

# The Ontology of Repeatable Artefacts

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## Abstract

Abstract for a thesis submitted to THE UNIVERSITY OF MANCHESTER for the degree of PH.D. by GEOFF M. J. STEVENSON, and titled *The Ontology of Repeatable Artefacts*.

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Many of those artefacts with which we are so familiar – including, for example, works of music, photographs, novels, essays, films, television adverts, and graphic designs – share a common ontological nature. I argue in this thesis that they are all repeatable, and set out to provide an ontological account of these entities that explains the phenomenon of repeatability. In a fruitful meeting of aesthetics and metaphysics, a great deal has been written recently on the ontological nature of musical works. More encompassing enquiries have sought to understand the ontology of artworks in general. I will be responding to and engaging with this body of literature insofar as it also offers accounts of the entities I describe as repeatable. However, my approach gives metaphysical concerns and the phenomenon of repeatability primacy over aesthetic concerns.

Here I argue that repeatable artefacts fall into the ontological category of kinds. I develop an account of repeatable artefacts as kinds that has two key components. Firstly, on my view kinds are physical rather than abstract. Secondly, I argue that repeatable artefacts, as kinds, have essences that are purely relational and historical.

The thesis begins with a discussion of method. The methodological issue has grown in prominence in recent years, as theorists have sought some higher level arbitration on the expanding number of theories and approaches being offered in response to ontological puzzles. Drawing on the work of Amie Thomasson, I defend a methodology according to which we should develop an ontological account using careful conceptual analysis that assesses our intuitions about the application of referring terms. This commitment to conceptual analysis is then defended from misunderstandings and objections.

I apply this method in giving an ontological explanation for the phenomenon of repeatability. I argue that repeatable artefacts are kinds. Kinds are strongly individuated by their essences, which are the conditions that must be satisfied for the kind to be instanced. I then develop an account of kinds as physical multiply located entities, that exist when and where they have instances. This stands in contrast to the prevailing view according to which kinds are abstract.

I then set out to give an account of the essences of paradigmatic repeatable artefacts. I argue that this can be done if we are willing to reject the default view according to which essences are at least partly structural, and replace it with an account of purely relational and historical essences. The essences of many paradigmatic repeatable artefacts, I claim, involve causal historical processes of copying.

## Declaration

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By far the biggest debt of gratitude is owed to my wife, Sarah, who has patiently stood by me, worked to keep food on the table, and offered continual love and support. This thesis is dedicated to our unborn child.

Finally, I thank God, who is simpler than the simplest axiom, and yet more complex than the deepest philosophical puzzle.

# Introduction

Suppose we ask “What is a novel?” There are a number of things we might be asking with such a question. We might want to know what a novel is in contrast to a poem, or what a novel is in contrast to a scientific work, or what a novel is in contrast to a film. The question I address in this thesis might be similarly clarified as: what is a novel in contrast to a *copy* of a novel?

The puzzle is immediately interesting for philosophers, because here we have things which seem familiar and commonplace yet when we look only a little beneath the surface what we are talking about is not so obvious at all. The work ULYSSES is not my copy of it, as Richard Wollheim has stressed (Wollheim, 1980, p.5). The novel has been read many times by many different people, but the same is not true of my copy. Nor is the work the original manuscript penned by Joyce. The manuscript could be lost or destroyed without the work being lost or destroyed. The manuscript could be worn and faded so that it is hard to read, or locked away so that it is impossible to read, but any difficulty one might have reading the work itself is not of this kind.

It would be equally implausible to say that the novel was identical with *every* copy of it, as Wolterstorff shows with a brief consideration of the transitivity of identity (Wolterstorff, 1980, p.35). Since any two copies are non-identical, they cannot both be identical with some third thing. The novel is not the same as any copy or all the copies, but what then is the novel?

It can be seen, I think, that the question thus put is not specific to novels. The question of what a novel is, in comparison to a copy of a novel, arguably has nothing to do with being a *novel* as such. We could similarly ask ‘What is a poem in contrast to a copy of a poem?’ and reasonably be taken to be asking the same question. The question being raised is no respecter of literary genre.

One of the premises of this thesis is that the puzzle is much broader still. We can extend it from literary works to text based artefacts in general: what is an academic essay in relation to a copy of that academic essay? What is this thesis in relation the copy of this thesis that you are holding now? Novel, poem or essay, it seems that the same kind of relationship is emerging. There can be many copies of a single novel, many copies of single poem, and many copies of a single essay, so what is this novel, poem or essay? What is its ontological nature?

Can the question be extended further beyond text-based artefacts? I believe it can. There is something strikingly similar about the puzzling relationship between a text-based artefact and its copies and the relationship between a photograph and its prints. There can be many prints of a single photograph. The photograph appears not to be

identical to any of its prints, but what then is the photograph and how is it related to its prints? Again a photograph may be a work of art, but it seems the puzzle holds independently of aesthetic properties. I suggest we can just as easily ask: what is a company logo in contrast to the many copies or prints of that logo?

Once the pattern is recognised, it can be spotted elsewhere with ease. The puzzle seems to equally apply to songs, films and plays, and even to such things as words, jokes and stories. In each case we have an artefact which appears identifiable and singular, and yet is encountered through its many copies, performances, showings and prints – what we can collectively call its *instances*. We can further call all those things that seem to share this relationship with instances *repeatable* things. Those repeatable things that I am interested in here I will describe loosely as *artefacts*. What I am not interested in doing is offering any tight analysis of the notion of an artefact. Rather, I will use the term as a broad and intuitive place holder, and operate for the most part with paradigmatic examples of the entities I am interested in (novels, essays, poems, photographs, logos, plays etc.). Thus in asking our original question – What is a novel? – we are asking about the *ontological nature of repeatable artefacts*.<sup>1</sup>

There are two points that emerge out of this initial presentation of the question. The first is that the question begins by collecting together a broad group of entities under what appears to be the same phenomenon, and setting out to provide an account of those entities. This is in contrast to an approach that picks some more specific kind of entity as the focus of an ontological enquiry. The recent relevant literature is dominated by discussions of the ontological status of works of music, and sometimes even more specifically, fully scored classical works in the western tradition (Levinson, 1980, p.6). Such approaches clearly have merit. The subject matter is precisely defined and the ontologist can work with a clear focus without having to continually consider broad examples. Moreover, it is much less likely that the subject matter will turn out to be ontologically diverse, not admitting of a single ontological theory.

However, taking the broader perspective adopted here has its own distinct advantages. If successful it will provide a theory with wider applicability and far greater explanatory power than a more focussed approach. Furthermore, the broader perspective lends itself more easily to spotting trends and categorical demarcations. By taking repeatable artefacts in general as the subject matter, we concern ourselves with entities that “share an ontological predicament” as Guy Rohrbaugh has put it (2003, p.177).

The second point to note is that the question being asked does not rely on any distinction between those artefacts that are considered works of art, and those that are not. Though many of the examples that I will use in discussing repeatable artefacts will be entities typically seen as works of art (broadly construed) the focus here is not on their status *as art*, but on their ontological nature. Thus the question is firmly one of metaphysics rather than aesthetics. As such I will be engaging, to a significant degree, with debates in the ontology of art, but it should be understood that my general theory

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<sup>1</sup>The use of ‘repeatable’ here thus involves a slight extension from our ordinary application of the term, which is usually reserved only for ‘event-like’ entities such as plays, films and songs.



is more encompassing. What I provide will stand as a direct competitor to ontological theories aimed specifically at repeatable works of art, though my theory includes, rather than is limited to, those things.

Others have developed ontological accounts according to which an artwork's ontological nature and its status as art are intimately connected.<sup>2</sup> Accordingly, being a work of art in some way *contributes to* or *specifies* the ontological nature of the entity in question. This type of approach has fostered the study of the ontology of art as a unique sub-discipline crossing the boundaries between metaphysics and aesthetics.

However, while I don't want to downplay the significance of aesthetic considerations, the approach I adopt holds that ontological distinctions are orthogonal to aesthetic ones. My reasons for thinking this are essentially the same as those offered by Andrew Harrison (1967). Harrison notes that we can distinguish "two general approaches to the question of what is a work of art, one entering the subject by way of the idea of judgement, one by way of the idea of an object" (1967, p.105). According to the former approach "[o]bjects of aesthetic judgement are seen, as it were, "bracketed off" from the rest of experience" (1967, p.105). However, this approach risks overplaying the significance of aesthetic judgement to ontology. Since, as Harrison notes, "virtually anything may be contemplated aesthetically" (1967, p.106), I think we should be suspicious of the idea that aesthetic judgement picks out an ontological category.<sup>3</sup>

Instead, though approaching works of art as objects may be the more "mundane" approach (Harrison, 1967, p.107) it seems more suited to ontological investigation. Rather than pick out entities that are all attended to aesthetically, the focus here is the phenomenon of repeatability itself. That some of those things are considered to be art is, on this view, not ontologically relevant.<sup>4</sup>

The question to be addressed, then, is the question of the ontological nature of repeatable artefacts. Much summarised, the answer that I will articulate and defend in this thesis is that repeatable artefacts are *copied kinds*. Kinds, I will argue, are physical multiply locatable entities that exist when and where they have instances. A kind has an *essence*, where this is the set of conditions that something must satisfy to be an instance of the kind. I will defend the view that the essences of many repeatable artefacts involve historical and relational properties: the property required for something to be an instance of a novel, for example, is that it be a suitable copy of a previous instance of that novel.

The thesis divides into three parts, with two chapters devoted to each part. In Part I,

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<sup>2</sup>In particular, see Currie (1989) and Davies (2004). Peter Lamarque (2002) adopts an approach that focusses on *works* (broadly construed) but allows that within this category there is ontological diversity, with some works being 'particulars' and some being 'types'.

<sup>3</sup>Robert Howell has similarly argued that literary works do not carve a single ontological category, but are "ontologically various" (Howell, 2002a, p.68). He argues that too many different sorts of thing are described as literary works and, like works of art in general, we should not expect ontological unity (2002, p.77). Amie Thomasson (2006) similarly compares the term 'art' with the term 'gift', which appears not to be "category specific" since a gift may be a trip to the beach, a T-shirt, or a poem, for example (2006, p.250).

<sup>4</sup>This point is contentious. We will return to this subject in §1.1 when I consider David Davies' methodological approach.

I discuss the issue of the correct methodology, a subject that has grown in prominence in recent years in this area. In Part II, I apply that methodology and develop an account of repeatable artefacts as kinds. In Part III, I tackle the question of the essences of repeatable artefacts.

## Method

Before addressing the primary question head on, a significant portion of the thesis will be given over to the methodological question. That is, before providing an account of the nature of repeatable artefacts I will look at the issue of how one should go about providing such an account.

We have a question, but how are we to know if we are looking for answers in the right place, and what constitutes a good answer? How can we know if we've got it right? To put it bluntly, why think that the ontological proposal set out here is going to be any more likely to hit the mark than any of the numerous others in the vicinity?

There are two reasons in particular for devoting this much attention to method. Firstly, and most simply, the methodological issue is extremely interesting. In asking how we should go about answering our ontological question we have to take a step back from repeatable artefacts and assess what it is that philosophers in this field are and should be doing. Secondly, the methodological issue has commanded a significant amount of attention in the recent literature,<sup>5</sup> and a full engagement with the existing ontological debates requires that the methodological issues be addressed.

One of the reasons for this growth in the interest in methodology is the large and expanding number of alternative ontological proposals that have been offered in response to what appears to be the same or similar questions. Amie Thomasson, a recent champion of the methodological issue, has described the variety of positions in the ontology of art as “an embarrassment of riches” (Thomasson, 2005, p.221) but it might as easily appear, as David Davies describes it, as “a philosophical “badlands,” a realm populated by entities as diverse as norm-kinds, indicated structures, action-types, continuants, and performances” (Davies, 2009, p.159).<sup>6</sup> When a question is asked, such as ‘what is the ontological nature of a work of music?’, and such an array of answers is offered as can be found in the recent literature, it is natural to suppose that there might be nearly as much variation in how the question is being answered as there is in the answers themselves. Unsurprisingly, then, questions of method are becoming increasingly important.

I begin the discussion of methodology in Chapter 1 by framing the question in terms of the relationship between our pre-theoretical beliefs or folk theories, and philosophical theory. The question to be asked is: to what extent, if at all, should ontological theory be beholden to our folk theories and beliefs? The responses *not at all* and *entirely*

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<sup>5</sup>See, for example, Thomasson (2005), Thomasson (2006), Kania (2008), Davies (2009), Steckner (2009) and Walton (2007).

<sup>6</sup>Though perhaps one would have to be in a rather cynical mood to agree with Kania that what we have here is a collection of “burnt-out wrecks on the ontological sea” (Kania, 2008, p.427).

are both implausible. Instead, we must look for some justified middle ground between these extremes. I consider and discuss Kendall Walton's position on philosophy as theory construction based on the relevant data (Walton, 2007). While there is a great deal of sense in this, I will argue that for the ontologist, how we should decide what counts as data is far from clear, and a methodological problem still remains. I then consider David Davies' methodological approach which places special emphasis on our critical and appreciative practices (Davies, 2009). I argue that Davies' methodology is at best unsuited to our project and at worst unsuitable even for his own.

I then introduce Amie Thomasson's methodological contribution. I believe that there is a great deal that is right in Thomasson's work, but that her account, which is effectively a defence of conceptual analysis based on semantic considerations, needs developing in certain key areas. The remaining half of Chapter 1 and the whole of Chapter 2 are then given over to elaborating on and defending this account. Briefly, I will argue that we should proceed in answering our ontological question using careful analysis of our intuitions regarding the application of the terms for those entities we are interested in. I will defend the view that language communities must associate terms with a tacit understanding of how those terms are correctly applied in possible scenarios if that term is to have a stable and determinate reference. As such it is these considered intuitions that provide a constraint on ontological theorising, if the ontologist is to avoid changing the subject.

## Kinds

With a methodology set out and defended, I then turn to the ontological question proper. There are two general introductory points that need to be made here. The first concerns a broad assumption that I will be making in this thesis – that anti-realism about repeatable artefacts is false – while the second concerns terminological choices.

It is certainly possible to adopt one of numerous forms of anti-realism concerning repeatable artefacts. Challenges to the existence of specific kinds of repeatable artefact are quite rare,<sup>7</sup> presumably because anti-realists of various stripes will regard their arguments as being more widely relevant, yet there is still plenty that could be said in response to anti-realist worries. Nevertheless, I will say very little, and effectively take the falsity of anti-realism as an assumption of this thesis. A philosopher must pick his or her fights, and this in one fight that I choose to leave to others.

My reasons for this are as follows. Firstly, it would be quite possible to dedicate a whole thesis to assessing anti-realist arguments (including, for example, versions of fictionalism, nihilism and nominalism about repeatable things) and realist responses, but I take that to be a distinct undertaking from the one set out here. This is in part because most of the relevant arguments will not be specific to repeatable artefacts, and instead will depend on broader metaphysical and meta-metaphysical positioning.

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<sup>7</sup>Though see Cameron (2008) for a defence of the view that musical works do not *strictly speaking* exist, and see Predelli (2009) for a reply. Kania (2008) has also defended a form of *fictionalism* for musical works.

As such, embarking on that journey would detract from an assessment of the positive proposal set out here.

Secondly, it appears to me that the default position should be realism. It at least seems there are such things as novels, works of music, films and essays, and unless one specifically sets out to engage with arguments to the contrary, we have perfectly valid grounds for offering a theory to explain what sorts of things these are.

Finally, the proof of the pudding is in the eating. Given that, as I will argue, we can provide a methodologically sound and coherent account of the ontological nature of repeatables – one that puts them in a broad ontological category with many other things – many of the reasons to doubt their existence, based on inconsistency and incoherence, for example, are undermined. I therefore begin my account of the nature of repeatable artefacts taking the fact that they exist and we refer to them as a given.

The second introductory point involves terminology. Anyone familiar with the literature will know that the most popular move in explaining the nature of words, novels and works of music involves an appeal to ‘types’. A work of music or a novel, it is said, is a ‘type’, and the performances of the music and the copies of the novel are called the ‘tokens’ of that type. The terminology is borrowed from C. S. Peirce (Peirce, 1933, p.243) who introduces it to draw a distinction between a word as a thing that can be repeated – the word type – and a word as a particular instance – the word token.

Now, when put to use as a *semantic* distinction Peirce’s terminology is invaluable. It helps clear up an ambiguity inherent in our talk about what a word is. It makes explicit a distinction that we are all fully aware of, or at least willing to accept once it is pointed out, but that has escaped clarification in pre-theoretical language. If we accept that words represent the same ontological phenomenon that we encounter with novels and copies of novels, and plays and performances of plays, the semantic use of the type/token distinction can then be rolled out further. We would then express the difference between a novel and a copy of and novel by speaking of novel types on the one hand and novel tokens on the other hand.

However, the numerous attempts to put ontological flesh onto the bones of the distinction have varied significantly. Peirce himself seemed to regard types as abstract “Forms” though he says also that a type “does not exist” and that it is impossible that the type “should lie visibly on a page or be heard in any voice, for the reason that it is not a Single thing or Single event” (Peirce, 1933, p.243). Though exactly what Peirce means here is rather obscure to a modern reader, there has been a strong tradition of following Peirce in holding that types are abstracta, (a notable exception is Eddy Zemach) but there has been little consensus as to what this exactly means. For some they are timeless abstract ‘universals’ (Dodd, 2002; Kivy, 1993) though for others they are abstract but creatable (Levinson, 1980; Howell, 2002).

Theories have not only offered different accounts of the nature of types, but also different takes on what should be properly called a type. Thus, for example, Jay Bachrach insists that the type/token scheme only applies to “linguistic elements” and should be used “for the express purpose of designating physical objects as meaningful units in a

language” (Bachrach, 1971, p.416). This, by his lights, rules out a type/token analysis for most works of art. Richard Wollheim, on the other hand, suggests that “we postulate types...where we can correlate a class of particulars with a piece of human invention” (Wollheim, 1980, p.78), and more recently Charles Nussbaum has put forward a view of types as only those things that “involve a process of historical reproduction” (Nussbaum, 2003, p.275). Other theories that appeal to types, such as Julian Dodd’s, have a thinner and as such broader conception of what a type is (Dodd, 2007, pp.8-19). Given this variation it can be a challenge to distinguish significant metaphysical differences in type theories from mere terminological differences.

As such, rather than leap into debates about whether repeatable artefacts are or are not types, I will avoid the type/token terminology as it applies to theories altogether, and retain it only as a handy semantic distinction between ‘words’ as those things that are repeated (types), and ‘words’ as individual occurrences (tokens). Instead, the theory I develop will describe repeatable artefacts as kinds. The choice here is partly cosmetic, and it would be possible, though somewhat tedious, to translate the theory offered here from one of kinds and their instances to one of types and their tokens.<sup>8</sup> However, as well as avoiding some of the ambiguities discussed above, the advantage of appealing to kinds is that it allows for a straightforward link with the natural kinds of science, a link that will prove to be significant for the theory developed here. In particular, it will help to emphasise the breadth and wide applicability of the ontological category into which I place repeatable artefacts (I will argue that repeatable artefacts belong in the same ontological category as gold and water, for example).

I will begin in Chapter 3 with a careful assessment of the phenomenon of repeatability as it arises out of our practices of identifying repeatable artefacts. It is at this stage that the methodology defended in Part I is put into practice. I will argue that the phenomenon is marked by the recognition of the identity of the repeatable entity despite the distinctness of instances. After rejecting two possible solutions to explain this (a set hypothesis, and a ‘scattered object’ hypothesis), I appeal to kinds as entities that can have multiple instances, and are thus intrinsically repeatable. The notion of a kind that I develop is rooted in our ordinary notion of things being *of the same kind, type or sort*. In particular, I argue that kinds are instanced whenever the *essence* of the kind is satisfied, where the essence is understood as a set of conditions, or properties, that are necessary and sufficient for something to be an instance of that kind. I accept the common assumption that the essence of the kind is modally essential to that kind.

In Chapter 4, I develop the metaphysics of kinds in more detail. My aim here is to propose an alternative to the entrenched belief that kinds are abstract, which I call *kind Physicalism*. I argue that we can coherently develop an account of kinds as physical multiply locatable entities that are *co-present* with their instances. On this view kinds exist when and where their instances exist. I will argue that this view ultimately

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<sup>8</sup>One prominent theory that uses the terminology of kinds and instances is Wolterstorff’s (1980). Wolterstorff suggests he would be equally comfortable with either terminological choice (1980, p.194).

provides a more satisfactory account of the nature kinds than appeal to abstracta.

## **Essences**

In the final part of the thesis I take up the challenge of providing an account of the essences of repeatable artefacts. This is not just a matter of filling in the details, however. The viability of the ontological theory proposed depends, to a degree, on the possibility of articulating reasonable essences for the kinds in question. If every repeatable artefact is individuated by an essence – the set of conditions that something must meet to be an instance of that artefact – then we had better be able to say, at least roughly, what those essences are.

The dominant view in the literature is that the conditions laid down are at least partly structural. This is unsurprising given that for many repeatable artefacts, it is the structure of the instances that we are most interested in: we value a novel for the word structure chosen by the author; we value a piece of music for its sonic features; and we value good photography at least in part for its visual structure. Structure is also clearly relevant to the identification of repeatable artefacts. All copies of the novel *EMMA* have the same word structure, and we can tell (usually) that a print is a print of a certain photograph by how the print looks. However, in Chapter 5 I consider this view and argue that it suffers number of difficulties which together provide good reason to doubt that essences of repeatable artefacts are in fact structural.

If the essences of repeatable artefacts are not at least partly structural, what else could they be? The rejection of structural essences might seem to be tantamount to rejection of essences, and thus a rejection of kinds, entirely (Rohrbaugh, 2003). However, in Chapter 6 I argue that non-structural essences can be provided for repeatable artefacts and that these non-structural essences do a better job of accounting for repeatable artefacts, including the seemingly central role of structure, than structural essences themselves. I will argue that repeatable artefacts are copied kinds with purely relational essences.

The position is inspired by accounts of relational essences for biological species, where the absence of structural essences has been apparent for some time. In what is an ongoing debate, the absence of structural essences has led some to reject the view that species are kinds. It has been responded, however, that a kind theory can be maintained (and thus that the rejection is an over-reaction) once we accept that the essences of species are relational and historical. What is required to be a member of a species is not that an organism have a certain structure, but that it emanate from the right historical lineage. I will argue that a similar view can be developed for repeatable artefacts. On the view defended, then, repeatable artefacts have a great deal in common with biological species. Both have essences that depend on processes of historical ‘reproduction’, the latter being biological, the former involving our cultural practices of making ‘copies’ (broadly understood) of that which we value, admire, or just find useful.

## *Introduction*

That concludes the introductory overview of what lies ahead. It remains to say just that though the specific question being addressed in this thesis is interesting in its own right, it is clear that much of the philosophical value of asking such a question lies in the methods and techniques that must be considered and employed in asking and answering that question. Hopefully in what follows there will be as much of worth in the process of this enquiry as in the outcome itself.

Part I.

Method



# 1. The Search for Methodological Guidance

If the first order ontological question about repeatable things asks what sort of thing an essay or a work of music or a logo is, then the methodological (or *metaontological*) question asks more generally how we should go about answering that first order question about essays, works of music, or logos. I have discussed the need to address this methodological issue in the Introduction. To recap, the ontological proposals offered to account for what appear to be the same entities are many and varied and there has been a growing feeling that if progress is to be made we must examine more closely the methods employed in reaching those conclusions. In heed of that, I do not want to add just another ontological theory to the pile. Instead I begin with a detailed assessment of the methodological questions that have been raised and answers that have been offered – an assessment that will take up these first two chapters.

In this chapter I assess the methodological state of play in the ontology of art and identify the proposal I take to be most promising. In the next chapter I will defend that proposal in greater depth. The position to be defended argues that ontology should proceed by careful conceptual analysis, where this amounts to an analysis of our understanding of the application and co-application conditions associated with a referring term. It is this understanding that determines (in the way to be explained) what our terms refer to, and so if we want to provide an ontology of what we ordinarily mean by ‘novel’ and ‘poem’, for example, we must offer an ontology in line with that analysis. We will see that a methodological approach of this sort provides methodological guidance while still leaving room for ontological theorising.

I start, in §1.1, by framing the question in terms of the relationship between philosophical theory and pre-theoretical intuitions and beliefs, or more generally our folk theory.<sup>1</sup> We can ask: how many, if any, of our folk beliefs about an entity should be respected by an ontological theory for that entity? The answers *all* and *none* are rejected. I then assess a proposal offered by Kendal Walton that suggests, roughly, that a philosophical theory should be constrained only by the data it seeks to explain. I argue that given the difficulties of separating theory from data, a methodological puzzle still remains. A more exact solution has been offered by David Davies. Davies’ account is steered by the belief that an ontology of art should be constrained principally by our aesthetic critical and appreciative practices. I argue, however, that Davies’ proposal is

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<sup>1</sup>I accept that this is a somewhat archaic term, but it is commonly enough used to make it worth adopting. It should be noted that for the most part ‘the folk’ are just us.

at best unsuitable for the ontological project undertaken here, and at worst unsuitable for his own project.

In §1.2 I give an account of Amie Thomasson's methodological argument. I outline how her proposal promises to succeed in answering the methodological question where Davies' failed. Thomasson claims that a consideration of how reference is fixed, and in particular a rejection of a 'pure causal theory' of reference, gives us reason to believe that reference is at least partly determined by certain elements of the conceptual content of language users. As such an analysis of that content must guide an ontology if we wish to provide an account of what the folk are talking about, and not inadvertently change the subject. Finally, a detailed analysis is undertaken of one of the key steps in Thomasson's argument (§1.3). I argue that the details of her rejection of a causal theory depend for their validity on precisely what kind of causal theory is up for rejection. In particular, I argue that Thomasson fails to distinguish between crucially different kinds of causal theory, and furthermore fails to distinguish *metaphysical* from *epistemic* indeterminacy in reference.

### 1.1. The Methodological Constraint in the Ontology of Art

The methodological debate in the ontology of artworks can helpfully be understood as tackling the following question: to what extent, if at all, can an ontological proposal concerning the nature of some entity *contradict* pre-theoretical beliefs about those entities? (Or, to what extent can folk theories *constrain* an ontological proposal?) The question suggests a scale with the following extremes. On the one hand, what Amie Thomasson has (critically) called the 'discovery model' claims that while the folk theory may point an investigation in the right direction, ultimately any or even all of that folk theory may be discovered to be false under the bright lights of philosophical enquiry. An ontological account offered for some kind of entity may therefore be highly *revisionary*, in the sense that it may claim that any or even all previous assumptions or beliefs should be revised (Thomasson, 2005, pp.222-224). The metaphysician in this case would be constrained only by the more familiar theoretical virtues, such as simplicity, explanatory power, and parsimony, and perhaps what Andrew Kania has called *metaphysical respectability* (Kania, 2008, p.436).

At the other extreme lies a position that has been called *descriptivism*. According to descriptivism the role of the ontologist, in this field at least, is merely to describe, in as much detail as possible, our existing conceptions of some entity.<sup>2</sup> On this view, surprising or revisionary answers will always be suspect and suggest either a failure to understand existing concepts, or a misunderstanding of the role of the ontologist.

Intuitively, there is something wrong with both of these extremes. Of the two, the former position is perhaps more familiar to modern metaphysics, where we have grown accustomed to surprising and revisionary claims regarding the fundamental nature of

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<sup>2</sup>Descriptivism is discussed in Kania (2008, pp.434-438).

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the world. However, if a line of enquiry is aimed at a certain kind of entity of which we already have some grasp (as, for example, in the ontology of art) and the philosopher is willing, along the way, to reject *all* our initial beliefs, then we might reasonably ask why we should believe that the resultant theory is still a theory of *those* original entities. It seems the philosopher would be in the same position as the biographer who sets out to write about a well known London-based artist from the 1950's, and turns out a book which describes a Spanish monk from the 15th Century. We would conclude that somewhere along the way they had changed the subject of enquiry. This does not constitute an argument, but is an indication of what strikes us as wrong about this unfettered revisionism. I doubt that any ontologist has or would locate themselves entirely at this end of the scale. Note that even if a theory is guided primarily by explanatory power, there is normally some pre-established phenomenon or data that the theory is supposed to be explaining.

The latter extreme faces even more obvious difficulties. If we set out to describe a particular kind of entity, merely describing our existing conceptions seems to miss the point. Surely we are interested in the entities themselves, not just what we happen to think about them? Our conceptions are sometimes inconsistent and often incomplete and we normally think that we can be mistaken about at least *some* of what we believe about the entities around us.<sup>3</sup> Nevertheless, descriptivism of this sort has received some attention in the literature. An indication of why one might want to adopt this view is given by Andrew Kania. With a focus on the ontology of music, Kania suggests that

[i]f we truly embrace descriptivism, we embrace the idea that when we do musical ontology, what we describe is our conceptions of musical works, rather than the things themselves. There is a certain irony in the idea that if we take descriptivism, and thus our conceptions of musical works seriously, we should conclude that those works have no existence *beyond* those conceptions of them. But this irony is counterbalanced by the security our conceptions of musical works gain against revisionist attacks. (Kania, 2008, p.441)

There are a few things we can note about this. Firstly, there is a puzzling ambiguity in Kania's account. On the one hand he suggests embracing descriptivism involves describing our conceptions *rather than the things themselves*, while on the other hand he speaks of works of music as having no existence *beyond* those conceptions. Perhaps Kania is best interpreted as equating descriptivism with a broad and controversial metaphysical position that, at least as far as works of music go, appears to be a form of anti-realism. As I have said in the introduction, arguing against such a position is outside the scope of this thesis (except insofar as the plausibility of the positive proposal set out here counts as a reason not to adopt an anti-realist stance). It is enough to note that the view being associated with descriptivism is distinctly at odds with the metaphysical project that we thought we were engaging in.

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<sup>3</sup>See, e.g. Devitt and Sterelny (1987, p.235).

## 1. *The Search for Methodological Guidance*

Further, adopting such a position would require a significant amount of metaphysical and ontological argument. We should not *slide into* anti-realism (and thereby justify descriptivism) out of either a desire to avoid revisionary theories or because of the general sentiment that our folk conceptions play *some* important role. Kania suggests that an advantage of descriptivism is that our conceptions are secure from ‘revisionist attacks’. But this gets the direction of argument backwards. The question of whether or not we should avoid revisionary theories (and what the subsequent ontological account should be) should depend on a worked out methodology, not vice versa.

### **Walton on Theory Construction**

Both extremes of the scale described above are unattractive. There appears to be something wrong with a rejection of all our pre-theoretical beliefs, but equally, merely describing our conceptions seems to either miss the point or else rely on a substantial theoretical claim that itself amounts to a metaphysical stance that is significantly controversial. If some middle ground is needed, the challenge becomes one of clarification and justification. Where between the two extremes should we locate ourselves and why? Kendall Walton’s recent discussion of methodology in aesthetics promises to provide some guidance on the relationship between folk theory and ontological theory (Walton, 2007). It should be noted that Walton is interested in very general methodological questions, and is not uniquely focussed on ontological issues. However, it seems admissible to interpret his discussion as at least encompassing the latter. Walton states a preference for viewing the philosopher’s work as the work of *theory construction*:

What philosophers do, on this conception, is pretty much what scientists do after the data are in: organizing the data in a perspicuous manner, devising conceptual structures, constructing theories, to clarify and explain the data.  
(Walton, 2007, p.151)

Walton’s take on conceptual analysis (and more generally the assessment of intuitions) is that it is to be used as a tool to reveal *one such competing theory* – the folk theory. We should be interested in pre-philosophical intuitions and beliefs just as far as they reveal the implicit folk theory. Moreover, we should respect the folk theory to a degree, Walton suggests, as it must have some merit to have evolved and become established as it has. However, it is far from sacrosanct and the job of the philosopher is to scrutinise and test this theory against competitors. If it is found wanting, rejection and revision are called for (Walton, 2007, p.155).

