truth depends on reality, without believing that this dependence consists in a relation to some entity, a ‘fact’.

If these three constraints on a groundedness intuition are legitimate — that it should ‘exhibit ontology’, suggest a picture of truthmaking as necessitation, and not be ‘theory-laden’ — then it may seem that it is already an impossible task to motivate (STM) in this way. In particular, the first and third conditions are in tension, for it may be claimed that any proposed statement of the intuition which exhibits ontology will, for precisely that reason, be ruled out as theory-laden. The situation is not entirely hopeless for the truthmaker theorist, for it may be that he can find *uncontroversial* statements of commonsense realism which exhibit ontology; alternatively he may be able to establish that there is latent ontological commitment to necessitating truthmakers to be found in some apparently uncommitted statement of realism. Nevertheless, I shall argue that the statements of groundedness intuition offered by truthmaker theorists do not succeed in motivating (STM); the suggested diagnosis is that (STM) cannot be derived from some intuition about truth that all commonsense realists share.

Our starting point for a statement of the groundedness intuition must surely be:

(G1) Truth is grounded in reality.

It seems that this fails both the first and second conditions we imposed on a successful motivation of the Strong Truthmaker Principle. It fails the first condition, that of ‘exhibiting ontology’, because it is not clear that talk of ‘reality’ is enough to commit us to some truthmaking *entity* for each truth; instead we may choose to understand ‘reality’ in the same way that Lewis construes talk of ‘being’: as covering not only what there is but also *how* it is (Lewis 1992: 218). Understood in this way, the fact that truth is grounded in reality does not license the inference that, for each truth, there is some entity in which the truth

is grounded: truths may be grounded in both what there is and how it is.

Moreover, it seems that (G<sub>1</sub>) fails the second condition, that of suggesting that the truth-making relation is necessitation, for even if 'reality' is restricted to cover only *what* there is, it is not clear that a truth's being grounded in reality requires that the entity in which it is 'grounded' should be a necessitating truthmaker for that truth. (G<sub>1</sub>) might be interpreted more in line with the commonsense realist's intentions as saying merely that truth is *about* reality: that truth has as its subject-matter parts of the world. But in this sense a truth succeeds in being 'grounded' in reality just so long as it is a truth about some actually existing thing. It may be said that '*Fa*' is grounded in reality simply because it is about an actually existing thing — the object *a*.

The challenge for the truthmaker theorist is to provide some elucidation of (G<sub>1</sub>) which closes off these alternative readings, leaving us with an account of the groundedness intuition which commits us to an entity, and an entity which is plausibly a necessitating truthmaker. Suppose we try to arrive at an elucidation that looks like it can satisfy the first condition, that of 'exhibiting ontology'. One such might be Armstrong's original formulation of the groundedness intuition:

(G<sub>2</sub>) Every truth depends for its truth on something outside it.

(G<sub>2</sub>) is ontologically significant, insofar as the talk of 'something' apparently commits us to an entity on which the truth of a proposition depends. But even if the first condition is met, the second is not, for we can claim that the entity on which the truth of 'The rose is red' depends is not a necessitating truthmaker, but is rather the rose itself (Liggins 2008: 186). Indeed, this can come to seem like a truism: of *course* the truth of this claim depends on the rose, for when the rose is red it is true, and when the rose is not red, it is false. What the truthmaker theorist needs to do, to block this response, is to make it clear that the 'something' on which truth depends is such that its mere existence guarantees the truth of the given proposition:

(G<sub>3</sub>) Every truth depends for its truth on the *existence* of something outside it.

But once the intuition is spelled out as (G<sub>3</sub>) it fails to meet the third condition for success, since (G<sub>3</sub>) is theory-laden: not all (not even most) commonsense realists would agree that (G<sub>3</sub>) is true, so (G<sub>3</sub>) cannot be used to motivate the Strong Truthmaker Principle (Liggins 2008: 186).

The most carefully-argued recent attempt to derive the Strong Truthmaker Principle from a groundedness intuition was made by Rodriguez-Pereyra (2005). He recognizes that it is insufficient to say that

(G4) Truth is determined by reality

because this fails to rule out the Lewisian interpretation that truth is determined by *how things are* as well as what things exist, and so fails to connect the groundedness of truth with entities which are necessitating truthmakers (Rodriguez-Pereyra 2005: 23). However, he has a counter-argument:

‘What makes true that the rose is red is not what makes true that the rose is light. What makes true that the rose is red is that it is *red*, while what makes true that the rose is light is that it is *light*. The idea that truth is determined by reality is the idea that different truths are determined by different portions of reality, or by different features of reality, and so different truths about the same subject matter are determined by different features of the subject matter in question... But to distinguish ways presupposes that we can identify them, count over them, and quantify over them. That is, ways, which are truthmakers, are entities.’<sup>147</sup>

The idea is that, even if it is conceded that truth is determined in part by how things are, rather than what things there are, it is necessary to distinguish between different aspects of ‘how things are’ to account for how different truths are determined by different portions of reality.

This response has been criticized by Hornsby on the grounds that it forces us only to recognize properties — ways things are — rather than some other kind of entity (tropes or states of affairs) that, unlike properties, are suited to play the role of necessitating truthmakers (Hornsby 2005: 40). But if we grant everything that Rodriguez-Pereyra claims, this objection cannot go through, since he speaks of different truths being *determined* by different features of reality, and there does not seem to be a sense in which a property could be said to *determine* a predicative truth. How could the colour *red* determine that it is true that the rose is red? As soon as we allow the claim that different features *determine* different truths, it becomes clear that the determining entities cannot be properties, but must instead be entities suited to play the role of necessitating truthmakers.

Instead we should ask whether to accept Rodriguez-Pereyra’s claim that

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<sup>147</sup>Rodriguez-Pereyra 2005, p. 23

‘the idea that truth is determined by reality is the idea that different truths are determined by different portions of reality.’<sup>148</sup>

He supports this claim with examples: what makes true that the rose is red is that the rose is red; what makes true that the rose is light is that it is light; therefore what makes true that the rose is red is not what makes true that it is light. So the argument begins with

(G4) Truth is determined by reality

and proceeds to a variety of statements of the form

(G5) What makes true that  $p$  is that  $p$

from which we are supposed to be able to derive the desired conclusion that

(G6) Different truths are determined by different portions of reality

It seems that the most questionable move here is not the attempt to derive (STM) from (G6), which Hornsby criticizes; instead it is the move to derive (G6) from various instances of the schema (G5). One problem is that on a natural reading (G5) collapses the truthmaking relation into identity, since ‘that  $p$ ’ is said to make true ‘that  $p$ ’. If we take ‘that  $p$ ’ to indicate a proposition, on the grounds that whatever is picked out by ‘that  $p$ ’ may be said to be true or false, then truthmaking is a relation that each true proposition holds to itself. Propositions are truth-*bearers*; we need to be convinced that there are entities that are truth-*makers*.

If (G5) is read in this way, it does not support (G6)’s picture of groundedness as a relation between truths and portions of reality. Our question, then, is whether there is a way to reformulate (G5) in such that it is both a plausible elucidation of the grounding intuition, and at the same time provides genuine support for (G6). I shall suggest that there is no satisfactory way of doing so. A first move might be to reformulate (G5) as this:

(G5\*) What makes true that  $p$  is the fact that  $p$

But (G5\*), in its overt commitment to a correspondence between truth and *facts*, is ruled out by our third condition, since it is theory-laden: it expresses a theory of truth that not all ‘commonsense realists’ share, therefore it cannot be proposed as an elucidation of

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<sup>148</sup>Rodriguez-Pereyra 2005, p.23

the groundedness intuition that is supposedly common currency among commonsense realists.

Can we improve the situation by removing the overt reference to ‘facts’? Then we could restate (G5) as

(G5\*\*) What makes it true that  $p$  is *its being the case that*  $p$

The noun-phrase ‘its being the case that  $p$ ’ certainly seems to denote an entity; so from two different instances of (G5\*\*) it may indeed be legitimate to infer (G6). But such an inference may be faulted for failing to take account of the difference between *apparent* and *genuine* ontological commitment. It may be that the opponent of (STM) can propose a paraphrase of (G5\*\*) that reveals that the apparent commitment to entities is *merely* apparent and not genuine.<sup>149</sup> One such candidate paraphrase for (G5\*\*) would be

(G5\*\*\*) It is true that  $p$  *because* it is the case that  $p$

If it is an option to paraphrase (G5\*\*) with the apparently uncommitted (G5\*\*\*), how can we be sure that the apparent commitment to truth-making entities in (G5\*\*) is genuine?<sup>150</sup> The availability of such a paraphrase raises the possibility that the commitment to entities in (G5\*\*) is merely apparent and not genuine; in that case, the opponent of (STM) can deny that the instances of (G5\*\*) genuinely license the inference to the principle from which Rodriguez-Pereyra derives the need for truthmakers, namely

(G6) Different truths are determined by different portions of reality.

In the absence of some means of ruling out such an objection, it cannot be assumed that the current attempt to motivate (STM) is a success.

I have argued that none of these attempts to motivate the Strong Truthmaker Principle (STM) from intuitions about groundedness succeeds. It may be that these failures are symptomatic of a general problem for the strategy. (STM) presents truth as determined by necessitating truthmakers: entities whose mere existence entails the truth of a proposition. Perhaps the reason that it has not proved possible to derive such a principle from statements of commonsense realism is that common sense embodies no commitment to entities of

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<sup>149</sup>In the terminology developed in Chapter 2, this would be ‘paraphrase as analysis’, since the paraphrase here is proposed to give a better account of the actual ontological commitments of the target sentence rather than as a replacement for it.

<sup>150</sup>A similar point is made against Rodriguez-Pereyra by Hornsby 2005, p.35.

this kind. It seems that the most we can derive from commonsense realism is that truth depends on *how* objects and properties are combined, but not on *whether* there is some entity guaranteeing that this combination is the case. For that reason I suggest that the second proposed justification for (STM) is a failure.

### 3. Catching Cheaters

It is sometimes claimed that an alternative reason to believe in truthmaker theory is its utility as a means of ‘catching cheaters’ (Sider 2001: 40), where ‘cheaters’ are taken to be proponents of philosophical positions which we know to be objectionable but which we otherwise could not fault by argument. If we adopt the Strong Truthmaker Principle, we can say why such theories are objectionable — they propose a theory whose ontology does not include adequate necessitating truthmakers for the statements of the theory. Examples of ‘cheaters’ given by Armstrong are: phenomenologists, who hold that truths apparently about objects are in fact truths about actual and possible sense-experience, and ‘Rylean behaviourists’, who hold that ascriptions of mental properties are really statements about dispositions to action which need not be realized at any point in a person’s life (2004a: 1-3); also ‘Rylean dispositionalists’, who say that claims about dispositional properties such as fragility are not ascriptions of genuine properties, but claims about what *would* happen if the object were in certain circumstances, and ‘operationalists’, who take the view that ascriptions of physical quantities are really counterfactual claims about what would happen if certain measuring instruments were used (Armstrong 1989b: 8-11).

I shall suggest two substantial problems for this way of motivating the Strong Truthmaker Principle. First, (STM) condemns many theories as ‘cheating’ which are not *obviously* unacceptable, so it should not be claimed that this principle in some way codifies a prior intuition about which theories are and are not acceptable. I shall argue that common sense about ‘cheaters’ does not justify the adoption of (STM), because that version of the principle is so much at odds with common sense’s view of what a cheating philosophical position is. Indeed, we might doubt whether *any* of the theories indicated deserves to be called ‘cheating’, unless the classification is driven by a prior theoretical concern that there should be no primitive modal truths. My second objection is that, even if these theories genuinely *are* cheating, all the cheater-catching work worth doing can be achieved with a much weaker truthmaker principle than (STM), so the need to catch cheaters does not justify (STM) itself.

The application of the term ‘cheater’ to theories that fail to recognize adequate necessitating

truthmakers is more plausible for some theories than others. It may seem that there is a real problem for a phenomenalist if all there is turns out to be actual and possible sense-experience, with no material objects to cause this experience. We might want to know *why* there are *these* possibilities for alternative sense-experience, rather than some other set of possibilities. But not every one of the ‘cheaters’ can be said to exhibit this kind of problem. For example, it is not *obvious*, prior to any theoretical commitment to (STM), that presentism is a cheating theory, even though presentists, who hold that past and future objects and events do not exist, lack adequate necessitating truthmakers for truths about the past and future (Bigelow 1996). It certainly is not the case that there is something *immediately* suspect about the view that ‘Socrates used to exist’ can be true although it is true in virtue of no existing thing: in this case, it is true in virtue of something (Socrates) which used to exist, but exists no more.

However, even if presentism *may* be counted a ‘cheating’ theory, it will be noticed that two other theories are caught by (STM) which it is implausible to describe as *obviously* cheating. They are, on the one hand, a traditional realism which says that reality contains particulars and universals but no composite states of affairs, and on the other, a traditional nominalism which says that reality contains only particular objects. Neither theory admits entities which could serve as necessitating truthmakers for contingent truths of the form  $Fa$  or  $aRb$ , for neither the existence of  $a$ , nor the existence of  $a$  and universal  $F$ , necessitates that  $Fa$  is the case. Traditional nominalism and traditional realism are both ‘cheaters’ according to the current proposal; if the extension of ‘cheater’ as determined by (STM) contains philosophical theories that we would not normally count as unacceptable, it seems that (STM) cannot demand our allegiance on the grounds that it codifies an intuition about unacceptable theories that we have already.