Walton also identifies a distinct activity that the philosopher may be engaged in, which is to theorise *about* the folk theory. Here the folk theory itself is the subject of the philosopher’s enquiry (Walton, 2007, p.154). In this case, conceptual analysis, as Walton understands it, takes a central role; this is “conceptual analysis more or less for its own sake” (Walton, 2007, p.155). Because the folk theory itself is the subject of analysis, revision here is not acceptable as it would amount to a distortion of that which is being analysed.

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The distinction Walton makes between these two kinds of viable activity allows him to rule out two further kinds of methodological approach. Firstly, he objects to an approach which aims to take the folk theory and clean it up “around the edges.” The explanation offered is quite simple: either we are interested in assessing the folk theory in and of itself, in which case any adjustments amount to “falsifying of data”; or our aim is to “understand what the folk theory aims to understand” in which case we should be prepared to replace the folk theory with a better theory if necessary. Taking the folk theory and tinkering with it is to take an unhappy middle ground between these two reasonable activities (Walton, 2007, p.155).

Secondly, Walton’s distinction allows him to rule out a methodological approach that generates theories that are so radically revisionary that they don’t even succeed in usefully explaining anything the folk might have originally been interested in. Nelson Goodman’s notorious theory of musical notation and performance is guilty of this, Walton suggests (Walton, 2007, p.155).<sup>4</sup> Walton argues that Goodman’s account of what it is to perform a musical work radically overthrows our normal understanding of performance, but fails, in its place, to offer anything that helps to explain or illuminate listeners’ experiences. According to Walton, Goodman is guilty of rejecting too much of the data and in doing so only succeeds in changing the subject.

On the face of it, Walton’s advice seems quite clear then: the philosopher must know what kind of project she is engaged in. If she aims to understand the folk’s theory and concepts, she should not offer suggestions to amend or revise aspects of that theory. Such a project would justify the kind of descriptivism discussed above, but would not normally be understood as ordinary ontological or metaphysical enquiry. This proposal is not in *conflict* with an alternative and more revisionary methodology because it is a different kind of undertaking altogether. On the other hand, if the aim is to provide the best explanation of the data that the folk seem to be interested in, the philosopher should be willing to look for and adopt the best available theory, however revisionary it may be. This revisionary project is constrained merely by a desire to ensure that one is still explaining the relevant data. Unfettered revision is therefore avoided, and we have some (quite minimal) constraint to allow us to establish a middle ground.

However, despite the fact that Walton’s position appears sensible, it can be seen that things are not so simple in practice. The most pressing worry is that, as Walton himself realises, distinguishing data from theory is no easy task (Walton, 2007, p.152). What counts as data to be explained for one philosopher may be merely a product of folk theory for another. As such, the point at which a revisionary theory ceases to address the relevant data may differ from one theorist to the next. Claims about the entanglement of theory and data, at least to some extent, are familiar and widely accepted in the philosophy of science, and it seems similar insights apply here.<sup>5</sup> This is most easily made clear by considering actual examples in the literature. Jerrold

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<sup>4</sup>For Goodman’s theory see Goodman (1968).

<sup>5</sup>Seminal works in the philosophy of science discussing this issue include Hanson (1958) and Kuhn (1996).

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Levinson, in a well known paper (Levinson, 1980), has insisted that one of the pieces of data that an ontology of musical works should explain is that musical works are *created*, where this is understood as being brought into existence. He puts this in terms of a *creatability requirement*:

(Cre) Musical works must be such that they do not exist prior to the composer's compositional activity, but are brought into existence by that activity. (Levinson, 1980, p.9)

Levinson does not insist on (Cre) without argument, but the arguments are all based on our ordinary pre-philosophical beliefs and intuitions. However, why might we not say that this belief, however strong it may be, and however integral it may be to our normal appreciation of musical works, is merely a product of our implicit folk *theory*, and therefore subject to rejection if a better theory is found? In other words, why think that what Levinson takes to be something that requires an explanation and that cannot be rejected by any philosophical theory is not in fact an erroneous aspect of our folk theory? Peter Kivy and Julian Dodd, who both defend accounts of musical works as eternally existing (and thus uncreated) entities, make responses of this sort to Levinson.<sup>6</sup> Dodd, for example, argues that the best theory of musical works shows that despite what we thought they are not literally created. Moreover, we can explain away the intuition about creatability in terms of creative *discovery* (Dodd, 2007, pp.112-121). The details of these arguments are not relevant here (there will be space to engage with them in later chapters). What is important is that what one philosopher takes as data to be explained, another can take as misguided theory to be overturned.

We can carry this line of thought further, however. For in order to reject the creatability of musical works on the grounds that it is overruled by theory (and thus reject it as data to be explained), both Dodd and Kivy must have some additional data that they take to be addressed by their theories. David Davies suggests, rightly I think, that Dodd's work aims primarily to explain the *repeatability* and *audibility* of works of music (Davies, 2009, p.162). However, the same question asked of Levinson's creatability requirement could be asked again here. How are we to be sure that repeatability and audibility are not themselves mere products of a folk theory, to be rejected if a better theory is found? It might seem surprising to the uninitiated that even the repeatability and audibility of works of music could be rejected in light of new theory, but that is precisely the move that has been made by Davies. Davies has thoroughly defended a theory of all art works, including works of music, as being *compositional action-tokens*. The work itself is an unrepeatable 'generative performance' (Davies, 2004, p.152) which gives rise to the thing that we normally identify as the work, which itself is in fact just the 'focus of appreciation' (Davies, 2004, p.26) or what he also calls the 'work product' (Davies, 2004, p.97).

To reach this conclusion, it is clear that Davies must reject much of what Dodd and Kivy take as data to be explained, and in turn replace it with further information that

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<sup>6</sup>See e.g. Dodd (2000), Dodd (2007) and Kivy (1993).

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provides the focus of the enquiry. Davies is actually quite explicit about this, and his methodological suggestion will be discussed in more depth shortly. Again, though, what is directly relevant here is that Walton's suggestion that we construct theories that best explain the data appears to be subject to serious disagreement about what counts as the relevant data. If we are to know how to move forward, it seems a significant amount of further methodological guidance is needed.

One way to make the most of Walton's advice might be to ask that philosophers are completely explicit about the data they take themselves to be explaining. With the data made explicit we might then find that theories that appeared to be at odds were really just theories that addressed different phenomena. However, things are not so straightforward when what are being proposed are ontological accounts of familiar entities. Though the above theories lean on different data as basic and in need of explanation, they all *purport* to offer ontological accounts of the same (or overlapping) subject matter. Thus Walton's proposal is complicated further, for the ontologist is engaged in a two-step process. The subject matter is first specified, and then decisions are made as to what is most central, and thus what counts as 'data', for an ontological account of such entities. The fact that all of the above theories are intended as theories that account for works of music means that the theories cannot simply be reconciled as being explanations of different data. Of course, no theorist wishes to 'change the subject' (even Goodman, presumably, took himself to be explaining something that he saw as of central importance to musical notation), and all take themselves to be explaining what they suppose is most central, or most important to our ordinary concept. The problem arises because there is significant disagreement about what is in fact most central or most important. There is disagreement about what is data and what is folk theory.

### **Davies on the Pragmatic Constraint**

I have already made reference to Davies' theory of artworks as compositional action-tokens and suggested that in arriving at such a theory, Davies is making assumptions about what is to be taken as 'data' that differ from other theorists such as Dodd. Davies is quite explicit about this, and I will now turn to considering Davies' methodological claims in more detail. Significantly, Davies provides reasons for thinking that his understanding of the relevant data is correct. Thus if Walton's proposal still leaves us with the question of what counts as data and what counts as folk theory, Davies' methodology can be seen as offering an answer. However, I will argue that Davies' constraint is at best irrelevant to the ontological project of this thesis, and at worst ill suited even for Davies' own purposes.

Initially, Davies' claims to be guided primarily by what he calls the Pragmatic Constraint (PC):

Artworks must be entities that can bear the sorts of properties rightly ascribed to what are termed 'works' in our reflective critical and appreciative practice; that are individuated in the way such 'works' are or would be indi-

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viduated, and that have the modal properties that are reasonably ascribed to ‘works’ in that practice. (Davies, 2004, p.18)

Davies takes this to be at least the beginnings of a constraint on what we should take as the relevant data in need of explanation by an ontological proposal. He later writes that “[t]o offer an “ontology of art” not subject to the pragmatic constraint would be to change the subject, rather than answer the questions that motivate philosophical aesthetics” (Davies, 2004, p.21). At first blush, there are two things that we can note about this proposal. The first is that appealing to what is “rightly ascribed” and “reasonably ascribed” to works of art leaves plenty of room still for disagreement and manoeuvre. How are we to know what properties it is right or reasonable to ascribe, and what considerations should weigh in on this? Moreover, might it not be that what counts as a rightly ascribed property depends itself on the correct ontology? In other words, the difficulty of distinguishing theory from data encountered above is still very much present. Secondly, we might wonder what the questions are that motivate philosophical aesthetics, and why the relevant question is not merely of the form ‘what is the ontological nature of . . . .?’

Looking for answers to these questions we can turn to a more recent paper by Davies (Davies, 2009). Here Davies argues more explicitly that it is our “practice as a whole” (2009, p.162) and our “practice taken holistically” (2009, p.163) that should constrain our ontology:

[I]t is our practice that has primacy and that must be foundational for our ontological endeavours, because it is practice that determines what kinds of properties, in general, artworks must have. (Davies, 2009, p.162)

So far, however, we are no closer to an answer to the puzzle raised by Walton’s view. Are we to take *all* of our ‘practice’ as data to be explained? What if the best theory of certain aspects of our practice overturns other aspects of our practice? Davies’ own theory, as he is well aware, is incompatible with some of this practice. He notes, for example, that “a commitment to the repeatability of musical works is implicit in our comparative assessment of different performances of a work” (Davies, 2009, p.163). Davies is quite willing to reject this practice in the light of his theory, and one might well wonder why *other* practice (that which leads him to conclude that works of music are non-repeatable action-tokens) takes precedence here. If the pragmatic constraint leaves us merely explaining some practice and rejecting other practice in light of theory, it seems we are no better off in choosing between data and folk theory than we were after Walton’s proposal. The pragmatic constraint as stated is too broad and open to interpretation to offer any detailed methodological guidance.

However, Davies’ choice is not entirely un-principled. Of all the beliefs and practices surrounding works of art, Davies stresses in particular our “critical and appreciative engagement with works” (Davies, 2009, p.163). Davies therefore focuses on our practices of aesthetic appreciation and criticism at the expense of our normal beliefs about



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identifying and picking out works of art. Andrew Kania has noticed this, remarking that Davies leaves out of his constraint “any reference to our pre-theoretical views about the ontology of artworks” (Kania, 2008, p.431).<sup>7</sup> In other words, the data that Davies regards as being in need of explanation is our ordinary aesthetically-oriented practice of appreciating artworks as works of art. Why is this of particular importance? Because, Davies argues, “our philosophical interest in artworks arises out of, and is an attempt to better understand, that practice” (Davies, 2009, p.163).<sup>8</sup> As such, Davies argues, “no acceptable ontology can require that we revise the basic conception of artistic appreciation to be found in that practice, for it is only by reference to this conception that we can get any firm grip on the very subject of the ontology of art” (Davies, 2009, p.163).

In light of this, I suggest a more accurate interpretation of Davies’ approach would be not as a *pragmatic* constraint, but as an *aesthetic* constraint, which might be something like the following:

**(AC)** Artworks must be entities that can bear all or many of the properties we reasonably attribute to ‘works’ in the process of aesthetic criticism and appreciation, because it is this aesthetic practice that motivates our interest as philosophers.

If this is a sensible clarification of Davies’ methodology (though note that there is still a lack of clarity in the notion of ‘reasonable attribution’), then we are in a position to more accurately assess the merits of Davies’ approach. Davies assumes that the data to be explained by Walton’s philosophical theory construction is, in the ontology art, the specifically aesthetic data. After all, Davies might say, isn’t this what made us interested the ontology of art in the first place? Davies seems to be saying that it is this aesthetic practice that, speaking loosely, *defines the subject matter*.

One response available here is to emphasise the fact that the ontological enquiry undertaken in *this* thesis is not principally motivated by aesthetic concerns. The ontological enquiry undertaken in this thesis is motivated by the puzzle of artefact *repeatability*. Consequently, even if Davies can show that when artists and critics make aesthetic claims they are actually, all things considered, talking about the compositional action tokens of artists, there will still be an ontological puzzle about repeatability left to be solved, because Davies will not have shown that there are no repeatable entities.

Digressing to non-repeatables for a moment, note that in arguing that all artworks are action tokens Davies is not making the extraordinary claim that the physical object that we normally call a painting does not exist. Davies would merely regard such an item as the ‘focus of appreciation’ rather than the work itself. But whatever it is that is made of canvas and paint and hangs in the gallery, even if it turns out it should not be called the ‘artwork’, still calls for a place in our ontology, and presumably would fall under the ontology of created ordinary physical objects along with tables, houses and jam jars. In a parallel manner, the puzzling ontology of repeatable artefacts still calls for an account whatever one thinks of Davies’ location of the artwork in artists’

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<sup>7</sup>See also Stecker (2009, p.337) for a similar point.

<sup>8</sup>See also Davies (2004, p.21).

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generative actions.

The upshot of arguing in this way is that even if Davies' methodological constraint is acceptable for *his* project, it offers no help to the ontological investigation carried out here. However, I think even this concedes too much to Davies. The reason is that while Davies is perfectly free to stipulate that his aim is to explain our aesthetic practice, or even to locate the object of proper artistic appreciation, it is far less clear that in doing so he can claim to be describing the true nature of those entities that we *normally* take to be works of art – those pieces of music, novels, and photographs with which we are so familiar. One reason for thinking this is that the fact that artists and critics have aesthetic views about certain objects seems, at least sometimes, to be accidental to those objects. Suppose a stone tablet is unearthed in an archaeological dig. The artefact may be the subject of a great deal of discussion before art historians start considering it as art and attending to it aesthetically. It would be surprising if, as soon as this happened, the subject of the investigation was redefined by their aesthetic criticism and appreciation. More importantly, however, having a primary interest in aesthetic practice does not, it seems, provide a good enough argument for thinking that the entity referred to by the name 'Emma' or 'Beethoven's Fifth' or 'Les Demoiselles d'Avignon' is fixed by that aesthetic practice alone. There is simply too much *other* practice (practice that directly contradicts Davies own ontological conclusion) that can lay claim to being ontologically relevant.

To conclude this section, it seems that Davies' argument is ultimately unsuccessful in solving the puzzle raised by Walton's account of theory construction, and so unsuccessful in establishing a principled middle ground between unfettered revisionism and descriptivism. An interest in aesthetic practice is not enough to establish the claim that aesthetic practice alone must guide an ontology. Of course, the nail in the coffin for Davies' methodological constraint would be an argument that showed that it is precisely data *other* than the critical and appreciative aesthetic practice that must be accounted for by an ontological proposal. Just such an argument has been put forward by Amie Thomasson. In the rest of this chapter and the whole of the next, I will argue that Thomasson's argument succeeds where Davies' argument fails.

### 1.2. **Thomasson's Methodological Argument**

In a number of recent articles and chapters, Amie Thomasson has sought to provide guidance on the methodological issue by appealing to the mechanisms of reference fixing. Thomasson argues that if we think about how it is that a term for some kind of entity comes to refer to that entity, we will see that there are certain aspects of the conceptions of ordinary language users about which they cannot be shown to be in error. According to this view, what we must account for as philosophers is not necessarily that which is most central or most important to the folk concept, but rather the aspects of that concept that *determine the reference* of the terms the folk use. The argument, taken

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principally from her (2005) and (2006), can be summarised as follows.<sup>9</sup>

Thomasson begins by suggesting that, either implicitly or explicitly, many ontologists have adopted the *discovery view* of ontological investigation (Thomasson, 2005, p.221). Accordingly, any proposition concerning a particular entity is available to be *discovered* to be either true or false. Thomasson suggests that such a view is “bolstered by causal theories of reference” which allow us to become acquainted with entities “so that we may go on to investigate their true nature, which may turn out to be at odds with our concepts or initial presuppositions” (Thomasson, 2005, p.222). However, this view of how our terms come to refer as they do faces what has been called the “qua problem”. Appealing to the work of Devitt and Sterelny (1987), Thomasson argues that the qua problem shows that a term cannot come to refer determinately just in virtue of causal acquaintance because for any act in which a name is given to that object, there will always be numerous kinds of entity present. Which of these actually *does* become the object of reference, Thomasson argues, must therefore depend on the language user having a conception of what sort of thing they intend to pick out. Thomasson writes that “external context alone is inadequate to determine what our terms refer to” (Thomasson, 2006, p.258) and elsewhere that “without some disambiguating concept specifying the sort of kind to be picked out, we cannot unambiguously ground reference to any kind” (Thomasson, 2005, p.222). Thus instead of a pure causal theory, Thomasson promotes a “hybrid theory of reference” (Thomasson, 2007b, p.38). Reference is fixed partly by causal relationships and partly by ‘competent grounders’ having some knowledge of what sort of thing they intend to refer to. The methodological consequences that Thomasson draws from this are best described in the following passage:

As a result, at least a background concept of the ontology of the work of art is needed to establish the reference of terms like ‘painting’ or ‘symphony.’ Such concepts determine the ontological kind, if any, picked out by the term, and so the ontology of the work of art must be something we learn about through conceptual analysis of the associated concepts of people who competently ground (and reground) the reference of terms like ‘symphony’ and ‘painting,’ not something we can seek to discover through investigations into mind-independent reality. Moreover, competent grounders cannot (as a whole) be massively ignorant of or in error about the ontological nature of the art-kind they refer to since their concepts are determinative of this. (Thomasson, 2005, p.223)

The phrase ‘background concept of the ontology’ in the above quote is potentially misleading, and should not be taken to imply that ordinary language users have an explicit ontological theory in mind when they use a term to refer. Elsewhere Thomasson is clearer about what it means to have an associated concept. According to Thomasson, the associated concept that fixes the reference of a term is a ‘categorical concept’ that may be tacitly held (Thomasson, 2007b, p.43). Having a categorical concept that

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<sup>9</sup>Similar versions of this argument also appear in her (2004), (2007a) and (2007b) Chapter 2.

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disambiguates reference involves having a tacit understanding of *application conditions* for the term and *co-application conditions* for the term. The application conditions are conditions “conceptually relevant” to the proper application of the term in various scenarios – i.e. the conditions in which one might correctly identify the referent. The co-application conditions, on the other hand, “specify under what conditions the term would be applied again to one and the same entity” (Thomasson, 2007b, pp.39-40).

As I have indicated, I am broadly sympathetic to Thomasson’s argument here. However, the issues raised are complex and controversial and deserve close scrutiny. For the rest of this chapter my aim will be to draw out some of the crucial points of Thomasson’s account and expand the argument in key areas.<sup>10</sup> Before doing that, however, it is worth making explicit how Thomasson’s proposal promises to provide the needed constraint on the data to be explained by an ontological proposal. The problem we found with Walton’s appeal to theory construction was that we still need some further way of deciding what counts as data to be explained by a philosophical theory, and what counts as an aspect of the folk theory which may later be rejected. This problem is exacerbated by ontological projects that take themselves to be constructing theories of the ontological nature of some pre-established entity or kind of entity. Here we need to know what counts as the relevant data for an ontological theory that aims to account for *those* specific entities. Thomasson’s proposal seeks to answer this by showing that if we have some specific entity in mind about which we wish to provide an ontology, the relevant data is the conceptual content of language users that itself fixes the reference of those terms to *those* entities. More precisely, it is the tacit knowledge of the application and co-application conditions for the terms.

If this account is right then any proposal that overrides these application and co-application conditions will be guilty of literally changing the subject. Davies’ ontological theory of works of art provides a likely example. According to Davies’ theory, ‘Les Demoiselles d’Avignon’ can only be correctly applied to a compositional action in the past. If so, the conditions under which it is right to apply and re-apply the name change radically. For a start it will never be possible to walk into a gallery, point at a canvas covered in paint, and say ‘That is LES DEMOISELLES D’AVIGNON.’ Ordinarily, I assume, we take it that that is exactly the kind of scenario in which one might correctly apply the name. For it to be a correct use of the name other facts must also obtain, of course. Davies may try to respond by saying that the only relevant fact is just whether or not the ostended object *is* LES DEMOISELLES D’AVIGNON. However, if

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<sup>10</sup>Note that there is an aspect of Thomasson’s metaphysical approach that I am not adopting here. Thomasson argues in a number of places that we can draw ontological conclusions only to the extent that they are settled by conceptual analysis, and that questions that are not settled by this process are unanswerable and are ‘ontologically shallow’ (Thomasson, 2005, p.227). I agree with Thomasson that some of the questions she mentions, such as “what percentage of the paint in a painting may be replaced in restoration while preserving the same painting”, are unanswerable. I do not think that ontological theorising can artificially remove such indeterminacy. However, on my view there is still room for ontological theory to provide some answers beyond conceptual analysis by drawing on broader theory and principles, and by looking to provide ontological explanation. My account of the persistence of repeatable artefacts in §4.7 is an example of this.

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Thomasson's argument is right then that rejoinder is unavailable, for the term 'Les Demoiselles d'Avignon' only has the reference it does have because of tacit knowledge of what criteria are relevant to applying the term. This criterion cannot itself appeal to which object is LES DEMOISELLES D'AVIGNON without the criteria of reference being circular. Thus, if this is right, Davies' theory cannot be an accurate theory of the ontological nature of LES DEMOISELLES D'AVIGNON. At best he can argue that the entities he describes are what art critics should be (or even, on occasion, are) interested in. He cannot say that the compositional action token is what we have been referring in all along when we use a name such as 'Les Demoiselles d'Avignon'. When critical and referential practices conflict, the methodological account defended here argues that referential practice wins out.

We are therefore steered away from the end of the scale that allows highly revisionary theories, where that revision involves large scale changes to the actual and hypothetical use of terms. However, the constraint has important limitations, allowing plenty of room still for ontological manoeuvre. For example, we can expect that there may often be multiple ontological theories compatible with the same careful analysis of application and co-application conditions. In this case, facing the metaphysical equivalent to the problem of under-determination in science, other factors such simplicity, explanatory power, coherence, and fit with other theories will be especially relevant. Furthermore, it is entirely possible that a theory compatible with the conceptual analysis defended here will yield additional results that are surprising or unexpected, if only because they had never been given much prior thought.

Thomasson's account promises a significant result, then. Rather than philosophers merely defending their theories on the grounds that those theories explain what they take to be most central or most important, or even on the grounds that they succeed in accounting for *more* of the folk beliefs than competing theories, what we have here is an independent argument for why one particular aspect of our folk conceptual content *must* be accounted for by ontological theory.

It is worth noting from the start that the way Thomasson sets up her methodological position runs the risk of targeting a straw man. Thomasson argues that many philosophers have implicitly adopted the *discovery* view of ontological investigation. As we saw above, however, very few are likely to accept entirely unfettered revisionism. Put in terms of Walton's theory construction, a quick survey of the ontological offerings suggests that most, if not all, philosophers take themselves to be constructing theories that account for or explain some existing phenomenon or data. The problem, as we saw, is that there seems to be little agreement about what this data should be. However, this should not detract from the important result of Thomasson's argument. What is important is not the general point that *some* pre-theoretical data must be accommodated by an ontological theory, but that there is an argument rooted in semantics and reference which clarifies *why* this is the case and at the same time tells *what* the relevant data is.

### 1.3. The Qua Problem and Causal Theories

With Thomasson's argument set out, and the case for its contribution to the methodological debate made, I now want to assess key parts of the argument in further detail and defend the general position against objections. That task begins by looking at causal theories of reference and the qua problem. Thomasson's argument moves very quickly from the potential problems of a casual theory of reference to a 'hybrid theory', which itself underpins her account of the conceptual content that fixes reference. However, the speed at which the argument moves obscures some of the subtleties of the issues raised, and the plausibility of the argument rests on drawing these out. In particular I will argue that more care needs to be taken to understand the relevance of the qua problem to causal theories. The cogency of the argument depends significantly on how we understand the notion of a causal theory being challenged. In what follows I will set out four different interpretations of the role of causal chains in reference and assess firstly the extent to which they provide any obstacle to the kind of conceptual analysis that Thomasson proposes, and secondly the extent to which the qua-problem constitutes an effective counter argument.

One of the key points to make in the following discussion involves the distinction between *epistemic* determination of reference and *metaphysical* determination of reference. Epistemic determination of reference concerns our ability to *know*, to some degree, what we are referring to. Metaphysical determination, on the other hand, is determination in the more usual sense of what *makes it the case that* we refer as we do. Thomasson seems to be concerned primarily with metaphysical determination and the interplay between causal theories and conceptual content. Her argument, much abridged, is that causal theories alone cannot (metaphysically) determine reference, so some reliable conceptual content is needed, or in other words, that without reliable conceptual content, reference is not determined. However, once we make the distinction between metaphysical and epistemic determination, we will see that what is needed for conceptual analysis is a commitment to conceptual content that *epistemically* determines reference. The qua problem is then only relevant and effective when understood in that context.

The reason that epistemic determination matters to conceptual analysis more than metaphysical determination can be put as follows. What conceptual analysis of this sort is committed to is the claim that language users have a tacit understanding of the application and co-application conditions for their terms. Language users must therefore be able to 'know' something about the referent of their terms.<sup>11</sup> They must then have conceptual content that epistemically determines the referent. Whether this conceptual content also metaphysically determines the referent, is a distinct, though related, question.

The interpretations of the causal theory divide into two groups. On the one hand a causal theory can be understood as claiming that reference is metaphysically determined

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<sup>11</sup>The kind of knowledge mentioned here should be understood broadly. It is not, of course, propositional knowledge about the referent.

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by real causal chains, and thus not determined by the conceptual content of language users. This category can then be further divided into two. Firstly (1) such causal chains can be taken to operate independently of any knowledge or ability on the part of language users, so that not only is conceptual content metaphysically irrelevant, but also epistemically irrelevant. Secondly, (2) the causal chains can be interpreted as metaphysically determining reference by being causally responsible for language users possessing certain discriminating knowledge of the referent.

On the other hand, the idea that reference is metaphysically determined by causal chains may be rejected in place of the view that reference is metaphysically determined at least in part by the conceptual content of language users. Here the causal chains are brought in elsewhere. They may be (3) thought of as simply *bringing it about* that a term refers as it does, though the causal chains are not what reference itself consists in, in the same way that an accident may bring it about that I have a bruise, though the accident is not what having the bruise consists in. Or they may be (4) included as *part* of the reference determining conceptual content of language users.

Account (1) is the account most similar to the causal theory that Thomasson sets herself up against. However, we will see that the qua problem as used by Thomasson does not constitute an effective argument against this kind of causal theory. If this theory is to be rejected as part of a defence of conceptual analysis, a further argument is needed. I will outline such an argument.

Accounts (2) and (3), on the other hand, do not pose a threat to conceptual analysis and so the relevance of the qua problem is immaterial. Finally, account (4) is both a problem for conceptual analysis and is a position against which the qua problem constitutes an effective argument.

### **(1) Pure Externalism**

The first account of a causal theory to be considered has it that many of our terms refer just in virtue of some mind-independent external causal links connecting the term with the object referred to. Here what it is for a term to refer is for certain causal chains to obtain, and reference occurs independently of any conceptual content, knowledge, or ability of competent language users. The following quote from Thomasson suggests that this is the kind of causal theory she has in mind:

[C]ausal theories of reference, at least in their pure form, have led many to hope that the meanings of terms can be understood as determined not at all by the concepts of competent speakers, but rather purely by a real causal relationship to things in the world...(Thomasson, 2007b, p.38)

A similar account is sometimes suggested in the literature. Describing a causal theory, Harrison suggests it implies a connection that “runs *directly* from expression to object” (Harrison, 1979, p.160). Assertions to this effect are often made in conjunction with a disavowal of a description theory of reference fixing (according to which a term refers

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to whatever fits some associated description), as if the failure of that theory implied that reference must therefore be fixed in this mind-independent way. For example, Hilary Kornblith claims that “it is now widely accepted that proper names and natural kind terms do not refer in virtue of associated definite descriptions; rather, these terms refer in virtue of some *real connection* between the user of the terms and an individual or kind of stuff” (Kornblith, 1980, p.109). Devitt and Sterelny similarly write that “the basic idea of causal theories of reference . . . is that a term refers to whatever is causally linked to it in a certain way, a way that does not require the speaker to have identifying knowledge of the referent” (Devitt and Sterelny, 1987, p.55). Call this view ‘pure externalism’.

It is clear that pure externalism would not allow any kind of reliable a priori analysis of conceptual content. On this view all such conceptual content reflects merely our fallible *beliefs* about an entity. Exactly which entity these beliefs are about is settled independently. It seems plausible, therefore, in conjunction with the quote above, that Thomasson takes herself to be arguing against just such a theory.

Thomasson’s account responds to such a theory by appealing to the *qua problem*. According to the kind of causal theory suggested, what makes it the case that ‘Moses’ refers to Moses is that there is a causal-historical chain running back from our use of the term to the first uses of the term when the name was ‘grounded’ on that actual person referred to. The qua problem is then supposed to challenge this view by pointing out that whenever a new term is given to an entity in a process of ‘grounding’ or ‘baptism’ there will always be numerous different kinds of entity present. Hence every instance of causal contact with Moses is both too broad and too narrow to independently fix reference to Moses. It is too *broad* because every case of contact with Moses will also be a case of contact with many other things, including parts of Moses, events, states of affairs and fusions of matter. Why is the name not ‘grounded’ in any of these things instead? Causal contact is also too *narrow*, because, for example, any encounter with Moses is a direct encounter with only a time slice of Moses. How does the name refer to Moses “qua whole object” rather than just to a time slice of Moses?<sup>12</sup> The qua problem thus calls into question the causal theory by claiming that the causal theory leaves reference radically indeterminate.

Thomasson’s response to this, following Devitt and Sterelny, is to conclude that when a term is ‘grounded’ to an object, the speaker must have “some very basic concept of what sort of thing (broadly speaking) they intend to refer to” (Thomasson, 2007b, p.64). Thus ‘Moses’ is grounded on Moses-the-whole-person rather than on a time slice of Moses or on Moses’ torso because the grounders of the term had a basic concept of what sort of thing they intended to name (as displayed in their understanding of application and co-application conditions of ‘Moses’). Reference determination is therefore a hybrid with both an external causal element and a conceptual element.

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<sup>12</sup>See Devitt and Sterelny (1987, p.63).



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I agree with Thomasson that this kind of causal theory should be rejected. I will discuss the reasons for this shortly. What I want to argue first, however, is that the qua problem as appealed to by Thomasson does not constitute an argument against pure externalism. To see why, recall that the qua problem states that when a person gives a name to an entity, there will always be numerous kinds of entity present, and so the argument is made that the causal theory alone leaves reference indeterminate. On the pure externalist view just set out, however, the fact that there are many kinds of entity present at any act of baptism or grounding need not be a cause for concern. According to this theory, the term refers just in virtue of external causal links. The presence of many potential causal sources does not jeopardise there being one actual causal source any more than there being many balloons in a room causes a problem for the claim that the piece of string I am holding is tied to just one of them.

The pure externalist can respond to the qua problem then by saying that it doesn't matter that there are a great number of potential causal links available, since as a matter of fact, the term refers as it does just in virtue of the one that is actual causal link. Nor does it matter that the grounder of the term failed to specify which was to be the relevant causal link when the term was grounded, since reference, on this view, is something that obtains independently of the language users' knowledge or ability. The argument from the qua problem goes wrong, it could be conjectured, in that it mistakes epistemic uncertainty with metaphysical uncertainty. According to pure externalism, reference is metaphysically determined by causal chains, and all the qua problem highlights is an epistemic indeterminacy. When asked how it is that 'Moses' refers to Moses and not to a time slice of Moses, the pure externalist can shrug their shoulders and say that if 'Moses' refers to Moses as a whole person, it is in virtue of (i.e. it is metaphysically determined by) external links between the name and 'Moses' and the whole person - end of story.<sup>13</sup>

Thus we have a causal account that Thomasson clearly needs to reject to defend her claims, and which she seems to be targeting, but against which the qua problem, as it has been put by Thomasson and prior to that by Devitt and Sterelny, is not a sufficient argument. If this is the kind of causal theory Thomasson has in mind, then her argument from the qua problem is unsound.