The fact that (STM) rules out such apparently acceptable theories suggests that we should look for an alternative criterion for being a ‘cheater’. Armstrong singles out for special criticism phenomenologists, Rylean behaviourists, Rylean dispositionalists, and operationalists. All four theories have one thing in common: they propose ‘brute’ or ‘primitive’ modal truths which are irreducible to non-modal truths about actual objects. The phenomenologist, told that the chair is behind the closed door, interprets this as a claim about what he would have seen under different circumstances; the Rylean behaviourists and dispositionalists call the untested warrior brave or describe the undropped glass as fragile because of what *would* have happened if they had been put in testing situations; while the operationalist can talk about the length of the unmeasured object because of what we would have observed had we measured it. In each case, the counterfactual is taken as a primitive



truth that permits of no further reduction in terms of non-modal truths about the actual properties of actual objects. It does not seem unreasonable to conclude that what is really objectionable here is the presence of primitive modality, rather than an ontology lacking in truthmakers. Perhaps the cheater-catching principle we really need is not (STM), but rather a prohibition on theories which admit primitive or ‘brute’ modal truths.

It may be that the truthmaker theorist has a response. He can accept that all of his examples of ‘cheaters’ are objectionable in point of their appeal to primitive modality; however, he might claim that (STM) is needed as part of the *explanation* of why primitive modality is unacceptable. If that is the case, then we must adopt (STM) to explain the unacceptability of cheating theories, and the demand for necessitating truthmakers is vindicated. Both Armstrong (1989c) and Lewis (1986) reject primitive modality: the former by appealing to recombinations of *actual* particulars and universals, the latter by espousing modal realism. Obviously, both regard it as a theoretical gain to be able to give an account of modal truths in terms of non-modal truths. Indeed, Armstrong claims that

‘Every systematic philosophy must give some account of the nature of possibility.’<sup>151</sup>

Armstrong’s own reason for thinking this is that the philosopher is under an obligation to specify necessitating truthmakers for every kind of truth, and modal truths are no exception. If we can find no better reason for rejecting primitive modality than its lack of truthmakers, it may seem that (STM) is justified after all.

However, the lack of necessitating truthmakers is not the only explanation we might give for dissatisfaction with primitive modality. One alternative account is that modal logic cannot be represented adequately if we are constrained to represent possibility and necessity with box and diamond operators, something which seems an unavoidable consequence of the view that possibility and necessity permit of no further analysis (Lewis 1986: 13-14). A second suggestion holds primitive modality to be unacceptable because it conflicts with a truthmaker principle, but this time not (STM), but rather the weaker version offered by Lewis:

‘Truth supervenes on being... I want to construe “being” broadly — it covers not only *whether* things are, but also *how* they are’<sup>152</sup>

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<sup>151</sup> Armstrong 1989c, p.3

<sup>152</sup> Lewis 1992, p.218

If we understand ‘how things are’ as restricted to ‘how they *actually* are’, then even Lewis’ weakened principle is sufficient to rule out primitive modality, for the theorist of primitive modality *denies* that modal truths about how things could have been are determined by non-modal truths about how things are. Since Lewis’ truthmaker principle is sufficient to explain the unacceptability of primitive modality, we may conclude that adoption of (STM) is not warranted by the need to explain why primitive modality should be excluded; that purpose is served by Lewis’ principle. Moreover, it can be claimed that Lewis’ principle facilitates a better overall account of ‘cheating’ theories, for it does not fault theories such as traditional nominalism and traditional realism, which see truth as depending on both what there is and how it is (in the case of traditional realism, *how* the particulars and universals are arranged), but which do not supply entities to serve as necessitating truthmakers for such truths.

It is possible to feel some hesitation about accepting the suggestion that all modal truths must be explicable in non-modal terms, and that theories which fail to do this are ‘cheaters’, *theoriae non gratae*. At least some of the time, we seek to explain non-modal facts in terms of modal facts, rather than the other way around. Thus a satisfactory explanation for some fact  $p$  might be that  $p$  could not have failed to be the case; once we understand that things could not have been otherwise, we no longer need to know why things are as they are. Moreover, as I alleged in the previous section, Armstrong’s ontology of states of affairs must appeal to primitive modal truths about the relation between a state of affairs and its constituents. If no theory can get by without primitive modality, why should any theory be faulted for failing to do so? Fortunately for the realist, he need not engage in discussion about the acceptability of primitive modality when debating (STM). As I have argued, even if theories which appeal to primitive modal truths *are* unacceptable, all the cheater-catching work worth doing can be achieved with Lewis’ weaker truthmaker principle. Consequently, the need to catch cheaters does not justify the adoption of (STM) and the demand for necessitating truthmakers.

I have considered three ways to motivate the adoption of the Strong Truthmaker Principle (STM), all of which I have argued to be unsatisfactory. If there is no reason to believe (STM), there is no reason to believe that truths of the form  $aRb$  must have necessitating truthmakers; but the current version of Bradley’s Regress threatens the realist only on the assumption that the realist is obliged to specify necessitating truthmakers for relational truths about the combination of a universal with the particular(s) that instantiate it. Once we accept that such a demand is illegitimate, we can answer Bradley’s challenge along the lines suggested by Russell and Lewis: that what is different in the case where  $aRb$

is not the existence of some additional entity, but rather a difference in *how* things are: when  $aRb$  is the case, what is different is that  $a$ ,  $R$  and  $b$  are combined as they are. This ‘combination’ need not, and should not, be considered to be an extra entity which plays the role of truthmaker for the truth that they are combined.

## 6. Bradley's Regress: II

### The Relatedness Regress

The previous chapter distinguished two kinds of Bradleyan regress, and argued that they are not genuine threats to the realist project. The Sameness regress, which attempts to show that the realist has no satisfactory account of sameness of type, was rejected because we found no good reason to believe that more than *one* relation is needed to account for the sameness of type between all cases in which particular(s) instantiate universals; moreover, even if a regress of different relations were conceded there was no reason to think that it must be vicious. A second regress, which challenged the realist to specify an entity which 'makes the difference' between the cases where a relation does, and does not, relate its terms, was rejected on the grounds that the challenge to the realist is illegitimate: the demand for 'difference-making' entities is nothing less than the demand for necessitating truthmakers for relational truths, but in fact there is no compelling reason for the realist to accept that truths about instantiation have truthmakers of this kind.

The suspicion remains that the two accounts considered so far do not get to the heart of Bradley's regress. Bradley in *Appearance and Reality* does not present himself as raising a problem for the analysis of sameness of type; nor does he give prominence to the need for an entity that plays the role of 'making the difference' or 'grounding' relational truths. In this chapter, I shall suggest that a third account gives a better reconstruction of the philosophical problem underlying Bradley's comments. Apart from this interpretative point, I shall make three claims about this 'Relatedness' regress. First, that the regress, if conceded, would be genuinely vicious; second, that the problem is not solved by the strategy, inspired by Frege, of appealing to the 'incompleteness' of universals to show how it is possible for particulars and universals to combine directly, without the need for an infinite hierarchy of intervening relations. Finally, I claim that the correct solution to the Relatedness regress is to deny that a numerically different instantiation-relation is introduced at each stage of the regress. If there is only *one* instantiation-relation, then there is no infinite regress of distinct instantiation-relations, and consequently no reason to think it impossible for universals and particulars to combine in instantiation.

## The Relatedness Regress as an Interpretation of Bradley

In *Appearance and Reality* there is little to suggest a requirement for some kind of ‘Difference-Making’ entity, as we find in the exchange with Russell in *Mind* of 1909-11; nor is there any suggestion that the realist project is undone by an incomplete analysis of qualitative sameness, as claimed by proponents of the ‘Sameness’ regress. Thus it is not clear exactly what problem Bradley is trying to raise for the realist. Bradley’s own remarks are not enough for us to be certain what he intended; nevertheless, I shall suggest that an account of the kind I present here makes good sense of many of Bradley’s comments. If this is not enough to justify the account as an exegesis of Bradley, we may treat it less as a reconstruction of his actual thought than as an attempt to clarify a genuine problem underlying his use of the regress argument, of which he may have been at most dimly aware.

The general form of the problem that Bradley poses is familiar: once we admit that a relation is an entity in its own right, distinct from its *relata*, it is necessary to introduce further relations that hold between the relation and its *relata*, and since these are also entities in their own right we need further relations in which they stand to *their relata*, and so on *ad infinitum*. We have already considered and rejected the idea that the problem is the absence of satisfactory truthmakers: that each fresh relation is introduced in the desperate hope that it will ‘ground’ or be *responsible for* the union of particular and universal. But what then is the problem here? Bradley states that the aim of his regress is to show that

‘The arrangement of given facts into relations and qualities may be necessary in practice, but is theoretically unintelligible.’<sup>153</sup>

In particular, we are told that

‘how the relation can stand to the qualities is, on the other side, unintelligible... If you take the connection as a solid thing, you have got to show, and you cannot show, how the other solids are joined to it.’<sup>154</sup>

These comments might suggest the view that Bradley’s challenge is one about *explanation*: that the realist has failed to explain something important about instantiation. In particular we might think that the realist has no satisfactory explanation of what instantiation *is*: no account of what it is for a relation to combine instantially with its *relata*. The regress is vicious because the initial demand for an explanation of the phenomenon of instantial

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<sup>153</sup>Bradley 1897, p.21

<sup>154</sup>Bradley 1897, p.27

combination recurs at each new stage of the regress: in supplying each new instantiation-relation we present it *as* instantially combined with the entities that have gone before. Rather than explaining what instantiation *is* by introducing an instantiation-relation, we presuppose an understanding of what it is for a relation to be instantially combined with its *relata*.

This view is defended by David Lewis, who claims that

‘Bradley’s regress shows that if we try to explain having *simpliciter* in terms of relational having, the explanation we seek will never be finished.’<sup>155</sup>

By ‘having’ Lewis means the ‘having’ that is the case when a particular instantiates a universal — that is to say, ‘instancial combination’ or ‘instantiation’. According to Lewis, Bradley’s objection to realism is that the realist cannot explain instantiation as a relation; nor can the realist explain instantiation in any other way; therefore instantiation is ‘theoretically unintelligible’.

I shall argue that this interpretation of Bradley should not be adopted, for it has considerable demerits, both philosophical and exegetical. The exegetical problem is that it leaves us with no understanding of how Bradley moves from the complaint that instancial combination cannot be explained in other, more basic terms, to the conclusion that relations and their *relata* cannot combine at all. Bradley’s view is that a pluralist ontology of relations and their *relata* is not merely poorly explained by its proponent; also, it is ‘infected and contradicts itself’ (1897:25), and is full of ‘inconsistencies’ (1897: 34). Bradley seems to claim that, properly understood, an ontology which treats relations as distinct entities from their *relata* is one in which relations *cannot* combine with their *relata*, one in which the relation and its terms ‘hopelessly fall asunder’ (1897: 31). A satisfactory exegesis of Bradley’s argument should reveal why we are supposed to accept that the regress demonstrates the *impossibility* of instancial combination, rather than merely the absence of an explanation in more basic terms.

This interpretation of the regress also faces a philosophical problem. It is commonplace to separate the ‘ideology’ of a theory from its ‘ontology’, in the terminology popularized by Quine (1951). We are also familiar with the idea that the ideology of any theory must incorporate some ‘primitive predicates’: predicates whose content is not to be analyzed in terms of other, more basic notions. No theory is to be faulted for the failure to do away with all unanalysed predicates, for this is an unattainable goal (Lewis 1983: 200). On the current interpretation, Bradley’s argument can be deflected simply by agreeing that instantiation

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<sup>155</sup>Lewis 2002, p.6

is to be treated as a theoretical primitive in the ideology of our theory; once we realize that every theory must incorporate some primitive predicates, what better primitive predicate than ‘...instantiates...’, for instantiation is the most fundamental combination of all. If Bradley’s complaint can be deflected simply by introducing a primitive instantiation-predicate, then the realist has no reason to take the regress argument seriously. Better to look for an interpretation of the problem which does not make it trivially easy for the realist to solve.

The way is open, then, for an account of Bradley’s thought which does not read him as posing a problem about explanation. Here it is worthwhile quoting his argument for the ‘unintelligibility’ of instantiation at length:

‘But how the relation can stand to the qualities is, on the other side, unintelligible. If it is nothing to the qualities, then they are not related at all... But if it is to be something to them, then clearly we now shall require a *new* connecting relation. For the relation hardly can be the mere adjective of one or both its terms; or, at least, as such it seems indefensible. And, being something itself, if it does not itself bear a relation to the terms, in what intelligible way will it succeed in being anything to them? But here again we are hurried off in the eddy of a hopeless process, since we are forced to go on finding new relations without end. The links are united by a link, and this bond of union is a link which also has two ends; and these require each a fresh link to connect them with the old. The problem is to find how the relation can stand to its qualities; and this problem is insoluble. If you take the connection as a solid thing, you have got to show, and you cannot show, how the other solids are joined to it. And, if you take it as a kind of medium or unsubstantial atmosphere, it is a connection no longer.’<sup>156</sup>

We do not need to read this passage as a problem about truthmakers — a problem about how a relation can ‘ensure’ or be ‘responsible for’ the unity of relation and *relata* — for there is another way to understand Bradley’s claim that relations ‘require each a fresh link to connect them with the old’. Bradley makes it clear that the need for a fresh relation or ‘link’ arises simply from the decision to treat a relation as an entity in its own right, as ‘something itself’ and a ‘solid thing’. Once the relation is a ‘solid thing’, he says, it must be ‘joined to’ the ‘other solids’ — i.e. it must be related to its *relata*. But the realist, who believes in the reality of relations, seems compelled to introduce a relation every time

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<sup>156</sup>Bradley 1897, p.33

there is a genuine case of relatedness, and the relatedness of a relation and its *relata* is no exception. So the relatedness of *a*, *R* and *b* requires that they stand in a relation *I*; since standing in a relation is a kind of relatedness, we must say that *a*, *R*, *b* and *I* are related, and therefore that they stand in a relation *I*<sup>\*</sup>, and so on without end. The sense in which each new relation ‘requires’ a fresh link, then, is this: the realist accepts that distinct things can be related *only if* they stand in a relation. But then *a*, *R* and *b* can be related only if they stand in relation *I*, which is to say that *a*, *R*, *b* and *I* are related; but this can be the case only if they stand in some further relation *I*<sup>\*</sup>. So on without end.

The current proposal has the consequence that the regress follows from three commitments *every* realist could be expected to share: first, that relations are entities in their own right; second, that genuine relatedness requires that the related entities stand in a relation; and third, that ‘standing in’ a relation is a kind of relatedness. For that reason it cannot be deflected simply by denying that relations (or any other entities) are needed to play the role of necessitating truthmakers; as long as we agree that two things can be related *only if* they stand in a relation, we shall be forced to say that the collection of things mentioned at each stage in the regress must stand in a relation, and as long as we treat relations as entities in their own right, we shall be compelled to recognize ever larger collections of related things, which in turn must stand in further relations if they are to be related as the realist claims they are.

It seems that a similar interpretation is proposed by C. D. Broad:

‘It is plain that Bradley thinks of *A* and *B* as being like two objects fastened together with a bit of string, and he thinks of [relation] *R* as being like the string. He then remembers that the objects must be glued or sealed to both ends of the bit of string if the latter is to fasten them together. And then, I suppose, another kind of glue is needed to fasten the first drop of glue to the object *A* on the one side and to the bit of string on the other; and another kind of glue is needed to fasten the second drop of glue to the object *B* on the one side and to the string on the other. And so on without end.’<sup>157</sup>

If the relatedness of a term and its *relata* requires the presence of a further relation in which they stand, and the relatedness of that term and its *relata* requires the presence of yet another relation in which they stand, then we become trapped in an infinite series of relations, each of which is a precondition of the relatedness of the entities already mentioned.

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<sup>157</sup>Broad 1933, p.85



This account of the regress reverses the direction of entailment from that supposed by the truthmaker theorist. A necessitating truthmaker is supposed to be *sufficient* for a truth; if each new relation  $I, I^*, I^{**}...$  is introduced as a truthmaker, then it is because  $I^*$  is supposed to be *sufficient* for the relatedness of  $I$  and its *relata*, and  $I^{**}$  is supposed to be *sufficient* for the relatedness of  $I^*$  and its *relata*, and so on down the hierarchy of instantiation-relations. But here the problem is that, since the realist believes that *standing in a relation* is a precondition of *being related*,  $I^*$  is a *necessary* condition for the relatedness of  $I$  and its *relata*, and  $I^{**}$  is a *necessary* condition for the relatedness of  $I^*$  and its *relata*, and so on down the hierarchy of instantiation-relations.  $aRb$  can be the case only if it is the case that  $a, R$  and  $b$  stand in some further relation  $I$ , and this can be the case only if  $a, R, b$  and  $I$  stand in some further relation  $I^*$ , and a further necessary condition of that being the case is that the five entities mentioned so far stand in yet another relation  $I^{**}$ .

Why does this regress constitute a genuine problem for the realist? A detailed answer to that question will take the whole of the following section; for now we should notice that the *prima facie* difficulty is easy to state. The realist seems committed to the view that standing in a relation is a *precondition* of being related. So a precondition of the relatedness between  $a, R$  and  $b$  is that these things stand in a relation  $I$ . But there is a further precondition which has yet to be met, for  $a, R, b$  and  $I$  can be related only if these four things stand in a further relation  $I^*$ . Again, there remains some precondition for the relatedness of the five things now mentioned which has not been met, namely that they stand in a further relation  $I^{**}$ . At each stage in the regress, some precondition for the relatedness of the entities mentioned has not been met, namely, the presence of a further relation in which these entities stand. This is problematic for the realist, for it is plausible that such an infinite series of preconditions cannot be met: at no stage in the regress will we have succeeded in providing *enough* to enable the relatedness of the original entities  $a R$  and  $b$ , for at each stage more will be needed than has been introduced so far.

Before considering the philosophical problem posed by this version of the regress, we might consider the merits of this proposal as an interpretation of Bradley. Perhaps the strongest points in its favour are, first, that it connects (as Bradley does) the need for a further relation with the decision to treat each relation as a 'solid thing' or entity in its own right, which may in turn *be related* to other entities; second, that it generates a problem for the realist which does not depend on relations playing a role as necessitating truthmakers. If Bradley *had* intended to criticize relations on this score, we should expect him to make it clearer that each fresh relation is introduced because its predecessor has failed in the task of 'ensuring' that relation and *relata* are combined, but this is no part of his presentation. A

final advantage of the current interpretation is that it is consistent with Bradley's insistence that his regress shows the combination of relation and *relata* to be *impossible*. He claims that 'the problem is to find how the relation can stand to its qualities'; the suggestion is that, when we follow the regress through, it reveals that the relation *cannot* be related to its *relata*. As he says in an earlier passage, if we treat relations as 'independently real' then

'the qualities and their relation fall entirely apart'.<sup>158</sup>

The problem for the realist is that, in treating relations as entities in their own right, he makes it impossible for anything to get related at all, for at every stage of the regress some precondition for the relatedness of the entities mentioned will not have been met; for that reason the realist is said to lack an account of how, in Bradley's phrase, the relation can succeed in 'being anything to' its *relata* (1897: 32).

## **The Infinitist Challenge and the Viciousness of the Regress**

A common view among realists is that, although at each stage of the regress the entities mentioned must stand in some further relation, this alone does not constitute a difficulty for realism. Russell takes such a position in *The Principles of Mathematics*: there he claims that Bradley's regress is only vicious when it arises in the analysis of meaning, forcing us to impart infinite complexity to what we say. When the regress describes the metaphysical situation, however, it is not to concern us:

'The endless regress is undeniable, if relational propositions are taken to be ultimate, but it is very doubtful whether it forms any logical difficulty'<sup>159</sup>

A similar approach may be found in more recent treatments of the regress. Wolterstorff claims that the fact that there must be an infinite series of further relations involved for a relation to relate its terms does not justify the conclusion that realism is incoherent:

'I see no incompatibility between the claim that these things are related, and the principle that for every relation, if some entities are to be in that relation, those entities plus that relation must be in a certain relation.'<sup>160</sup>

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<sup>158</sup> Bradley 1897, p.21

<sup>159</sup> Russell 1903, p.99

<sup>160</sup> Wolterstorff 1970, p.102

Likewise, Reinhardt Grossmann says of the regress argument that ‘there is nothing vicious about it. It merely shows that an infinity of relations exists’ (1982: 135).

Following the terminology developed by Klein (1998, 1999, 2003) for the ‘regress of reasons’ problem in epistemology, we might call this position ‘Infinetism’. It is the view that there is nothing vicious about a regress of different instantiation-relations of the kind proposed by Bradley; the regress may be conceded, for it reveals no flaw in the realist proposal. The Infinetist must be prepared to defend himself against the accusation of ontological extravagance, requiring as he does an infinite hierarchy of distinct instantiation-relations involved in each case of particular-universal combination; however, the need for such a large ontology of universals does not make the theory obviously untenable. If Infinetism is a viable response to Bradley’s regress, then it can no longer be claimed that the regress argument demonstrates that realism must be abandoned. In this section I argue that Infinetism, although plausible, is not a satisfactory solution to the regress.

The Infinetist’s refusal to credit that the current version of the regress is vicious is understandable. The proponent of the Relatedness regress claims that each stage of the regress can be the case only if the subsequent stage of the regress is the case. But it might seem strange to feel so confident about deriving from this infinite chain of preconditions the conclusion that there is a problem about the relatedness of the original entities. All that can be established by looking at each individual stage is a conditional conclusion, that  $I_n$  can relate its terms only if  $I_n$  and its terms are related by a further relation  $I_{n+1}$ . But a conditional conclusion of this kind only tells us about which things have to be the case if some other thing is the case; it does not tell us anything about whether the original situation can be the case or not, and at least in the finite case we do not think that the discovery of a chain of dependencies has any bearing on whether the situation in question is problematic.

Comparison with the truth regress makes the question urgent. Here also we seem to have an infinite chain of preconditions, since a proposition  $p$  is true only if it is true that  $p$  is true, which is true only if it is true that it is true that  $p$  is true, and so on. But no-one should argue from (i) the mere fact that each stage in the regress is true only if the succeeding stage is true and (ii) the fact that the regress is infinite to the conclusion (iii) that the first stage in the regress is not the case and no proposition  $p$  is true after all. So the nominalist cannot expect to argue from (i) the mere fact that each stage of the Bradleyan regress is true only if its successor is, and (ii) the fact that the regress is infinite to the conclusion (iii) that the union of universal and particular is impossible.

The question at issue, then, is whether there is any good reason to agree with Bradley that the need to posit an infinite hierarchy of instantiation-relations in each case of relatedness shows that relation and *relata* cannot combine in the first place — that they, in Bradley’s expression, ‘hopelessly fall asunder’ (1897: 31). The apparent situation is that we have an impasse: on one side, the friend of Bradley points out that, at each stage of the regress, some precondition for the relatedness of the entities has not been met, namely that these entities stand in some further relation, as yet unmentioned. But if, at every stage, some precondition has not been met, then at no stage will enough have been done for universal and particular to be able to combine in instantiation. On the other side, the Infinitist claims that, since his hierarchy of universals is *infinite*, there is every reason to suppose that every precondition required *has* been met. If the hierarchy of universals is infinite, then for every relation  $I_n$  there will be a relation  $I_{n+1}$  in which it stands. While Bradley supposes that relation and *relata* never get related, because we never reach a stage at which enough has been done to enable them to get related, the Infinitist responds that, because the hierarchy of universals is infinite, each relation  $I_n$  *does* stand in some further relation  $I_{n+1}$ , and this is all that is needed in order for it to be possible for relational combination to take place.

If we think about the infinite hierarchy of universals along these lines, then Infinitism comes to seem very plausible. Unfortunately for the realist, there is a way of stating Bradley’s challenge which reveals a genuine problem with the relatedness of the entities mentioned at each stage of the regress. For that reason, I suggest, Infinitism should be abandoned. The problem is this: the fact that each relation  $I_n$  turns out to be a *relatum* of some higher-order relation  $I_{n+1}$  shows that none of the relations introduced by the realist functions *as* a relation; introducing more and more relations gives us more entities in need of relating, but never an entity that plays the role of relating them.

Candlish suggests an account of the viciousness of the regress with some affinities to the current suggestion:

‘Suppose I am given the task of making a chain out of some loose metal rings, and when I come to join any two of them, I respond by asserting that we need a third ring to do the job, so that the most I can achieve is the addition of more rings to the collection. It is quite clear that no matter how many rings I add, I shall never get a chain’<sup>161</sup>

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<sup>161</sup>Candlish 2007, p.170

Candlish takes the problem to be that each relation turns out to be merely something else in need of relating (a 'ring') rather than an entity that could genuinely do the job of relating the things introduced already. However, an account of this kind raises more questions than it answers. In particular, the friend of Bradley needs to say *why* it is that each new relation functions only as a potential *relatum* rather than (as the Infinitist claims) serving to relate its terms. It should not be claimed that each relation  $I_n$  fails to relate its terms simply because  $I_n$  is itself a term of a higher-order relation  $I_{n+1}$ : to make such a claim would be to deny that a relation could function both *as* a relation and as a term of a higher-order relation, but the Infinitist will respond that no reason has been given to believe this prohibition on relations functioning both as relations and as *relata*. After all, it is commonplace to suppose that universals can both *be instantiated by* particulars, and also *instantiate* other universals in turn. If the denial that the relations in the hierarchy relate their terms rests on a general principle of that kind, it will be too easy for the Infinitist to reject Bradley's argument.

So the friend of Bradley must answer one crucial question. *Why* does it follow, given that each relation introduced in the regress is at the same time a term of some higher-order relation, that none of the relations actually relates its *relata*? To answer this on behalf of Bradley, it is necessary to reconsider the realist's position *qua* relatedness. The realist believes that *being related* is a matter of standing in a *relation*, where the relation itself is construed as an entity in its own right. So the realist should be willing to say that the correct account of the relatedness of some entities sees them as standing as *relata* for some relation. It seems the realist is committed to a constitutive thesis about what relatedness is, namely that, in every case, the relatedness of two or more entities consists in their standing as terms to some relation. But then it seems legitimate to ask, what does the relatedness consist in when a relation  $R$  combines with its *relata*  $a$  and  $b$ ? What is the underlying metaphysical situation? Since standing in a relation is a kind of relatedness, the realist is compelled by his own analysis to say that the underlying metaphysical situation is not one where  $a$  and  $b$  are the two terms of a relation  $R$ ; instead the real situation is that  $a$ ,  $b$ , and  $R$  are the three terms of a relation  $I$ . So, it seems, the correct account of the situation is not, as we thought, a situation where  $R$  functions *as* a relation, whose terms are  $a$  and  $b$ ; instead, the connection between  $a$ ,  $b$  and  $R$  is that all three entities play the role of terms of some further relation.

But the realist cannot stop there, for his account of relatedness ought to apply no less to the new statement of the situation: in describing a situation in which  $a$ ,  $b$  and  $R$  stand in some relation  $I$ , he describes a case of relatedness, for  $a$ ,  $b$  and  $R$  are related to  $I$ . But then

the realist's account of relatedness forces him to say that we do not yet have the correct account of the underlying metaphysical situation, for we should see *I* playing the role, not of a relation whose terms are *a*, *b* and *R*, but rather as itself one of the four terms of a further relation *I*\*. So again, the realist should say that the correct account of the role played by the relation *I* is not that it occurs *as* a relation; rather, it occurs as a term in need of relating. But then, given *any* relation in the infinite hierarchy, we can establish by similar reasoning that the *correct* account of the role it plays is that it serves as *relatum* of some higher-order relation.