If we are to arrive at Thomasson's conclusions, we need an alternative argument against pure externalism. Fortunately, however, there are other good reasons for rejecting this pure externalist theory. I will argue that the pure externalist theory should be rejected because by entirely separating conceptual content from reference, it makes reference radically epistemically inscrutable. Though it claims metaphysical determinacy, by suggesting radical epistemic inscrutability it makes reference impossible. To see this, note that if what a word refers to is a fact that obtains independently of any knowledge or ability of language users, we would be in a situation in which we would never know or be able to find out what our terms refer to. The problem is not just that

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<sup>13</sup>Of course, it cannot actually be the end of the story, as I argue below.

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we could not know a priori, but that we could not come to know anything at all. This is because to gain any a posteriori knowledge about a subject that we take ourselves to be referring to, we need some reliable place to start. Without any kind of initial knowledge or ability, broadly speaking, with regard to a referring term, any a posteriori search for knowledge about the subject would be unguided, and would be as likely to lead us to false information as to true information.

It might be responded that we could uncover the referent of a term by investigating the relevant causal-historical chains. Couldn't we just 'follow the string back to the relevant balloon', to pick up on a previous analogy? In some cases this might seem plausible. After all, don't we sometimes find out who a name refers to by looking into the history of the name? However, if we take the claims of pure externalism seriously, even this would be impossible, the trouble being that we would never know what counted as the *correct* causal history, and so never know that we had followed the chain back to the right entity. We cannot appeal to the knowledge or intentions of previous language users to guide the investigation, because they may be wholly mistaken, according to this view. Note that even if we were present at the initial grounding of the term onto the entity, we would still not know what the term had been grounded on because we would still not know which causal chain was relevant.

As such, on this view, a whole language community may be universally and systematically mistaken about what a term in their language refers to and how and when the term should be used. For all we could know according to this view, 'red' may refer to the colour blue, and 'blue' to the colour red. Thus in practice, all our referring terms would be useless as we would never know which proposition a sentence containing them expressed. That the pure externalist theory looks, in this light, so highly implausible is not a consequence of how I have described it (except insofar as I have drawn out the commitments), but a consequence of the fact that it so seriously mis-characterises how it is that terms in our language relate to objects in the world.

I said that qua problem as described is unsuccessful here, but it worth noting that a close cousin of the qua-problem is relevant. This is because we may run a version of the qua problem as an *epistemic* problem rather than a problem about what *metaphysically* determines reference. Even here, though, the crucial step in the argument is not the epistemic qua problem, but the claim that complete epistemic indeterminacy is deeply problematic for a theory of reference. I think this epistemic problem is quite distinct from the problem of indeterminacy that Thomasson actually appeals to. The pure externalist theory can and should be rejected, but if Thomasson had such a theory in mind, she chose an argument unfit for the purpose.

### **(2) Causal Chains and Reliable Content**

The above view had it that reference is metaphysically determined by external causal chains, but is radically epistemically indeterminate. An alternative view may be adopted, however, according to which reference is similarly metaphysically determined by exter-

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nal causal chains, but which holds that what lies at the ‘effect’ end of such causal chains is not just that the term refers, but that we have certain discriminating knowledge or ability with respect to the referent.

A theory of this sort is suggested by Richard Miller (1992) as part of an attempt to give a purely causal solution to the *qua* problem. His claim is that “the sample upon which the term is grounded causes the reliable ability to discriminate the kind in virtue of its membership in that kind itself.” He formulates this more precisely as:

The speaker S can use his perceptual contact with x to ground ‘N’ on the kind Q if x *qua* Q causes S to acquire the reliable ability to discriminate Qs.  
(Miller, 1992, p.429)

Miller takes it that entities themselves, *qua* entities of one sort or another, have unique causal powers that can determine reference. As such we can walk away from a grounding situation referring to kangaroos rather than marsupials, to use one of Miller’s examples, because the kangaroo *qua* kangaroo caused the reliable ability to discriminate kangaroos. Similarly, we refer to Moses rather than a time slice of Moses because the causal powers of Moses *qua* whole person caused our ability to reliably discriminate Moses. Miller indicates that he believes that reference to Q consists not in the reliable ability but in the causal chains themselves. If so, we have a view according to which reference is determined by causal chains, but where causal reference always goes hand in hand with language users having some ‘discriminating ability’. What is it to have a reliable discriminating ability? It can’t be that one is never or even nearly never mistaken in using a term. This simply doesn’t stand up to real world examples. A farmer can name a lamb with unusual markings and use those markings to identify the lamb on future occasions, even if unbeknownst to him a lamb with nearly identical markings from a nearby farm has wandered into his fields. He will misidentify his own lamb as much as half the time yet refer to it nevertheless. More plausibly the relevant kind of discriminating knowledge might be taken to be something very much like Thomason’s understanding of application and co-application conditions – roughly speaking, an understanding of what counts, or would count, as using the term correctly.

Miller heads towards this view later in the paper. He considers the now familiar ‘disjunction problem’ which is also a problem for his view.<sup>14</sup> The disjunction problem arises because it seems that what is gained is not a reliable ability to discriminate kangaroos, but a reliable ability to discriminate the disjunctive class of kangaroos and things that look like kangaroos. How then does ‘kangaroo’ not refer to this larger class? Miller suggests that here we pick out the right causal chain *counterfactually* (Miller, 1992, p.433). In other words, we appeal to what we *would* say given certain situations. Moreover, our recognitional capacity “includes the disposition to explore the object thoroughly and use our full perceptual resources to check and cross check our initial impressions” (Miller, 1992, p.434). Thus the kind of conceptual ability associated with

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<sup>14</sup>The disjunction problem has been raised for early accounts of this sort, such as Dretske (1981), and Fodor (1990). Miller’s account is clearly heavily indebted to these views.

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referring is, on Miller's account, quite substantive. In any case, this view is committed to *some* reliable ability or knowledge relating language users to the entities they refer to.

What is important is that this view does not attempt to open up a gap between the reference fixing mechanisms and the knowledge or ability of competent language users, at least broadly understood. One could hold a theory of reference of this sort and yet *still agree* that an ontology must be guided by an analysis of conceptual content on the grounds that that content is a causal product of referring to that entity – it is an *epistemically reliable guide* to what is referred to, even if the content itself doesn't metaphysically determine reference. The position Thomasson is interested in defending holds that conceptual content determines reference, and an analysis of that content must therefore guide an ontology if the account is to avoid changing the subject. But clearly epistemic determination is sufficient for this argument to work. This is a point that Thomasson has either failed to pick up on, or has failed to make clear. Miller has therefore offered a causal solution to the qua problem that still has a central place for conceptual content. In fact, I have significant doubts about the plausibility of Miller's account of what it is to refer, but those doubts need not be addressed here because the accuracy or inaccuracy of Miller's suggestion has no bearing on the role of conceptual analysis.<sup>15</sup> At the most, if Miller is right he can object to Thomasson's claim that conceptual content metaphysically determines reference. What he *cannot* object to is the claim that it epistemically determines reference. As far as conceptual analysis is concerned, metaphysical reference determination is beside the point; what matters is whether or not a given theory of reference accommodates *epistemic* determination (and as we saw with the first theory, any theory that rejects epistemic determination is unworkable).

### (3) Causal Chains with a Metasemantic Role

The two interpretations of the causal theory suggested above operated on the assumption that reference is metaphysically determined by causal chains. It is possible to reject this assumption and still hold a place for causal chains. The following two accounts hold that reference is at least partly metaphysically determined by the conceptual content of language users.

According to the first of this second pair, causal chains are taken out of a theory of reference entirely. One way to put this is that the causal chains here *bring it about* that I refer as I do rather than *make it the case* that I refer as I do. They are causally responsible for the fact that the term refers, but do not themselves constitute that fact (as the fall may be causally responsible for the broken leg, though the occurrence of the fall and its causal role are not what having a broken leg consists in.) When I use 'Moses' in a sentence I refer to Moses because of a series of causal chains going back, ultimately,

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<sup>15</sup>My doubts lie in the viability of appealing to causation as something metaphysically capable of determining reference in this mind independent way. See e.g. Anderson (1993).

to Moses, but my reference to Moses cannot be reduced to those causal chains. On this view the causal chains do not explain what my referring amounts to, they merely give a causal explanation for *why* I now do actually refer in that way.

Such a view is suggested by Joseph Almog (1984). Almog argues that causal historical links between uses of a word serve merely to pass on the semantic value of that word. If the word is a referring term, then what is passed on is that the word refers as it does. The causal chains on this view have nothing to do with what it is for the word to refer as it does, i.e. they are not part of a theory of reference at all, and so have a metasemantic rather than a semantic role (Almog, 1984, p.486). I think this interpretation of the causal theory is, at least in some cases, relatively uncontroversial. In every case we might ask how a term came to refer as it does (note: not how it *does* refer as it does), and in some of those cases it seems plausible to give a causal account.

Crucially, though, this understanding of a causal theory need not deny that terms refer as they do in virtue of associated conceptual content. It merely gives a story about where that reference determining content came from. As such, this interpretation of the causal theory can be passed over quite quickly. Its merits are not relevant to a defence of Thomasson's conclusion.

#### **(4) Causal Descriptivism**

Some passages in the text notwithstanding, it is possible that Thomasson had a fourth causal theory in mind that again does not attempt to separate the mechanisms of reference from the abilities or conceptual content of language users. Such a theory, like the one immediately above, makes reference determination a matter of the conceptual content of language users. However, it holds that this knowledge and ability sometimes *just* involves appeals to causal chains. As such one need not have any *further* discriminating conceptual content for the term to refer determinately. For instance, on this view 'Moses' refers to Moses because competent users intend the term to refer to whatever lies at the end of a particular causal chain of use. Similarly 'water' refers to water because we associate with 'water' the intention to refer to the stuff causally present at the grounding of that term. Here (contra Thomasson's description of the causal theory) the meanings of our terms, and the reference of those terms, *is* determined by the concepts of competent speakers. Nevertheless, this causal theory would stand opposed to the use of conceptual analysis to guide an ontology because the reference determining concepts of speakers merely involve appeal to a certain causal-history and thus reference requires no a priori ontologically relevant conceptual content.

This causal theory avoids many of the pitfalls of pure externalism. Reference is not epistemically inscrutable, it seems, because we as language users specify the relevant causal chains, and our use of the terms is guided by our knowledge of the relevant causal chains. The metaphysical determinacy itself depends on epistemic determinacy. Frederick Kroon has defended a theory of this sort under the name of *casual descriptivism* which is the view that "what names refer to is determined by descriptions couched in

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causal terms” (Kroon, 1987, p.1). It is worth noting a terminological issue here. Kroon describes his theory as a kind of descriptivism, and it is often assumed that a trademark of description theories is that associated descriptions form part of the semantic content or meaning of terms. However, Kroon explicitly rejects this interpretation of his view: “The causal descriptivism argued for does not purport to give the meaning of names, only the way in which their reference is determined” (Kroon, 1987, p.10). Likewise, the kind of causal theory discussed here is not a theory about semantic content, though it is a theory about reference-determining conceptual content.

It is not clear whether Thomasson had this kind of causal theory in mind, but it seems that perhaps she should have had this in mind given firstly that this theory denies that we need have any categorical conception to refer determinately, and secondly that this theory provides an ideal target for the qua problem.

This theory is an ideal target for the qua problem because, unlike pure externalism, this theory is committed to the view that language users have discriminating conceptual content, but merely holds that this discriminating conceptual content is *not sufficient to restrict an ontology*. The qua problem can be brought in here, however, and used to point out that a general appeal to whatever entity lies at the end of the causal chain is not sufficiently discriminating, since there will always be many different sorts of entity present. If it is claimed that ‘Moses’ refers to whatever was present at the baptism of that name, the qua problem responds by pointing out that there were many kinds of thing present.<sup>16</sup> If that thin causal account is all that has been offered to fix reference, it will not be enough to determine that it is Moses *qua* whole person that is referred to, rather than a time-slice of Moses or a spatial part of Moses.<sup>17</sup> This understanding of how the qua problem can be yielded is strongly reminiscent of the problem of ostensive definition. The theory of ostensive definition carries with it the assumption that we define a name by having certain beliefs and intentions (contra pure externalism) but suggests that we can do so by pointing and saying ‘that will be called such and such.’<sup>18</sup> The problem, though, is that if this is all that is going on then we ought to be puzzled about how anything determinate is being named at all, for on any given occasion there will be numerous candidates for what is being indicated by the pointing. In the PHILOSOPHICAL INVESTIGATIONS Wittgenstein writes:

Now one can ostensively define a proper name, the name of a colour, the name of a material, a numeral, the name of a point of a compass and so

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<sup>16</sup>This echoes the objection originally offered by Thomasson against pure externalist theories. However, as I argued above, if the pure externalist is challenged on grounds of metaphysical indeterminacy, they can respond by insisting that the qua problem only highlights epistemic indeterminacy. They may hold that reference is metaphysically determined by external context despite our epistemic inadequacy. Thus the real argument against the pure externalist was that epistemic indeterminacy itself is implausible for any theory of reference. The account discussed here accepts that, but merely holds that our knowledge of the referent is insufficient to guide an ontology.

<sup>17</sup>Note that the causal descriptivist theories of Lewis (1984) and Jackson (1998b) are not likely to be phased by the qua problem as they already allow that causal elements form only part of reference determining descriptions.

<sup>18</sup>Harrison (1979, p.18).

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on. The definition of the number two, “That is called ‘two’” - pointing to two nuts – is perfectly exact. - But how can two be defined like that? The person one gives the definition to doesn’t know what one wants to call “two”; he will suppose that “two” is the name given to this group of nuts! ... That is to say: an ostensive definition can be variously interpreted in every case. (Wittgenstein, 1953, §28)

Wittgenstein’s puzzle here is about how one teaches a name with an ostensive definition but we can just as easily see it as a problem for how one can assign a name in the first place using just ostensive definition. The point is that we cannot think that all that is going on is an act of pointing and the saying of a name – some other discriminating knowledge is required.<sup>19</sup> Perhaps it is helpful, in seeing the connection between Wittgenstein’s puzzle and the problem of how a word comes to refer in the first place, to imagine the act of baptising something with a name as an act of teaching oneself a name. The problem now, however, is not how it is you know what has been named, but how it is that anything determinate has been named at all.

At this point, then, we can bring back in Thomasson’s suggestion that the reference is further discriminated and so made determinate (to the degree that it is determinate – Thomasson allows, sensibly, that degrees of both indeterminacy and vagueness may remain (Thomasson, 2007b, p.41)) by competent language users having a categorical concept which consists of a tacit understanding of application and co-application conditions.<sup>20</sup>

Thus with causal descriptivism we have located a position that denies that language users need have any ontologically relevant knowledge (tacit ability/ conceptual content), but which is susceptible to indeterminacy as raised by the qua problem. What I want to further point out is that the qua problem in this context applies not just to the determination of what *sort* of thing a term picks out, but sometimes to *which* thing within an ontological category is picked out as the referent. This is especially obvious for kind terms. What we see is that even if we assume that eligible candidates are narrowed down to just kinds (or even just natural kinds) which specific kind is actually referred to needs to be determined in some way that goes beyond an appeal to ‘the kind causally present’. This is because every sample is a sample of a great many different kinds.<sup>21</sup> Thomasson’s solution is still relevant here, however, for we can say that which kind is picked out is settled (to the degree that it is settled) by the language users’ tacit understanding of how the kind term should be applied and how it should be re-

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<sup>19</sup>Note that there is an important difference between what Wittgenstein seems to have in mind with the notion of teaching a name, and the possibility of a member of a language community using a name to refer successfully merely by uttering the name in the right context. This point, about the social character of semantics, will be discussed more fully in §2.2.

<sup>20</sup>Though I do not claim to interpret Wittgenstein, it is interesting to note that his hints at solving the problem of ostensive definition similarly involve knowledge and use: “We may say: only someone who already knows how to do something with it can significantly ask a name” (Wittgenstein, 1953, §31). It does not take a great leap to read ‘knows how to do something with it’ as an early version of ‘has a tacit understanding of application and co-application conditions.’

<sup>21</sup>See Devitt and Sterelny (1987, p.73), and Devitt (1991, p.463).

applied to one and the same kind. Thus we refer to water-as-chemical-kind with ‘water’ (rather than to ‘potable liquid’, for example) in part because of an understanding about how the term should be applied in various situations.<sup>22</sup> The reason for stressing the relevance of the qua problem to the reference of kind terms is that I will argue in this thesis that repeatable artefacts are kinds. Conceptual analysis of reference-determining content then becomes highly relevant to which kind a given term refers to (Chapter 5).

The final point to make here is that when the qua problem is clarified as a problem specifically for causal descriptivism, one particular attempt to respond to the qua problem without admitting language users have relevant conceptual content can be dismissed. In a discussion of Thomasson’s methodological approach, Robert Stecker has suggested that the qua problem can be solved without appeal to the conceptual content of language users because what will be referred to in some cases will just be what is most *salient* (Stecker, 2009, p.318). However, simply suggesting that one entity will be more obvious to us as observers than other entities does not have any relevance to the question of conceptual content. Even if there is a most salient candidate, the question of how we come to refer to that candidate still remains. As an analogy, suppose a child is offered a choice of sweets. The child chooses one sweet. If we believe that she has determinately chosen one sweet out of the batch, we might ask what it is that makes it the case that she has chosen that sweet rather than another. Suppose also that she chose the sweet that was most salient to her. Here the salience of the chosen sweet cannot be offered as part of the answer to what it is to have chosen that sweet. The answer being sought will perhaps appeal to something like the actual and dispositional actions of the child and her mental states and beliefs. The fact that that sweet was most salient to her is an answer to the wrong question. Perhaps it is an answer to the question of why she always chooses that sweet, or why she chose that sweet on that occasion. It is possible then that Stecker has confused the question of what makes it the case a term refers as it does (i.e. what reference *consists* in), with the distinct question of what brought it about that the term refers as it does (i.e. what is causally responsible for reference).

## Conclusion

My ambitions in this section have not been to analyse and pronounce on every possible role of causal chains in theories of reference. I have even resisted arguing in favour of either of the two basic frameworks discussed (reference metaphysically determined by external causal chains, vs. reference metaphysically determined by conceptual content). What I have argued is that the effectiveness of Thomasson’s argumentative move depends significantly on what kind of causal theory is on the table. There are indications

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<sup>22</sup>Devitt and Sterely’s tentative suggestion is similar: “the grounder of a natural kind term associates, consciously or unconsciously, with that term, first some description that in effect classifies the term as a natural kind term; second, some descriptions that determine which nature of the sample is relevant to the reference of the term” (Devitt and Sterelny, 1987, p.74). Harrison offers a more Wittgensteinian suggestion: “the logical category of a term defined by a given act of dubbing depends on what we go on to do with a term” (Harrison, 1979, p.161).



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that Thomasson has something like pure externalism in mind. However, it is not the qua problem that needs to be employed here, but rather a more general argument against the plausibility of metaphysical determinacy despite epistemic inscrutability. Accounts (2) and (3), on the other hand, are compatible with the role for conceptual analysis being defended, and thus do not need to be rebuffed to formulate this methodological argument. Finally, account (4) – causal descriptivism – presents both an obstacle to Thomasson’s conceptual analysis, and a target for the qua problem. More generally, the crucial element for a defence of Thomasson’s conceptual analysis is *epistemic* scrutibility, rather than metaphysical determination. Had Thomasson been aware of this, or made it explicit, it would have been clear that the qua problem, as a problem of metaphysical determinacy, is only relevant as applied to a theory that *rests* metaphysical determination on epistemic determination, as with casual descriptivism.

What then should we make of Thomasson’s appeal to a ‘hybrid theory’? If the causal account on the table is causal descriptivism, then the new proposal can be clarified as requiring that reference to terms is fixed by conceptual content that partly consists of appeals to causal chains, and partly involves a tacit understanding of the application and co-application conditions. It is a hybrid theory in that the descriptions involve both a causal and a non-causal component. What if, instead, she had something more like pure externalism in mind? I argued against pure externalism on the grounds that it leaves reference epistemically indeterminate. As such, any acceptable modification must allow some reliable epistemic access to the referents of our terms. It need not, however, be a theory according to which reference is *completely* determined – epistemically or metaphysically – by mental content. Thus reference can be understood to be ‘hybrid’ to the extent that what we refer to is partly determined by conceptual content and partly determined by how the world actually is.

## 2. Conceptual Analysis Defended

I argued in the previous chapter that Amie Thomasson's methodological constraint promises to provide an answer to the question of which aspects of our folk beliefs and intuitions should guide an ontological proposal. To recap, the position I am defending has it that the reference of a term is determined (at least epistemically) by the tacitly held application and co-application conditions associated with the term. Thus an ontological proposal must conform to a careful analysis of that conceptual content if it is to avoid changing the subject.

Before going any further, a point of terminology is called for. I have said that the key issue is epistemic determination rather than metaphysical determination, and so the defence of conceptual analysis need not be committed to any particular theory concerning the metaphysical determination of reference. However, for the sake of simplicity, in what follows I will speak of just 'determination of reference' rather than 'epistemic determination of reference.' Readers who share doubts about the plausibility of separating metaphysical from epistemic determination of reference (such as those in support of some form of modern description theory) may harmlessly read 'determination' as conveying both the epistemic and metaphysical sense. Those more sympathetic to the idea that there is a metaphysical fact of the matter about reference that is independent of conceptual content should take claims about determination only in the epistemic sense.

In this chapter, then, I will defend the claim that reference is determined by conceptual content. I do that by addressing four actual or potential sources of difficulty. Some of these issues have been addressed in part by Thomasson, but Thomasson's responses are brief and fail to tackle the broader issues involved. What follows will, I hope, be a more holistic and robust defence of the kind of position Thomasson is appealing to. I start by considering the use of thought experiments that appeal to intuitions about the reference of our terms in possible scenarios (§2.1). These 'externalist thought experiments' – so called for their use in motivating various forms of externalism about semantic or mental content – are sometimes taken as evidence against the claim that conceptual content determines reference. I will argue that though the thought experiments deliver important semantic results, that conclusion is unwarranted. Most importantly, however, the thought experiments themselves rely on conceptual content epistemically determining reference, so any attempt to deny that conceptual content determines reference using these thought experiments is bound to fail. In §2.2 I consider the issues of social externalism and direct reference. As part of the broader aim of defending and clarifying the proposed thesis, I discuss how these widely recognised phenomena do not contradict that thesis.

In §2.3 and §2.4 I turn to more direct criticisms of conceptual analysis. In §2.3 I consider a set of objections offered by Stephen Laurence and Eric Margolis to the idea that we can have a priori access to the conceptual content that determines reference. Laurence and Margolis' objections are targeted at Frank Jackson's theory of A-intensions. However, the similarities between Jackson's A-intensions and Thomasson's application and co-application conditions are considerable and if Jackson's account fails here, then so does Thomasson's. I argue against Laurence and Margolis that their objections are based on misunderstandings of what is being proposed. Finally in §2.4 I address a challenge to this view based on concept change and reference stability. It is argued that history attests to the fact that reference remains stable despite radical shifts in associated concepts. As such past language users have sometimes been so mistaken that no aspect of their conceptual content could have determined the reference of their terms. I will argue against this, however, on the grounds that it relies on shaky intuitions about reference stability that should be rejected in favour of a more plausible understanding of reference.

### 2.1. **Intuitions About Possible Cases**

In the previous chapter I discussed Richard Miller's purely causal theory of reference. I suggested that while Miller seems to hold that reference is metaphysically determined by external causal chains, he is still committed to language users possessing some epistemically determining conceptual content which he puts in terms of counterfactual recognitional abilities. As such, the kind of causal theory he proposes, I argued, need not be seen as competing with the thesis being defended here. Nevertheless, Miller also mounts an argument against a 'hybrid' theory of reference which certainly could be construed as a challenge to the claim that conceptual content even epistemically determines reference (his own commitment to recognitional capacities not withstanding).<sup>1</sup> As such, it will be worth considering Miller's argument understood as an argument against epistemic determination, not least because Thomasson has herself responded to Miller's argument as a direct criticism of her own view.

Miller argues by appealing to familiar thought experiments about the reference of our terms in possible scenarios, where those possible scenarios are considered as being actual. Miller's understanding is that on a hybrid theory, the reference of 'kangaroo' is partly determined by its association with a categorical concept such as 'species'. However, Hilary Putnam's Martian robots example shows that 'kangaroo' would still refer even if we discovered that all kangaroos are not animals but are highly deceptive Martian robots. Thus Miller concludes that the reference of 'kangaroo' is not determined by the associated categorical concept 'species' (Miller, 1992, p.427).

Thomasson has responded to this objection, but her response is insufficient. It involves constructing an alternative thought experiment in which what ornithologists take

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<sup>1</sup>As before, the argument is in his (1992).

## 2. *Conceptual Analysis Defended*

to be a new species – dubbed ‘Key Sparrow’ – turns out to be a sophisticated animatronic hoax. Here, Thomasson argues, our intuitions suggest that we would say that it has been discovered (once the hoax is uncovered) that there are no Key Sparrows, not that Key Sparrows are small robots (Thomasson, 2007b, pp.49-50). Thomasson’s point is then that reference does not automatically survive any failure of associated conceptual content. Instead, when empirical discovery shows the failings of associated categorical concepts, we have a decision to make regarding how to go on using the term. If, as in the Martian robot case, we do decide to keep the term and adopt a new categorical concept, “that need not be taken as a sign that these were never part of the meaning of the term” (Thomasson, 2007b, p.50).

However, Thomasson concedes too much here. For despite Thomasson’s response, hasn’t Miller shown that in some cases at least we can refer despite erroneous categorical concepts? Thomasson’s Key Sparrow example merely shows that this need not always happen. But if, as Thomasson claims, the associated categorical concept determines reference, and if that categorical concept is ‘species’, wouldn’t the Martian robot example have to have resulted in failed reference? Thomasson says that an adjustment in associated categorical concepts – from ‘species’ to ‘robot’, for example – need not be taken as a sign that these were never part of the meaning of the term. But if it was part of the meaning of the term ‘Kangaroo’ (in the reference determining sense being discussed) that it referred to a species, then the empirical discovery of Martian robots would show that there were no (and had never been any) kangaroos (contra our intuitions).

Fortunately there is a more successful and more general response to Miller, a response that takes into account any argument based on these kinds of thought experiments. To begin with, I think that Miller is in fact right to say that the reference of ‘Kangaroo’ is not determined by the associated categorical concept ‘species’. This is because the conceptual content that determines reference need not involve developed concepts such as ‘species’ or ‘animal’. Instead, the defensible claim is just that language users are tacitly aware of the application and co-application conditions of their terms, and that those application and co-application conditions determine reference. To think that knowledge of such conditions amounts to the concept ‘species’ or something similar is to adopt an account that is, as Miller suggests, too much like the fated description theories of reference (see below). A thought experiment in which kangaroos turn out to be robots may show that KANGAROO is not a species, but it does not (and, as we will see shortly, cannot) show that the reference of that term is not fixed by associated tacit application and co-application conditions. It is not clear why Thomasson, who elsewhere stresses that the associated conceptual content is to be understood in terms of application and co-application conditions, allows Miller’s point that the relevant concept associated with ‘Kangaroo’ is ‘species’.

The point being made is not specific to kangaroos and the Martian robot example, and to develop the point, it will be helpful to consider these ‘externalist’ thought experiments more generally. To begin, we can note that these thought experiments have been used

## 2. Conceptual Analysis Defended

to counter a traditional description theory of reference, according to which reference is determined for proper names by an associated definite description. Suppose it is claimed that a name refers to an entity in virtue of that entity satisfying some unique description. Does ‘Gödel’ refer to whatever satisfies the description ‘discoverer of the incompleteness theorem’? Following Kripke (1980, p.83), we agree that this is not how the reference of ‘Gödel’ is determined, because if it happened to turn out that Schmidt actually discovered the incompleteness theorem, ‘Gödel’ would still refer to Gödel and not to Schmidt.<sup>2</sup> Indeed, many take Donnellan and Kripke, among others, to have shown that no description associated with a name will serve to determine the reference of that name. For with any given description (barring certain “question begging” suggestions (Donnellan, 1970, p.344)) it is plausible that we can construct a scenario in which the referent does not satisfy that description.<sup>3</sup>

Hilary Putnam’s discussions about the stereotypes associated with natural kind terms appear to give a similar result for natural kinds (Putnam, 1970). That is, though we may associate a stereotypical description with a kind term, the reference is not fixed by that description in virtue of the referent satisfying that description. It is claimed that we might imagine, for any associated description, that it is in fact not true of the objects referred to.<sup>4</sup> Hilary Kornblith has argued that the same conclusion can be extended beyond natural kinds to artefact kinds (Kornblith, 1980).

However, though these experiments deliver important semantic results, the results consist only in negative claims to the effect that some given description does not determine the referent of the term. This has led some to argue that all that these thought experiments show is that we have failed to hit on the right description.<sup>5</sup> The view offered here is not committed to the success of that descriptive project. What is important is that the negative conclusion about what it is for some terms to refer – that they don’t refer in virtue of the satisfaction of an associated definite description – does not warrant the claim that reference is not determined by any conceptual content at all.

It might be suggested, however, that the failure of the description theory of names is just a symptom of the more encompassing fact that the mental states of speakers do not determine the referent of a term, and this is something we can know by testing *other* intuitions about possible scenarios. Michael Devitt seems to adopt this line of thinking when he attributes the failure of the description theories to a broader issue characterised by Putnam’s slogan that ‘meanings just ain’t in the head’ (Devitt, 1996, p.160).

This leads us to a slightly different kind of thought experiment involving the reference of our terms in possible scenarios. Putnam’s Twin Earth scenarios, specifically those in which Twin Earth is a planet located somewhere in the actual universe, are the most well known examples here.<sup>6</sup> One of the intuitive results of the Twin Earth scenario is

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<sup>2</sup>See also Donnellan (1970).

<sup>3</sup>See, e.g., Carlson (2004).

<sup>4</sup>C.f. Laurence and Margolis (2003, p.261).

<sup>5</sup>E.g. Jackson (1998b).

<sup>6</sup>In Putnam (1973) & Putnam (1975).

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that when the internal state of speakers is held constant and the external environment is changed (as on Twin Earth), the reference of the corresponding term ‘water’ is different (i.e. in the mouths of Twin Earthians, ‘water’ refers to XYZ). The important semantic result here is that the extension of a term is not wholly determined by how things are in the heads of language users – duplicates from the skin in can nevertheless refer differently if their external environments are different.<sup>7</sup> A slight variation on this thought experiment that yields the same result involves us imagining (as actually being the case) that the stuff in our lakes and rivers is not H<sub>2</sub>O but XYZ, though we don’t know that yet (perhaps we have been blinded to it by some persistent scientific error). Now in this scenario, our term ‘water’ refers not to H<sub>2</sub>O, as we think, but to XYZ even though the scenario involves no change in the internal states of language users.

After a discussion of Twin Earth scenarios, Devitt and Sterelny conclude that “there is no internal state of the speaker that determines the reference and hence the meaning, of his words” (Devitt and Sterelny, 1987, p.52). If this was right, then the failure of the description theory would be a symptom of this more significant semantic truth. However, I suggest this inference is a mistake. It doesn’t follow from these scenarios that the reference of ‘water’ is determined independently of any language user’s conception or ability. All this shows is that for some terms, what the actual world is like has an *influence* on what a term refers to. In other words, Putnam has shown the failings of what we might call *strong internalism* – the idea that exactly what we refer to is determined just by mental states from the skin in, so that same mental state (narrowly construed) necessarily means same referent. It is a big and unwarranted leap to go from a rejection of this to the conclusion that mental states are irrelevant to what we refer to.<sup>8</sup>

It may be useful at this point to introduce a distinction between strong and weak determination. Twin Earth scenarios and environmental externalism in general show that mental states do not *strongly* determine reference. However, they do not show that they do not *weakly* determine reference. A simple analogy can help us understand this distinction. If a pack of cards are shuffled and spread face down on the table, an observer may pick a card by pointing. The actions of the observer determine which card type is picked, but the actions only weakly determine which card type is picked, in that what the chosen card is also depends on how the cards are arranged face down on the table. The actions *and* the actual arrangement of cards together determine which card is chosen. On the other hand, if the observer picks a card by naming the card explicitly, by uttering ‘Jack of Spades’ for example, that utterance alone strongly determines which card (type) is picked.<sup>9</sup> Devitt and Sterelny may be right to say that mental states do

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<sup>7</sup>Here ‘how things are in the head’ is understood in the narrow sense according to which Earthians and Twin Earthians do not have different things in the head just because of their different external environments. For a discussion see the introduction to Pettit and McDowell (1986).

<sup>8</sup>As Jackson has pointed out, even traditional descriptivism is world involving to an important degree – it holds that reference is determined by descriptions, but what we refer to also depends on what in the actual world satisfies those descriptions. See Jackson (1998b, p.205).

<sup>9</sup>The distinction used here is developed from Chalmers (2002).

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not strongly determine the reference of many terms, but it has not been shown that they do not weakly determine reference. Moreover, as we saw that conceptual analysis is only committed to epistemic rather than metaphysical determination of reference, so we can say also that conceptual analysis is committed only to weak rather than strong determination of reference.

Thus we have two significant results derived from these thought experiments. Neither result, however, justifies the conclusion that conceptual content does not (weakly & epistemically) determine reference. However, there is still a yet more important observation to make about these thought experiments. Above, I claimed that Putnam's Martian robots example does not imply that reference is not determined by a tacit understanding of application and co-application conditions. I also said that it *cannot* show this. This, crucially, is because the thought experiment itself relies on a tacit understanding of the application and co-application conditions associated with the term 'Kangaroo'. The thought experiment asks us to consider what the term refers to in a given scenario, and our answer reflects the conceptual content that epistemically determines the reference of the term in that scenario.<sup>10</sup>

This point is quite general. Any thought experiment that appeals to our intuitions about the referent of a term in a possible scenario *must* itself be committed to a tacit understanding of the application and co-application conditions of the term. The very fact that we can give any answer at all in these scenarios proves the existence of conceptual content that (epistemically) determines the reference of our terms. I think that with this argument set out we can see that Miller's attempt to deny that conceptual content determines reference, and any other similar attempt employing different examples, cannot be successful. This is the argument Thomasson should have made but didn't.<sup>11</sup>

### 2.2. Direct Reference and Social Externalism

Hopefully I have shown that the traditional externalist thought experiments that rely on intuitions about possible cases cannot be used to counter the claim that conceptual content determines reference. In this section I want to turn my attention to two related areas familiar to the philosophy of language: direct reference and social externalism. In addressing these issues I will rule out any worries that might arise based on these topics, and in doing so further develop the claim being defended.

#### Direct Reference

The issue of direct reference can be addressed fairly quickly, for while there is some dispute about how direct reference (henceforth DR) should be formulated precisely<sup>12</sup>

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<sup>10</sup>A similar point is made in Haukioja (2009).

<sup>11</sup>Recall, however, that Miller's own causal theory is itself committed to just such conceptual content in terms of a 'reliable discriminating ability' – see §1.3.

<sup>12</sup>For a discussion see, e.g. Recanati (1993, pp.7-27).

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no reasonable characterisation stands opposed to the thesis being defended here. Nevertheless, there is space for confusion that it will be best to avoid. Why might one think that DR stands as a stumbling block to the claim that conceptual content determines reference? It might be supposed that DR stands as a stumbling block to this claim because DR is typically contrasted with a Fregean sense theory of meaning, and it may seem that what is being proposed here is tantamount to a theory of senses.

The account being defended here is not committed to any particular understanding of senses, though it is certainly the case that some closely related philosophical positions have drawn links with Fregean senses of sorts.<sup>13</sup> Whether or not one regards a theory of conceptual content that epistemically and weakly determines reference as amounting to a theory of senses depends entirely on what one takes a theory of sense to be committed to. To clear things up, it will be helpful to separate the kind of senses that DR is opposed to from the account of conceptual content being defended here. We will see that they are not the same, and so the theory being defended here is not in conflict with DR. François Recanati has described the broad and intuitive notion of direct reference as follows:

A (directly) referential term is term that serves simply to refer. It is devoid of descriptive content, in the sense at least that what it contributes to the proposition expressed by the sentence where it occurs is not a concept but an object. (Recanati, 1993, p.3)

If this is how DR is understood, we can note that it will be opposed to any account of senses that holds that a referring term contributes a sense to the proposition expressed. David Kaplan's landmark paper originally set up direct reference as being an alternative to just such an understanding of Fregean senses (Kaplan, 1989, p.486). Kaplan claims explicitly that in Fregean semantics it is the sense that is contributed to the proposition expressed.

However, the theory being defended here is committed to no such thing. The conceptual content that epistemically determines reference need not be understood as being contributed to the proposition expressed. All the theory defended here is committed to is that if a term contributes an object to the proposition expressed, we have conceptual content that allows us to (weakly and epistemically) determine which object is contributed.

We could leave matters there, but Genevieve Martí has recently claimed that there is an additional stronger notion of DR. Martí describes a *propositional* characterisation of direct reference which is as above: "a directly referential term is one that contributes an object, its referent, to the propositions expressed by the sentence containing it" (Martí, 2003, p.163). In contrast, however, what she calls the *Millian* characterisation describes "directly referential terms as those that refer directly without the mediation of a Fregean sense or a semantic profile whose function it is to select and single out the

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<sup>13</sup>David Chalmers' two-dimensionalism, for example, promises a role for "quasi-Fregean senses" in Chalmers (2002).



## 2. Conceptual Analysis Defended

referent” (Martí, 2003, p.163). Now one *may* read Martí’s Millian characterisation here as being opposed to conceptual analysis, for haven’t I claimed that conceptual content can ‘select and single out the referent’ even if only epistemically and weakly?

However, I think that even this Millian characterisation (as far as it *is* distinct from the propositional characterisation) is compatible with the thesis being defended here. This is because even this account is committed to language users having the ability to epistemically determine reference. To see this note that in his *Demonstratives* Kaplan contrasts a Fregean semantic theory with a direct reference theory and in a diagram of the latter he suggests that a singular term refers to an individual “by the conventions and rules of language use” (Kaplan, 1989, p.486). A similar claim is made by Martí concerning her Millian characterisation: “the Millian proponent of direct reference argues that a name is associated by convention with an object” (Martí, 2003, p.166). But now suppose that we ask: what is it for a name to be associated by conventions or rules with an object? How does that occur? How is it that a name is associated with one object and not another, for example? I do not know how this question can be answered without appeal to the conceptual content of language users, especially as ‘conventions’ surely depend on a kind of knowledge or ability on the part of language users. In other words, if language users have conventions and rules that associate words with objects, then they do have conceptual content, (expressed in rules and conventions, perhaps) that (at least) epistemically determines reference. Drawing out a full account of how conventions fit in with an epistemic determination of reference would be a delicate and lengthy task that I will not undertake here, but there is no reason to think it could not be done. The important point for us is just that these accounts of DR do not stand opposed to the conceptual analysis being defended.

### **Social Externalism**

The second issue to be addressed concerns another well known feature of meaning and reference that has so far not been mentioned but about which we ought to be aware in order to avoid a particular class of potential problems. This feature concerns the social aspect of meaning and reference. There are two related points to draw out here. The first is that when an individual uses a word in a common language, the meaning of the word is often the meaning that that word has in that language, irrespective of any facts about the user other than that she is judged to be using that word in that language. Tyler Burge made this point well with a number of now well known examples (Burge, 1979). To use just one, Burge asks us to consider someone who speaks English well and has had arthritis for years. He has many beliefs about arthritis and has discussed it often. However, he then develops a pain in his thigh and goes to the doctor to complain that his arthritis has spread to his thigh. He now betrays a misunderstanding of arthritis because arthritis is an ailment that only affects the joints. Nevertheless, ‘arthritis’ in his mouth refers to a rheumatoid ailment of the joints despite his misunderstanding. Burge then considers a Twin Earth scenario with a Doppelganger of our patient. The only

## 2. Conceptual Analysis Defended

difference on Twin Earth is that there ‘arthritis’ doesn’t mean *arthritis* (as we mean it) but instead is used to refer to an ailment of the joints *or limbs*. The Doppelganger and the original patient are otherwise exactly alike. Now the Doppelganger says something correct when he says that his arthritis has spread to his thigh, despite the fact that the patients are alike from the skin in (Burge, 1979, pp.77-79).<sup>14</sup>

One upshot of this is that what a speaker says with a word depends on what the word actually means in the common language that they are speaking. The two patients express different propositions merely by being in different socio-linguistic environments. This is important to us because in the light of it we must understand the conceptual content that determines reference as the conceptual content associated with a term by the community of language users, and not just by some lone user who may refer with the term. The crucial question for us is how a word refers in a language, and not how an individual comes to be using that word.<sup>15</sup>

The second and clearly related point has been emphasised by Putnam, who notes than within a language community there is a “division of linguistic labour” Putnam (1973, p.704). We commonly defer to experts and the wider linguistic community when speaking so that we mean ‘whatever they mean’ – indicating the experts in the relevant field, or even the people in the pub whose conversation we have just entered halfway through. There is a question here about whether such ‘borrowed’ uses of words require some epistemically determining conceptual content be possessed by *each* user of the word if they are genuinely to refer with it. Both Kripke and Evans have suggested that in such cases the otherwise ignorant user must at least *intend* for such a deference to take place if it is to take place, and one might construe this as some minimal reference determining conceptual content.<sup>16</sup> As far as the conceptual analyst is concerned, however, that point of detail is relatively unimportant. The conceptual analyst is not interested in analysing the concepts of users who merely borrow the term from others in the linguistic community in this parasitic way.

The point to be emphasised then is that the conceptual analysis being defended and practised here is the conceptual analysis of terms in a social context. It is conceptual analysis of what we mean by ‘novel’ or by ‘photograph’, or by the name of a particular novel – ‘Emma’, for example. If I, as an individual, were to apply the name ‘Emma’ in a manner wildly at odds with normal accepted usage then I could hold my ground and insist that I am interested in the ontological status of what I mean by ‘Emma’, which wouldn’t yield very interesting results for anyone else. Alternatively I could accept the error of my ways and concede that my desire was to refer to the same thing as everyone else all along despite my ignorance, and that, as it happened, I was too linguistically incompetent to be a reliable source. The failure here would be with me and not with the method of conceptual analysis. As an analogy, if a scientist discovers truths about protons but, thinking they are electrons, publishes a paper of false claims

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<sup>14</sup>See also Pettit and McDowell (1986, p.7) for discussion.

<sup>15</sup>For this distinction see Evans (1982, p.77).

<sup>16</sup>See Kripke (1980, p.163), and Evans (1973, p.191).

about electrons, the problem is with the scientist, not the scientific method. Likewise, it cannot be held as a charge against conceptual analysis that any particular individual may have unorthodox ideas about the meanings of words, nor can it be held against conceptual analysis that an individual may defer to the rest of the linguistic community with their concept of ‘sonata’ for example. Only if they are going to do any ontology, they had better go and find out what everyone else does mean by ‘sonata’.

### 2.3. **Objections to A Prioricity and a Connection with Two-Dimensionalism**

So far I have dealt with intuitions about possible cases and the issues of direct reference and social externalism. What I will turn to now are a set of arguments that have been levelled not at this specific characterisation of this thesis, but at a closely related thesis defended by Frank Jackson. Jackson has been a staunch defender of conceptual analysis, and his argument is strongly dependent on a theory of *two-dimensional semantics* (Jackson, 1998a). The aspect of two-dimensionalism that is relevant to us is what Jackson calls the A-intension of a term (Jackson, 1998a, p.48). The A-intension of a term is a function that assigns an extension to a term in a possible world  $w$  under the supposition that that world  $w$  is actual. So, for example, if we suppose the actual world is thus and so, the A-intension of a term assigns an extension to that term in that supposed scenario. Note how similar this idea is to the thought experiments considered in §2.1. We are asked to imagine that we discover, for example, that all the kangaroos on the planet are actually Martian robots. We then respond by saying that the robots are indeed kangaroos, but that we have been mistaken about their nature all along. In other words, we are given a scenario and are asked to consider it as actually being the case, and make a judgement about the reference of our terms given that scenario. According to Jackson, this judgement is based on the A-intension of the term, which is a function that assigns an extension in just such a scenario. I have said, using Thomasson’s terminology, that this judgement is based on tacit understanding of the application and co-application conditions associated with a term. It is clear then that the two notions are closely related.<sup>17</sup>

If this association of A-intensions with Thomasson’s application and co-application conditions is right, then a recent challenge presented by Laurence and Margolis to the possibility of having a priori access to A-intentions will also count as a challenge to the claim that language users have (accessible) conceptual content that determines the reference of their terms (Laurence and Margolis, 2003). For the most part, addressing Laurence and Margolis’ worries will provide an opportunity to re-apply some of the

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<sup>17</sup>Thomasson goes only so far as to mention in a footnote that two-dimensionalism represents an alternative formulation of her general position. See Thomasson (2007b, p.210). I think in fact that two-dimensionalism represents one of the most promising methods of formalising the conclusion defended here, but doing so, and properly defending two-dimensionalism from, e.g. Soames (2004), would require a thesis in itself. Moreover, a defence of Thomasson’s thesis, in her terms, will be sufficient for the methodological needs of our ontological project.

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points already made. However, their discussion also highlights some additional confusions that ought to be cleared up.

First, a note about the claim of a *prioricity*. The view being defended here is not committed to the claim that an understanding of application and co-application conditions for a term is somehow innate. It is perfectly possible, and indeed quite likely, that we have the conceptual content we do as a direct result of our experience of the world. It can even be allowed that generally speaking our experience has shaped and continues to shape that conceptual content. The sense in which our understanding of these conditions is *a priori* (and the sense in which access to A-intensions is *a priori* for Jackson) lies in the fact that for a term to genuinely refer, language users must associate the term with conceptual content that (epistemically and weakly) determines the referent of that term. Such conceptual content is not something that can be revised or learned after the term has become an established referring term in the language, because until there is such conceptual content, the term does not refer at all. Moreover, understanding this conceptual content, which involves understanding the application of the term in possible scenarios considered as actual, does not depend on how the actual world is. The answers we give to the Martian robot case, or the Gödel/Schmidt case, do not themselves depend on whether or not Kangaroos are actually robots, or on whether or not Gödel actually did discover the incompleteness theorem. Thus what Laurence and Margolis are proposing, in rejecting *a priori* access to A-intentions, is that any content that determines reference is empirical and subject to revision in light of discovery. In other words, they are denying that such content determines reference at all.

They begin by claiming that we do not have *a priori* access to the A-intensions of our terms because we do not have *a priori* access to a description that picks out the referent of that term in any world considered as actual:

[P]eople can't have *a priori* access to a description that picks out the referent of WATER in each world, *w*, since people don't even have *a priori* access to a description that picks out the referent of WATER in the actual world.  
(Laurence and Margolis, 2003, p.261)

This is because, they argue, “any element in a natural kind concept’s stereotype is open to revision in the light of empirical findings.” The claim then is that A-intensions are as subject to refutation just as much as any other information associated with a term, and so cannot be *a priori*. However, this criticism is based on a misunderstanding of A-intensions. Firstly, Laurence and Margolis assume that A-intensions are descriptions understood as ‘stereotypes’. In other words, they assume that epistemic determination of reference involves knowing an infallible description in virtue of which we determine the reference in a possible scenario. However, the epistemic determination of reference does not involve possessing infallible stereotype descriptions. As discussed in §2.1, Putnam’s examples do show that reference is not fixed to natural kind terms by satisfaction of superficial properties, but this does not warrant the further conclusion that reference is not epistemically determined at all.

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Moreover, to pick up on another point made in §2.1, all of the examples that Laurence and Margolis appeal to themselves depend on an ability to specify the referent of our terms in possible scenarios considered as actual. Laurence and Margolis note, following Putnam, that we may be under an illusion about the colour of lemons, or that a new gas in the atmosphere may change them from yellow to blue. They then assume that these empirical findings would result in us changing our A-intension of lemons from including the criteria of being yellow to including the criteria of being blue. But in fact what these examples show us is that we are *already* in a position in which ‘being yellow’ is not essential to being a lemon in a possible scenario considered as actual – if it was essential, then our response to the story about the new gas turning lemons from yellow to blue would be to say that lemons had ceased to exist.<sup>18</sup>

Laurence and Margolis then go on to object to a priori knowledge of A-intensions on slightly different grounds. They consider a person who “grows up with no exposure to lakes or oceans – maybe a nomad in the Sahara” (Laurence and Margolis, 2003, p.262). However, since, as they assume, knowing a priori the A-intension of water would mean knowing a priori that water was ‘the stuff in our lakes and rivers’, either the nomad would have a priori knowledge of such a fact, or “lack the concept WATER all together.” It seems that neither of these options are very attractive.

However, this is a false dichotomy based on a number of mistakes. Firstly, as before, the conceptual content that epistemically determines the reference of ‘water’ is not a description that includes the belief that water is ‘the stuff in our lakes and rivers’. Secondly, it is very unclear what is meant by “the concept WATER”. The implication is that the conceptual analyst is committed to saying that to have the concept WATER is to know the common A-intension associated with that term. It is further implied that this is clearly not what it is to have the concept WATER (since the nomad presumably has the concept WATER). However, the conceptual analyst is not committed to any particular understanding of the phrase “the concept WATER”. If, for example, to have the concept WATER is just to be able to refer to water then the nomad may have the concept either by deferring to the wider language community (if we make the unlikely assumption that they are speaking English) or by possessing their own conceptual content that epistemically determines the reference of their term.

A third challenge is mounted against a priori access that involves arguing that it relies on a description theory of reference determination, and that a description theory is not obviously the right theory (Laurence and Margolis, 2003, p.263). Furthermore, since, as they claim, “the nature of reference determination is itself a broadly empirical question” (Laurence and Margolis, 2003, p.264) we cannot know a priori the extension of our terms in a possible scenario considered as actual, because we cannot know a priori what determines the extension in any scenario. There are a few things to note here. The first is that as we saw from §1.3, what matters for conceptual analysis is epistemic determination of reference. Though Jackson is very sympathetic to a description

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<sup>18</sup>The example is from Putnam (1970, p.142).

## 2. Conceptual Analysis Defended

theory,<sup>19</sup> conceptual analysis is not committed to a description theory. All conceptual analysis is committed to is that for us to use a term to refer, we must be able to say what counts as a correct use of the term in some scenario considered as actual.

Secondly, it is true that conceptual analysis is based on some broad theoretical arguments about reference and semantics, many of which have already been outlined. It is also true that there are competing theories of reference determination. However, the existence of alternative theories of reference cannot be used as an argument against the consequences of one such theory. Indeed, the understanding of reference needed for conceptual analysis may be mistaken – as with any philosophical theory – but the conceptual analyst believes there are good reasons for holding the kind of theory that requires A-intensions to which we have a priori access. I doubt whether Laurence and Margolis are right to say that the nature of reference determination is an empirical question, but the point is that the fallibility of our theories should not stop us drawing conclusions from what we take to be the best theory available.

The final reason Laurence and Margolis offer for doubting that A-intensions can be knowable a priori is put as follows:

Suppose that some variation of the causal theory of reference is right, and that what determines the extension of a kind term is a given world is a matter of causal links, not satisfaction of descriptions. In that case it seems clear that we will not have a priori access to the A-intensions which are determined by these causal links, since we presumably do not have a priori access to the relevant causal relata. (Laurence and Margolis, 2003, p.264)

At this point, however, we can appeal to the argument made against pure externalism in §1.3. We have good reasons for doubting that the kind of external causal link theory suggested is a coherent theory of reference. This point was argued for in the previous chapter, but there is a deeper issue here that is worth emphasising. The kind of theory being suggested by Laurence and Margolis would imply that in a scenario considered as actual we would not know, without empirical investigation, what the extension of our term was because we would not know which were the relevant causal relata without empirical investigation. But recall that if the reference of a term really were epistemically inscrutable, we would not be able to empirically investigate the relevant causal relata as we would not know *which* causal chains would lead us towards the referent and which would lead us away.

To summarise, then, the objections that Laurence and Margolis make to our ability to have a priori access to conceptual content that determines the reference of our terms (or to A-intensions, as they put it following Jackson) are based on general misunderstandings about the position involved. In particular, once the relevant conceptual content is separated from the possession of an infallible reference fixing description, many of the objections can be dealt with.

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<sup>19</sup>See, e.g., Jackson (1998b).

## 2.4. **Conceptual Change and Reference Stability**

In this section I want to consider one last and particularly substantial objection to conceptual analysis. The broad idea here is fairly straightforward and can be summarised as follows. Recall that according to conceptual analysis, the reference of our terms is weakly and epistemically determined by conceptual content. Consequently an analysis of our intuitions about the use of a term in possible scenarios can reveal details about the referent; details that any further assertions about the referent (in particular, ontological theories) must be compatible with. We saw that this position is extremely resilient to counterexamples, because any attempt to show that some current conceptual knowledge is not involved in determining the referent of a term must itself rely on intuitive knowledge about the referent in a possible scenario.

However, it is often argued that both experience and intuition show that in some cases there can and has been radical conceptual change *despite* continuity of reference. The aim here is not to try to show that what is referred to now is not determined by conceptual content but to show that past reference must not have been determined by conceptual content given that past users sometimes possessed radically mistaken concepts that have since been overturned. Thus even details revealed by the most insightful and accurate conceptual analysis may be rejected given future developments. Conceptual analysis could not then provide the kinds of restrictions on ontological proposals that we are hoping for.

By far the most common source of examples used to imply radical conceptual change despite stability of reference involve developments in scientific theory. It is argued that past speakers had radical misconceptions about the things they referred to. Subsequent discoveries have revealed the true nature of these things, and have resulted in significant conceptual shifts.

In what follows I will argue that this direct challenge to conceptual analysis is weaker than its proponents assume and relies on shaky intuitions about reference stability. In particular I will argue that we should be suspicious of assumptions about the reference of past speakers based purely on current conceptual content. While such moves retain the stability of reference, they do so artificially and at the expense of a plausible theory of reference.

The strength of the examples employed varies considerably, with intuitions towards reference stability being much stronger in some cases than others. As such, the ease with which reference stability can be called into question varies. I will start with a more easily overturned example, offered by Robert Stecker. Stecker has suggested that conceptual content is not a reliable guide to the nature of the entities referred to since “we might be fundamentally mistaken about the kind a term refers to – as when it was widely believed that . . . planets were gods” (Stecker, 2009, p.382). The suggestion is that at a previous point in history, language users referred to the planets with various names, though they believed that what they were referring to were gods. Of course, as we have seen conceptual analysis does not depend on explicit beliefs about the nature of the entities

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we talk and think about. Rather, it depends on our considered intuitions about the use of terms in possible scenarios considered as actual (application and co-application conditions). What we are to believe then, if the example is to have any force, is that an analysis of past language users' understanding of these conditions would have been compatible with god-like entities but not with planet-like entities, though unbeknownst to them they were actually referring to planets.

We can fill out the details of this example in two ways, neither of which will give Stecker the result he needs. Firstly – and I think this is the most plausible way to fill out the scenario – we can imagine that past speakers had a host of different beliefs about their 'gods', and an aspect of those beliefs involved associating the 'gods' with certain heavenly bodies. However, it is now very implausible that they were actually referring to those heavenly bodies when they spoke of their gods. They were simply *failing* to refer. If reference failure is ever possible, and it surely is, then this seems to be one of the better candidates. Stecker has merely assumed that they were referring to planets when they spoke of gods, while also assuming that their reference determining conceptual content was wholly 'god like'. The conceptual analyst's response is that *if* their reference determining conceptual content was wholly god like, they merely failed to refer.

Alternatively, it may be the case that there were people who were aware of the 'heavenly bodies' and referred to them and individuated them as those things that move through the sky, are visible each night, etc., and *of* those things thought that they were gods. Here it is plausible that they had false beliefs and made false claims about planets. This scenario won't give Stecker the result he needs however, because *ex hypothesi* the conceptual content of the past speakers is not sufficiently different from our own. However, I think this scenario is very unlikely. Note that if this was the case then they would not have come to the conclusion that the god of war (for example) didn't exist.

The primary response to Stecker then is that he is just wrong to suppose that past speakers referred to the planets when they spoke of gods. They merely failed to refer. Moreover, any move Stecker makes to fill out the story to persuade us otherwise will involve him painting the scenario such that it no longer achieves his aims.

A more developed attempt to make the same point has been provided by Laura Schroeter (2004). Schroeter's examples focus on Aristotle's classical elements: air, earth, fire and water. The argument begins by noting that Aristotle's metaphysical world view and explanatory models were considerably different from ours, involving potential for change and rest, among other things. As such, the kinds of things Aristotle took himself to be talking about, based on his beliefs and concepts, were radically different from what we now know he was talking about. So, taking water as an example:

a modern analyst might be tempted to say Aristotle's "water" concept referred to that basic configuration of prime matter which most closely matched Aristotle's own criteria for identifying water in his actual world.



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If that is what Aristotle had in mind, however, his “water” concept did not manage to refer to anything at all. (Schroeter, 2004, p.437)

But it is then assumed, in line with our ‘standard externalist intuitions’, that these terms as used by Aristotle do in fact refer to the same things that we refer to when we use them. So “Aristotle’s ‘water’ concept referred to H<sub>2</sub>O just as ours does,” Schroeter claims (Schroeter, 2004, p.437). Ergo, the modern analyst is mistaken in thinking that concepts can reveal facts about the referent, since Aristotle managed to refer to the same stuff as us despite his radically different conceptual content.

My response here will essentially be the same as that offered to Stecker, but overcoming the strength of the ‘externalist intuitions’ will require slightly more work. I will begin by discussing briefly Schroeter’s specific claims concerning earth, air and fire, where I think it is possible to push our intuitions away from reference stability. The example of water, where intuitions are arguably the strongest, will require a more detailed discussion.

Schroeter is asking us to accept that the Greek words used by Aristotle that have subsequently been translated as ‘earth’, ‘air’ and ‘fire’ referred in Aristotle’s mouth to the same things that we refer to with our terms ‘earth’, ‘air’ and ‘fire’. The first thing to note is that there is a real danger here of making false assumptions about the accuracy of translation. Languages have idiosyncratic and untranslatable terms, and meanings (and subsequently references) can and do shift especially over such long periods. We should not blindly assume accuracy of translation. A second point to note is that none of these terms even have clear well defined extensions in modern English. Does ‘air’ refer to any gas, or any planetary atmosphere, or any gas with the same molecular ratios as Earth atmosphere at ground level? Likewise, is the extension of ‘earth’ any inanimate naturally found solid, or just the soil-like compounds in which things can be grown? ‘Fire’ is also a slippery term: we speak of building fires, or relighting the same fire, or having a coal fire installed. Are we to believe that the words Aristotle used has exactly the same vague extension as our words now have, and if so, in virtue of what is this assumption made? Schroeter claims that Aristotle was referring to fire as a process which today is more precisely picked out by the term ‘combustion’, but it seems unlikely that Aristotle had anything like the concept of combustion at work. Why are we to think that he was referring to combustion rather than, say, flames, or heat, or some other related kind of phenomenon? Schroeter addresses this worry as follows:

When we judge that Aristotle’s “fire” concept referred to the process of combustion, we justify our interpretation by appealing to the most important elements of Aristotle’s own conceptual practice. But Aristotle himself couldn’t tell what the most important elements of this practice really were purely on the basis of a priori conceptual analysis. Aristotle’s strongest tacit assumptions about which sort of thing he was thinking about were just plain wrong. (Schroeter, 2004, p.442)

Yet notice that this proves too much, for this rules out even the *possibility* that Aristotle

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was thinking of and referring to flames rather than combustion, on the grounds that combustion is the most scientifically interesting aspect according to modern theory (and even that is a questionable claim – scientifically interesting according to which standards?). Yet surely the possibility that Aristotle was thinking of and referring to flames is very real. After all, one certainly *can* think of and refer to flames rather than combustion. Schroeter might *assume* that that Aristotle’s fire concept referred to the process of combustion, but this is a bad assumption to make.

What emerges here, I think, is a tendency to make uncritical assumptions about what past speakers were referring to based on modern theories and concepts. We should not put the reference of Aristotle’s words into his mouth simply based on our own understanding of terminology and theory, regardless of what our initial intuition suggests.

Schroeter might respond at this point, however, by arguing that even if intuitions about reference stability can be questioned for ‘earth’, ‘air’ and ‘fire’, our intuitions about the reference of ‘water’ are surely strong enough to support the claim of reference stability despite radical conceptual change. It is likely that discussions of the reference of ‘water’ benefit from its familiarity in externalist thought experiments. We have become accustomed to accepting that speakers in 1750 referred to H<sub>2</sub>O despite not knowing the chemical constitution of water, and it seems that we can extrapolate backwards to Aristotle without too much difficulty.

There is a problem here for Schroeter, however, and it is a problem that she recognises. The problem is that with the example of water, it is not clear that Aristotle’s conceptual content would have been sufficiently *different* from our own to undermine the claims of conceptual analysis (Schroeter, 2004, p.442).

Nevertheless, I think that even the intuition that the reference of ‘water’ has remained stable can be brought into question. To make this point we can begin by noting that it is often taken for granted that developments in science lead to straightforward discoveries about the true nature of the entities being referred to. Speakers in the past referred to H<sub>2</sub>O with the term ‘water’ it is claimed, despite not knowing its chemical nature, or even that it is a chemical kind. However, there are good reasons to think that even though we now specify the referent of ‘water’ as H<sub>2</sub>O, reaching this conclusion was a matter of *decision* about what ‘water’ should refer to rather than pure discovery. As such we cannot assume that prior to these scientific developments the relevant terms referred just as they do now. The presence of decision points in scientific progress in general should put the brakes on the uncritical supposition that reference is always stable through conceptual change.

The way to show this is to appeal to examples that demonstrate the inevitability of decision points following certain scientific discoveries. Both actual and hypothetical examples can be found in the philosophical literature. The point in every case is to show that the precise reference that our terms have *now* was not mandated by any previous (relevant) state of affairs, and thus it is implausible that previous speakers referred to what we do now.

For example, Keith Donnellan has offered a Twin Earth thought experiment of his

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own to show that the precise reference of a kind term such as ‘gold’ was not fixed as it is now prior to key scientific developments. In Donnellan’s example, Twin Earth and Earth differ only in the following respect: scientists on Twin Earth decide that *isotope* number is more interesting than atomic number. On Twin Earth as on Earth early language users had the vernacular term ‘gold’ and used it in the same way. However, as the chemistry of elements developed, scientists on Twin Earth identified the vernacular term ‘gold’ “not with the element having atomic number 79, but with a certain isotope having a certain isotope number” (Donnellan, 1983, p.100). Thus after the rise of modern chemistry ‘gold’ on Twin Earth has a different extension to ‘gold’ on Earth even though the only relevant differences on Earth and Twin Earth arose following the discovery of atomic number and isotope number. Given the possibility of this scenario, the point then is that we cannot just assume that early language users on Earth had the same extension for their term as we have now, without assuming that the extension of their terms was fixed by *future* scientific decisions. As Donnellan put it, “it is bizarre to suppose extension depends on future historical accidents” (Donnellan, 1983, p.104). Donnellan concludes: “I do not see how we can accept Putnam’s view that it is clear that natural kind terms in ordinary language have the same extension before and after scientific discoveries” (Donnellan, 1983, p.104).