This way of thinking about the regress enables us to answer the Infinitist's challenge on behalf of Bradley. Two considerations combine to mandate the conclusion that relations cannot relate their terms even if we are granted an infinite hierarchy of them. One is the fact, mentioned already, that the realist makes a *constitutive* claim about relatedness: that the relatedness of some entities consists in those entities standing as terms to some relation. This has the consequence that, for the realist, each fresh stage in the regress is not *merely* a precondition of the preceding stage; in fact each fresh stage of the regress ought, by the realist's lights, to be a *better* statement of the underlying situation than the preceding stage, for each fresh stage is the result of reapplying the realist's account of relatedness to the situation we had already. To deny that each new stage is a better account of the underlying situation would simply be to deny the correctness of the realist's thesis that relatedness is a matter of standing in a relation.

The second consideration is the need to distinguish between two roles we expect relations to play: as terms of other relations (and properties), and as relations which relate their terms. By recursively applying the realist's account of relatedness, we can show, of any relation in the hierarchy, that the proper account of its function sees it playing a role, not as a relation, but rather as a *relatum* of some higher-order relation. This is because, in every case in which a relation is alleged to combine with its terms, we are forced to conclude that its combination with its terms is effected, not by the relation having its terms *as* terms, but rather by the relation and its terms serving as terms of some further relation. To borrow a phrase from Gaskin's account of the semantic problem of the Unity of the Proposition, the role of actually relating the *relata* is 'endlessly deferred' (Gaskin 1995: 176): each of the relations in the hierarchy functions as a term in need of relating, rather than playing the role of *relating* some terms. But it seems reasonable to suppose that any case of genuine relatedness must be a case in which some relation occurs functioning *as* a relation. If *no* relation in the hierarchy functions as a relation, but instead serves only as a candidate *relatum* for a higher-order relation, then we shall be forced to conclude that

nothing gets related. Each relation turns out not to play any role in relating the entities already mentioned; instead the role it plays is that of ‘just another *relatum*’.

This account of the viciousness of the regress requires that we make some conciliatory remarks to those who would explain the problem in terms of truthmakers or by appealing to an obligation to provide an explanation of instantiation. If the current account is correct, then the truthmaker theorist was right to claim that the regress is problematic because it shows that every relation in the regress fails to fulfil some special role of ‘relating’ its *relata*; however, it is not necessary to say that this role is that of necessitating truthmaker for the truth that the *relata* are related. Instead, the problem is that each relation serves as ‘just another *relatum*’, and if that relation’s only connection to its terms is that relation and terms stand as the *relata* of some further relation, it seems that the relation does not function *as* a relation after all — instead of relating its terms, it is just another term in need of relating.

To the proponent of an ‘explanation’ account of the regress, we should say that he was right to assert that the regress arises as a consequence of an account, or explanation, offered by the realist. This is the realist’s ‘account’ of relatedness — the theory that *every* case of relatedness is constituted by the related things standing in a further entity, which is a relation. However, the viciousness of the regress does not consist in a demonstration that the realist has not *provided* an account of relatedness, in the way in which Armstrong’s ‘relation’ regress attempts to show that various theories fail to give an account of sameness of type. Instead, the regress is vicious because it seems to show that, if the realist’s account of relatedness is correct, then nothing is related to anything else. The reason Bradley calls the combination of a relation and its terms ‘unintelligible’ (1897: 21) is that, when we apply the realist’s own account of relatedness in this case, it becomes apparent that the combination of relation and *relata* is impossible; but if we reject the thesis that the relatedness in this specific case involves a ‘solid thing’ (1897: 33), a relation, standing between the related entities, then Bradley will object that the connection between these entities ‘is a connection no longer’ (1897: 33). The sense in which we may be said to lack an explanation of instantiation is that instantiation turns out to be impossible if subsumed under the realist’s general account of relatedness; but if this account is rejected, there is no obvious alternative which the realist might put in its place.

I have argued that there is a way of understanding the viciousness of Bradley’s regress which does not demand that each relation serve as a truthmaker, and does not accuse the realist of failing to account for some *explanandum* of which he owes us an explanation. The charge against the realist is that, if we recursively apply his account of what ‘relatedness’ is,

we are forced to introduce an infinite hierarchy of relations, and to conclude that *each* of these relations functions, not as a relation, but rather as another potential *relatum* of some higher-order relation. If the regress is infinite, then *all* of the relations in it will function as *relata* rather than relation; in which case none of the relations relates its terms. As Bradley says, all of the entities in the regress ‘fall entirely apart’ (1897: 21), and nothing is related to anything else. If this reconstruction is correct, then Informatism is no longer a genuine option for the realist.



## **‘Relatedness without Relations’ and Incompleteness**

It is clear that the current regress arises only if we agree that the relatedness of universal and particular(s) should be subsumed under the realist’s general account of relatedness, and explained as a case in which the entities in question stand as terms to some further relation of *instantiation*. If  $a$  and  $F$ , or  $a$ ,  $R$  and  $b$ , can combine directly in instantiation without the need for a further entity — an instantiation-*relation* — then the regress cannot get started, for there is no further entity that must in turn be combined in instantiation with its terms. The situation would simply be that universal and particular combine directly in the first place. The denial that instantiation is an entity in its own right has a wide constituency. In this section I argue that such a denial raises a problem for the realist that he is unable to answer satisfactorily; therefore the denial of an instantiation-relation is not an adequate solution to the Relatedness regress.

The problem for the realist is this: instancial combination between particular and universal is a case of relatedness. So the realist must accept that the instantiation of universal by particular(s) is a case of ‘relatedness without relations’, in Fisk’s phrase (1972: 139). But the realist claims that, everywhere else, relatedness requires an intervening entity, a relation. He needs to explain what is special about the case in which particular and universal combine which relieves him of the obligation to posit a relation in this case. Indeed, if the realist concedes that two things can be related without standing in a relation in the specific case of the combination of particular and universal, why then should we believe his claim that, in general, relatedness between things requires that they stand in a relation? Why not claim that, always and everywhere, things may *be related* without standing in further entity, a relation? The objection against the realist, then, is that if he admits that it *is* possible for two things to be related without an intervening relation, it seems there is no longer any reason to hold that a relation is needed in other cases of relatedness, and so it may come to seem that there is no reason to believe that relations are needed at all. Worse, if it is conceded that  $a$  can stand to  $b$  in some way without there being an entity — a relation — in which they stand, what is there to prevent a nominalist from drawing the further conclusion that  $a$  can be a certain way on its own without there being some entity — a property — which  $a$  instantiates? One thing’s being a certain way is simply the monadic case of two thing’s standing to each other in a certain way; if there can be relatedness without relations, perhaps there is no need for universals of *any* kind.

To answer this objection, the realist needs to say more about these special cases in which two things are related without an intervening relation. In particular, he must answer

this question: given that, in general, relatedness requires an intervening relation, *how is it possible* for there to be relatedness with no relation in the special case of particulars combining with universals? In this section I consider and reject two realist proposals that are best understood as attempts to answer this ‘how-possible’ question. One is the suggestion that universals and particulars are fundamentally different kinds of entity; the other that universals are to be understood as in some sense ‘incomplete’. Since each fails, I conclude that the realist should not deny that there is a genuine relation of instantiation; to do so raises the suspicion that relations are not required in any case of relatedness, contrary to realist hypothesis.

One suggestion which might dispel our worries is that particular-universal combination requires no intervening relation because particulars and universals are fundamentally different kinds of entity. When *particulars* are related, the suggestion goes, they need a relation between them, but it would be a mistake to think that universals (which are a completely different kind of thing) likewise require intervening relations to join them to anything. This, it is hoped, will explain why an instantiation-relation is not required in the case of instancial combination, although relations *are* required in other cases of relatedness.

This suggestion is found already in Broad, who complains that the regress argument

‘depends on insisting that they [relations] shall behave as if they were particulars like the terms which they relate.’<sup>162</sup>

Such a response is also endorsed by the later Russell, who claims that the ‘essential error’ in Bradley’s argument is that

‘Bradley conceives a relation as something just as substantial as its terms, and not radically different in kind.’<sup>163</sup>

This passage has been subject to misinterpretation. Candlish claims that Russell finally came to agree with Bradley in treating relations as unreal, alleging that Russell by 1927 had

‘surreptitiously switched sides to become the new champion of the insubstantiality and unreality of relations.’<sup>164</sup>

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<sup>162</sup>Broad 1933, p.85

<sup>163</sup>Russell 1927, p.263

<sup>164</sup>Candlish 2007, p.168. Wolterstorff 1970, p.96 reaches the same conclusion about Russell’s change of heart.

This interpretation is not persuasive. It is clear from the following discussion that Russell still holds that universals *exist*, for he takes care to distinguish an ‘actual relation’ from the ‘word for a relation’ (1927: 264). It is true that he denies that a relation is ‘just as substantial as its terms’ (1927: 263), but this cannot be understood as the denial that relations are features of mind-independent reality; instead Russell is using the ‘substantial’ as the adjectival form of the general term ‘substance’, where substances are opposed to attributes as one of two kinds of thing that exist. To say that universals are not ‘substantial’ is merely to say that they are unlike substances — not to say that they are unreal.

Can Broad and Russell’s proposal meet the current challenge? I shall argue that merely appealing to a difference in kind between particulars and universals does not explain how is it possible for them to combine without an intervening relation, given that relatedness (in general) requires relations. Let it be granted that the two kinds of entity are radically different, such that particulars require, and universals do not require, intervening relations when they are related. This will explain why there must be an intervening relation when two particulars *a* and *b* are related; it can also explain why no relation is needed when two universals, *F* and *G*, are related; however, it does not give any clue as to what we should think about the transcategorial relationship between universals and particulars which we get in instantiation. Suppose particulars *a* and *b* stand in relation *R*. What the realist is trying to deny is that a further relation *I* is required in order for the particulars to be related to the relation *R*. It might be claimed that one constituent of the situation — *R* — is a universal, and so requires no intervening relation to join it to *a* and *b*. After all, universals are fundamentally different in kind from particulars. But it might equally well be pointed out that *a* and *b* are particulars and therefore *do* require an intervening relation to join them to the relation *R*. If it is conceded by the realist that a relation is needed when particulars are related, how can it be denied that the situation *aRb* is one in which particulars *a* and *b* are related, albeit to a universal *R*, and so it is a situation in which an extra relation, *instantiation*, is required to relate the particulars to what they are related to? We may conclude that, even if universals and particulars are radically different kinds of thing, this gives us no suggestion why the transcategorial relationship of instantiation does not itself require a relation.

An alternative proposal may be seen as an attempt to say precisely what it is about universals that makes them capable of combining with particulars without an intervening instantiation-relation. This strategy, endorsed by Read (2005), Forrest (1984), and Armstrong (1997), is inspired by the Fregean doctrine of ‘unsaturated’ concepts. The ability of universals to combine directly with particulars is explained by the universals having

the special characteristic of being ‘incomplete’ or ‘unsaturated’ or ‘in need of completion’ (Frege 1892a: 31). We may call this theory of instantiation ‘Incompleteness’ for short. But what is it for a universal to be incomplete, and does this ‘incompleteness’ show how universals and particulars can combine directly with each other? In the following paragraphs I argue that it does not: that Incompleteness does nothing whatsoever to answer the current challenge to the realist.

Gregory Currie has shown that Frege had two accounts of what it is for a concept to be ‘incomplete’. One is that it indicates ‘the mere ability on the part of the concept to lock immediately with the object’.<sup>165</sup> If this is all we mean by ‘incomplete’ it is clear that we have got no closer to explaining how it is possible for universals and particulars to combine directly without requiring an intervening relation. The realist’s answer is no less vacuous than the ‘explanation’ that opium puts people to sleep because it has a ‘dormitive virtue’.<sup>166</sup> If the realist is to give a satisfactory answer to the ‘how-possible’ question, it must be one which has at least some non-vacuous content.

Fortunately, Frege offers a second, more contentful characterization: concepts deserve to be called ‘unsaturated’ because they are incapable of existing on their own – they can only exist in combination with objects. Part of Frege’s motivation for introducing Incompleteness is to avoid the regress which threatens if we suspect that concepts and objects do not combine directly, but instead require an intervening relation. Incompleteness is intended to block this regress by showing that no intervening relation is necessary in the special case of concept-object combination (Currie 1984: 334); this is why Frege claims that

‘Not all the parts of a thought can be incomplete; at least one must be “unsaturated”, or predicative; otherwise they would not hold together.’<sup>167</sup>

Frege’s account needs some modification, for the kind of combination between concept and object he wanted to explain was something that could happen even when the concept did not actually apply to the object in question. In Frege’s mature theory, concepts are functions which map objects onto truth-values; when  $a$  is not  $F$  the concept ‘...is  $F$ ’ nevertheless combines with the object  $a$  to map it onto the False. In order to use Frege’s suggestion to explain the ability of a universal to combine with a particular when the particular instantiates the universal, it is necessary to hold, not that universals are incapable of existing without combining with particulars to determine a truth-value, but that universals are incapable of existing without actually being instantiated by particulars. The thought

<sup>165</sup>Currie 1984, p. 337. Currie’s source is a letter written by Frege to Anton Marty in 1882.

<sup>166</sup>*virtus dormativa cuius est natura sensus stupefire*. From Act III of Molière, *Le Malade Imaginaire* (1673)

<sup>167</sup>Frege 1892a, p.54

is this: by showing that universals are incapable of occurring without combining with particulars, we show what is special about universals, such that they are naturally suited to combine directly with the things that instantiate them. The natural metaphor is that universals have ‘gaps’ into which particulars slot directly, whereas relations are required when particulars are related to other ‘non-gappy’ particulars.

However, I shall argue that Incompleteness should not be accepted by the realist, because it fails to explain what it sets out to explain, namely how it is possible for universals and particulars to combine without an intervening relation, given that relatedness in general requires relations. Consider this caricature of the debate:

*Bradley:* Given that (according to you) relatedness in general requires relations, how is it possible for  $a$  and  $F$  to combine directly, without an intervening instantiation relation?

*Realist:* It is possible for  $a$  and  $F$  to combine directly, because the universal  $F$  is incomplete, which is to say that  $F$  could not exist unless something or other instantiated it.

The realist response does not seem at all satisfactory. The point is a simple one: if there is genuine doubt about whether universals are able to combine directly with the things that instantiate them, this doubt cannot be dispelled by saying that universals are the sort of thing which cannot exist without combining with the things that instantiate them. Perhaps it is true that universals, if there were any, would not be able to exist without combining with something or other; however, this may show no more than that universals cannot exist after all. An example should make the point more clearly: suppose that two philosophers are discussing square circles. One of them doubts whether there can be square circles with right-angled corners. However, the other one responds in this way:

“What you have to understand about square circles is that no square circle could exist unless it had right-angled corners. This explains how it is possible for there to be square circles which have right-angled corners.”

Schematically, the reasoning is this: there cannot be  $F$ s which are not  $G$ s, so there can be  $F$ s which are  $G$ s. It seems that the same fallacious reasoning is employed by those who attempt to show that there *can* be universals which combine directly with the things that instantiate them by claiming that there *cannot* be universals which *do not* combine with the things that instantiate them. Such an argument is invalid: the premise (there cannot

be universals which fail to combine) can be true and yet the conclusion (there can be universals which do combine) be false — in the situation in which there are no universals. But whether there are universals is precisely the question at issue!

We may conclude that Incompleteness fails in its attempt to show how it is possible for particulars and universals to combine directly, without needing an intervening instantiation-relation. Showing that universals could not exist unless they combined directly with particulars does not show that universals *do* exist and combine directly with particulars, or even that universals *could* exist and combine directly with particulars. Since we cannot explain how there can be relatedness without relations in the special case of particular-universal combination, either by appealing to a ‘radical difference in kind’ between particulars and universals, or by saying that universals are ‘incomplete’, it seems that the current realist strategy should be abandoned. We should not evade the Relatedness regress by denying that an instantiation-relation is required when universals and particulars combine in instantiation, for the realist has no good answer to the question, how is it possible for universals and particulars to combine directly, given that all other cases of relatedness require an intervening relation? In the next section I propose an alternative response to the Relatedness regress.

## An Alternative to Incompleteness

The current problem is the most threatening of the regress arguments considered so far. We ought not appeal to Informatism, for there is good reason to believe that the regress of distinct instantiation-relations is genuinely vicious: if the regress is conceded, it follows that the relatedness of universal and particular is impossible, for each new relation can be shown to function only as a term of a higher relation, and not as a relation that plays the role of relating some terms. Yet denying that the relatedness between particular and universal is achieved by these entities standing as terms to some further relation leaves the realist with no good account of why he should be allowed to suspend his thesis that ‘relatedness requires relations’ in this case, without suspending it more generally.

In this section, I propose an alternative solution. The suggestion is that the realist should recognize *instantiation* as a entity — a genuine relation in which particulars and universals stand — yet deny that a numerically *different* instantiation-relation is involved at each stage of the regress.<sup>168</sup> If there is only one instantiation-relation, then there is no further distinct relation intervening between it and the particular and universal it relates; therefore we may say that the instantiation-relation succeeds in combining with particular and universal *directly*. There is no regress of ‘preconditions’ that have to be met before this can be the case, and so no reason to believe that the combination of instantiation-relation with particular and universal is impossible.

It is a surprising feature of treatments of the regress that they do not say why a numerically different instantiation-relation is required at each stage. As we saw in the Ryle-Armstrong version of the regress, it cannot be merely that there must be as many different instantiation-relations as there are *instances* of that relation, for the realist supposes the instantiation-relation to be a universal, numerically the same across many different tokens or instances. This objection to the regress is raised by Wolterstorff

‘Some argument for the nonidentity of relation [I] and relation [I\*] seems necessary, if we are to have a convincing proof that in the series of sentences... an infinite number of different relations is said to be.’<sup>169</sup>

<sup>168</sup>I made this suggestion in my 2006 MPhil Thesis for the University of London; it has since been made in print by Meinertsen (2007). However Meinertsen thinks that the problem for the realist is to say ‘in virtue of what’ a relation combines with its terms, and it is clear that an instantiation-relation, even a self-relating one, cannot be that in virtue of which a relation combines with its terms, since the instantiation-relation could have existed even if the relation had not combined with its terms. Thus Meinertsen suggests, as it were, the right answer to the wrong problem.

<sup>169</sup>Wolterstorff 1970, p.93

The realist need not merely observe that there is no good reason to suppose a *different* instantiation-relation is needed at each stage. He can also provide two cogent arguments for his position. One is an argument from ordinary language: at each stage of the regress we notice that there is some link between the relation just introduced and its *relata*. It is claimed that this link is a *different* relation each time. But everyday discourse about characteristics and qualities recognizes only *one* kind of connection between relation and *relata*: when we find that entities stand in a relation, we say that one ‘bears’ the relation to the other, or that the two things ‘stand in’ the relation. Our everyday theory does not distinguish between different *ways* in which terms may stand to their relation; why then should we conclude that the relation which joins the terms *a*, *b* and *R* to their relation *I* is a *different* relation from that which joins the terms *a* and *b* to their relation *R*?

A second argument we might call the Argument from Consistency. Here the realist claims that the very principle to which the nominalist appeals to motivate the regress requires that we introduce no more than *one* instantiation-relation. We might put the point this way. Suppose a nominalist wanted to find fault with a realist who on the one hand claims that, when *a* and *F* combine in instantiation, there is a relation *instantiation* in which *a* and *F* stand, but on the other hand claims that, when *a*, *F* and *instantiation* combine in instantiation, there is *no* further relation in which these entities stand – it is merely a ‘brute fact’ about the world that these three entities are instantially combined without standing in a relation. Here the nominalist (with some justification) may raise an objection of inconsistency: the realist recognizes that *some* cases of instancial combination require that the combined entities stand in the relation of *instantiation*, but other cases do not.

To make such a charge against the realist, the nominalist will appeal to a principle of consistency:

(PC) The realist’s account of instancial combination must be the same regardless of which entities are said to be instantially combined.

But (PC) does not merely rule out theories in which some kinds of instancial combination require an intervening relation and others do not; it also rules out theories in which instancial combination is sometimes a matter of standing in the relation *I*, sometimes of standing in the relation *I*<sup>\*</sup>, sometimes of standing in the relation *I*<sup>\*\*</sup>, and so on. If the realist’s theory introduces a hierarchy of *different* instantiation-relations, he will have failed in the attempt to give a unified account of instancial combination. Nominalist and realist alike should recognize that (PC) requires that we posit only *one* instantiation-relation, in which any collection of entities stand iff they are instantially combined.



So there is good reason to suppose that the realist's theory should have room for only one instantiation-relation. But if there is only one instantiation-relation, the regress can be blocked. The realist notices that the combination between *a* and *F*, say, is a case of relatedness, and accordingly he introduces a *relation* in which *a* and *F* stand; call this relation *I*. Now Bradley will claim that the combination between *I* and its terms *a* and *F* must in turn be a case of relatedness, and so there should be a *further* relation introduced in which *a*, *F* and *I* stand. But the realist will point out that the combination between *I* and its *relata* is a case of *instantiation*; therefore consistency demands that the relation in which *I* stands to its terms is not some *new* relation *I*\*, but rather the very same instantiation-relation we have already — *I* itself. The situation, then, is this:

(IC) *a*, *F* and *I* stand in the relation *I*.

Can the regress continue beyond (IC)? I shall argue that it cannot. In the original regress, the progression from each stage to the next was mandated by the fact that at each stage we found a collection of related entities whose relatedness required us to recognize a *new* relation to which they stood as terms. But if we have a situation as described in (IC), it is one in which all three entities mentioned *already* stand in a relation, for *a*, *F* and *I* stand as terms to the relation *I*. There is no further relatedness which requires another relation, because the relatedness between *a*, *F* and *I* has already been accounted for by the relation in which they stand. By allowing that the instantiation-relation *I* can have itself among its *relata* — in other words, that *I* is *self-instantiating* — we arrive at a situation which cannot be accused of involving us in 'relatedness without a relation', because all three related entities *already* stand in a relation.

Does this proposal enable us to answer the challenge posed by the regress? I have argued that the viciousness of the original regress derives from the fact that each of the relations introduced functions merely as a term to some further relation, rather than playing the role of a relation which relates its terms. But if the ultimate situation is as described by (IC), one where we have an instantiation-relation whose terms are *a*, *F* and *itself*, then the problem is avoided. The original problem was that each relation in the regress functioned *only* as a term, and not as a relation. But now, it seems, we have a relation which functions *both* as a term *and* as a relation. It cannot be claimed that we are left without any entity playing the role of relating some terms, because the ultimate situation is that the relation *I* *does* relate some terms — provided, of course, that it is legitimate to suppose that a relation might instantiate itself as our instantiation-relation is said to do.

## Problems for the new theory

The view that there is only one, self-instantiating instantiation-relation invites two major objections. First, there is the powerful feeling that there must be something “wrong” with a description of a situation in which some entities bear a relation *I* to that relation itself. Wouldn't this be like a town being North of *being North of*? The second objection is harder to state precisely, but consists in criticizing the appropriateness of the realist's response: isn't there something unpleasantly *ad hoc* about the positing of a self-instantiating instantiation-relation? At the very least, doesn't the triviality of the solution suggest that the seriousness of the original problem has been underestimated? I answer each of these concerns in turn.

### 1. Are there self-instantiating relations?

Despite our initial suspicions about a self-instantiating instantiation-relation, there is good reason to believe that self-instantiating universals are not a metaphysical impossibility. Perhaps the initial doubt is due to the fact that any case in which a universal self-instantiates is, by its very nature, a higher-order situation — one in which a universal is instantiated not by a particular but by a universal. Since there is only a limited range of universals that it makes sense to see as instantiated *by* other universals, we should not be surprised that examples of self-instantiation are hard to come by. But examples there are. In particular, we should notice that *any* universal that is a characteristic of all universals will *ipso facto* be a characteristic of itself, and so self-instantiate. One such might be the universal which marks the fundamental respect in which all universals are similar — *being a universal*. If there is such a universal, it must be instantiated by itself as well as every other universal. By the same reasoning, if universals lack spatio-temporal location, then we might think that *being non-spatiotemporally located* is a self-instantiating universal, while if universals inhabit a Platonic heaven, then all universals bear the relation *inhabiting* to a place, namely the Platonic heaven. Since the relation *inhabiting* is itself a universal, it too will stand in the relation *inhabiting* to this Platonic heaven; in other words, it will be a self-instantiating relation just as we are claiming *instantiation* to be. A similar example arises for 'aristotelians' who hold that universals are spatio-temporally located, for the universal *being located at* will be a relation which every universal bears to some or other places; consequently *being located at* will be a self-instantiating relation, since according to the aristotelian it too must be located.

We need not be convinced of the claims of either 'aristotelian' or 'platonist' to concede the

point; rather, we should notice that their proposals are not to be deemed impossible simply because they require self-instantiating universals. It seems there is nothing *obviously* wrong with introducing self-instantiating universals. What the nominalist needs to challenge the current proposal is some argument to show that self-instantiating universals are *entia non grata*.

One such argument is proposed by Armstrong (1978b: 143). His argument is: nothing can bear a relation to itself, therefore no universal can instantiate itself. We should accept the validity of his reasoning, since we accept that to instantiate a universal is to stand in the instantiation-relation to that universal, and so for a universal to instantiate itself will be for that universal to stand in the instantiation-relation to itself.<sup>170</sup> If there is good reason to think that nothing can bear a relation to itself, then there can be no self-instantiating universals, and there can be no self-instantiating instantiation-relation. In that case, the realist will be compelled to recognize a *different* instantiation-relation for each fresh iteration, and the regress will threaten us again.

The realist must find a way to reject Armstrong's premise that nothing can bear a relation to itself. Here it will be necessary to make both a positive and a negative case. The positive case will give other reasons why we should allow things to bear relations to themselves in general. The negative case will attack Armstrong's reasons for denying that we should allow genuine instances of the schema  $Rxx$ . (I shall use the phrase 'reflexively instantiated relation' to talk of a relation that some entity bears to itself.) Perhaps the positive case is easiest to make. Armstrong himself recognises that our language has meaningful expressions of the form  $Rxx$ :

'somebody can love, hate, kill, wash or contradict himself as well as other people... Ropes can be entangled with themselves as well as with other objects.'<sup>171</sup>

For some of these relations we might use a theory of parts to produce a non-reflexive account of the situation. For example it might be more correct to say that 'one part of the rope is entangled with another part of the same rope'. However, this strategy will not work for every case. We are also accustomed to say that

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<sup>170</sup>It is not at all clear why *Armstrong* should deny that there can be self-instantiating universals on the grounds that nothing can bear a relation to itself, for Armstrong *denies* that when  $x$  instantiates  $y$  the two entities stand in a relation of *instantiation*. He claims that, if we reject situations of the form  $R(P,P)$ , consistency requires that we also reject situations of the form  $P(P)$ , which is 'simply the monadic case' (1978b, 143). But how can it be said that a situation  $P(P)$  where the same entity occupies argument-position and predicate-position is a 'monadic case' of a situation  $R(P,P)$  where the universal found in the predicate position is not also occupying an argument position?

<sup>171</sup>Armstrong 1978b, p.92. So too following quotes.

‘a particular is identical with itself, resembles itself, is the same size as itself, and so on.’

Armstrong himself has done a great deal of the work needed in enumerating the positive reasons for believing in reflexively instantiated relations. Another, even more compelling example is given by Russell (1910): since

‘in order to assert that a term may be related to itself, it is only necessary to show that a term may occur twice in one proposition’<sup>172</sup>

we may conclude that a term may be related to itself simply because ‘ $x + y = 4$ ’ asserts a relation between  $x$  and  $y$ , and this relation ‘is one which 2 has to itself’, since  $2 + 2 = 4$  (Russell 1910: 375).