In case it is thought that a lot rests here on questionable hypothetical scenarios, Joseph LaPorte has more recently used actual world examples to make the same point (LaPorte, 2004, pp.103-108). LaPorte notes that following the discovery of  $D_2O$ , a heavy isotope of  $H_2O$ , in 1931, it was scientific ‘decision’ rather than discovery which resulted in  $D_2O$  falling under the extension of ‘water’ (it is often called ‘heavy water’). This is because nothing prior to the discovery of  $D_2O$  mandated one extension of ‘water’ rather than the other.  $D_2O$  and ‘normal’  $H_2O$  are the same microstructural kind in one sense (they are both  $H_2O$ ) but different microstructural kinds in another sense (they have different mass numbers). Furthermore, as LaPorte points out, it makes no difference to accurate scientific theory *which* kind ‘water’ picks out. We happen to have chosen to have ‘water’ refer to all  $H_2O$  (heavy and light), but we need not have done. As such we can bring into question the entrenched assumption that ‘water’ in the mouths of previous speakers picked out the very kind that we refer to now. LaPorte argues that in 1900, for example, it was simply indeterminate as to whether  $D_2O$  lay in the extension of ‘water’. To say that it *did* include  $D_2O$  simply in virtue of decisions made 30 years later is to be guilty of the kind of retrospective semantics that Donnellan called ‘bizarre’.

LaPorte further discusses other actual examples that show that in some cases vernacular terms do not come to stand for micro-structural kinds at all. Jade, for example, has come to refer for the Chinese to two different chemical substances, despite the fact that one of them was only introduced at the end of the 18th Century. The relatively new stone (‘new jade’ or ‘jadeite’) was superficially similar to (though distinguishable from) the traditional stone (‘old jade’ or ‘nephrite’). With the introduction of the new material, the Chinese certainly *could* have retained the term ‘jade’ just for the nephrite they were already familiar with. However, they chose not to, and now ‘jade’ in the

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Chinese language applies equally to the old and new forms (LaPorte, 2004, pp.94-96).

The term ‘ruby’ provides a slightly different kind of example. As LaPorte notes, the term was used for a long time for a red mineral identified only by superficial properties. When the chemical composition of the mineral was discovered, it turned out that the mineral could also be found in a blue form, the colour being changed by minor impurities. It might seem then that ‘ruby’ should have come to refer to the mineral in its blue form as well (given our tendency, in other cases, to refer to microstructure), but it did not. The term has been reserved only for the red variety of the mineral (LaPorte, 2004, p.101).

The point in both cases is that there is a great deal more referential decision making following empirical discoveries than our ‘externalist intuitions’ would have us believe. It is tempting to assume, as Schroeter does, that Aristotle was referring to  $H_2O$  with his term, given that that is what we now refer to. But there are no guarantees of this. Things could have gone differently following the same scientific discoveries, and, to repeat Donnellan’s point, it is implausible to think that the extension of Aristotle’s terms depended on future accidents and psychological quirks.

So what was Aristotle referring to with his terms for ‘earth’, ‘air’, ‘fire’ and ‘water’? This is a difficult question to answer with any precision, though if we trust the translations we can certainly get some idea. The important point is that it is a question for historians and translators, and not just a question for modern science. If Aristotle did associate very different application and co-application conditions with his terms than we currently associate with our terms, we should doubt that his terms had the same extension as our terms.

It should be noted that this tendency towards retrospective attribution of meaning and extension becomes even less plausible when we move away from scientific terms. For example, for most of us, ‘poor’ is currently a vague term (it has borderline cases) and a context sensitive term (what counts as being poor will change with context). It is sometimes useful, however, to make the meaning of ‘poor’ precise, usually in terms of how much a person lives on in a day. Research bodies and governments do this all the time. Now suppose that one of these precisifications becomes so widely used that ‘poor’ is no longer a vague term of English. In virtue of one of the definitions catching on, ‘poor’ now only applies to those living on less than a certain fixed amount. It is quite clear that we don’t want to say that ‘this is what we meant all along’. We don’t retrospectively interpret our previous uses as having this new precise meaning. Instead, we just say that the meaning of ‘poor’ has changed. We might even suppose that some more research is carried out and data gathered, and a new definition proposed that is in some sense more accurate, or more useful. But it is still clear that we have changed the meaning of ‘poor’ – we have not discovered something about what the term meant in the past.

## 2.5. Conceptual Analysis in Practice

Defending the claim that language users have conceptual content that determines reference has taken us some way from the original methodological question. Along the way, many broad and thorny philosophical issues have been touched upon, about which a great deal more could be said. However, at some point we must move on from methodology to our motivating ontological question. Hopefully enough has been said to at least ward off any premature objections to the methodological claim being defended here.

What remains in this chapter is firstly to discuss, briefly, some of the consequences that this commitment to reliable conceptual content has for our understanding of reference in general, and secondly to set out more fully how ontological theorising can be guided by this methodology.

In terms of the general consequences for reference, the first point to pick up on is that it follows from this account that significant disagreements between two parties about the application and co-application conditions for a given term implies that the term is actually being used to refer to different entities by each party. However, this need not worry us for two reasons. Firstly, this phenomenon is quite familiar. Sometimes debates can continue for some time before it is realised that contributors are actually talking about different entities – they are ‘talking past each other.’ This phenomenon agrees well with conceptual analysis. We know we are talking past one another when our understanding of how the term should be applied in scenarios differs significantly.<sup>20</sup>

At other times, a genuine debate can arise where the debate is not about some agreed subject matter where this is understood as an entity, but about the most accurate or suitable conceptual framework needed to explain some phenomenon. In these cases, theorists may reject old entities and introduce new ones. A debate about what caused the pattern in a cliff face (is it caused by nesting birds or by a storm?) is not a debate about what some established entity is like, but about which entity the best theory should posit. In these cases of course the kind of conceptual analysis defended here is ineffective. We cannot provide an ontological account of what caused the pattern in the cliff face if we as of yet have not identified in any clear way *which* entity we are interested in.

The second concern that the method of conceptual analysis might invite is this: if our application and co-application conditions are indeterminate in some key area then would it not follow that reference of our terms is indeterminate? The answer is that this is exactly what follows, but that this also needn’t worry us. For a start, it doesn’t follow from this that, of any particular entity that is picked out, it itself is indeterminate. Rather, it follows that sometimes, perhaps often, it is indeterminate what exactly is being picked out by a term in a common language. In other words, the indeterminacy is semantic rather than metaphysical. This kind of indeterminacy is commonplace and

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<sup>20</sup>This kind of disagreement is not to be confused with the disagreement that takes place when debators disagree about the correct use of a term whose reference is established by experts or in general by the consensus of the wider linguistic community.

## 2. Conceptual Analysis Defended

usually quite harmless. We noted already that, as LaPorte argued, prior to the discovery of  $D_2O$ , it was indeterminate whether  $D_2O$  was in the extension of ‘water’. It may also have been somewhat indeterminate what Aristotle was referring to with his term for ‘earth’. Along with indeterminacy, there will be degrees of ambiguity. For example, Paul Bloom has argued that while ‘water’ is often used to refer to  $H_2O$ , the term also has a strong vernacular use that suggests that we are picking out not  $H_2O$  but an artifact kind that includes swamp water and radiator water (both called ‘water’) but excludes tea, tears and Sprite (Bloom, 2007, pp.151-156).

The example of biological species, a topic that we will return to §6.1, is very useful here. In most non-technical contexts language users discuss animal species with little trouble. We say things like ‘THE TIGER is a dangerous carnivore’ or ‘THE AYE-AYE is an unusual animal, if ever there was one’ and there are no real problems with this. However, we might ask what exactly the speakers are referring to. Likely they will reply ‘the species by that name, whatever a species is’ or something to the same effect. In other words, the reference is given by deferring to the experts. But there are in fact around 20 different species concepts, each of which picks out animals in a given species according to different criteria (Griffiths, 1999, p.221). So they may say instead that the ordinary users mean by ‘species’ whatever a species *actually* is, according to best future science. However, this makes the assumption that there is such a thing as the one correct species concept that all the current uses of the term actually pick out. Species pluralists argue on the contrary that different concepts are useful in different contexts and so ‘species’ has a number of different but equally ‘correct’ meanings.<sup>21</sup> If the pluralists are right about this then ordinary users can use the term ‘species’ perfectly well without referring determinately. The possible species concepts are ‘close enough’ to each other in most contexts for the indeterminacy to be communicatively unimportant. If it is asked ‘Is THE TIGER a carnivorous animal?’ it would be inappropriate to ask in reply ‘what exactly do you mean by TIGER?’ because the answer is ‘yes’ in every case. It is only in more technical contexts that clarification is needed.

I think we should likewise expect a degree of indeterminacy and some cases of ambiguity to arise in our talk of entities such as novels, works of music, and poems. Many of these will be minor and unproblematic, others are already well known. For example, ‘book’ is ambiguous between a copy of a book and ‘book’ as a work of literature. Here the differences in how we apply the terms in each case make the distinction easy to recognise. That does not, of course, mean the distinction is ontologically straightforward. It will be the goal of the remaining chapters of this thesis to spell out the relationship between copies and works of literature in more detail.

We can see now that this will be done by assessing our intuitions about the application and co-application conditions for those terms. We can use these intuitions to reach ontological conclusions by picking out patterns and similarities in our use of various referring terms. We are looking for generalisations of the kind of information that we

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<sup>21</sup>See, e.g., Dupré (1993), and Ereshefsky (1998).

## 2. *Conceptual Analysis Defended*

use to give answers to questions about the application of terms in possible scenarios. The grouping of things into ontological categories depends on similarities in the way that terms for individual things are applied. So for example, if we are interested in the ontological nature of novels, we are not interested in when it is or is not correct to apply the term ‘novel’ in a scenario. That would amount to conceptual analysis of the concept ‘novel’ instead of an investigation into the ontological nature of novels themselves. Rather we want to know what the application and co-application conditions for terms referring to specific novels have in common both with each other and with other entities. Given that these conditions provide the extension of the term in possible scenarios, we are interested, when identifying an ontological category, in what those conditions have in common.

The grouping that provides the target of the enquiry undertaken in this thesis is itself based on just such an implicit analysis. So, for example, the logic behind the conditions of application and co-application for novels appears very similar to that for works of music, company logos, and photographs (or so it seems). The claim that certain things are all repeatable is based on the implicit recognition that terms for these things apply in scenarios in the same kind of way, and according to the same principles – principles that we will attempt to extract and set out in terms of an ontological theory.

It is clear that conceptual analysis, as being put forward here, is not a matter of language users being aware a priori of a set of propositions that are true, and obviously true, about the entities in question. (On the contrary, many of the conclusions drawn in this thesis will turn out not to be obvious at all.) Rather conceptual analysis is the process of exploring one’s intuitions about the application of a term in possible scenarios, and working to develop conceptual descriptions and generalisations of those intuitions. The process is thus reflective: explanatory hypotheses are offered to account for these intuitions, which are then tested against further intuitions.

Moreover, it should be pointed out that there is still plenty of room for manoeuvre within this methodological approach. Intuitions about application and co-application conditions need analysing and interpreting, and ontological theories must be developed that both accommodate those intuitions and fill in the gaps, so to speak. As such, there is still plenty of work to be done even with this guiding methodology in place. It is to that task that I turn now.

Part II.

Kinds



### 3. Explaining Repeatability

We are now in a position to address the central question of this thesis head on. What sort of thing is a repeatable artefact? We have names for novels, works of music and graphic designs and regularly employ those names in sentences in order to make claims about, ask questions about, and generally discuss the bearers of those names. We say things like “The 2012 Olympic Logo received a bad reception when it was first unveiled.” We also, of course, appear to quantify over these repeatable things (“All the proposed logos were flawed in some way”), though as I said in the introduction, I will not be mounting any substantial defence of realism here. Given that we accept that they exist, we ask: what sort of thing *is* the 2012 Olympic Logo? In this part of the thesis I will articulate and defend an answer to this question. In doing so I will seek to assess and take seriously our practices of identifying and individuating these sorts of entities, and of our understanding of application and co-application conditions. That is, we must now try to apply the method outlined previously to a specific end.

To cut to the chase, I will argue in this chapter that repeatable entities are kinds. Kinds are entities whose identity and persistence conditions are grasped in terms of the instantiation of collections of properties. They are not like ordinary material objects in that they do not trace continuous paths through space-time in the form of more or less cohesive collections of matter. Instead, they are located and can be identified wherever the relevant collection of properties are instantiated and so can be ‘multiply located’. They are real physical entities in an ontological plane that cuts across the non-repeatable objects and events with which we are more familiar.

There are two key elements to the theory of kinds indicated in that brief description. The first is that kinds have *essences*. The essence of a kind is the condition or conditions that must be satisfied for the kind to be instanced. It provides a rationale for identifying and individuating the kind and provides a unifying principle that binds the instances of the kind together as instances of that kind. Motivating and expanding on this position will be the task of this chapter. The second element to this theory of kinds is that, contrary to the prevailing view, kinds that have physical instances, and so repeatable artefacts, are not abstract entities.<sup>1</sup> Instead, kinds that have physical instances are themselves physical. They represent a different ontological categorisation of the same material world occupied by chairs, mountains and electrons. I will leave a discussion of this view, which I call *Kind Physicalism*, until the next chapter.

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<sup>1</sup>I restrict the claim I am making to kinds with physical instances, because I do not want to rule out the possibility of kinds of abstract entity. There may be kinds of number (such as the kind PRIME NUMBER) which has abstract instances and is itself abstract.

### 3. Explaining Repeatability

The theory I present accounts for the ontological nature of repeatable artefacts, but extends far beyond those. To pave the way for a fuller explanation we can note that according to this theory a paradigmatic example of a kind is gold. On this view gold is a real physical *mass*<sup>2</sup> that is found across the world instantiated by all the ingots and pieces of jewellery that are made of gold. The essence of gold is (we can assume) the atomic number: wherever there are atoms with atomic number 79 collected together there is gold. However, these atoms will also form a piece of gold. To see and touch a piece of gold is to see and touch gold itself as gold is instantiated by the ordinary material object that is the piece of gold.

I will defend the view proposed here primarily on the following grounds. Firstly it makes good sense of our intuitions concerning the application and co-application conditions for these entities. That is, it adheres with a careful analysis of our concepts of repeatable things, and so provides an account of what we ordinarily mean by ‘novel’ and by ‘work of music’, for example. Likewise, it takes seriously and explains how we can encounter artworks and other artefacts – how we can read a novel and see a play as well as read a copy of a novel and see a performance of a play. Secondly it locates repeatable artefacts, and more specifically repeatable artworks, in a broad ontological category with far reaching explanatory virtues. Repeatable artefacts are not obscure entities that call for their own ontology. Rather they represent a familiar (if philosophically slippery) ontological category that encompasses many other things.

The structure of this chapter is as follows. I begin in §3.1 by presenting the ontological task in terms of explaining the phenomenon of repeatability. I argue that to understand repeatability we need to carefully assess those practices that give rise to the phenomenon. We need to see clearly what is going on when we identify something as repeatable. Doing so will reveal a conflict between the identification of sameness and the identification of difference. I will argue that we have no reason not to interpret these as ascriptions of literal identity and non-identity. In §3.2 I consider two solutions to the puzzle of repeatability. The first claims that the entity which is strictly identical in cases of repetition is a set or a class, while the second claims that a repeatable entity is a large scattered object (or scattered event). I argue that while both of these suggestions can handle our practices of identifying repeatable entities to some degree, they ultimately provide unsatisfactory frameworks for understanding the relationship between a repeatable entity and its instances. Instead I defend the view that when we make an identity claim in the case of repeatability we are identifying an entity uniquely suited to be repeatable - namely a kind (§3.3). I introduce the kind/instance relationship as an explanation of repeatability and tie kinds to the notion of an essence. The chapter ends with a discussion of this theory of kinds in relation to the notion of natural kinds. I argue that worries about kinds being highly prolific can be eased by recognising that kinds come in degrees of naturalness.

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<sup>2</sup>I adopt the term ‘mass’ following Quine (1960, p.90), though ‘material’ or perhaps ‘substance’ would be alternative terms to use, if they weren’t so likely to carry unwanted implications.

### 3.1. The Puzzle of Repeatability

The method of conceptual analysis defended in Part I requires that if we want to know what sort of thing a novel is we need to know what sort of thing ‘novel’ in English refers to. The reference of ‘novel’ is partly determined by our understanding of the application and co-application conditions for that term. It is because of that understanding that we have placed novels into the broad ontological category of repeatable things. But what exactly is this phenomenon of repeatability, and what does it imply about the nature of the repeatable thing? In order to bring the question into focus it will be useful to have some concrete scenarios to assess:

#### Example 1

Bill and Jane each give Mary a gift for her birthday. Unfortunately, they both give Mary PRIDE AND PREJUDICE in hardback, aware as they are that Mary is fan of Austen. As such the following statements are both true:

- 1) Bill and Jane both gave Mary the same present for her birthday.
- 2) Bill and Jane gave Mary different presents for her birthday.

(1) is true because they both gave Mary PRIDE AND PREJUDICE. They both gave one and the same novel. (2) is true because Bill and Jane did not club together to get one copy. They both gave different copies.

Here we are identifying and making claims about the novel PRIDE AND PREJUDICE. We are also identifying the copies of the novel. What this scenario highlights, it seems, is that the difference between copies of the novel and the novel itself can be understood in terms of differing ascriptions of identity and non-identity.

#### Example 2

The second example is taken from David Armstrong in a discussion of universals. Armstrong offers the following diagram:



He then asks:

How many words are there in this display? It is obvious that the question has two good answers: There are two words there. There is only one word there. Pierce would have said there are two tokens of one type. (Armstrong, 1989, pp.1-2)

Again, what is going on here is the juxtaposition of an ascription of identity with an ascription of non-identity. There is something identical, which we count as one, while there are also two distinct things.<sup>3</sup>

<sup>3</sup>To repeat a point made in the Introduction, I will henceforth adopt the type-token terminology when discussing words merely as a useful way of distinguishing between ‘word’ as a repeatable and ‘word’ as a non-repeatable.

### 3. Explaining Repeatability

#### Example 3

Thirdly, suppose that Bill and Jane go to see the same film over the weekend. However, they do not meet because they went to different *showings* of that same film. Thus what they went to see was the same, and what they went to see was different. The first claim is about the identity of the film, while the second is about the non-identity of the showings.

#### Example 4

A final example takes us beyond what we normally think of as repeatable entities, but will be useful to highlight what I perceive to be the extent of the phenomenon. Suppose instead of a novel, Bill and Jane both buy Mary the same whisky, knowing how much she likes GLENFIDDICH. Mary is disappointed because she would have preferred to get two different whiskies, rather than getting the same whisky twice. In this case the identification and individuation of the whisky can be contrasted with the identification and individuation of the *bottles* of whisky.

To take these practices seriously is to accept that when we talk of whiskies in this way we are identifying what we might call a ‘mass’ (substance, material) and the relationship between the mass and the samples of that mass (bottles, shots, etc.) appears to follow the same logic as the relationship between word types and word tokens, between novels and copies of novels, and between the films and the showings. Let us say that in each case we have the repeatable entity on the one hand and the *instances* of the entity on the other. The use of this term pre-empts the ontological account defended in this thesis, but can be understood for now just as a useful label.

It is because of this similarity that I am going to argue that each of the four entities discussed belongs to the same ontological category, and in understanding that ontological category we can understand the ontological nature of repeatable entities. However, the examples also bring to light differences within that category that we need to be aware of. The differences lie in the different sorts of instances that a repeatable entity can have. Nicholas Wolterstorff has made a distinction between repeatable works of art that are occurrence-works and those that are object-works (Wolterstorff, 1980, pp.36-37). However, ‘works’ here implies a work of art, and since I am interested in repeatable artefacts more broadly I will adapt the terminology and talk of *occurrence-repeatables* and *object-repeatables*. An occurrence-repeatable is a repeatable artefact that has as its instances events or occurrences. Thus films are occurrence repeatables, as are plays and works of music. An object-repeatable, on the other hand, is a repeatable artefact that has what we would commonly think of as non-occurring objects as its instances. Graphic designs, logos, photographs, and castings are all object-repeatables, as are masses like whisky. Finally, we can note that some repeatable artefacts have instances that are either occurrences or objects. Words, for example, have instances that are inscriptions and instances that are utterances (events). Poems certainly seem to fall into the same

### 3. Explaining Repeatability

category, in that we can identify a poem through an inscription or an utterance.<sup>4</sup>

It is important to note that there is a thorny issue of *what it is* to be an object of this sort that is being ignored here. For example, a logo has as its instances particular images, but one might think that a particular image is *on* an object rather than being an object itself, especially if we think of an image being displayed on a computer screen. A similar puzzle holds for text based artefacts. Is a token of a word an object in itself? It seems to be an arrangement of ink on a page or an arrangement of pixels on a screen, but what exactly is this? A state of affairs? Because my focus here is on the ontological nature of the repeatable entities, I am not going to try and answer these questions. It will be enough to say that image-based and text-based artefacts have instances of *some sort or another*. Moreover, though it is simplifying matters somewhat, I will continue to call all these instances ‘objects’ where the primary demarcating feature is that they are not events. Hence we can treat the object-repeatable/occurrence-repeatable distinction as exhaustive. Fortunately the proposal defended in this thesis does not rely on an articulation of the precise ontological nature of the non-repeatable instances of repeatable entities. For the same reason I will not discuss the nature of events in this thesis.

I want to suggest that these examples present us with the ‘raw data’ of the puzzle of repeatability. There are two word tokens and one word type in the display, but there are not, straightforwardly at least, three separate entities in the display (but see §4.1). But then what *do* we have?

In an attempt to strip our language of as many preconceptions as possible, we can say that there is ‘sameness despite difference’ when there is repeatability. And this is an attempt to capture the notion of repeatability as ‘same again’. However, not just any case of ‘same again’ counts as repeatability. Each day I sit at the same desk even though the day of sitting has changed, and I can identify the same cat at different locations throughout the day, but the desk and the cat are not ‘repeatable’ in the relevant way. What is unique about genuine cases of repeatability is that we do not just have the same entity in different circumstances, or identified under different descriptions, but we seem to have the sameness of an entity being identified ‘in connection with’ the difference of an entity. So when we have a situation in which one entity is repeated twice we have the *same* entity twice but we also have *different* entities. Moreover, we cannot, as it were, physically separate the state of affairs of there being two word tokens with the state of affairs of there being one word type. One could not take a rubber to the display and remove the two word tokens without also ‘removing’ the word type, nor could one make it so that there was no word type there without also removing the word tokens.<sup>5</sup>

We can also bring clarity by contrasting genuine repeatability with the relationship

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<sup>4</sup>Wolterstorff has suggested that all literary (all text-based?) works have diverse instances in this respect, but one might wonder whether, for example, we really do identify the novel itself when we have a reading of the novel (1980, p.38). Fortunately that matter need not be decided here.

<sup>5</sup>With talk of ‘removing’ a type here, there is a danger of begging the question against an abstract account, according to which it is not possible to literally remove a word type. However, all that is being implied is that without word tokens there is no sense in which one can identify the word type.

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between an original thing and a copy of that thing. If prints are produced from a painting it may be that some *image* is repeated across the prints, but the painting itself is not repeated. If we have two prints of a painting side by side, we do not have two different prints and one painting, because we do not have the painting at all. The actual painting may be hanging in some distant gallery. We have seen the print, but it is not true that we have literally seen the painting.<sup>6</sup> Contrast this with two copies of a novel. If I have two copies of the novel, I also have the novel. If I pick up a copy and read it, it would not be right to say that I have not read the novel but only a copy of the novel (as it would be right to say that I have not seen the painting, but only a print or copy of the painting). ‘Copies’ in this case cannot mean what it means for a painting and its prints. The novel, unlike the painting, is repeatable, and the difference lies in the genuine identification of the repeatable thing despite the different instances.<sup>7</sup>

The reason for labouring at this phenomenon is not because it is unfamiliar but if anything because it is *over*-familiar. As Armstrong says, the distinction is ubiquitous (Armstrong, 1989, p.2). There is a danger that we accept the distinction without taking enough care to see what is actually going on when we make the identifications that give rise to the phenomenon. By saying that we are identifying one word twice when we say that there is one type in the display I am claiming that we have here genuine identification of the type going on – we are *identifying something as being literally identical* alongside the distinctness of the tokens. It is essential to my account that such literal identity claims are being made when we identify repeatable artefacts. When we say that Bill and Jane gave the same whisky or the same novel, or when a person says that they saw the same film three times before, I am claiming that a strict and literal identity claim is being made in each case. This is important because, as I have argued, it is these practices which determine what it is that is being picked out.

There is a possible line of objection to my reasoning here that needs to be addressed before we can move on and consider the consequences of this identification. It might be argued that I am reading more into the situation than I am entitled to. One might want to distinguish between strict and literal identity and a more relaxed sense of sameness, and then say that what is going on here is the identification of two distinct word tokens (or novel copies, to use the earlier example) that are the same as each other only in some loose sense. That is, we do not look at the display and literally pick out the same identical thing twice, according to this objection. Armstrong, who once thought this kind of interpretation implausible, came to accept it after considering examples of the

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<sup>6</sup>Though of course there is a *sense* in which one can say they have ‘seen’ a painting when they have only seen a reproduction.

<sup>7</sup>As Peter Lamarque (2002, pp.144-145) has argued, though, we should perhaps be cautious of the assumption that categories of art and artefacts neatly fall on either side of the repeatable/non-repeatable divide (Lamarque talks of ‘particulars’ and ‘types’). Some pictures that we may think of as ‘paintings’ may be such that to see a copy just is to actually see the painting (paintings made using computer software may be like this). In other words, something that we may consider as a painting may also be identified as repeatable. Similarly, some pieces of music may perhaps be identified as non-repeatable so that, as Lamarque puts it, “[t]o have heard the work you would have had to have been present at the performance” (2002 p.145). The point is that whether an entity is repeatable is a matter of how we identify the entity, not of which artefact category it belongs to.

### 3. Explaining Repeatability

identification of parts and wholes (Armstrong, 1989, pp.1-7). I think that Armstrong is mistaken here, but the mistake he makes is quite revealing.

The first example Armstrong considers arises from an attempt to understand the identity of a person over time in relation to Leibniz's Law. Leibniz's Law tells us that when two things are strictly identical they will have the same properties.<sup>8</sup> Now consider the case of a person yesterday and the same person today. The person will have different properties on different days, perhaps being happy one day and sad the next. On the face of it, this might seem like a violation of Leibniz's Law. Does this mean they are not in fact the same person? Very few philosophers draw that conclusion, and rightly so. Armstrong's own suggestion, however, is that following Leibniz's Law we should accept that "the person yesterday is not strictly identical with "the very same person" today" but instead we should say that "what we have when we speak of a person yesterday and the very same person today is identity only in a "loose and popular" sense of the word 'identity'" (Armstrong, 1989, p.4).

Armstrong favours a 'temporal parts' account of existence through time (perdurantism) such that the same person exists yesterday and today in virtue of the existence of different temporal parts of the one person. Thus, according to Armstrong, when we say that the person yesterday is the same as the person today we are actually ascribing sameness to "different temporal parts of a single four-dimensional entity" and thus it is sameness only in a 'loose and popular' sense. The loose and popular sense of sameness occurs, according to Armstrong, when we apply "'the same' to different parts of the same thing" (Armstrong, 1989, p.4).

The second example considered doesn't rely on a commitment to temporal parts, and instead involves two spectators looking at an elephant from different sides:

We can properly say that you two are seeing the same elephant. At the same time, though, we would agree that each of you can only see different parts of that one elephant. So in this case talk of seeing the (very) same thing only amounts to talk of seeing different parts of the very same thing. I am inclined to think that when 'the same' or 'the very same' is used in the loose and popular sense, it always involves applying 'the same' to different parts of the same thing, where that last phrase 'the same thing' has the sense of strict identity. (Armstrong, 1989, p.4)

The inference drawn from these examples is that we might be doing something similar when confronted with repeatability: the two tokens are the same in the loose sense and there is no literal identity claim being made. Thus when we speak of Bill and Jane giving the same whisky, or of someone seeing the same film three times, we would not actually be identifying some entity as being identical.

However, I would argue that Armstrong is describing these situations inaccurately. If we accept the account of persons as having temporal parts we can certainly provide

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<sup>8</sup>Or that whatever is true of one is true of the other, to adopt a formulation from E. J. Lowe (2002, p.41).

### 3. Explaining Repeatability

an explanation of change: the person is sad one day and happy the next because one temporal part is happy and one temporal part is sad. Yet it doesn't follow from this that when we speak of the person yesterday and the very same person today we are only identifying the *parts* of the person. Rather than assume that we are applying a loose sense of sameness to the parts, why not take our language at face value and accept that we are applying sameness to the whole person, which we encounter twice in virtue of encountering temporal parts?

Or consider the example of the elephant. Armstrong says that we must be “applying ‘the same’ to different parts of the same thing”. But this is emphatically not what we are doing when we say that we are seeing the same elephant. When we say that the elephant I am seeing is the same as the elephant you are seeing we are applying ‘the same’ to the *whole* elephant. The ascription of sameness here is strict and literal identity – everything that is true of the elephant I am seeing is true of the elephant you are seeing.<sup>9</sup>

The key point here is that when we say that what you are seeing is the same as what I am seeing, given that we do not mean that we are having the same phenomenal experiences (in which case strict identity claims would be almost entirely impossible), there must always be some implicit understanding of what the ‘thing’ in question is. We cannot simply appeal to ‘that which we are confronted with’ because – recalling the discussion from §1.3 – that would not be enough to pick out anything determinately. The ‘that’ of the ostentation must somehow be made determinate between, for example, the part of the elephant (and then it would be reasonable to ask ‘which part?’) and the whole elephant. Since we are asserting that what you see and what I see are the same, we have every reason to think we are talking about the whole elephant and making a strict identity claim, and no reason to think that we are merely talking about the parts.

If we return to the case of repeatability, the moral is that we should take seriously what appears to be the case: we can recognise the non-identity of the two word tokens and thus count them *as* two, while also recognising and identifying the word type which we ‘count as one’: the word type that occurs on the left of the display is literally identical to the word type that occurs on the right of the display. Rather than write off or explain away these practices we should be looking for an ontology that accounts for them if we want to provide an ontological account of those things we call novels, poems and works of music.

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<sup>9</sup>It might be objected that I am failing to account for the puzzle raised by property ascription and Leibniz's Law. Armstrong introduced the identification of parts because of the worry that that which is said to be sad cannot be identical with that which is said to be happy, as this would violate the Law. However, I would claim – though I cannot argue for it here – that the solution to this puzzle, whatever it may be, should pay heed to the identifications being made rather than vice versa.



### 3.2. Unpromising Solutions

Having set out the phenomenon of repeatability, I now want to consider two potential solutions to the problem which I will argue are unsatisfactory. This will pave the way for the introduction of kinds. Though I argue against these solutions, it is worth noting that they deserve attention here as they both reasonably successfully accommodate our practices of identification and re-identification of repeatable things, and our general understanding of application and co-application conditions. Thus they are both compatible with the methodology set out in Chapters 1 and 2.

#### The Set Hypothesis

To begin with, I will consider, and in due course reject, an explanation of repeatability by appeal to classes or sets. How would an appeal to sets explain the identification of supposedly repeatable entities? Well, it might be thought that when we identify one word type and two word tokens, for example, we are identifying one set of which the two word tokens are members. It might be claimed that we identify the set ‘in virtue of’ its members much as we identify the whole elephant in virtue of its parts. The members are strictly distinct, though the set of which they are members is identical.

This view can be dispatched fairly quickly. One pressing difficulty for this view is that, at least on the most common understanding of sets, whatever members a set has it has essentially.<sup>10</sup> Thus all the copies of *ULYSSES* that exist now must form a different set from the one containing all the copies of *ULYSSES* twenty years ago (given that between now and then some copies have been produced and some have gone out of existence.) Which set should we then identify with *ULYSSES*? Identifying the right set seems somewhat problematic. One might be forced to say that each new copy brings about a new novel (since the collection of copies then form a new set), but then it would be impossible to produce a new copy of a previous novel, and the novel I enjoy now could not be the same novel that my grandfather enjoyed. Sets also face a related modal problem. Given that a set is defined by its members, it could not have had fewer or more members than it actually does have.<sup>11</sup> Yet intuitively, how many instances there are of a given repeatable artefact is a highly contingent matter.<sup>12</sup>

A further worry about this view is that an appeal to sets offers no explanation of why all the copies of a novel or all the performances of the play are grouped together as being instances of the same work. Is it simply that they are members of the same set? But in virtue of what? Sets are defined by their members, so one can’t appeal to the set itself to provide a rationale for which members the set has. The feeling is that

<sup>10</sup>See Wolterstorff (1980, p.44), and Van Cleve (1985).

<sup>11</sup>Dodd (2000, pp.424-425), Rohrbaugh (2003, p.201), and Sharvy (1968).

<sup>12</sup>One could propose a counterpart-theoretic account to avoid this problem. Roughly speaking, on this view, the modal properties of the novel depend on the non-modal properties of the novel’s counterparts in other worlds, and which counterpart relation is relevant depends on the conversational context. The novel (as a set) could then have had different members if there is a counterpart to the novel with different members. See e.g. Lewis (1971), Lewis (1986); also Caplan and Matheson (2006) for similar application.

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the set/member relationship is, to speak loosely, too ‘thin’ to offer any robust account of the relationship between a novel and a copy of the novel. If a theory of sets is going to be viable, then, a great deal of work would need to be done. I think the prospects for such an account are dim.

#### The Scattered Object Hypothesis

Perhaps, though, instead of the repeatable entity being a set and the instances its members, the repeatable entity is an entity that has the ‘instances’ as *parts*. We might suppose that a repeatable artefact is the collection of its instances in the way that a chess set is a collection of its pieces. On this view a novel is a ‘scattered object’ and each of its copies is a part of that object. This view interprets our claims of identity and difference as claims about the identity of the whole object despite the difference of the parts. One initial observation is that this move attempts to account for repeatability using a more familiar ontological distinction, rather than treating it as a basic ontological phenomenon. Repeatability in this case is just the ability of an entity to have multiple parts.<sup>13</sup>

This view avoids many of the difficulties faced by an appeal to sets. Unlike a set, a scattered object can gain and lose parts just as more commonplace objects do (replacing a broken car part does not generate a numerically different car). Similarly, the scattered object could have had more or fewer parts than it actually does have and still be numerically the same object. Furthermore, because scattered objects are not defined by their parts, this theory does distinctly better than an appeal to sets in explaining why two word tokens are tokens of the same word type. For in this case one can make an appeal to the ‘nature of the whole’. While there is a certain amount of hand waving involved in this, it is the same kind of hand waving made when we try to understand what unifies parts into wholes in general. Thus the scattered object theorist can claim that spelling this out in more detail is a general problem for any metaphysical account that admits ordinary objects.

Something like the scattered object view has recently been defended for musical works by Ben Caplan and Carl Matheson (Caplan and Matheson, 2006).<sup>14</sup> Caplan and Matheson complicate matters somewhat by presenting their view as ‘Perdurantism about Musical Works’ and offering repeated close analogies to perdurantism about persons. However, the scattered object hypothesis need not be tied to perdurantism in this way. This is because perdurantism is normally understood as a metaphysical account that interprets common-or-garden concrete objects as actually being ‘event-like’ in that they have duration and temporal parts.<sup>15</sup> However, since a work of music is an occurrence-repeatable, the instances of the work of music – the performances or playings – are

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<sup>13</sup>Though note that it would be strange to say that a chess set was repeatable, simply because we can identify different pieces of the same chess set.

<sup>14</sup>For a similar position applied to works of literature see Ghiselin (1980). Ghiselin’s account is inspired by a scattered object account of species, which will be discussed in depth in §6.1.

<sup>15</sup>Lowe (2002, p.49).

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*already* events with duration and temporal parts. The claim that Caplan and Matheson are making, which is just that the performances are parts of a larger whole, is more simply understood as the claim that they are parts of one larger event.<sup>16</sup> Thus rather than draw an analogy with perdurantism about persons, a simpler analogy would surely be with some other large ‘scattered’ event.<sup>17</sup> We might say, for example, that the performances of a work are related to the work itself in the same way that the individual games in the World Cup are related to the World Cup as a whole – they are shorter events that are parts of a bigger and longer event. An advantage of putting the matter in this way is that one does not need to think perdurantism about persons is at all plausible in order to assess the proposal.

Comparing musical works with large events like the World Cup reveals some interesting results that show that our talk about large events and their parts is not so different from our talk of occurrence-repeatables. For example, a spectator may say that they are ‘watching the World Cup’ even though they are watching only one of the games that make up the whole event. Perhaps, then, when we say we are listening to Beethoven’s Fifth we can similarly do so by listening to only part of the whole work of music. We also talk of a large event and its parts as occurring at the same time. The World Cup is going on *and* the match is going on. Moreover, the world cup is going on because of (or *in virtue of*) the playing of the match. Notice how this parallels the claim that the song is being played *and* the performance is under way, and that the song is being played because the performance is under way.

Having compared an occurrence-repeatable to a large scattered event, we can similarly compare an object-repeatable to an ordinary scattered object (in which case the associations with perdurantism are even less helpful). Again there is some overlap between the way that we talk about scattered objects and their parts and the way that we talk about repeatable artefacts and their instances. A pair of gloves is a scattered object with which we are familiar. If a pair of gloves is placed on a table and we ask ‘how many objects are on the table?’ we have an ambiguity which might be seen to echo the ambiguity in the earlier question ‘how many words are there in the display?’ There is one *pair* of gloves but two *individual* gloves; there is one word *type*, but two word *tokens*.

A similar line of thought is pursued by Quine in relation to ‘masses’ such as water and sugar (Quine, 1960, pp.90-100). Here Quine notes that certain terms (‘water’, ‘sugar’, ‘gold’) take the grammatical role sometimes of singular and sometimes of general terms. In predicative sentences they may appear before the ‘is’ as a singular term (‘water is fluid’), or after the ‘is’ as a general term (‘that puddle is water’). I want to leave aside

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<sup>16</sup>Though the theory applied to occurrence-repeatables would more accurately be called a ‘scattered event hypothesis’ for simplicity I will apply ‘scattered object hypothesis’ to cover the application both to occurrence-repeatables and to object-repeatables.

<sup>17</sup>They might reply by saying that their account is *technically* a perdurance account nonetheless. My point is that emphasising this seems to be beside the point given that instances of works of music are already events. Moreover, if they were to extend their theory to object-repeatables such as novels, the truth of perdurantism becomes entirely irrelevant (see below). One could hold a scattered object account of novels without accepting that that scattered object was a perduring entity.

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the role as general term for now and look at Quine's interpretation of a mass term in subject position. Quine writes:

A mass term used thus in subject position differs none from such singular terms as 'mama' and 'Agnes', unless the scattered stuff that it names be denied the status of a singular sprawling object. (Quine, 1960, p.98)

Thus when we say 'Water is fluid' we are referring to a scattered object which is the "aqueous part of the world" that has "sundry parts which are lakes, pools, drops, and molecules" (Quine, 1960, p.98). If we accept, as I have proposed, that masses fall into the same ontological category as repeatable artefacts then we can interpret this as a proposal that the instances of water are *parts* of water.

The scattered object hypothesis has much in its favour. As well as what I have claimed about the similarities between our talk of repeatable artefacts and their instances, and our talk of scattered wholes and their parts, this view also has the advantage of reducing repeatability and the ontological nature of repeatable entities to something we are already familiar with. One might take that to be a significant advantage of the view, citing simplicity and parsimony. If we can get away only with the ontological relationship between parts and wholes then something that appeared complex will have been reduced to something more simple. The scattered object hypothesis ought not be dismissed lightly.

However, it should still be dismissed. Once we start looking at the problems this view faces, its appeal fades away. Firstly, our ordinary language betrays as many differences as similarities. As Dodd argues, to accept this assimilation would be to accept that works of music can (almost) never be heard in their entirety, and this is a hard pill to swallow (Dodd, 2007, p.157).<sup>18</sup> It is quite right that we are normally willing to accept that one can hear or see something by hearing or seeing a part of it – just as we see the elephant by seeing a part of the elephant, and as we see and hear the fireworks even if we only see the last half of the display. But in none of these cases do we claim to see or hear the *whole* entity by seeing or hearing a part of it. When I catch the last half the firework display I cannot claim that I saw the whole display, because I saw only part of that event. Similarly, though one can watch the World Cup by watching a match within it, one cannot watch the *whole* World Cup by watching just one match. Nor can one study a single piece of a chess set and claim to have studied the *whole* chess set. Yet there is a very strong intuition to the effect that we do hear the whole of a work of music when we hear a single whole performance, and we do see the whole photograph when we see a single print of the photograph.

The same discrepancy can be highlighted by the fact that if a teacher asks a child to write the word 'cat', and the child inscribes 'c-a-t' the teacher will not complain that the child has written only part of the word, and if I ask you to perform Greensleeves and you play the song through, I will not complain that you have played only part of Greensleeves. Likewise, as Eddie Zemach argues, if I ask for water and you bring a cup

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<sup>18</sup>See also Zemach (1970, p.243).

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of water, I cannot complain that you have brought me only part of water. Yet if I ask for my pair of gloves and you bring only one glove, or if I ask for the chess set and you bring only the white king, just such a complaint will be appropriate (Zemach, 1970, p.243).

One of Caplan and Matheson's responses to this line of attack is to say that they are only as badly off here as the perdurantist about persons is – for they have to say that contrary to supposition even when a doctor inspects a patient from head to toe they have not seen the whole person but only a temporal part of the whole (Caplan and Matheson, 2006, p.62). The idea is that we have an intuition about seeing the whole person, but the intuition is wrong given perdurantism. Therefore our intuitions may well be wrong about works of music and photographs. However, having separated the scattered object hypothesis from perdurantism, this move has far less argumentative force. We see merely that there are parallel problems for perdurantism about persons and for the scattered object hypothesis, without reason to believe that either is correct.

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The scattered object hypothesis also suffers because there is an unexplained asymmetry between wholes and parts on the one hand and repeatable entities and instances on the other. According to the hypothesis, an entity is an instance of a repeatable entity if it is a part of a whole. However, even if we accept that every instance is a part of the whole, it is not the case that every part of the whole is an instance. For example, if we think of a work of music as a large scattered event, an individual performance, which we recognise as an instance of the repeatable entity, would be a part of the event. However, the opening ten bars of the performance will also be a part of the large scattered event, but the opening ten bars cannot properly be identified as an instance of the repeatable entity. If instances just are parts, as the scattered object hypothesis maintains, then the hypothesis cannot account for this asymmetry. In other words, for the scattered object theorist, being a part of the whole is a necessary but not sufficient condition for being an instance. The theorist must then provide some *further* sufficiency criteria. It is hard to see how this will not be ad-hoc and available only on a case by case basis. Moreover, the need for these additional conditions demonstrates that the relationship between a repeatable entity and its instances is not fully captured by the part-whole relationship.<sup>20</sup>

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<sup>19</sup>Dodd has a further objection against Caplan and Matheson's perdurantism about musical works but this becomes moot once we separate perdurantism from a scattered object (or scattered event) theory. The objection was that musical works as perduring entities would be "ontologically multifarious", having unified temporal parts when there was only one performance occurring but scattered temporal parts when there were two or more concurrent performances. If musical works are thought of as scattered events, however, this problem does not arise as there seems to be nothing preventing the same event occurring sometimes in one location and sometimes in two or more distinct locations (Dodd, 2007, p.158).

<sup>20</sup>Note that the scattered object hypothesis here is similar to a take on universals that Armstrong calls mereological nominalism. On this view *a is F* obtains in virtue of *a* being a part of the aggregate of all the Fs. Armstrong rejects the view on the grounds that while it may be a necessary condition of *a*'s being F that it is part of the aggregate of F's, it is not generally sufficient. Something can be part of the aggregate of F's though not itself be F. See Armstrong (1978, pp.34-35).

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This asymmetry leads Quine into difficult waters in his discussion of masses as scattered objects. After claiming that ‘water’ in subject position (e.g. as in the phrase ‘water is fluid’) refers to a single scattered object he goes on to consider the possibility that ‘water’ functions as a singular term even when it appears after the ‘is’ of a predicative sentence. In these cases the ‘is’ would be interpreted as ‘is a part of’, so that when one points to a pool and says ‘That pool is water’ this means that that pool is *a part of* the scattered object that is water. An earlier comment suggests Quine is committed to this: “There remain, besides the world’s water as total scattered object, sundry parts which are lakes, pools, drops, and molecules” (Quine, 1960, p.98). However, Quine rejects treating water as a singular term when it appears after the copula ‘is’ on the grounds that “there are parts of water, sugar, and furniture too small to count as water, sugar, furniture” (Quine, 1960, p.98). In other words, his problem is that claims that something is a part of water do not coincide with claims that something is water. Though the instances are parts, not every part counts as an instance. His response is to treat mass terms as general terms when they appear after the copula, and they thus have a protean character, being “singular in the subject and general in the predicate” (Quine, 1960, p.98). Quine’s disjunctive account of the role of kind terms might provide a fix to the asymmetry problem, but he still must then explain the nature of the general term ‘water’ and why, if ‘water’ as singular term refers to a large scattered object, the general term only applies to certain (sufficiently sized) parts of that object.

In short, though the scattered object hypothesis looked promising as an explanation of the basic ascriptions of identity and difference surrounding repeatable entities and their instances, once we take a closer look at the relationship between wholes and parts that the scattered object hypothesis requires, we can see that the explanatory power of the theory breaks down. Nevertheless, what is noticeable about the scattered object hypothesis is that it very closely accommodates our understanding of application and co-application conditions, departing only perhaps with the asymmetry problem noted above. As such, this theory cannot be ruled out simply by conceptual analysis of the kind defended in Chapters 1 and 2. The scattered object hypothesis should rather be rejected on broader theoretical grounds. Note then that we *could* bite the bullet in the face of all conflicting evidence. The problem is that we have been given no reason to do such a thing. The evidence points to the fact that the scattered object hypothesis is a far from ideal theory. If there are better contenders available, we should prefer them.

### 3.3. A Theory of Kinds

If repeatability doesn’t involve the identification of a class or set and its members, and it doesn’t involve the identification of a scattered object and its parts, then what is being identified? The answer I defend here is that the phenomenon of repeatability is best explained by an appeal to the ontological category of *kinds*. What is strictly and literally identical in each of the discussed cases is a kind. What are kinds? The short answer is that kinds are entities that are repeatable in the way described above.

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In other words, I want to suggest that in identifying repeatability we are identifying a category defining phenomenon. Repeatability is not something to be explained away, but rather points to a unique ontological category.<sup>21</sup>

This does not mean, however, that kinds have been invented purely to plug a hole. Rather they provide what has been called a ‘self evidencing explanation’ (Hempel, 1965, p.370). That a person walked by wearing snow shoes explains the snow tracks in front of the house, even though those snow tracks are the only evidence for such an explanation. Likewise the ontological category of kinds explains the data of repeatability even though it is that data – the identifications of sameness and difference discussed above – that provide the very evidence for there being such an ontological category. As Peter Lipton points out, even though these explanations have a distinctive circularity, “the circularity is benign: it spoils neither the explanation of the track nor the justification for the belief that someone did pass on snow shoes” (Lipton, 2004, p.24). Thus we need not worry that appealing to a theory of kinds in this way is cheating. We have justification for believing in kinds in the form of repeatability, and kinds themselves provide the ontological explanation of repeatability.

In discussing the ontological nature of kinds, we cannot avoid discussing the relationship between kinds and instances. However, we can approach the relationship either from the direction of the instances, or from the direction of the kinds. Both will be useful. I will begin with the former, by looking at what it is to be an instance of a kind. We can understand this by appeal to the pre-philosophical notion of entities being of the same kind, type or sort. When we say that two entities are of the same kind in this sense, we mean usually that the entities have something in common – they share certain properties. To say that two things are ‘of the same kind’ can be understood as a shorthand way of saying that they are both instances of the same kind. Thus two entities are instances of the same kind in virtue of sharing certain properties. It is *because* they share those properties that they are instances of that kind. These properties are the essence of the kind, for these properties are essential (in a way that will be clarified shortly) for an entity to be an instance of the kind.

If we approach the relationship instead from the direction of kinds themselves, we can say that a kind is something that is instanced whenever the essence of the kind is instantiated. The kind itself is *repeatable* because it can have multiple instantiations. When we identify two word tokens and one word type, or two copies of a novel and one novel, we are identifying two instances of a kind and one kind of which there are two instances.

I have argued that in cases of repeatability we genuinely identify something as being strictly identical despite the difference of the instances. Here I am claiming that when

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<sup>21</sup>The following account owes much to Wolterstorff’s understanding of kinds (Wolterstorff, 1980). However, my approach differs to his in an important respect. Wolterstorff introduces kinds by way of a list of formal definitions, and then seeks to demonstrate that those definitions describe a category that accounts for repeatable works of art. My own approach looks instead to describe the nature of kinds first and foremost by drawing on our practices of identifying and individuating repeatable entities.

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this happens we identify an entity that is individuated not by spatial or temporal location, or by spatio-temporal continuity, but by an essence. To help make clear what is going on when we identify a kind like this, it can be helpful to entertain a fiction in which a person *only* identifies the kind without also identifying or individuating the instances. This will allow us to recognise the process involved in the identification of kinds without the distraction of identifying instances.

Just such a notion has been developed by P. F. Strawson.<sup>22</sup> Strawson's aim is to isolate the logical point at which ordinary particulars are introduced into thought. To this end, he considers the possibility of what he calls 'the naming game'. In the naming game, we imagine that the speaker names only kinds and is blind to the individuation of the particular instances of the kinds that he names. Thus the speaker utters "the general name for a kind of thing in the presence of a thing of that kind" (Strawson, 1959, p.202). So in the presence of a ball the speaker says 'ball' and in the presence of a duck the speaker says 'duck' etc. What the speaker is *not* doing in this game is naming a particular duck or ball, nor are they saying 'there is a duck' or 'there is a ball'. Each kind has what Strawson calls 'corresponding features', and when the features are recognised the name of the kind is uttered. Though Strawson resists regarding the kinds being named as actual entities, talking instead of "feature universals or feature placing concepts", my claim here is that the person involved is making a genuine identification of kinds – an entity that is found whenever its corresponding features (i.e. its essence) are found (Strawson, 1959, p.202). We might imagine a person who can identify only kinds being presented with the question asked by Armstrong: how many words are there in the display? Their answer would be that there was only one word, and if we tried to convince them otherwise by pointing first to the left inscription and then to the right they would merely say that we had pointed to the *same thing twice*. They see and identify the kind, but not the individual instances.

Of course, none of us lack the ability to identify instances in this way – whenever we identify a kind we also recognise an instance. Hence we say that not only is there one word type in the display, there are also two word tokens. Our ordinary practices approach this fiction most closely when we identify masses, in which cases there is the identification of a kind with little thought of the instances. When a child looks out of the window and remarks 'There's snow!' or simply 'Snow!' they are not naming an ordinary individual but rather recognising and naming the kind snow, rather than giving a name to this fall of snow, or this pile of snow.

#### The Importance of Essence

Central to the introduction of kinds given above is the notion of an essence: the set of properties that the kind requires of its instances. The idea of every kind having an essence is familiar, both in realist discussions of natural kinds and in discussions

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<sup>22</sup>Strawson (1959, pp.202-209), see also Quine (1960). Strawson's discussion was brought to my attention by Zemach (1970).



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that favour only nominal kinds. I will then address the question of ‘naturalness’ in the next section, but for now we can note that whether essence is a hidden trait to be ‘discovered’ in things of the same natural kind, or whether it be seen as a mere Lockean ‘workmanship of the understanding’ (nominal essences), I would argue that it is difficult to get a grip on the notion of a kind without thinking in terms of essences (Locke, 1964, Book 3, III).<sup>23</sup> The essence is for the kind as spatio-temporal continuity is for ordinary persisting objects: the essence provides us with a rationale for identifying and individuating the kind. What the essence is in any given case may not be obvious, and we may think there is an essence that a collection of objects share so that they are all of the same kind and be mistaken in this. But if the unifying role of the essence is rejected completely I would claim that we have nothing more than a group of objects that happen to be called by the same general name.<sup>24</sup>

I have said that we cannot understand the concept of a kind without an essence. However, one might question this. Wittgenstein famously remarked that, if we *really* look, we will see that there are no necessary and sufficient conditions for an object’s falling under many of our general terms:

Don’t say: “There must be something in common, or they would not be called ‘games’” – but look and see whether there is anything common to all. – For if you look at them you will not see something that is common to all, but similarities, relationships, and a whole series of them at that. (Wittgenstein, 1953, §66)<sup>25</sup>

Should this be seen as a counter example to the claim that kinds have essences? My tentative suggestion is that it should not, for if Wittgenstein is right (and it is not certain that he is) about the term ‘game’, why not conclude from this that the given general term appears not to correspond to any single kind? If Wittgenstein is right then we will look at all the things that we call games and discover that contrary to supposition, they are not all of a kind. Moreover, the fact that we expect there to be something that all the things called ‘games’ have in common points to the centrality of essence in our aims to identify kinds. There is certainly scope for taking the discovery about ‘games’ as a failure of these aims, rather than as a truth about the nature of kinds. As an analogy, consider the name ‘The FA Cup’. We might suppose that this names an individual cup which is passed from team to team each year. If we then discover that, in fact, over the years there have been a number of distinct cups used interchangeably, so that there is no ‘one’ FA Cup, we would not go on to conclude that ordinary persisting objects do not follow continuous paths through space-time, and can be in two places at once. Rather we conclude that ‘The FA Cup’ does not uniquely pick out a single object. Likewise I suggest that we should not conclude from our use of ‘game’ that kinds

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<sup>23</sup>It is interesting to note that recent work in psychology suggests that from a young age children operate with the assumption that kinds have essences that are causally responsible for other non-essential properties of instances. See Ahn et al. (2001).

<sup>24</sup>See Hacking (1991, p.115).

<sup>25</sup>See Armstrong (1978, p.76) for a similar worry.

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don't have essences, but rather that not every general term successfully corresponds to a single kind. Certainly in the debates over the ontological nature of species (discussed in §6.1), the presumed absence of any properties that all and only members of a given species possess is taken as a knock down argument against the claim that species are kinds. My response to the Wittgensteinian position is given in this spirit.<sup>26</sup>

An alternative objection to the view that kinds have essences might arise from the thought that even in cases in which entities of a kind share more than “family resemblance” it would seem disingenuous to insist that one can always state whether or not an entity is an instance of a kind thanks to some necessary and sufficient conditions.<sup>27</sup> Given this, we can note that although every kind has an essence, it does not follow that there need always be a fact of the matter about whether or not a kind is instantiated at a given point. An aversion to either semantic or (more likely) metaphysical indeterminacy might make one sceptical of these scenarios, but there is nothing about kinds themselves that rules this out. The actual extension of a kind – that is, where the kind is actually instanced at a given time – may be indeterminate ‘around the edges’, in much the same way that we might identify a mountain but not know for every location,  $\ell$ , whether  $\ell$  is within the boundaries of the mountain. Whether such indeterminacy is at root purely semantic, or whether it is also (or primarily) metaphysical is a matter of some contention, and I will not try to settle it here.<sup>28</sup>

Either way, there is no need to think that an ontology of kinds committed to essences poses a special problem here. If there is metaphysical indeterminacy then kinds will share in that indeterminacy; if all indeterminacy is semantic, then neither will kinds be exempt from that. The upshot of this is that a commitment to kinds having essences commits one neither to holding that every general term picks out a unique kind, nor to holding that it will always be determinate whether or not a kind is instanced. We can be sure that natural language is messy, and it may be that the world is also, but there are entities to be referred to nonetheless.

#### The Essential Nature of Kind Essences

I have argued that essences should play a central role in our notion of kinds. Can this claim be strengthened further? It follows from the discussion above that we pick out and identify kinds according to the instancing of certain properties. Two things are ‘of the same kind’ when they share properties, and which kind they are both of depends on which properties they share (so that some third thing may be of the same kind as the first two if it also has those properties).

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<sup>26</sup>It might be further objected my position is theoretically motivated, and that Wittgenstein has shown that our ordinary notion of some common names does not admit of essences. I agree, however, that this position is theoretically motivated. The motivation comes from the clarity that essences bring to kinds. Notice that if ‘game’ was to be brought into some kind of scientific taxonomy, the first thing to do would be to ‘clean up’ the concept of a game by applying stipulated necessary and sufficient conditions.

<sup>27</sup>See Boyd (1991, p.142).

<sup>28</sup>But see Williams (2008) for a useful introduction to the topic.

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Given this, we can reasonably make the following individuating claim:

**(PIK)** For kinds  $K_1$  and  $K_2$ , and the respective required properties  $P_1$  and  $P_2$ ,  $K_1 = K_2$  if and only if  $P_1 = P_2$ .

Some immediate consequences of this are that if  $K_1 = K_2$ , anything that is an instance of  $K_1$  at time  $t$  is an instance of  $K_2$  at  $t$ , and if it is possible for there to be, at  $t$ , an entity that is an instance of  $K_1$  and not an instance of  $K_2$ , then  $K_1 \neq K_2$ .

A further result of this individuating claim is that a kind cannot change with respect to the properties it requires of its instances over time: If at time  $t_1$ , a kind  $K_1$  requires that its instances have properties  $P_1$ , and at time  $t_2$  a kind  $K_2$  requires that its instances have  $P_2$ , and if  $P_1 \neq P_2$ , it follows that  $K_1 \neq K_2$ . Arriving at this result involves taking the individuating claim at face value, and not relativising the identity of the required properties to a time. For example, Van Cleve, in a discussion of the essential nature of set membership, mentions a principle for individuation for ‘bodies’ which is sometimes given as follows: necessarily, for bodies  $x$  and  $y$ ,  $x = y$  if and only if  $x$  and  $y$  occupy the same place (Van Cleve, 1985, p.589).

If this principle is to be plausible, of course, we have to include a temporal dimension, perhaps reading ‘place’ four-dimensionally. This is just because bodies change place through time without ceasing to be the bodies that they are. The individuating claim given for kinds, however, should be read as implying that kinds do *not* change the properties they require of their instances over time. The reason for insisting on this is that if this was not the case an entity at  $t_1$  and an entity at  $t_2$  could be ‘of the same kind’ despite not sharing the relevant properties, a scenario which violates the intuitive principle according to which kinds were introduced. Kinds are thus *temporally inflexible* with respect to the properties required of instances (c.f. §5.4).

There is a further reading of PIK that is widely assumed, and that I will accept here, but which is somewhat harder to decisively prove. This is that if a kind,  $K_1$ , requires that its instances have properties  $P_1$ , then  $K_1$  could not have required anything other than  $P_1$  from its instances. More formally, for kinds  $K$ , possible worlds,  $w$ , and required properties  $P$ : if  $K_1$  requires  $P_1$  in  $w_1$ , and  $K_2$  requires  $P_2$  in  $w_2$ ,  $K_1 = K_2$  if and only if  $P_1 = P_2$ .

Kinds are thus *modally inflexible* with respect to the properties required of instances (c.f. §5.3).<sup>29</sup> It is important to note that the truth of this claim does not follow (straightforwardly) from the truth of PIK. This can be seen by noting that other principles of individuation do not entail modal commitments. For instance, the principle of individuation for bodies mentioned above may be true (if ‘place’ is understood four-dimensionally) without that entailing that bodies occupy the places they do occupy essentially. Similarly, Davidson’s account of the individuation of events (according to which events  $x$  and  $y$  are the same event if and only if they have the same causes

<sup>29</sup>This is assumed in LaPorte (2004, p.61), and in Bird (2009). Wolterstorff’s account of kinds also appears committed to just such a condition. The equivalent claim is also made for types: types are modally inflexible with respect to the properties they require of their tokens. See Rohrbaugh (2003), Dodd (2007, pp.53-54) and Caplan and Matheson (2008, p.500).

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and effects) seems not to imply that events have their causes and effects essentially (Davidson, 1969). Van Cleve has even argued the stronger claim that “no essentialist consequences ever follow from any [principle of individuation]” (Van Van Cleve, 1985, p.589). A similarly strong claim has been made more recently by Guy Rohrbaugh who argues that “[a]nswers to individuation questions never settle questions of what is called cross-world identity” (Rohrbaugh, 2005, p.212).

However, although Rohrbaugh and Van Cleve might be right about the kinds of individuating claims that they are considering, I think there is room here to strengthen a principle of individuation so that essentialist consequences do follow. Let us call the individuating claims with no essentialist consequences *weak* individuation claims, and individuation claims with essentialist consequences *strong* individuation claims.<sup>30</sup> If Davidson’s principle for events does not have essentialist consequences, it is a weak individuation principle. It allows us to recognise identity and distinctness in the *actual* world, but does not tell us whether or not an event *could* have had different causes and effects. Guy Rohrbaugh’s consideration of authorship is similar. Rohrbaugh argues that we might decide that paintings with different authors cannot be the same painting without thereby committing ourselves to the claim that a work could not have had a different author (Rohrbaugh, 2005, p.212). As far as this *does* constitute a principle of individuation, it is only a weak principle.<sup>31</sup>

A strong individuation principle, however, provides a criterion for sameness and distinctness for both actual and possible entities that fall under the principle. If Davidson’s principle was given a strong reading, it would imply that an actual event would be the same event as some possible event if and only if the events had the same causes and effects. Though this reading may not be acceptable for Davidson’s principle, what is widely assumed, and what I will accept here is that the strong reading *is* acceptable for PIK. It provides us with a criterion not just concerning the identity of kinds in the actual world, but also concerning identity of kinds across possible worlds.

Why should PIK get the strong reading? I think that the most promising defence of this position involves arguing that an entity in  $w_1$  and an entity in  $w_2$  *must* share the same properties if they are to be (instances) of the *same* kind (as an entity at  $t_1$  and an entity at  $t_2$  must within the same possible world). That is, it can be argued that our intuitive notion of ‘being of the same kind’ applies across possible worlds as well as

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<sup>30</sup>Caplan and Matheson (2008, pp.501-502) introduce a similar but slightly finer distinction. Where I have weak and strong individuation, they recognise *weak*, *medium* and *strong* individuation conditions. According to their weakest level of individuation, an entity is individuated by a property if that entity is *contingently* the only entity in the actual world with that property. A medium principle of individuation implies instead that in any possible world in which an entity has that property, only one entity has that property. Thus according to Caplan and Matheson, Davidson’s principle for events is a *medium* rather than a *weak* individuation principle (2008, p.504). Their strong principle corresponds to my strong principle. I have no problem with this finer three-tier system, but it is not needed for our purposes since the emphasis here is on those principles that *have* essentialist consequences, and those that do not. Caplan and Matheson agree that only the strong principle delivers the modal essentialism under discussion.

<sup>31</sup>We can note that the condition only constitutes a necessary condition for individuation – sameness of author is *not* sufficient for sameness of painting. Moreover, we can note in passing that it is not clear that such a claim is not merely a consequence of Leibniz’s Law.

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across different times in the actual world.

So, for example, if two entities are instances of kind  $K_1$  in virtue of having properties  $P_1$ , then it is impossible that they could have been instances of  $K_1$  without possessing properties  $P_1$ , for had they failed to have properties  $P_1$ , they would not have been of the same kind as they actually are. (There is no possible world in which entities are instances of  $K_1$  without possessing  $P_1$ ).

However, there is a worry here that this argument relies on the assumption that the same kind relationship must be trans-world and thus begs the question in favour of modal inflexibility. If kinds *were* modally flexible, it might be argued, then the entities could have been of the same kind despite lacking  $P_1$ , because the very same kind could have required that its instances had properties other than  $P_1$ . To say otherwise is just to assume the conclusion we set out to prove.

The problem, of course, is the familiar one of trans-world identity. The position that is widely assumed and that we are trying to defend is that kinds are identified across worlds, as well as within a world, according to the properties they require of their instances. But providing independent grounds for trans-world identity conditions is notoriously difficult.<sup>32</sup>

Perhaps the argument given already can be strengthened by insisting that if we give up on the modal inflexibility of the properties required of instances, then we give up on the only grip we have on kind identity in any circumstances. This kind of reasoning is sometimes offered with respect to sets and their members. It is claimed that a given set could not have had members other than the members it does have, and a supposition to the contrary involves misunderstanding what it is to identify a set in the first place.<sup>33</sup> As far as sets go, a change in members is a change in the set, and likewise it might be argued that a change in essences for kinds just *is* a change in kind; it is a consequence of what we mean by kinds that a kind could not have required different properties of its instances. We could say that as a set is defined by its members, so a kind is ‘defined’ by the properties required of instances.