The case that there can be reflexively instantiated relations is strong; all that remains is to address the argument that such relations should not be admitted. Armstrong’s argument against reflexive instantiation relies almost entirely on his rejection of *a priori* knowledge of universals. The problem with relational situations of the form  $Rxx$  is that they

‘exhibit the... disadvantage that we can determine *a priori*, without any need for empirical investigation, that the particular has the “relation” to itself.’<sup>173</sup>

Armstrong’s rejection of reflexively instantiated relations, and hence of self-instantiating universals, rests on the paradoxical principle that he calls ‘Irish’:

If it can be proved *a priori* that a thing falls under a certain universal, then there is no such universal.’<sup>174</sup>

Since, he believes, cases where a relation are reflexively instantiated are cases where we can know *a priori* that the thing bears the relation to itself, the ‘Irish’ principle will prevent us from recognizing the universal in question as genuine.

Two objections to Armstrong’s argument present themselves. First is a question of fact: *is it* really the case that all reflexively instantiated universals are such that we know *a priori* about anything that stands in them? If we take reflexively instantiated relations seriously, then for many of them it will *not* be an *a priori* matter whether a given particular bears

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<sup>172</sup>Russell 1910, p.375

<sup>173</sup>Armstrong 1978b, p.92

<sup>174</sup>Armstrong 1978b, p.11

that relation to itself. If we allow that *loving* is a genuine relation, no-one would want to deem it an *a priori* question whether a given person loves himself. Armstrong's main example of a reflexively instantiated relation is *identity*, which every thing bears to itself (1978b: 10). However, it is a mistake to generalize from this to a universal principle that *every* such relation may be known to hold *a priori*. If the Irish principle only applies to *some* reflexively instantiated relations and not others, we cannot appeal to that principle to establish a general prohibition against reflexively instantiated relations.

A second objection is that the Irish principle itself lacks a secure footing in Armstrong's 'scientific realist' epistemology. Armstrong characterizes 'scientific' realism in opposition to '*a priori*' realism, which is the view that we should recognize a universal 'wherever there was a corresponding predicate' (1978b: 7). Thus what is 'scientific' or '*a posteriori*' about Armstrong's realism is that

'what universals there are is not to be determined simply by considering what predicates can be applied to particulars. Instead, it is the task of total science, conceived of as total enquiry, to determine what universals there are.'<sup>175</sup>

This position is reiterated at the start of the second volume of *Universals and Scientific Realism*, with an expanded account of what is meant by 'total science':

'what properties and relations there are in the world is to be decided by total science, that is the sum total of all enquiries into the nature of things. (Philosophy is part of total science, but a mere part and not the most important part).'<sup>176</sup>

Armstrong's 'scientific realism', conceived as the denial that a universal should be recognized corresponding to every predicate, is congenial to the realist project developed in the preceding chapters. If universals are needed primarily to play the role of *respects* in which things are the same, and *features* which they share in common, then predicates and universals cannot stand in one-one correlation, for not every predicate will mark a genuine respect in which things are the same.

However, if *this* is how we are to understand 'scientific' realism, then it is hard to see why a Scientific Realist must endorse the Irish principle on which Armstrong bases his rejection of self-instantiating universals. There are three problems with the attempt to derive the Irish principle in this way. One is that there is nothing in Scientific Realism, as stated, to

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<sup>175</sup> Armstrong 1978a, p.xiii

<sup>176</sup> Armstrong 1978b, p. 8

rule out the possibility of gaining some knowledge of universals *a priori*, provided the *a priori* argument can be counted within some discipline which is part of ‘total science’. For example, philosophy, mathematics, and geometry may all be parts of ‘total science’, although it is not implausible to think that they are *a priori* disciplines; moreover, they are *a priori* disciplines which the realist will treat as dealing with the existence of, and relations between, *universals*. Armstrong’s real target is the realism which, as a matter of policy, treats each predicate as picking out a universal; but the rejection of such a realism is compatible with the view that knowledge of universals can be achieved through *a priori* means, provided our method is more than simply inferring the existence of the universal from the application of the predicate.

A further problem with the Irish principle is that, even if it is conceded to be a consequence of Scientific Realism that the existence of universals cannot be established *a priori*, this is not sufficient to motivate the ‘Irish’ ban on universals where we can ascertain facts about the combinations they form *a priori*. Armstrong endorses

‘the Empiricist rejection of the notion that one can establish the existence of entities by *a priori* reasoning’.<sup>177</sup>

However, the Irish principle requires more than that: it requires that we reject the notion that we can establish the patterns of instantiation in which a genuine universal stands by *a priori* reasoning. This latter claim is less immediately compelling, for even a realist who believes that our inventory of universals should be decided by empirical enquiry might believe that some of the facts about the patterns of instantiation formed by universals and particulars can be known *a priori*. A persuasive example is the fact of the asymmetry of instantiation: that particulars can only *instantiate*, and cannot *be instantiated by* universals. Why should we accept the Irish principle, if the rejection of *a priori* knowledge of the patterns of instantiation universals enter into is not mandatory even for a realist who believes that there is no *a priori* knowledge of the *existence* of universals?

A final, decisive, problem for the epistemological status of the Irish principle is that this principle, far from being consistent with Armstrong’s empiricism, seems to *contradict* it. His empiricism prohibits us from deciding whether or not a given universal exists on the basis of purely *a priori* considerations; yet the Irish principle enables us to decide that certain universals *do not* exist purely on the basis of *a priori* considerations. If it is possible to show *a priori* that a thing falls under a given universal *F*, then the Irish principle licenses us to conclude that *F* does not exist. What is this, if it is not reaching an ontological

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<sup>177</sup> Armstrong 1978b, pp.7-8

conclusion on the basis of purely *a priori* considerations (assuming that the Irish principle, as a piece of methodology, itself counts as *a priori*)?

For these reasons, I suggest, we should accept neither the Irish principle nor its use in the argument that there are no reflexively instantiated relations. Since Armstrong's case against self-instantiating universals depends upon the prior rejection of reflexively instantiated relations, our rejection of the Irish principle undermines his entire case, and we may conclude that he provides no good reason to reject the possibility of a self-instantiating instantiation-relation.

Before leaving this question, however, we should consider another way of thinking about universals which might encourage the rejection of self-instantiating universals. If a universal is thought of as having 'gaps' or 'slots' into which it fits the things that instantiate it, then reflexive instantiation is a problem, since it requires us to think of one and the same thing fitting simultaneously into two different 'gaps'; worse, in the situation where the instantiation-relation is instantiated by *a*, *F* and *itself*, we have a case where the third gap in the universal contains the universal itself. Surely this is an impossibility?

The right answer here is that the pictorial and spatial metaphors of universals having 'gaps' which are 'occupied' by the things that instantiate them are a relic of a theory of universals as 'incomplete' that was introduced only in an ill-fated attempt to do without an instantiation-relation, and which we now see there is no need to adopt. Moreover, it would be a mistake to allow our metaphysics to be driven by spatial imagery, imagining universal-blobs combining with particular-blobs and then asking ourselves what this would look like. To draw a tentative comparison with chemistry, the plastic models we make of molecules are assessed by how well they live up to the theory they (imperfectly) represent; it is not the case that the theory is constrained by the limitations of what can be represented using plastic models. Likewise, our ability to form visual images of the metaphysical situation when particulars and universals combine should not constrain our theory, so long as that theory does not fall into absurdity or inconsistency.

## **2. Trivializing the problem**

We have found no reason to deny the possibility of self-instantiating universals, and so no reason to deny that the relation *instantiation* might instantiate itself. Nevertheless, another charge against the realist remains. Isn't this all just sleight of hand, *ad hoc*-ery, or at best a response that unduly trivialises the problem? The Relatedness regress suggested that particulars and universals could not combine instantially; this problem is avoided

by refusing to recognize a numerically different instantiation-relation at each stage; yet to do this we had to adopt the desperate measure of allowing the instantiation relation in question to be self-instantiating. We may distinguish two related charges against the realist: one, that he is rewriting the metaphysics to suit the conclusion he wants to derive; the other, that his solution does not take the Relatedness regress seriously enough.

I shall suggest that neither of these charges is ultimately justified. Against the charge of *ad hoc*-ery, that the realist is pressing his metaphysics of instantiation into an unusual form simply to avoid the problem posed by Bradley's Regress, it can be replied that as early as the discussion of the Sameness regress in the previous chapter, it was suggested that we require no more than one relation of instantiation, if we are to account for the sameness of type between pairs or *n*-tuples of entities that form instantial combinations. Moreover, since all of these pairs or *n*-tuples are alike in the *same* respect, it was argued that consistency requires us to posit one and the same instantiation-relation shared between *all* cases of instantiation, even when this was the higher-order situation in which a particular and universal stand in the relation of *instantiation*. So the idea of a *single* instantiation-relation shared between every case of instantial combination is suggested by the analysis of the sameness of type between instantiation-situations, as well as being required for the current response to the Relatedness regress.

With regard to the charge of 'trivializing' the regress, we may respond that this solution no more trivializes the regress than does the strategy of denying that there is a genuine instantiation-relation in the first place, in the manner proposed by adherents of Incompleteness. In each case, rather than denying the viciousness of the regress, the realist blocks it before it can get going. In the Incompleteness view, this is done by finding a way for particulars and universals to combine without the need for an intervening relation of *instantiation*, while in the current proposal it is done by allowing only *one* instantiation-relation, literally identical across *all* its instances. So the current proposal does not 'trivialize' the regress; instead it recognizes its force, but finds a way to prevent it from getting started.

The idea of a single self-instantiating instantiation-relation is not, as might have been thought, a desperate measure to avoid disastrous confrontation with the regress, for this idea is suggested by our thought about universals even before we become aware of the regress; nor does it trivialize the regress itself. Moreover, we have found no good argument why such a relation constitutes a metaphysical impossibility. Therefore this proposal represents the best solution to the last, most threatening form of Bradley's regress.



## Conclusion

I have suggested that Bradley's regress has been persistently misconstrued, both by those who think that the structure of the regress supports only *one* argument against the realist, and by those who believe that it presents a problem for the realist only if entities such as instantiation-relations or states of affairs are introduced in addition to universals and particulars. But the greatest deficit in presentations of the regress arguments is the failure to see what form an adequate solution should take in each case. I have argued that the 'Sameness' regress proposed by Armstrong and Ryle is unthreatening because it rests on a mistake about the realist's obligation to provide an analysis of sameness of type, while the 'Difference-Maker' argument requires that we accept a Strong Truthmaker Principle which lacks theoretical rationale, and should not be considered an indispensable part of the realist's theory. Finally, the Relatedness regress, with which this chapter has been concerned, has been persistently underestimated, both by those who believe that an infinite hierarchy of instantiation-relations threatens nothing worse than a profligate ontology, and by those who believe it is possible to deny that instantiation is a genuine relation while maintaining that relatedness (in general) requires relations. However, this third regress may be solved, once we realize that there is no reason to introduce a numerically different instantiation-relation at each stage of the regress. For this reason we may conclude that no version of Bradley's Regress succeeds in defeating realism.

## Afterword

I have asked whether there is any good reason to believe that universals exist, and whether nominalists have provided any good reason *not* to believe this realist hypothesis. The answer to the first question is, Yes. Universals are needed as the entities over which we quantify when we quantify over *respects* in which things are the same, and *features* which things share in common. The nominalist should not reject such quantifications as part of a mistaken ‘community ontology’ because they are needed in the analysis of statements about qualitative sameness and difference whose truth cannot be denied. I have argued that there is no prospect of suggesting that these quantificational idioms might fail to be ontologically committing, either by offering an uncommitted ‘paraphrase’, or by providing alternative, ‘non-objectual’ semantics for the quantifier itself. The only remaining option for the nominalist is to provide some account of the entities over which we quantify, as an alternative to the realist’s account of respects of sameness as *universals*. There is good reason to suppose that no such alternative account could succeed; for that reason we should accept that shared features and respects of sameness exist, and that they are universals.

To address the second question, whether there is any good reason *not* to believe that universals exist, I have considered two kinds of problem facing the realist. One consisted of issues surrounding the theory of predicate-reference, a theory which I argued was an inescapable corollary of realism itself; the other was the family of arguments known as ‘Bradley’s regress’. Neither set of problems presents insuperable obstacles for a theory of universals, provided certain adaptations to the realist theory are permissible. In particular, the realist must be willing to accept that the reference-relation holding between predicates and universals can be one–many rather than one–one; that predicates, as well as picking out universals, have an additional function of ‘indicating propositional combination’; that universals play no ‘truthmaking’ role; and that there could be a self-instantiating instantiation-relation. I have argued that each of these views is reasonable. It seems we may answer the second question in the negative: at any rate, the problems considered here do not provide a reason to reject realism.

In closing, I should return to a question postponed from my preface: what kind of thing is a universal? In particular, are universals to be considered as ‘aristotelian’, co-located with the particulars than instantiate them, or as ‘platonic’, lacking spatiotemporal location altogether? We have encountered considerations which push us in each direction. In favour of an ‘aristotelian’ theory, it will be noticed that a theory of universals as ‘respects’ in which things are the same is most naturally understood as locating the universals *at*

their instances, for we become aware of respects in which things are the same, and features which they share, in perception, and it seems strange to say that we *perceive* universals which lack a spatiotemporal location. In favour of the alternative 'platonic' theory, it will be noticed that uninstantiated universals are needed to serve as referents of predicates which are true of no actually existing things, and uninstantiated universals must be 'platonic', for if they lack instances there is nowhere for them to be located.

Faced with this dilemma, we might be inclined to ask whether the choice between located and unlocated universals is exclusive. Is there any good reason not to take a view that locates instantiated universals at their instances yet allows uninstantiated universals which lack location altogether? 'Platonists' assume that, if universals lack spatiotemporal location, they are necessarily unlocated, while 'aristotelians' assume that, if universals have spatiotemporal location, they are essentially located in some place or places. The reason for the exclusivity of the choice is that unlocated universals are supposed to be a radically different kind of entity from the universals proposed by the aristotelian: unlocated universals are 'transcendent' (Armstrong 1978a: 64), and do not depend for their existence on being instantiated by one or another entity. They are *universalia ante res*, not *universalia in rebus*. Another factor emphasizing the difference between platonic and aristotelian universals is that, in the former case, it is mandatory to recognize instantiation as a genuine relation. As Armstrong puts it, for the platonist instantiation becomes 'a very big deal' (1989a: 76).

The *prima facie* appearance of a substantial difference in kind between platonic and aristotelian universals may be challenged. A consequence of our discussion of Bradley's Regress was that *no* theory of universals should deny that there is a genuine relation of instantiation holding between particulars and universals: if an instantiation-relation is not recognized, we have no account of the sameness of type between different instantiation-situations, and we have no account of how there can be 'relatedness without relations' in the special case of universal-particular combination. Even the aristotelian, then, needs to take instantiation seriously as genuine relation, and the status of the instantiation-relation does not constitute a major difference in kind between platonic and aristotelian universals.

The first point of difference, that located universals are dependent for their existence on the things that instantiate them, may also be challenged. Located universals are dependent on the things that instantiate them only if they are *essentially* located — if they could not have existed uninstantiated, lacking spatiotemporal location. But the point at issue is precisely whether located universals are *essentially* located; if we deny this, and say that a located universal *could* have existed unlocated, then there is no reason to think that even located universals are 'dependent' on particulars for their existence, and consequently

no reason to think that located and ‘platonic’ universals are fundamentally different in kind *qua* dependence. It would be circular to argue, against the current proposal, that located universals must be essentially located because platonic and aristotelian universals are radically different in kind, if our only reason for believing that platonic and aristotelian universals are radically different in kind is that the located, aristotelian universals are essentially located and consequently dependent on particulars in a way that platonic universals are not.

It seems that there is a third option for the realist, which synthesizes the most appealing features of platonic and aristotelian views of universals. From the platonic picture we adopt the idea that universals are related to particulars by a genuine relation of instantiation, and the idea that universals do not depend for their existence on being instantiated by something in the world. From the aristotelian picture we take the idea that an instantiated universal is located *at* its instances. The tension between these approaches evaporates if we allow that instantiated universals, although located, are not *essentially* located. If a given universal had *not* been instantiated, it would have existed without location.

Perhaps it will be objected that entities which can be either located or unlocated are far stranger than entities which are unlocated as part of their essential nature, for we usually think of the location of an entity as fundamental to its identity. How could a located universal have *lacked* a location and yet been the same entity; indeed, how could it be the same *kind* of entity? It seems the proper answer to this concern is to point out that, for the aristotelian at least, location is *not* fundamental to the identity of the universal, for the aristotelian is already willing to say that a universal could have been *wholly* present at a different number of locations: if the universal *F-ness* has been instantiated by *a* and *b* rather than just *a*, then *F-ness* would have been wholly present at two places, although in actuality it is present in only one location. If we are already willing to accept that a universal can be wholly present at *different* numbers of distinct locations, why not also credit the universal with the ability to be present at *no* location at all? If the number of locations can vary from one upwards, why couldn’t the number of locations a universal has also be zero?

This suggestion is advanced only tentatively; if it is rejected then some decision must be made between the ‘platonic’ and ‘aristotelian’ approaches. However, there can be no doubt that the realist needs to make up his mind on the issue. The reason is a happy one: it should not be doubted that universals exist, for they are the entities that we talk about when we talk about respects in which things are the same, features which things share, and characteristics that they have in common.

## References

- Abelard, P. 1992. 'On Universals'. In A. B. Schoedinger (ed.) *The Problem of Universals*. New York: Humanity Books.
- Alston, W. P. 1957. 'Ontological Commitments'. In *Philosophical Studies* 9: 8-17.
- Armstrong, D. M. 1974. 'Infinite Regress Arguments and the Problem of Universals'. In *Australasian Journal of Philosophy* 52: 191-201.
- 1978a. *Universals and Scientific Realism. Volume I: Nominalism and Realism*. Cambridge: Cambridge University Press.
- 1978b. *Universals and Scientific Realism. Volume II: A Theory of Universals*. Cambridge: Cambridge University Press.
- 1980. 'Against "Ostrich" Nominalism: A Reply to Michael Devitt'. In *Pacific Philosophical Quarterly* 61: 440-9. Reprinted in Mellor and Oliver 1997.
- 1983. *What is a Law of Nature?*. Cambridge: Cambridge University Press.
- 1989a. *Universals An Opinionated Introduction*. Boulder, Colorado: Westview Press.
- 1989b. 'C. B. Martin, Counterfactuals, Causality, and Conditionals'. In John Heil (ed.) *Cause, Mind, and Reality*. Dordrecht: Kluwer.
- 1989c. *A Combinatorial Theory of Possibility*. Cambridge: Cambridge University Press.
- 1992. 'Properties'. In K. Mulligan (ed.) *Language, Truth and Ontology*. Dordrecht: Kluwer. Reprinted in Mellor and Oliver 1997.
- 1997. *A World of States of Affairs*. Cambridge: Cambridge University Press.
- 2004a. *Truth and Truthmakers*. Cambridge: Cambridge University Press.
- 2004b. 'How do Particulars Stand to Universals?' In D. W. Zimmerman (ed.) *Oxford Studies in Metaphysics* 1: 139-154.
- Azzouni, J. 2004. *Deflating Existential Consequence A Case for Nominalism*. Oxford: Oxford University Press.
- 2007. 'Ontological Commitment in the Vernacular.' In *Noûs* 41: 204-226.
- Bambrough, R. 1961. 'Universals and Family Resemblances'. In *Proceedings of the Aristotelian Society* 61: 209-222.

- Barcan Marcus, R. 1962. 'Interpreting Quantification'. In *Inquiry* 5: 252-259.
- 1972. 'Quantification and Ontology'. In *Noûs* 6: 240-250.
- 1978. 'Nominalism and the Substitutional Quantifier'. In *The Monist* 61: 351-362.
- Barwise, J. 1984. 'Situations, Sets and the Axiom of Foundation'. In Paris, Wilkie and  
Winners (eds.) *Logic Colloquium '84*. Amsterdam: North-Holland.
- Barwise, J. and Etchemendy, J. 1987. *The Liar: an Essay in Truth and Circularity*. Oxford:  
Oxford University Press.
- Bealer, G. 2006. 'Universals and the Defence of *Ante Rem* Realism'. In P. F. Strawson  
and A. Chakrabarti (eds.) *Universals, Concepts and Qualities: New Essays on the  
Meaning of Predicates*. London: Ashgate Publishing, Ltd.
- Beebe, H., and Dodd, J. (eds.) 2005. *Truthmakers the Contemporary Debate*. Oxford.  
Oxford University Press.
- Benacerraf, P. 1962. 'Tasks, Super-Tasks, and the Modern Eleatics'. In *Journal of Philosophy*  
59: 765-784. Reprinted in Salmon 2001.
- 1965. 'What Numbers Could Not Be'. In *The Philosophical Review* 74: 47-73.
- Bergmann, 1964. *Logic and Reality*. Wisconsin: University of Wisconsin Press.
- Bigelow, J. 1990. 'Sets are Universals'. In A. D. Irvine (ed.) *Physicalism in Mathematics*.  
Dordrecht: Kluwer Academic Publishers.
- 1993. 'Sets are Haecceities'. In J. Bacon, K. Campbell, and L. Reinhardt (eds.)  
*Ontology, Causality and Mind: Essays in Honour of D. M. Armstrong*. Cambridge.  
Cambridge University Press.
- Bradley, F. H. 1897. *Appearance and Reality*, 2nd edition. London: Swan Sonnenschein &  
Co. Ltd.
- 1911. 'Reply to Mr Russell's Explanations'. In *Mind* 20: 74-76.
- Broad, C. D. 1933. *An Examination of McTaggart's Philosophy*. Cambridge: Cambridge  
University Press.
- Cameron, R. 2008a. 'Turtles all the way down: Regress, priority and fundamentality in  
metaphysics'. In *The Philosophical Quarterly* 58: 1-14.
- 2008b. 'Truthmakers and Ontological commitment: or, how to deal with com-  
plex objects and mathematical ontology without getting into trouble'. In *Philosophi-  
cal Studies* 140: 1-18.

- Campbell, K. 1981. 'The Metaphysic of Abstract Particulars'. In *Midwest Studies in Philosophy VI: The Foundations of Analytical Philosophy*. Reprinted in Mellor and Oliver 1997.
- 1990. *Abstract Particulars*. Oxford: Basil Blackwell & Co.
- Candlish, S. 2007. *The Russell / Bradley Dispute*. Basingstoke: Palgrave MacMillan.
- Cartwright, R. L. 1954. 'Ontology and the Theory of Meaning.' Reprinted in H. Feigl, W. Sellars, K. Lehrer (eds.) 1972. *New Readings in Philosophical Analysis*. New York: Meredith Corporation.
- Chihara, C. S. 1968. 'Our Ontological Commitment to Universals.' In *Noûs* 2: 25-46.
- Clark, R. 1988. 'Vicious Infinite Regress Arguments'. In *Philosophical Perspectives* 2: 369-380.
- Crane, T. and Farkas, K. 2004. *Metaphysics A Guide and Anthology*. Oxford: Oxford University Press.
- Currie, G. 1984. 'Frege's Metaphysical Argument'. In *The Philosophical Quarterly* 34: 329-42.
- Daly, C. 1997. 'Tropes'. In Mellor and Oliver 1997.
- Davidson, D. 1967. 'The Logical Form of Action Sentences'. Reprinted in Davidson 1980.
- 1977. 'The Method of Truth in Metaphysics'. Reprinted in Davidson 1984.
- 1980. *Essays on Actions and Events*. Oxford: Oxford University Press.
- 1984. *Inquiries into Truth and Interpretation*. Oxford: Oxford University Press.
- Devitt, M. 1980. "'Ostrich Nominalism' or 'Mirage Realism'?" In *Pacific Philosophical Quarterly* 61: 433-9. Reprinted in Mellor and Oliver 1997.
- Dodd, J. 1997. 'Farewell to States of Affairs'. In *Australasian Journal of Philosophy* 77: 146-160.
- 2000. *An Identity Theory of Truth*. Basingstoke: Palgrave MacMillan.
- 2002. 'Is Truth Supervenient on Being?' In *Proceedings of the Aristotelian Society* 102: 69-85.
- Dudman, V. H. 1972. "'Bedeutung" in Frege: A Reply'. In *Analysis* 33: 21-27.
- Dummett, M. 1955. 'Frege on Functions: A Reply'. In *The Philosophical Review* 64: 96-107.
- 1981. *Frege Philosophy of Language*, 2nd ed. London: Gerald Duckworth & Co.
- 1993. *The Logical Basis of Metaphysics*. Cambridge, MA: Harvard University Press.

- Dunn, J. M. & Belnap, N. D. Jr. 1968. 'The Substitutional Interpretation of the Quantifiers'. In *Noûs* 2: 177-185.
- Fine, G. 1980. 'The One over Many'. In *The Philosophical Review* 89: 197-240.
- 1993. *On Ideas: Aristotle's Criticisms of Plato's Theory of Forms*. Oxford: Oxford University Press.
- Fisk, M. 1972. 'Relatedness Without Relations'. In *Noûs* 6: 139-151.
- Forrest, P. 1984. 'Bradley and Realism about Universals'. In *Idealistic Studies* 14: 200-212.
- Fox, J. F. 1987. 'Truthmaker' in *Australasian Journal of Philosophy* 65: 188-207.
- Frege, G. 1891. 'Function and Concept'. Reprinted in Geach & Black 1980.
- 1892a. 'On Concept and Object'. Reprinted in Geach & Black 1980.
- 1892b. 'On Sense and Meaning'. Reprinted in Geach & Black 1980.
- Gaskin, R. 1995. 'Bradley's Regress, The Copula, and the Unity of the Proposition'. In *The Philosophical Quarterly* 45: 161-180.
- Gaskin, R. 2008. *The Unity of the Proposition*. Oxford: Oxford University Press.
- Geach, P. T. & Black, M. (eds.) 1980. *Translations from the Philosophical Writings of Gottlob Frege*, 3rd edition. Totowa: Barnes and Noble Books.
- Gibbard, A. 1975. 'Contingent identity'. In *Journal of Philosophical Logic* 4: 187-221.
- Gibson, M. 2004. *From Naming to Saying: the Unity of the Proposition*. Oxford: Blackwell.
- Gillet, C. 2003. 'Infinetism Redux? A Response to Klein'. In *Philosophy and Phenomenological Research* 66: 709-717.
- Glock, H-J. 2002. 'Does Ontology Exist?' In *Philosophy* 77: 235-260.
- Goodman, N. 1966. *The Structure of Appearance*. Indianapolis: The Bobbs-Merrill Company, Inc.
- 1972. *Problems and Projects*. Indianapolis: Bobbs-Merrill.
- Goodman, N. and Quine, W. V. O. 1947. 'Steps towards a Constructive Nominalism'. In *The Journal of Symbolic Logic* 12: 105-122.
- Gottlieb, D. 1980. *Ontological Economy: Substitution Quantification and Mathematics*. Oxford: Oxford University Press.
- Grossman, R. 1982. Review of *Universals and Scientific Realism Volume 1: Nominalism and Realism; Volume 2: A Theory of Universals*. In *Noûs* 16: 133-142.
- Haack, S. 1978. *Philosophy of Logics*. Cambridge: Cambridge University Press.