However, the air of question begging is still not cleared: it might yet be asked *why* kinds should be understood in this way? If that is just what we mean by kinds, why do we mean that and not something else? I will not try and settle this matter here conclusively. Rather, I will follow tradition and take modal inflexibility as an assumption, for which at least some argument has been offered. In other words, I will assume that the essences of kinds are essential to kinds, and thus are *essences* in the usual sense of that term. We can at least note that to depart from this line, though it may not be as strongly grounded as we would like, would involve quite a considerable departure from standard interpretations of kinds.

What sorts of properties can be part of the essence of a kind? I see no reason not to allow that the essence of a kind can include any property whatsoever. Hence a kind may demand that its instances have internal or external properties, as those are traditionally

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<sup>32</sup>See, e.g., Chisolm (1967), Plantinga (1974), Loux (1979), Forbes (1980), Lowe (2002).

<sup>33</sup>See, e.g., Van Cleve (1985, p.585).

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understood, with external properties including relational properties. A kind may require that its instances have a certain chemical composition, or that its instances be loved by Socrates, or both.

Given this relationship between kinds and their essences, it is possible that at a given time in the actual world every instance of one kind exactly ‘overlaps’ with every instance of another kind though the kinds are not thereby identical. For example, suppose that as a matter of coincidence (though obviously not pure coincidence), every swan that exists at a certain time is white. Then every instance of the kind SWAN will also be an instance of the kind WHITE SWAN at that time. However, there is no barrier to the coincidence being broken at a later date following the birth of a brown swan.

Using this example we can also see that the kind SWAN and the kind WHITE SWAN are related so that whatever is an instance of the kind WHITE SWAN will necessarily be an instance of the kind SWAN but not vice versa. This is because the essence of WHITE SWAN ‘includes’ the essence of SWAN. In Wolterstorff’s terminology the kind WHITE SWAN is a *species* of the kind SWAN (Wolterstorff, 1980, p.55).

We can note finally that on this understanding kind terms are rigid designators in the following sense: a kind term ‘K’ picks out the same kind in every possible world – that is, the kind with the essence such that instances must have properties P. However, a kind can have different instances in different possible worlds, as far as it is possible for different objects to possess the demanded properties. So a kind does not have the instances it actually has essentially. The theory of kinds espoused here is also neutral as to whether or not entities that are instances of a kind are instances of that kind essentially.<sup>34</sup>

#### 3.4. Natural and Unnatural Kinds

With this description of kinds in place one might wonder firstly what this has to do with the familiar appeal to natural kinds, and in particular the claim that natural kinds are the only ‘real’ kinds, and secondly whether kinds on this account turn out to be worryingly prolific: if kinds are individuated by their essences and there are no theoretical restrictions on what those essences can be, does it not follow that we live in a world populated by seemingly limitless numbers of overlapping kinds? Is there such a gerrymandered kind which has as its essence the requirement that its instances be currently on my desk? Surely we would not identify some ‘thing’ as being identical across the different items on my desk in the same way that we identify a novel or a song?

Let me consider two approaches to this worry. The first approach involves biting the bullet on proliferation, but sweetening the pill by noting that only some of those many or even infinite kinds will be interesting or relevant to us. We can draw a parallel here between this issue and the fact that an ontological account of the nature of ordinary

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<sup>34</sup>For discussion see Okasha (2002), and LaPorte (1997).

### 3. Explaining Repeatability

persisting objects still faces what van Inwagen called the ‘special composition problem’ which asks the difficult question of when a plurality of objects composes some other object (van Inwagen, 1990). The question is motivated by the thought that many collections of things do seem to compose legitimate entities, but if composition were granted for every possible collection we would have to admit a plethora of gerrymandered objects such as the fusion of my pen lid, the second story of the Empire State Building, and Gordon Brown’s left ear. One might respond here by allowing that strictly speaking every possible fusion does exist, though many of them are uninteresting to us.

However, discussions of natural kinds sometimes have more than this in mind, and make the claim that what is natural should be equated with what is real, or what exists in “ontological strictness” (Elder, 2007, p.44). Would this cause problems for the realist view of repeatable artefacts as kinds being defended here? I think not, for dominant interpretations of naturalness still leave room for repeatable artefacts.

Returning to the special composition question, my inclination, though I will not argue for it here, is to reject the need for a single universal answer that covers every case.<sup>35</sup> I prefer instead to take the lead of David Lewis who has defended the view that eligibility for reference is a matter of degree, with some things being more naturally eligible as referents than others:

The mereological sum of the coffee in my cup, the ink in this sentence, a nearby sparrow, and my left shoe is a miscellaneous mess of an object, yet its boundaries are by no means unrelated to the joints in nature. It is an eligible referent, but less eligible than some others. (I have just referred to it.) (Lewis, 1984, p.227)<sup>36</sup>

Similarly, some kinds might be regarded as being quite natural and eligible entities (e.g. gold, water) while others will be far more ‘grue-like’ and lie at the other end of the scale (e.g. LEFT HANDED PEOPLE BORN ON A TUESDAY).

The view that the naturalness of kinds is a matter both of perspective and degree is familiar and well defended in the philosophy of science. Ronald de Sousa argues persuasively that naturalness is not an objective binary matter, but will always depend on particular interests and epistemic priorities (de Sousa, 1984). Ian Hacking also rejects the idea that there is an objective and exhaustive list of natural kinds as being something that “does not make sense, not even as an idea to which we strive” (Hacking, 1991, p.111). It doesn’t follow from this that there are no criteria for recognising naturalness, or that every kind is ‘as good as’ any other. Joseph LaPorte has argued, for example, that “the naturalness of a kind consists in its explanatory value” (LaPorte, 2004, p.20). While ‘explanatory value’ may well be perspectival, it doesn’t follow that naturalness is a myth – it is at least as real as explanatory value. Also important in LaPorte’s discussion is the idea that what counts as natural will be relative to a context,

<sup>35</sup>For a defence of this view see Thomasson (2007a, pp.126-236).

<sup>36</sup>Note that ‘natural’ here is not to be understood as being opposed to ‘man-made’. A desk may be highly natural as a referent in a way that the fusion of a random collection of ‘naturally occurring’ objects would not be. See also Lewis (1983, p.372).

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much as what counts as ‘flat’ will depend on context. In some contexts, standards for naturalness will be very high. In many scientific contexts, it may seem that the only truly ‘natural’ kinds are those that are included in a description of the universe in terms of fundamental physics. In other contexts many more kinds will count as being natural. As LaPorte points out, Green-Kind is generally assumed to be quite un-natural, since it has a fairly eclectic collection of things as instances (green trees, green frogs, grass, green cars etc). Yet compared with a Goodman style Grue-Kind it is quite natural (LaPorte, 2004, pp.23-27).

If repeatable artefacts are kinds, as I have argued, they are not gerrymandered in the way that the kind LEFT HANDED PEOPLE BORN ON A TUESDAY seems to be. For a start, they are familiar, regularly referred to and (often) easily identified. In fact, we will see in Chapter 6 of this thesis that many repeatable artefacts are similar in important respects to biological kinds and that the motivations for regarding biological kinds as natural transfer comfortably over to novels, photographs and the like.

My aim here is not to defend any particular theory for judging naturalness in a given context, but merely to make use of the popular notion that naturalness is not an ‘all or nothing’ matter.<sup>37</sup> Significantly for us, just because novels, films and plays are called ‘artefacts’ it doesn’t follow that they are unreal, even if one favours the view that we should not admit every possible kind (or every possible fusion) into an account of what exists. If naturalness is a matter of degrees there will be no neat natural/artefactual divide that corresponds to a real/unreal divide.<sup>38</sup>

## Conclusion

There are many things that might interest us about novels, works of music, plays, films and photographs. I have argued that from the perspective of metaphysics it is the repeatability of these entities that both groups them together in an interesting way, and introduces an important ontological question. We must ask ourselves how it is possible that there can be two different inscriptions but only one single word, or how two people can see different showings but also see the same film.

The best explanation of these scenarios, one that takes our understanding of application and co-application conditions seriously (and as innocent until proven guilty) appeals to the ontological category of kinds. Rather than explain repeatability in terms of some other phenomenon such as wholes and parts, a theory of kinds has the possibility of repeatability built into the very ontological nature of kinds. There is one word type despite there being two inscriptions because a word type is a kind, which can be identified wherever the essence of that kind is instantiated. Two distinct inscriptions can instantiate the same essence and thus we have the sameness of word type despite the difference of the inscriptions. Two showings can instantiate the same essence and thus there can be one film though there are two showings.

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<sup>37</sup>For a discussion of common criteria for judging naturalness, see Boyd (1991, p.129).

<sup>38</sup>For more on the reality of ‘man-made’ entities see Thomasson (2003).



### 3. *Explaining Repeatability*

This provides us with the beginnings of an ontological account, but there are still important details to fill in. To this point, the view defended here departs from other type/token or kind/instance theories in the literature only in the detail. However, any type or kind theory that posits entities that are instanced (or tokened) just when certain conditions are met will need to provide some account of what those conditions are. In Chapters 5 and 6 of this thesis I take on that challenge and provide an account of the essences of repeatable artefacts that overcomes many of the difficulties other type and kind theories face.

Before that, however, there are more immediate matters to be settled that pertain not to the specifics of repeatable artefacts as kinds, but to the ontological nature of kinds in general. It is nearly universally assumed that kinds or types, if they exist, are abstract entities. However, I think the dominance of this view must be called into question. In the coming chapter I defend the view that kinds (at least those with physical instances) are in fact physical, rather than abstract, entities.

## 4. Kind Physicalism

In the previous chapter I claimed that the ontological nature of repeatable artefacts should be teased out of an analysis of our practices of identifying and individuating them. I argued that doing this places repeatable artefacts into the broad ontological category of kinds. Kinds are entities individuated (in the strong sense) by essences, where the essence can be understood as the requirement that must be met for the kind to be instanced. A novel is a kind, and an arrangement of paper and ink is an instance of the kind if it satisfies the essence of the kind. What more can we say about the relationship between a kind and its instances? Most theories of kinds (or types) that fall within this general framework add at this point that kinds (or types) are abstract entities.<sup>1</sup> It is claimed that the relationship between a kind and an instance is a relationship between something abstract and something concrete. In this chapter I will challenge this dominant position and defend the view that kinds, like their instances, are concrete.

What it means precisely for an entity to be abstract is a matter of some contention,<sup>2</sup> but there is one key element that we can focus on for our purposes. Arguably the most important feature of an abstract entity is that it is *non-spatial*. Mathematical entities – paradigms of abstract entities *par excellence* – invite this assumption with a degree of naturalness. The question ‘where is the number four?’ seems to either have no good answer, or the trivial answer of ‘nowhere’. Whatever numbers are, they seem not to be entities that have a location or occupy space. To this condition of being non-spatial it is sometimes added that abstract objects are also non-temporal, though the appeal of including the criterion is not nearly so strong, and the view of abstracta that I am interested in here need not be committed to it. More popular is the further condition that abstract objects are causally inert. While I think that causal inefficacy is a plausible consequence of being non-spatial, it also need not be regarded as a necessary condition for abstracta (we will encounter in due course an argument for the causal efficacy of abstract entities). Thus I want to adopt a minimal definition of an abstract object as any entity that is non-spatial. In challenging the view that kinds are abstract I am challenging any view which regards kinds as non-spatial. For the sake of this discussion, I will call any view of this sort *Platonism* about kinds.<sup>3</sup>

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<sup>1</sup>See, e.g., Wolterstorff (1980); Levinson (1980, 1990); Kivy (1987); Dodd (2000, 2007); Howell (2002b).

<sup>2</sup>For a discussion see Caplan & Matheson (2004 pp.117-122).

<sup>3</sup>This is in contrast to, e.g. Levinson (1980) and Fisher (1991), where *Platonism* is limited to those theories that hold that the entities in question are eternal or timeless, in which case Levinson's type theory is *anti-platonist* because it allows that musical works are created. According to my terminology, however, Levinson's position *is* a version of Platonism.

#### 4. *Kind Physicalism*

By approaching the topic in this way my critique of the view that repeatable artefacts are abstract entities differs from what is commonly found in the literature. A common move is to assume that if a novel or a work of music is abstract then it faces a number of associated problems – being causally inert and being eternal or timeless (and so not created), for example – that should persuade us to look elsewhere for a theory.<sup>4</sup> These issues are not irrelevant to the present discussion – the view I defend has the distinct advantage of avoiding them altogether – but the argument in this chapter looks to undercut the assumption that kinds are abstract by challenging the assumption that they are non-spatial before these further issues can take hold. That is, I want to bring into question the status of Platonism as the default theory of kinds.

Rejecting Platonism and accepting that kinds with physical instances are themselves physical will require a significant shift in the way that theorists have traditionally thought about kinds. However, I will argue that the move is not unprecedented, and brings some distinct advantages. We will see that it allows us to recognise that kinds have parts, where these are the parts we thought they had (in contrast to claims of the scattered object hypothesis). We will be able to allow that the opening movement is part of the symphony, and the last chapter is part of the book. This view also allows that kinds are the objects of sensory experience. We can literally see a photograph and hear a song. Further, with some caveats, it allows that kinds, and thus works of music and novels, can be created.

A couple of points need to be made about the scope of this chapter. Firstly, my aim is not to reject all abstract entities. I am sympathetic to the belief that there are some abstract entities – mathematical objects seem to be excellent candidates – but a full and general discussion of abstract objects is outside the scope of this thesis. Secondly, I cannot hope to offer a knock down argument against kind Platonism here. Rather my more modest aim will have been achieved if the dominance of kind Platonism as a realist theory of kinds is undermined. If I cannot convince the reader that kind Platonism (henceforth, just ‘Platonism’) is wrong, perhaps I can convince them that there is a serious contender in the arena. My tactic to this end will be to introduce and defend a theory of kinds as being spatially located physical entities. I call this view *Kind Physicalism* (or for brevity just ‘Physicalism’, though it should be remembered that my thesis is only a thesis about kinds, and only about those kinds that have physical instances). It should also be noted that though Physicalism is motivated in part by the methodology of Chapters 1 and 2, no other part of this thesis depends on an acceptance of Physicalism, and to this degree the positive argument of this chapter is independent of the rest of the thesis.

In §4.1 I introduce the proposal that kinds are concrete or physical entities.<sup>5</sup> I argue that without prior metaphysical commitments, it is more natural to view kinds as physical entities. I then develop the view in §4.2 by drawing on an account offered by

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<sup>4</sup>Caplan & Matheson (2006); Rohrbaugh (2003).

<sup>5</sup>Note that I take being ‘concrete’ and being ‘physical’ as amounting to the same thing, which I analyse only intuitively and roughly as being part of the physical or material nexus of the universe.

Eddy Zemach of the ontological category of *pure continuants*. If we are to view kinds as physical entities, they must be understood as entities that are continuous (or unbound) in both space and time. This allows them to be multiply located in both space and time. I argue in §4.3 that this has important consequences for the notion of kinds having parts.

In sections §§4.4 - 4.6 I turn my attention to the more traditional Platonist proposal. I argue that ultimately motivations for adopting Platonism over Physicalism are unpersuasive. In §4.4 I consider the Platonist's argument that Physicalism falls foul of the axiom of localisation. I suggest that when probed, the axiom amounts to no more than an unjustified assumption. In §4.5 I consider instead the claim that it is in fact quite natural to think of repeatable artefacts as being non-spatial. In §4.6 I discuss the plausibility of the Platonist's attempts to explain our encounters and causal interactions with kinds. I will show that while the Platonist can make progress here, it comes at a price and it is a much less attractive explanation of the evidence than Physicalism. Finally, I end the chapter by drawing out the consequences that this view has for the persistence of kinds (§4.7). I will argue that most sensible account of the persistence of kinds has it that kinds exist when and where they have instances.

## 4.1. Kind Physicalism Introduced

The view that I am calling Platonism about kinds – the view that holds as a minimum condition that kinds are abstract (non-spatial) entities – has been the default realist view about kinds in the recent literature. This is perhaps unsurprising given the historical association of kinds with universals and the common view that universals are abstract. However, I would argue that given our practices of identifying and individuating repeatable artefacts – those practices that themselves provided the basis for postulating a theory of kinds – it is not clear that Platonism is the most straightforward or *prima face* obvious view to adopt. Take, for instance, the example of the following display:

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How many words are there in the box? There are two word tokens and there is one word type; that is the answer Armstrong gave. But notice the question asks how many words there are *in the box*. Thus if the latter answer is a good answer, and we have been assuming that it is, then the answer implies that there is one word type *in the box*, as well as two word tokens.<sup>6</sup> The temptation will be to move quickly to explain away this apparent claim about the location of a word type, but if we can approach the claim without the prior assumption that word types are abstract then this temptation can perhaps be resisted long enough to assess the claim at face value. What appears to

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<sup>6</sup>Does this mean there are *three* things in the box? Well, there are *at least* three. There are also three letter types and six letter tokens. We could say 'there are three words in the box', but this obscures the distinction between word types and word tokens.

#### 4. Kind Physicalism

be the case is that when we identify the word tokens and then identify the word type we are identifying all three as being there in the box.

This apparent commitment to location is not an isolated incident. We seem to make similar claims about many other repeatable artefacts. For example: ‘the family photograph from last Christmas is pinned to the fridge in our house, but my parents have it on their mantelpiece.’ Taken at face value, the claim here seems to be that the *same* photograph is both on my fridge and in my parents’ house. It is located in two places. As well as claims about location, we also make claims about hearing, seeing and touching repeatable artefacts. Songs can be heard, films and plays can be watched, and novels and theses can be read. While it may be possible to argue that abstracta can be encountered in this way, these claims certainly don’t *suggest* a theory of abstracta. Moving away from paradigm repeatable artefacts to other kinds, a similar trend emerges. We claim the same whisky is in both bottles, that gold was found in California, and that ice is cold to the touch.

The entities that we are to be talking about, and that I have labelled kinds, at least seem to be physical things that are spatially located – things that we physically interact with as easily as we interact with ordinary physical objects like rocks and chairs. While there are responses that the Platonist can make here – in the next section we will encounter a Platonist-friendly interpretation of what it is to see and hear repeatable artefacts – I want to present an account that makes no excuses about such claims, but rather takes them to be data to be interpreted by an ontology. If such an account is possible, then it surely deserves the status of being the default view.

An ontology that allows that both the word tokens and the word type are in the display, and that allows that the photograph is both on my fridge and in my parents house, is an ontology that allows that kinds are, in the way to be described, *co-present* with their instances.<sup>7</sup> Thus this is a realist ontology of kinds that – insofar as kinds can be seen as a sort of universal – resembles David Armstrong’s ‘immanent realism’ about universals, though, as I have said, my aim here is not to argue that there are no abstract entities at all.<sup>8</sup> Nor, in developing this view, am I committed to any particular theory of properties. This theory is put forward only as a theory of kinds, as described in the previous chapter.

#### 4.2. Kinds as Continuants

What does it mean to say that a kind is co-present with its instances? Understanding this claim will be the key to understanding Physicalism about kinds. I will defend the view that if kinds are physical they must be continuous both spatially and temporally. To make progress on this front I want to discuss an ontological proposal put forward by

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<sup>7</sup>The term is from Lewis (1983, p.345).

<sup>8</sup>See Armstrong (1978) and (1989). Are kinds universals on my view? This is not straightforward to answer because it is not obvious what exactly a universal is. They may warrant the label ‘universal’ simply in virtue of their relationship to their instances. However, they are not straightforwardly ‘true of’ or ‘applied to’ multiple things except insofar as many things are instances of them.

#### 4. Kind Physicalism

Eddy Zemach – a proposal to which the Physicalism defended here is heavily indebted. I will look briefly at Zemach’s overall position, and then focus on the aspect relevant to kind Physicalism.

Zemach (1970) has made the unorthodox claim that there are four basic ‘ontologies’ of the physical world, differentiated upon whether or not the entities of the ontology are ‘bound’ or ‘continuous’ in time and space.<sup>9</sup> A rough account of what this means (and one that will need to be amended shortly) is this. If an entity is continuous in time then at any time at which the entity exists, the whole entity exists. Likewise if an entity is continuous in space then at any spatial point at which the entity is located, the whole of the entity is located. On the other hand, if an entity is bound in time then any temporal slice of the entity contains part of the entity. Likewise, if an entity is bound in space then there are regions of space occupied by the entity that contain part of the entity:

Hence, four kinds of ontology: an ontology whose entities are bound in space and in time, an ontology whose entities are bound in space and continuous in time, an ontology whose entities are bound in time and continuous in space, and an ontology whose entities are continuous in space and in time.  
(Zeamach, 1970, p.233)

The first ontology can be seen to correspond to the perdurantist’s ‘space-time worms’. Entities in this ontology are spread out in space and time so there that are both spatial and temporal cross sections that contain parts of those entities. Zemach associates this ontology with the category of events. The second ontology, with entities bound in space but continuous in time, corresponds to the endurantist’s entities. These entities can have spatial slices that contain parts of the entity, but no temporal parts – any region of time occupied by the entity contains the whole entity. As Zemach points out, this ontology is the ‘natural’ ontology that accounts for how we tend conceive of ordinary objects, long before we are introduced to the possibility of space-time worms in undergraduate metaphysics. The third ontology has entities bound in time but continuous in space. This is the most difficult of the four to conceptualise, but Zemach offers the examples of ‘the rain’, ‘the Industrial Revolution’ and ‘the Roosevelt era’. The entities, dubbed *processes* by Zemach, have temporal parts (“the ‘start’ of the revolution”) but no spatial parts – at every place at which the process is, the whole process is found. Whilst you may experience the beginning of the heat wave, and I the end of the heat wave (temporal parts), we cannot both experience different spatial parts of the heat wave. (The difficulty of this ontological category is that it is very tempting to construe something like a heat wave as an entity of the first category – an event with both spatial and temporal parts. To grasp Zemach’s processes one must put this interpretation of ‘heat wave’ to one side and allow the formal definition of the category to take hold.)

Much more could be said about these three ontological categories and Zemach’s treatment of them, but it is the fourth category that is most relevant to us. The entities of

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<sup>9</sup>For a further exposition, see also Zemach (1975).

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the fourth ontology are continuous in both space and time. Zemach calls these entities pure continuants, or types, and gives, as paradigmatic examples, gold, THE COMMON ELM, the letter Q, and WAR AND PEACE. I believe that with this final category, Zemach has put his finger on the ontology of kinds. However, it should be noted that my theory of kinds differs from Zemach's account of 'pure continuants' or 'types' in at least two respects. Firstly, Zemach makes no mention of entities of this ontology having essences. On my view, however, essences are vital to the individuation and recognition of kinds and are central to understanding the ontological category of kinds. Secondly, Zemach argues that each one of these four ontologies is on its own sufficient to represent the whole of physical reality (Zemach, 1970 p.231). I make no such commitment about the ontology of kinds in this thesis.

Given this rough description of what it is to be continuous or bound in time or space, we can characterise Zemach's pure continuants as entities that are wholly present at any point in space or time that they are found. (This is not quite right, as we will see, but nevertheless provides a helpful first take.) Entities like gold and water provide a useful way in to this category, especially as they are paradigmatic examples of kinds in philosophical discussion. The view being put forward here is that water is a kind and is a genuine physical entity – a 'mass', as I have been calling it – that is wholly located wherever and whenever it is found. Just as the endurantist insists that the same person is found in her entirety at 5 pm and at 6pm in the same place, so the kind physicalist insists that the kind is found in its entirety both in one room and in another at the same time. It also follows from this that when we have an instance of water we do not have proper part of water (recall the rejection of the scattered object hypothesis in §3.2). Zemach writes:

If I want water and you bring me a cupful I cannot object saying, "You brought me only part of water, not water itself," but I am likely to make this objection if I want Fido and you bring me his ear. (Zemach, 1970, p.243)

Of course, whenever we have water, we also have a quantity of water – a cupful, or a litre, for example – which we *can* divide into parts. I can throw part of this cup of water on the fire, or turn part of that lump of gold into a ring. But I cannot throw part of water on the fire or turn part of gold into a ring, and the explanation given here is that this is because water and gold do not have spatial (or temporal) parts.<sup>10</sup> The cup of water, and the lump of gold are entities of one ontological category, and they are, to introduce my terminology, co-present with gold and water – entities of another category. The kind and its instance (gold and the gold ingot, or water and the cup of water, for example) can be thought of as overlapping one another (or *coinciding* with one another) in physical reality at that point. This is the sense in which the kind and

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<sup>10</sup>Recall that the scattered object hypothesis claims just the opposite: when I throw a cup of water I do throw part of water on the fire (§3.2). The point being made here is that pre-theoretically, the claim that one can throw part of water onto the fire appears to involve some kind of mistake.

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its instances are co-present.<sup>11</sup>

At this stage, however, we need to be more precise in the description of what it is to be continuous in time and space, and in doing so we will qualify the claim that continuous entities have no parts. The rough characterisation was that at every place at which the entity is found, the entity is present in its entirety, and so there is literally no sense in which the entity has parts. However, even for stuffs like gold and water, there will be a *minimum* region of space in which the kind can properly said to be found in its entirety. If region  $\ell$  is the region containing only a single hydrogen atom in a water molecule, then it would not be right to say that  $\ell$  contained water in its entirety. Rather genuine instances of water have a certain minimum size. We may be able to divide up a sample of water, but it is not the case that each division produces smaller and smaller samples of water (as Quine realised – see again the discussion in §3.2). Once a small enough size is reached, we no longer have an instance of water within the divided area.<sup>12</sup>

Thus we need to make a distinction that Zemach does not make, between being purely continuous along a dimension, and having what we might describe as ‘chunky’ or ‘quantified’ continuity along a dimension. The entities of the endurantist’s ontology are purely continuous in time (though bound in space) and so there is no minimal temporal region in which they can be found. Similarly, mass kinds like gold and water are purely continuous in time but have ‘quantified’ continuity in space. Could there be a kind that was purely continuous in space, with no limit on the region of space in which it is found? Ether, if it had existed, would perhaps have named such a kind. A quantity of ether could be divided infinitely and each part of the quantity would itself contain ether. Thus ether would be purely continuous in both space and time.

Given the possibility of quantified continuity, a more accurate account of what it is to be continuous in time and space is that there are multiple regions of space and time that contain the entity in its entirety.<sup>13</sup> This weaker description allows for both pure continuity and quantified continuity.

The point that I want to emphasise, then, is that we can understand repeatability

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<sup>11</sup>Simons uses the term *superposition* for what I am calling being co-present (1987, pp.210-254). Simons draws a distinction between objects that are *superposed* and those that are *conincident*. Coincident objects share their parts, where as superposed objects need not. Simons writes:

“There are certain cases where objects are superposed which for categorical reasons cannot coincide. A continuant and an occurrent involving all of it occupy the same spatial region for a while, but clearly, since they belong to different categories, cannot have a common part, and equally clearly they do not *compete* for this region” (Simons, 1987, p.211)

Using Simons’ terminology, then, it would be right to say that kinds and their instances do not coincide but are superposed (since kinds and their instances belong in different ontological categories). See Simons (1987, pp.210-254) for a discussion and defence of superposition. See also (Wiggins, 1968) and (Doepke, 1982).

<sup>12</sup>Note that the point is not that kinds are not located at all in regions smaller than these minimal regions, just that they are not wholly located in those regions. Similarly, a car can be said to occupy a region  $\ell$  marked out by the drivers seat, but region  $\ell$  does not contain the whole car. The account also translates to the temporal dimension. If a performance of a work of music lasts from 1pm to 1.30pm, then the time period 1.10pm to 1.15pm does not contain the whole work of music, but it does contain the work of music in virtue of containing part of the work. See §4.3.

<sup>13</sup>This is weaker than the requirement that every region at which the entity is located contains the entity in its entirety. All that is required is that there are many regions at which this is the case.



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using the notion of continuity along a dimension. However, that an entity can be found multiple times in its entirety along a dimension is not sufficient for an entity to be repeatable. Enduring entities are continuous through time, but we do not think of them as being repeatable. Rather, our notion of repeatability seems to correspond to continuity through *both* space and time. It makes it possible for us to genuinely identify the same entity in multiple places at once, and at multiple times, as we do with repeatable artefacts. To clarify then: the kind is co-present with its instances. It is located when and where its instances are located, but is distinct from them. This is possible because the kind, unlike the instances, is continuous in time and space and thus can be multiply located.

### 4.3. Kinds and Their Parts

That certain kinds have minimum spatial or temporal regions has important implications for the notion of kinds having parts. It is common to talk as if repeatable artefacts do have parts. We talk of the first half of a song, or the last few pages of a novel, or a section of the new company logo which we don't like. Can we take this talk of kinds having parts seriously? The initial account that was given of what it is for an entity to be continuous along a dimension stated that the entity has no parts along that dimension. Hence a purely continuous entity would have no parts at all. However, if a kind has a minimal region in which it can be instanced, and the kind is located where its instances are located, then we *can* allow that the kind has parts. That is, kinds can be thought of as having parts as long as it is understood that those 'parts' exist just in the same ontological category as the kinds themselves. We might say that kinds have *kind-parts*. For example, the region of space occupied by an instance of a photograph will contain the whole photograph, co-present with the instance (and indeed every instance of the photograph will be co-present with the whole photograph). But the top right quadrant, containing part of the instance, will not contain the whole photograph but a *kind-part* of the photograph. As the whole photograph is co-present with the whole instance, the kind-part of the photograph is co-present with the corresponding part of an instance. The top right quadrant of each instance of the photograph is co-present with the same (strictly identical) kind-part of the photograph kind in each case, and the kind-part is repeatable just as the kind itself is.

As an instance of a photograph can be divided into spatial parts, each part being an instance of the corresponding part of the photograph kind, so an instance of a work of music can be divided into temporal (kind-)parts, and each temporal part will be co-present with the corresponding temporal part of the work itself. The opening bars of Beethoven's Fifth are a temporal (kind-)part of that work of music. They are identifiable and repeatable as a part of the whole kind. Thus we can see that a further advantage of kind Physicalism over Platonism is that it makes good sense of our talk of parts of repeatable artefacts. The talk is legitimate because repeatable artefacts do have parts, though we must understand them as kind-parts.

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We can contrast this with a Platonist account, according to which the view that kinds (or types) have structures and parts is untenable. For if kinds are abstract objects, it is hard if not impossible to make sense of the claim that they are spatially or temporally arranged. If a photograph is non-spatial, it makes little sense to identify the top right quadrant of the photograph itself; the Platonist must interpret such talk as talk about the spatial arrangement of instances of the photograph, and they are likely to conclude that the kinds themselves are structureless.<sup>14</sup>

However, there is a further problem with viewing abstract kinds as having parts and structure that one might think is also a pressing problem for Physicalism. This problem was raised for abstract types and their tokens by Peter Simons (Simons, 1982, p.196). Consider the expression ‘*Faa*’.<sup>15</sup> We might ask: how many letters does the expression type ‘*Faa*’ contain? It is plausible to reply that there are three letters in ‘*Faa*’. However, if these are letter *tokens* that are being counted, we seem to be saying that the type contains three letter tokens. This is problematic because a type, as an abstract object, cannot literally contain tokens (which are physical, locatable particulars). However, if we mean letter types, then there are not three but *two* letter types in the expression ‘*Faa*’. But if ‘*Faa*’ is an expression composed of two letter types, there would be no distinction between ‘*Faa*’ and the expression ‘*Fa*’, which also contains the same two letter types.

It seems possible to generate the same worry for a physicalist account of kinds. If the physical word kind ‘hoot’ is a structured entity, it cannot be composed of four letter *instances*, because instances are not repeatable, whereas the word itself is. We said above that parts of kinds are kinds themselves, and so we called them kind-parts. But if it is composed of letter kinds, there are only three distinct letter kinds to be counted. There would then be no difference between the structure of ‘hoot’ and the structure of ‘hot’.