- Hawking, S. W. 1989. *A Brief History of Time*. London: Bantam Doubleday Dell Publishing Group Inc.
- Heil, J. 2003. *From an Ontological Point of View*. Oxford: Oxford University Press.
- Hermann, J. 2000. *Vulgar Latin*. Pennsylvania: The Pennsylvania State University Press.
- Hodes, H. T. 1990. 'Ontological Commitment Thick and Thin.' In Boolos, ed. 1990. *Meaning and Method: Essays in Honor of Hilary Putnam*. Cambridge: Cambridge University Press.
- Hofweber, T. 2005. 'A Puzzle about Ontology.' In *Noûs* 39: 256-283.
- Hornsby, J. 2005. 'Truth without Truthmaking Entities'. In Beebe and Dodd 2005.
- Hossack, K. 2007. *The Metaphysics of Knowledge*. Oxford: Oxford University Press.
- Jackson, F. 1977. 'Statements about Universals'. In *Mind* 86: 427-429.
- Jackson, F. C. 1980. 'Ontological Commitment and Paraphrase.' In *Philosophy* 55: 303-315.
- 1989. 'A Puzzle about Ontological Commitment.' In J. Heil (ed.), *Cause, Mind and Reality*. Dordrecht: Kluwer Academic Publishers.
- Jubien, M. 1996. 'The Myth of Identity-Conditions'. In *Noûs Supplement: Philosophical Perspectives*. 10: 343-356.
- Kahn, C. 1973. *The Verb "Be" in Ancient Greek*. Dordrecht: Reidel.
- Keinänen, M. 2005. *Trope Theories and the Problem of Universals*. Helsinki: University of Helsinki Press.
- Klein, P. 1998. 'Foundationalism and the Infinite Regress of Reasons'. In *Philosophy and Phenomenological Research* 58: 919-925.
- 1999. 'Human Knowledge and the Infinite Regress of Reasons'. In *Noûs* 33: 297-325.
- 2003. 'When Infinite Regresses are *not* Vicious'. In *Philosophy and Phenomenological Research* 66: 718-729.
- Kripke, S. 1976. 'Is there a Problem about Substitutional Quantification?' In G. Evans and J. MacDowell (eds.), *Truth and Meaning*. Oxford: Clarendon Press.
- 1981. *Naming and Necessity*. Cambridge, Mass.: Harvard University Press.
- Lewis, D. K. 1973. *Counterfactuals*. Oxford: Basil Blackwell.
- 1983. 'New Work for a Theory of Universals'. In *Australasian Journal of Philosophy* 61: 343-77. Reprinted in Mellor and Oliver 1997.

- 1986. *On the Plurality of Worlds*. Oxford: Blackwell Publishing.
- 1990. 'Noneism and Allism'. In *Mind* 99: 23-31.
- 1991. *Parts of Classes*. Oxford: Blackwell.
- 1992. Critical Notice of *A Combinatorial Theory of Possibility*. In *Australasian Journal of Philosophy* 70: 211-224.
- 2002. 'Tensing the Copula'. In *Mind* 111: 1-13.
- Lewis, H. A. 1985. 'Substitutional Quantification and Nonstandard Quantifiers'. In *Noûs* 19: 447-451.
- Liggins, D. 2007. 'Truthmakers and the Groundedness of Truth'. In *Proceedings of the Aristotelian Society* 108: 177-196.
- Linsky, B. 2003. 'The Metaphysics of Logical Atomism'. In N. Griffin (ed.) *The Cambridge Companion to Bertrand Russell*. Cambridge: Cambridge University Press.
- Loux, M. J. 1978. *Substance and Attribute*. Dordrecht: Reidel.
- 2002. *Metaphysics A Contemporary Introduction* 2nd edition, revised. London: Routledge.
- MacBride, F. 2002. 'The Problem of Universals and the Limits of Truth-making'. In *Philosophical Papers* 31: 27-37.
- 2005. 'The Particular-Universal Distinction: A Dogma of Metaphysics?' In *Mind* 114: 565-614.
- 2006. 'Predicate Reference'. In E. Lepore & B. Smith (eds.) *The Oxford Handbook of Philosophy of Language* Oxford: Oxford University Press.
- Maurin, A-S. 2002. *If Tropes*. Dordrecht: Kluwer Academic Publishers.
- Meinertsen, B. 2007. 'A Relation as the Unifier of States of Affairs'. In *Dialectica* 62: 1-19.
- Melia, J. 1992. 'A Note on Lewis's Ontology'. In *Analysis* 52: 191-192.
- Melia, J. 2005. 'Truthmaking without Truthmakers'. In Beebe and Dodd 2005.
- Mellor, D. H., and Oliver, A. (eds.) 1997. *Properties*. Oxford: Oxford University Press.
- Michael, M. 2008. 'Implicit Ontological Commitment'. In *Philosophical Studies* 141: 43-61.
- Moseley, J. L. 1983. 'Jackson, Criteria, and Ontological Commitment.' In *Australasian Journal of Philosophy* 61: 192-201.
- Mumford, S. 2007. 'Negative Truth and Falsehood'. In *Proceedings of the Aristotelian Society* 107: 45-71.

- Neale, S. 1993. 'Logical Form and LF'. In C. Otero (ed.) *Noam Chomsky: Critical Assessments*. London: Routledge and Kegan Paul.
- Newman, A. 1992. *The Physical Basis of Predication*. Cambridge: Cambridge University Press.
- Nolan, D. 2001. 'What's Wrong with Infinite Regresses?' In *Metaphilosophy* 32: 523-538.
- Nozick, R. 1981. *Philosophical Explanations*. Oxford: Clarendon Press.
- Oliver, A. 1996. 'The Metaphysics of Properties.' In *Mind* 105: 1-80.
- 2005. 'The Reference Principle'. In *Analysis* 65: 177i)a('87.
- Orenstein, A. 1978. *Existence and the Particular Quantifier*. Philadelphia: Temple University Press.
- Palmer, A. 1988. *Concept and Object: The Unity of the Proposition in Logic and Psychology*. London: Routledge.
- Pap, A. 1959. 'Nominalism, Empiricism, and Universals: I'. In *The Philosophical Quarterly* 9: 330-340.
- Parsons, C. 1971. 'A Plea for Substitutional Quantification'. In *Journal of Philosophy* 68: 231-7.
- Parsons, J. 2006. 'Negative Truths from Positive Facts?' In *Australasian Journal of Philosophy* 84: 591-602.
- Parsons, T. 1970. 'Various Extensional Notions of Ontological Commitment'. In *Philosophical Studies* 21: 65-74.
- Price, H.H. 1953. *Thinking and Experience*. London: Hutchinson's University Library.
- Prior, A. N. 1971. *Objects of Thought*. Oxford: Clarendon Press. University Press.
- Pruss, A. R. 2006. *The Principle of Sufficient Reason A Reassessment*. Cambridge: Cambridge University Press.
- Quine, W. V. 1951. 'Ontology and Ideology.' In *Philosophical Studies* 2: 11-15.
- 1960 *Word and Object*. Cambridge, MA: The M.I.T. Press.
- 1969a. *Ontological Relativity and Other Essays*. New York: Columbia University Press.
- 1969b. *Set Theory and its Logic*. London: Harvard University Press.
- 1980. *From a Logical Point of View*. 2nd edition, revised. London: Harvard University Press.

- 1986. *Philosophy of Logic*. 2nd Edition. London: Harvard University Press.
- Quinton, A. 1957. 'Properties and Classes'. In *Proceedings of the Aristotelian Society* 58: 33-58.
- Read, S. 2005. 'The Unity of the Fact'. In *Philosophy* 80: 317-342.
- Rodriguez-Pereyra, G. 2000. 'What is the Problem of Universals?' in *Mind* 109:255-274.
- 2002. *Resemblance Nominalism A Solution to the Problem of Universals*. Oxford: Oxford University Press.
- 2005. 'Why Truthmakers'. In Beebee and Dodd 2005.
- 2006. 'Truthmaker Maximalism Defended'. In *Analysis* 66: 260-264.
- Russell, B. 1903. *The Principles of Mathematics*. London: Allen and Unwin.
- 1905. 'On Denoting'. In *Mind* 14: 479-493. Reprinted in Russell 1951.
- 1910. 'Some Explanations in Reply to Mr Bradley'. In *Mind* 75: 373-378.
- 1911. 'On the Relations of Universals and Particulars'. In *Proceedings of the Aristotelian Society* 12: 1-24. Reprinted in Russell 1951.
- 1912. *The Problems of Philosophy*, 2nd ed. Oxford: Oxford University Press.
- 1918. *The Philosophy of Logical Atomism*. Reprinted in Russell 1951.
- 1927. *An Outline of Philosophy*. London: Allen and Unwin.
- 1946. *A History of Western Philosophy*. London: Routledge.
- 1951. *Logic and Knowledge*. London: George Allen & Unwin.
- 1992. *Theory of Knowledge, the 1913 Manuscript*. London: Routledge.
- Ryle, G. 1939. 'Plato's "Parmenides"'. In *Mind* 48: 129-151.
- Sainsbury, R. M. 2005. *Reference without Referents*. Oxford: Oxford University Press.
- Salmon, W. C. 2001. *Zeno's Paradoxes*. Indianapolis: Hackett Publishing Company, Inc.
- Schaffer, J. 2004. 'Two Conceptions of Sparse Properties'. In *Pacific Philosophical Quarterly* 85: 92-102.
- 2008. 'Truthmaker Commitments'. In *Philosophical Studies* 141: 71-91.
- Scheffler, I. and Chomsky, N. 1958. 'What is Said to Be.' In *Proceedings of the Aristotelian Society*, New Series 59: 71-82.
- Searle, J. R. 1969. *Speech Acts: An Essay in the Philosophy of Language*. Cambridge: Cambridge University Press.

- Sellars, W. 1960. 'Grammar and Existence: A Preface to Ontology'. In *Mind* 69: 499-533.
- 1980. *Naturalism and Ontology*. Reseda, California: Ridgeview Publishing Co.
- Schiffer, S. 1987. *Remnants of Meaning*. Cambridge, MA: Bradford Books.
- Sider, T. 2001. *Four-Dimensionalism: An Ontology of Persistence and Time*. Oxford: Clarendon Press.
- Simmons, K. 2000. 'Sets, Classes and Extensions: A Singularity Approach to Russell's Paradox'. In *Philosophical Studies* 100: 109-149.
- Soames, S. 2002. *Beyond Rigidity The Unfinished Semantic Agenda of Naming and Necessity*. Oxford: Oxford University Press.
- Sorabji, R. 1983. *Time, Creation and the Continuum*. London: Gerald Duckworth & Co.
- Strawson, P. F. 1950. 'On Referring'. In *Mind* 69: 320-344. Reprinted in Strawson 1971.
- 1959. *Individuals*. London: Methuen & Co. Ltd.
- 1971. *Logico-linguistic Papers*. London: Methuen & Co. Ltd.
- 1974a. 'Positions for Quantifiers.' In M.K. Munitz and P. Unger (eds.) *Semantics and Philosophy*. Reprinted in Strawson 1997.
- 1974b. *Subject and Predicate in Logic and Grammar*. Aldershot: Ashgate Publishing Ltd.
- 1976. 'Entity and Identity'. In H. Lewis (ed.) *Contemporary British Philosophy, Fourth Series*. London: George Allen and Unwin. Reprinted in Strawson 1974
- 1979. 'Universals'. In *Midwest Studies in Philosophy, Vol. 14: Studies in Metaphysics*. Minnesota: University of Minnesota Press. Reprinted in Strawson 1997.
- 1997. *Entity and Identity and Other Essays*. Oxford: Oxford University Press.
- Szabo, G. N. 2003. 'Nominalism'. In *The Oxford Handbook of Metaphysics*, ed. Loux, M. J. & Zimmerman D. W. Oxford: Oxford University Press.
- Teichmann, R. 1989. 'Three Kinds of Realism about Universals'. In *The Philosophical Quarterly* 39: 143-165.
- 1992. *Abstract Entities*. Basingstoke: Macmillan.
- Thomson, J. 1954. 'Tasks and Super-Tasks'. In *Analysis* 15: 1-13.
- 1970. 'Comments on Professor Benacerraf's Paper'. Reprinted in Salmon 2001.
- Tomberlin, J. 1990. 'Belief, Nominalism, and Quantification'. In *Philosophical Perspectives Vol.4: Action Theory and Philosophy of Mind* 573-579.

- Tugendhat, E. 1970. 'The Meaning of "Bedeutung" in Frege'. In *Analysis* 30: 177-189.
- Vallicella, B. F. 2002. 'Relations, Monism, and the Vindication of Bradley's Regress'. In *Dialectica* 56: 3-35.
- Van Cleve, J. 1994. 'Predication without Universals? A Fling with Ostrich Nominalism'. In *Philosophy and Phenomenological Research* 54: 577-590.
- Van Inwagen, P. 1981. 'Why I Don't Understand Substitutional Quantification'. In *Philosophical Studies* 39: 281-285.
- 2000. 'Quantification and Fictional Discourse'. In A. Everett and T. Hofweber (eds.) *Empty Names, Ficton, and the Puzzles of Non-existence*. Stanford: CSLI Publications.
- 2004. 'A Theory of Properties'. In D. W. Zimmerman (ed.) *Oxford Studies in Metaphysics*. Oxford: Oxford University Press.
- Vlastos, G. 1954. 'The Third Man Argument in the Parmenides'. In the *Philosophical Review* 63: 319-349.
- Wiggins, D. 1984. 'The Sense and Reference of Predicates: A Running Repair to Frege's Doctrine and a Plea for the Copula'. In *The Philosophical Quarterly* 34: 311-328.
- Williams, D. C. 1953. 'On the Elements of Being: II'. In *Review of Metaphysics* 7: 71-92. Reprinted in Crane and Farkas 2004.
- Wolterstorff, N. 1970. *On Universals*. Chicago: University of Chicago Press.
- Wright, C. 1988. 'Why Frege did not Deserve his *Granulum Salis*: A Note on the Paradox of the "Concept Horse" and the Ascription of Bedeutung to Predicates'. In *Grazer Philosophische Studien* 55: 239-263.
- Yablo, S. 1995. 'Singling Out Properties'. In *Philosophical Perspectives* 9: 477-502.