I think that to answer this problem, the physicalist must recognise that kind-parts of a word are *not* identical with letter kinds. Thus we can understand ‘hoot’ as being composed of four kind parts. The second part and the third part are numerically distinct kind parts of that word, but neither of those parts are kind-identical with the letter ‘o’. This is because the second part of the word ‘hoot’ and the letter ‘o’ have different requirements of their instances (and thus different essences). Note, for instance, that in writing ‘o’ in this sentence I am producing an instance of that letter, but not an instance of either the second or third kind-part of the word ‘hoot’. To instance the second and third kind-part of the word ‘hoot’ I have to write the word ‘hoot’ itself. Alternatively, the difference can be seen from the fact that if I point first to one ‘o’ in ‘hoot’ and then to the other, I am pointing to the same (identical) letter, but *different* parts of the word. Because the second and third part are non-identical, they cannot be identical to some third thing. Thus none of the parts of the word are identical with the letter ‘o’ itself (though, of course, every instance of the second and third part will also

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<sup>14</sup>See Dodd (2007, p.51).

<sup>15</sup>I have adapted the argument slightly from Simons’ presentation.

#### 4. *Kind Physicalism*

be an instance of the letter ‘o’). Hence the answer to the problem might be that words are not in fact composed of letters (in that no part of the word is identical with any letter), though they do have parts.

Before moving on to consider Platonism in more detail, more can be said to flesh out the details of Physicalism. For instance, the discussion of photographs and works of music above reveals the different ways that kinds can be continuous. We can see that while a photograph has quantified continuity in space and pure continuity in time (there is no minimal temporal region in which a photograph can be found in its entirety) a work of music has quantified continuity in time but something approaching pure continuity in space.<sup>16</sup> A film, on the other hand, has quantified continuity in both space and time: one can see part of a film either by only watching in the first half hour or by covering up part of the screen for the duration of a showing.

The final point to emphasise here is that the kind of continuity that a repeatable artefact has along a dimension is not ad hoc. Rather it depends entirely on the essence of the kind in question. A photograph, as an object-repeatable, requires that its instances have properties that relate directly to spatial arrangement (relative spatial locations) and so instances have minimum sizes, but there is no temporal dimension to the required properties. A work of music – an occurrence-repeatable – is just the opposite. An instance must have certain temporal properties – it must be a sound event with certain duration – but there is no strict spatial requirement. Thus minimum spatial or temporal regions are set by the minimum regions in which the required properties, whatever they may be, can be found.<sup>17</sup>

This concludes my introduction of kind Physicalism which presents kinds (with physical instances) as being spatially located repeatable entities. They exist in the physical world though they do not belong to the ontological category of ordinary physical objects like chairs and rocks.<sup>18</sup> I argue that accepting this as an ontological category allows us to make better sense of our talk about repeatable artefacts and other kinds than can be gained by appealing to abstracta. In §§4.4-4.6 I will argue that once Physicalism is on the table, the arguments for Platonism about kinds are not nearly so appealing.

#### 4.4. **The Axiom of Localisation**

A Platonist about kinds, as that term is being used here, is anyone who thinks that kinds are not spatially located. Sensible Platonists object to claims that such entities are ‘outside of space’ because this adopts a unhelpful spatial metaphor (saying ‘where’ they are) and leads to metaphysical queasiness and jeers about ‘Platonic realms’ and

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<sup>16</sup>The minimum spatial region in which a work of music can be instanced will be the minimum spatial region in which one can identify a sound event as being located, whatever that may be.

<sup>17</sup>I will argue in Chapter 6 that these structural properties are not in fact part of the essence, though they are entailed by the essence.

<sup>18</sup>Note that I remain neutral in this thesis as to whether chairs and rocks are enduring or perduring entities.

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‘wraith-like hinter-worlds’.<sup>19</sup> Instead, the Platonist claims simply that they are not the sorts of things that can be located. They cannot be at a particular place, nor can they have a volume, any more than ‘4.30pm yesterday’ can have a mass, or the number four can have a velocity.

However, I will argue here that the arguments for Platonism are surprisingly weak and do not justify the banishing of repeatable artefacts to the realm of the abstract. I suggest that the weakness of these arguments has generally gone unnoticed thanks to a scarcity of alternatives. What the Platonists get right is that many things that we want to say about instances of kinds, especially with regard to their relationship to space and time, we cannot say about the kinds that they are instances of. This is because kinds and instances are in fundamentally different ontological categories. However, it has too often been assumed that this difference must be a difference between the abstract and the concrete.

I will consider three arguments that have been given for Platonism: the argument from the axiom of localisation (this section); the argument from the peculiarity of “where is X?” questions, where X is the name of a kind (§4.5); and, finally, the argument that Physicalism is unnecessary (§4.6). The first two arguments for Platonism are designed to show that any view which tries to locate kinds is untenable. I will claim that neither of these arguments provides us with enough reason to reject Physicalism. The third argument tries to show that Physicalism isn’t needed because a Platonist account can explain everything that needs to be explained about kinds. I will argue that while a Platonist explanation can be offered, Physicalism offers a better explanation.

Before launching into these arguments it should be noted that while Platonism has the weight of tradition on its side, Physicalism does a better job from the outset of allowing for our ordinary talk of repeatable artefacts and other kinds. It allows that we can straightforwardly causally interact with kinds, it allows that kinds can have parts (though we must understand them as kind-parts) and it allows for our claims to see, hear and touch kinds. In that sense, at least, it begins with the upper hand and the onus should be on Platonism to show that it offers a better account.

This first argument hinges on an insistence that a central feature of kind Physicalism is intuitively false. Kind Physicalism holds that a kind can be multiply located; i.e. that it can be wholly located in different places at once. However, it has been claimed that this cannot be possible because the contrary is a truism to which any ontology must conform. This ‘truism’ has been put forward as the axiom of localisation. Quoting from Julian Dodd, we can present the axiom as follows:

**AL** “no entity whatsoever can be wholly present at different places at once.” (Dodd, 2007, p.48)<sup>20</sup>

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<sup>19</sup>See, for example, Glock (2002, p.249): “Platonists hold that abstract objects . . . inhabit a supernatural world beyond space, time and causation...Nominalists protest that this hinterworld is a myth...” See also Howell (2002, p.124) who talks unhelpfully of an abstract pattern “hovering ghost-like over the musical landscape”.

<sup>20</sup>J.P. Moreland (2001, pp.9-10) presents AL as the claim that “no entity can exist at different places

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Clearly, if there were independent reasons to believe that AL was a universal truth, then kind Physicalism would have to be rejected. But why should we think that AL is a universal truth? It might be thought that it is true because it is impossible to conceive of it being false. However, this seems to indicate a lack of conceptual imagination more than anything else. Most philosophers are willing to accept that a person can be wholly located at different times (or at the least that this is a conceptual possibility). Is it really that much of a conceptual step to also allow that an entity can exist wholly at different locations?

Alternatively, though in the same ball park, it might be insisted that AL is true because it is true by definition. In other words, it might be claimed that it is just what we mean by ‘wholly located’ that if an entity is wholly located in a region  $\ell_1$  then it cannot be simultaneously located at any other place outside of  $\ell_1$ . On this view it is contradictory to say that an entity is wholly located at  $\ell_1$  and wholly located at  $\ell_2$  where  $\ell_1$  and  $\ell_2$  are non-identical, in the same way that it would be contradictory to say that a stone had a mass of one kilo and a mass of two kilos, or that a race began at time  $t_1$  and at time  $t_2$  (where  $t_1 \neq t_2$ ).

However, it is far from clear that our concept *being wholly located* has such a strict definition. If one were to define ‘wholly located’ or ‘wholly present’ in this way then AL would be trivially true. But then we could re-state the central claim of Physicalism in different terms that avoided this conceptual contradiction. We might say, for example, that when a kind is multiply located it is found ‘in its entirety’ at each location, thus avoiding the claim that it is ‘wholly’ located in multiple places. This might seem like slight of hand, but note that merely defining a term so that a claim cannot be made using it does not mean that there is no true claim to be made. The truth or falsity of Physicalism cannot lie in a contestable definition of the phrase ‘wholly located’.

More importantly, however, I argue that a reflection on the nature of kinds provides precisely the sort of evidence that should persuade us that our concept of being ‘wholly located’ is more encompassing than we thought. For example, I noted above that it appears contradictory to claim that an event had two beginnings. On the face of it this strikes us as plausible. It might seem that it is part of what we mean by ‘beginning’ that something can only ‘begin’ once. However, doesn’t it make good sense to say that a film begins at the cinema at 16.30, 18.30 and 22.30? Similarly, to someone who missed the opening sequence of a video installation on loop, we might say that they needn’t worry because it will begin again in five minutes.<sup>21</sup> We have every reason to take this as evidence that, contrary to what we thought, some things – namely kinds – can begin more than once. Our mistake was to limit the concept of what it is to

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or at interrupted time intervals”. Interpreting Moreland (with a little charity) as meaning that ‘no entity can *wholly* exist at different places at once’, his is the slightly stronger formulation of the axiom, but we will do well enough to stick with Dodd’s more careful formulation.

<sup>21</sup>In a similar manner, Simons (1987, pp.195-196) distinguishes a global from a local sense of ‘beginning’ for activities, so that one may only begin climbing (for example) in the global sense once (‘When did you *first* begin climbing?’) but each episode of climbing will have a new beginning in the local sense. See also the discussion of intermittent existence in §4.7.

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begin to non-repeatable entities. Likewise, I would argue that reflection on kinds gives us good reason to think that we were mistaken to think that no entity can be wholly located in two places at once, or at the very least that our prior understanding of what it is to be wholly located was unnecessarily restrictive.

An alternative and more persuasive version of the ‘true by definition’ argument can be found in the work of Jonathan Lowe (Lowe, 2006, p.24). Lowe begins by claiming that the suggestion that something can be wholly present in two different places at the same time “seems to make no sense”. He then argues this is because the relation of *being wholly in the same place as* appears to be a symmetrical and transitive relation. (The axiom of localisation was not put in terms of being ‘in the same place as’ but that formulation can easily be derived from the axiom. If a kind is wholly present in a region  $\ell$ , and the instance is wholly present in a region  $\ell$ , the kind and the instance are wholly located in the same place.) Thus if we want to say that the kind is wholly in the same place as instance A, it follows – if the relation is symmetrical – that instance A is wholly in the same place as the kind. But then if the kind is also wholly in the same place as instance B (and thus B is wholly in the same place as the kind) it seems we would have to say, by transitivity, that instance A is wholly in the same place as instance B, which is obviously false.

But we can reply here very much in the same spirit as before: we can either deny that our concept of being wholly located is symmetrical and transitive in this way, or argue that a reflection on kinds should persuade us to rethink this understanding of being ‘wholly located’. How can the relationship not be symmetrical and transitive? Well, if instance A is wholly in the same place as the kind, and instance B is wholly in the same place as the kind we can insist that it doesn’t follow that A and B are in the same place, precisely because the kind can be wholly in two places at once! So, contrary to supposition, though the relationship may be symmetrical, it is not transitive. In this respect it is like the relationship of *being the same nationality as*. If Ben has the same nationality as Mary, Mary has the same nationality as Ben. But if Ben is also the same nationality as Jane, it doesn’t follow that Mary is also the same nationality as Jane. This is because Ben could have dual nationality.<sup>22</sup>

Lowe anticipates this move, however. He notes that someone may simply reject the claim that the relation is transitive, but charges this as being “both unprincipled and question begging” (Lowe, 2006, p.24). Lowe’s reason here is that if one gives up on this condition then it becomes “altogether obscure” what one does mean by ‘wholly located’. In other words, a supporter of the view that an entity can be wholly in two places at once cannot reject the most central components of what we mean by being ‘wholly located’ and still be understood as saying something coherent.

This challenge could equally be made in response to the previous point: if, by ‘wholly located in region  $\ell$ ’ it is not meant that the entity is not located in any other region, then what is meant by such a claim? Kind Physicalism must be able to meet this challenge

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<sup>22</sup>That being co-present is not a transitive relation is regarded as obvious by David Lewis (1983, p.345).

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or Lowe's charge of it being "altogether obscure" will be justified.

Fortunately, this challenge can be answered. What we mean when we say that a kind is wholly located in a region  $\ell$  is that in that location every part of the kind is located. By parts, here, we mean of course kind-parts. To say then that the kind is wholly located in region  $\ell$  is to say that every part of the kind is located in region  $\ell$ , and so if the kind is located outside region  $\ell$ , say in region  $m$ , then  $m$  will not contain a numerically different part of the kind, but the same kind and the same parts again. In other words, we mean that one cannot look elsewhere and find a different part of the entity, but we do not mean that one cannot look elsewhere and find the same entity and same parts again.

This is not an ad hoc move that defines 'being wholly located' in a question begging way. Rather it picks out something that is surely central to our meaning of being wholly located in every case of using that term. If a house is wholly located in region  $h$ , then one cannot find an additional part of the house somewhere else outside of  $h$ . Of course, because the house is an entity bound in space, it also follows that the entity cannot be found outside of  $h$  at all. But consider this example: if an enduring person is wholly located in a space-time region, one cannot find an additional *part* of the person in another space-time region, even if the person can be found to exist in other space-time regions. For what one will find in these other regions is not a different part of the same thing, but the same parts (and the same whole) again.

Now Lowe and others might still object that this doesn't really do full justice to our ordinary sense of 'wholly located' and our ordinary understanding of parts and wholes, but this need not trouble the Physicalist. All the Physicalist needs is that there is an understandable and reasonable sense in which the kind can be said to be wholly located in a region. It can be claimed that 'wholly located' as applied to kinds was never meant to be used in this strong 'ordinary sense', since that strong ordinary sense is the sense that only applies to ordinary spatially bound (i.e. non-repeatable) entities.

It is no doubt because we are so familiar with spatially bound entities that the axiom of localisation has the degree of intuitive pull that it does have. However, when we move onto kinds we have no reason to think that the axiom should apply. In fact, I have argued that consideration of kinds provides a solid antidote to the claim the axiom is a universal truth. Entities *can* be wholly located in two places at once: the word THE is both in the left half of the display *and* the right half of the display; water is underground *and* in lakes and rivers.

#### 4.5. The Peculiarity of "Where is X?" Questions

I have argued that the axiom of localisation cannot be used to show that Physicalism is false because we have no independent reason to think that the axiom applies to kinds. It therefore provides no reason to think that Platonism should be preferred over Physicalism. However, the Platonist might attempt to counter one of the motivations for Physicalism by claiming that at least some of the time it is far more natural to think

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of kinds as not being located. For example Dodd notes that

“[t]he question ‘Where is Beethoven’s Fifth Symphony?’ has a curious ring about it: its occurrences take place in concert-halls and living rooms, but we do not describe the work itself as inhabiting such spaces.” (Dodd, 2007, p.92)

Dodd then counts this as (at least some) intuitive evidence for his version of Platonism. However, I suggest that the oddity of the proposed question is really just evidence of the fact that works of music are kinds of sound or noise, and that it is unusual to ask ‘where’ a noise is. Kind Physicalism is the thesis that kinds are physical entities that are co-present with their instances. If the instances are noises, or sound events, then a kind has no obvious location just as a noise has no obvious location, but the kind is still physically present in the way that the noise is present. The oddity arises from the ontology of noises, and should not be taken as evidence for the non-physicality of repeatable artefacts in general.

However, this isn’t the whole story. It might be objected that it still sounds odd to ask where a repeatable artefact is even for artefacts that have easily locatable instances (such as photographs). ‘Where is the 2012 Olympic logo?’ sounds just as peculiar as ‘Where is Beethoven’s Fifth Symphony?’ though instances of the logo are not noises.

However, we can easily identify two further reasons why the question sounds peculiar, without rejecting the view that kinds are located. The first is that because a kind can be multiply located the question is poorly worded and ambiguous as to the kind of answer being sought. Is the question asking for every point at which the logo is to be found, or is there a contextually relevant instance about which the enquiry is aimed? As an analogy, consider the question ‘How much gold is there?’ – in the whole world? In this room? Now? It sounds odd because it is a poorly formed question, not because gold cannot be quantified.

The second reason follows from the first and is that because kinds can be multiply located while instances cannot, the question, in appearing to ask for a *single* location, seems to betray a category mistake. When presented with the question its ambiguity suggests that the questioner has mistaken the name of a kind for the name of an ordinary object. Thus while Platonism would say that the question sounds odd because kinds are not located at all, the Physicalist’s claim is equally plausible: the question is odd because kinds are multiply located. Again, we have no reason in this argument to prefer Platonism over Physicalism.

#### 4.6. The Argument Against the Necessity of Physicalism

The previous two arguments tried to show that Platonism is to be preferred over Physicalism because in the first place Physicalism has insurmountable problems, and in the second place there is intuitive evidence for Platonism rather than Physicalism. This third argument for Platonism is less direct. The aim here is to show that we should not



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be wooed by Physicalism in the first place because Platonism can account for everything that Physicalism can account for. Recall that the advantage of Physicalism was that straight off the bat it could allow for our apparent claims about locating kinds, and it made it simple to see how kinds could be seen and heard, and how they could be causally efficacious. The Platonist will argue that all these claims can be made sense of, or otherwise dealt with, by a Platonist account. I will argue that while ground can be made by the Platonist to this end, it comes at a price. Physicalism still provides the better explanation.

We should note that this final line of attack will only be effective for the Platonist in conjunction with the success of an additional argument to show that Physicalism is flawed, for otherwise it seems that the simpler option of Physicalism should be preferred. I have not found such an argument, but that doesn't mean that there isn't one. Thus it is worth assessing the plausibility of the Platonist's hopes in this respect. The version of the Platonist argument that I will focus on comes from Dodd (2007).

Firstly, we can note that Dodd's argument deals with the two issues of encountering kinds and of kinds being causally efficacious in one blow. We can encounter kinds *because* kinds can be causally efficacious, according to Dodd. Specifically, Dodd argues that we can hear a work of music because the work participates in a sound event which causally affects us. I will argue that while this account may be workable for music it is not without problems. More significantly, however, it does not translate well to other repeatable artefacts such as photographs.

Firstly, Dodd fully accepts the need to account for the fact that we can hear works of music:

Someone who had clearly listened attentively to a performance of *In This House, On This Morning*, but who nonetheless insisted that she had never heard the work, would be looked upon with bewilderment by her fellow concert-goers. An ontological proposal that had as a consequence that such a person had spoken truly should only be adopted in extremis. (Dodd, 2007, pp.12-13)

He then notes that the objection to his Platonism, given the force of the above, is based on the claim that:

types of sound-event cannot themselves be perceived because, lacking location in space, they cannot enter into causal relations, and hence cannot figure in the causal process that ends with an auditory experience. (Dodd, 2007, p.13)<sup>23</sup>

It is here that Dodd ties being heard to being causally efficacious. Something can be heard if it can "figure in the causal process that ends with an auditory experience."

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<sup>23</sup>Note that Dodd uses the terminology of types here, but for the purposes of this argument they can be taken as sufficiently similar to kinds.

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The traditional line of thought is then that because abstract entities cannot figure in a causal process in this way, they cannot be heard.

To be clearer as to the argument involved here, we can helpfully distinguish between different kinds of hearing. If a bird is singing, we can talk of hearing the *noise*, or of hearing the *bird singing*, or of hearing the *bird itself*. The first two are closely related. I suggest we can understand the noise as being the audible aspect of the event of the bird singing. When an event occurs, if part of that event involves vibrations of a suitable frequency travelling through a suitable medium there will be noise available to be heard. On the other hand, we hear the bird itself in a slightly different sense. We hear the bird because it participates in the event in the right kind of way. Let us say that we directly hear the event, but indirectly hear the object that participates in the event. Thus we directly hear a musical performance (an event), because we hear the audible aspect of the event (which is the noise), and we indirectly hear the orchestra which participates in that event in a suitable manner.

The conceptual connection with causal efficacy seems relatively straightforward. Our directly hearing the event, in particular the audible aspect of the event, is a result of the event causing an auditory experience. The indirect hearing of the object (the orchestra or the bird) is also connected to the causal powers of the object but in a slightly different way. To see why, we need to note that it is events that are commonly taken to be the causal relata.<sup>24</sup> Nevertheless, as Dodd notes, our concept of what can be a cause seems to stretch to objects (orchestras, birds) in a derivative way (Dodd, 2007, p.13). An object can be said to cause something if it is suitably involved in the causing event: the nail caused the tyre to burst by being suitably involved in the event of the nail coming into contact with the tyre; the alarm clock caused me to wake up by being suitably involved in the event of the alarm clock going off. This derivative way in which an object can be causally efficacious nicely mirrors the sense in which we indirectly hear an object thanks to its involvement in an audible event. Thus it is natural to say that we (indirectly) hear an object as a result of its (derivatively) causing an auditory experience. Being a cause, either straightforwardly or derivatively, seems to be a necessary condition of being heard, either directly or indirectly.

We can be more specific, then, and say that the traditional thought is that abstract objects, being non-spatial, cannot cause either straightforwardly or derivatively, and cannot be heard, either directly or indirectly.<sup>25</sup> That an abstract object cannot straightforwardly be a cause, and that it cannot directly be heard, is quite clear. Because it is non-spatial it is not a physical event, and it cannot have an audible aspect involving vibrations. However, Dodd's argument is that it is far from clear that an abstract object cannot be a cause *derivatively*, and cannot thus be heard *indirectly*.

To be a cause derivatively, and so to be heard indirectly, we have said that an entity must participate in or be involved in the event in the right kind of way. The orchestra seems to satisfy this condition in relation to the performance. Thus the orchestra

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<sup>24</sup>See, e.g. Davidson (1967), Lewis (1973), and Collins et al. (2004).

<sup>25</sup>See, e.g., Cheyne (2001, p.3).

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(derivatively) causes the auditory experience and can be (indirectly) heard. Can we be so sure that an abstract object cannot play an analogous role to the orchestra here? Any objection to this thought would have to rely on a worked out and principled theory of what it is to participate in an event in the right kind of way, Dodd argues, and no such theory appears to be available. Thus the Platonist should be free to hold that their abstract objects are as good candidates as anything else for participating in this way (Dodd, 2007, pp.15-16).

Dodd's argument here certainly serves to undercut the assumption that it is obvious that an abstract object cannot be heard and cannot be causally efficacious. However, one might wonder about the relationship between intuitions and inferences at play here. We have the intuition that a work of music can be heard, and if theory says that a musical work is an abstract object, we might infer that we have reason to believe that an abstract object is an entity suitable to be heard, at least in an indirect way. But if one is unsure about the theory that a musical work is an abstract object, then the prior intuition to the effect that something abstract cannot be heard will continue to be a sticking point. If all else was equal, they will say, a theory of musical works that didn't have us saying an abstract object could be heard would be preferable. Platonism that holds that musical works can be heard will hold it a price, even if Dodd has persuaded us that the price was much less than we thought.

Perhaps more importantly, however, Dodd's account of how we can hear a work of music doesn't translate well to visual repeatable artefacts, such as plays and photographs. While our concept of hearing allows that we can hear something indirectly in virtue of it participating in an audible event, it is not obvious that there is an analogous sense of indirectly seeing something. That is, in seeing we cannot draw a distinction parallel to the one drawn between a sound and an object that makes a sound.

Now, what it is to see something might not be as straightforward as having our retina impinged by light reflected or emitted from an object. The concept also allows us to see using electronic equipment to transmit this kind of visual data over long distances, so that we can 'see' the person we are in a video call with. We may even see an object using sonar or a heat sensitive camera. However, all of these ways of seeing involve us receiving visual data relating to an object's physical presence in a physical environment. Seeing is in this way quite a singular concept that doesn't allow for the seeing of something non-spatial. There are of course non-literal uses of 'see' as in 'I see your point' or 'I don't see why I should' but here 'see' is being used to mean 'understand' or 'grasp' and the fact that it is a non-literal use of the verb 'to see' is obvious. If this is right, it is much harder to see how an account could be given of how we could literally see an abstract object.

There may be a Platonist response here. They may argue that our concept of what it is to see something should be expanded in light of our claims to see repeatable artefacts, coupled with the theory that such artefacts are abstract. Or they may argue that the singular sense of seeing that I have described is subject to as yet unknown counterexamples. However, the point is that the Platonist has their work cut out for

them here, and a theory that has it that the plays and photographs that we see are not abstract provides a considerably more straightforward explanation. According to Physicalism we can see photographs and plays because they are physical and literally reflect light.<sup>26</sup> Similarly, we can hear pieces of music just because, as physically located event kinds, they can involve vibrations of a suitable frequency that cause auditory experiences, just as ordinary events can. In summary, then, though the Platonist is not entirely at a loss when it comes to explaining how we can hear repeatable artefacts, the waters muddy considerably when we turn to seeing artefacts, and, moreover, Physicalism provides a simpler explanation in both cases.<sup>27</sup>

### 4.7. The Persistence of Kinds

A theory of kinds has been set out as the best explanation of repeatability, and I have defended the view that kinds are physical multiply located entities. However, we have yet to address an important issue concerning when kinds exist. (The related question of *where* kinds exist, which only comes into play given the rejection of kinds as abstracta, needs also to be addressed). I have said that kinds exist and are real physically located entities, but do they come into existence at a certain point, and if so when? Do they cease to exist, and if so under what circumstances? These questions, concerning the persistence of the entities, have proved surprisingly difficult to answer.

The ontological theory developed so far, however, suggests a quite specific response to this question. If kinds are multiply located physical entities, then I argue that they must exist (and only exist) when and where they are located. With respect to repeatable artefacts, this position is certainly contrary to our intuitions, as we will see. However, the view seems unavoidable for anyone committed to the physicality of kinds. I will argue that our intuitions are weaker than they seem here, and in terms of a cost-benefit analysis, the view defended here does remarkably well.

Take the issue of spatial location first as this is the least problematic. If kinds are physical then the question of where they exist is just the question of where they are, which is a simple matter of their locations. Being located at  $\ell$  is sufficient for existing at  $\ell$ , and being located at  $\ell$  is also necessary for existing at  $\ell$ .

What of the temporal aspect of their existence? I am claiming that if kinds are physical they exist when they are located. I suggest that the truth of this is an example of a more general principle that applies to all physical entities, which we can call the Principle of Physical Existence (PPE):

**PPE** A physical entity exists at a time  $t$  if and only if it is located at  $t$ .

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<sup>26</sup>Note the the photograph and the print of the photograph will both reflect the *same* light given that they are co-present.

<sup>27</sup>I have focussed on hearing and sight, but the discussion could easily be extended to the other senses. We say we can smell a perfume, though the perfume is a kind of stuff, taste a herb though the herb is plant kind, and touch gold or water. Any theory that regards these as physical entities will always be more straightforward in accounting for these claims than one that does not.

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Why should we believe the bi-conditional PPE? One half of the bi-conditional, that an entity exists at time  $t$  if it is located at  $t$  (that location is sufficient for existence), is straightforward. That an entity is located at  $t$  *entails* that the entity exists at  $t$ , for something cannot be at a place without being simpliciter.

The second half of the bi-conditional is the claim that an entity is located at time  $t$  if it exists at time  $t$  (that location is necessary for existence). This is not quite as straightforward as the first conditional, but I still think it deserves our assent. To see why, note that the falsity of this claim would imply that a physical entity could exist without being located.

Is this possible? One immediate response is that this is impossible because, on pain of contradiction, an entity cannot be physical and lack a location. However, this is only true if for an entity to be physical it must have a physical location at every time that it exists. One might propose that some entity could be physical in virtue of being only *occasionally* located. I do not know how to show conclusively that such an entity is not possible, but I think we have good reason to doubt that there are such entities. An entity of this sort would be sometimes abstract and sometimes physical, but a common characterisation of what it is to be abstract is that something is categorically unsuited to having a location. Perhaps it would be responded that though the entity is sometimes not located, it does not follow that it is abstract at such times; it is physical though sometimes lacking a location. The problem, though, is that we normally take the existence of a physical thing to be bound in an important way to its physicality. If it exists though it is not located, how or why does it exist? In what sense does it have being? Again, I do not know how to show that such an entity is impossible but I think we should avoid postulating such entities if we can.

Of course, PPE as applied to kinds has the following consequence. If a kind is a physical entity, then according to PPE it exists at multiple times and places, just as it is located at multiple times and places. However, there may be a time,  $t$ , at which the kind is not located and so does not exist, even though the kind exists at a time both before and after  $t$ . In other words, PPE implies that kinds can have *intermittent existence*.<sup>28</sup>

It is likely that this will be seen as a problem, and we can identify two lines of objection. On the one hand there is likely to be a principled objection to intermittent existence. The claim here is that intermittent existence violates more basic ontological principles and is universally implausible. On the other hand there is likely to be an objection to accepting intermittent existence for certain specific entities which supposedly fall within this ontological category. It is important to keep these objections separate and not allow worries from one bleed into our consideration of the other. I will argue firstly that there are no good reasons to object to intermittent existence in principle,

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<sup>28</sup>Note that the theory of intermittent existence here is quite independent of Joseph Margolis' theory that all works of art (repeatable or not) have intermittent existence. According to Margolis, a work of art exists only when some 'lower level' material object which 'subvenes' the work receives some "properly orientated attention" (Margolis, 1958). The work then exists intermittently as it is perceived intermittently. See also Hein (1959).

and secondly that worries about the intermittent existence of specific repeatable artefacts are inconclusive and insufficient to undermine the broader ontological proposal set out here.

### Objections in Principle to Intermittent Existence

To object to intermittent existence in principle is to advocate a principle of No Intermittent Existence (NIE). NIE may be defended on the grounds that if an entity goes out of existence at a time,  $t$ , it cannot exist at any time after  $t$ . After all, the conjecture goes, isn't this what 'ceasing to exist' means? Similarly, it may be insisted that for an entity to begin to exist at  $t$ , it must not have existed at any time before  $t$ . Locke, for example, made the claim that "one thing cannot have two beginnings of existence" (Locke, 1964, Book 3, 27:1). The proper response, I think, is that this is an indefensibly weighty meaning of what it is to come into and go out of existence. All we are committed to, I suggest, is that an entity ceases to exist at  $t$  if it exists immediately before  $t$  and does not exist immediately after  $t$ , and an entity begins to exist at  $t$  if it does not exist immediately before  $t$  and it does exist immediately after  $t$ . To insist that something can only come into existence if it has not existed at any time before is to beg the question against intermittent existence. Of course, we may want to infer *special* status to the event in which an entity comes into existence for the first time (and perhaps in such a way to appease Locke's intuitions about singular beginnings), or to the event in which an entity ceases to exist permanently, but it does not follow that coming into existence and going out of existence are concepts that only apply in this limited way.<sup>29</sup>

The principle NIE may instead be defended on grounds of individuation. Suppose an entity,  $E$ , comes into existence and then goes out of existence, and subsequently an entity  $E^*$  comes into existence. If kinds can have intermittent existence, it would be possible that  $E$  and  $E^*$  are identical. However, it may seem that  $E^*$  must be non-identical with  $E$  because of basic principles of individuation that require that we count them as two just in virtue of the discontinuity of existence. An illuminating discussion by Michael Burke is helpful at this point (Burke, 1980). Burke argues that there are some cases where ordinary material objects exist intermittently. Because Burke is not writing about kinds, the fine details of the discussion are not relevant to us, but his argument is roughly as follows. He considers a table made of thirty pieces of wood. The table is dismantled and those same thirty pieces of wood are then used to make a chair. Later the chair is dismantled and the pieces are put back into their original arrangement to make a table. Burke argues that in this scenario we have good reason to accept that the table ceases to exist when the wood is arranged as a chair, but that the original table exists again after the wood is put back into its former arrangement. The table then has intermittent existence. Towards the end of the paper, Burke defends the claim that the table at the end of the story is identical with the table at the beginning. He recognises that objectors will be motivated by a "criterion of particular identity".

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<sup>29</sup>For a similar point, see Simons (1987, pp.195-196).

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This criterion is described by quoting Henry Laycock:

[A]n object  $m$  at  $t_1$  is identical with an object  $n$  at  $t_2$  only if  $m$  is spatio-temporally continuous with  $n$  between  $t_1$  and  $t_2$  under some covering concept F. (Laycock, 1972, p.28)<sup>30</sup>

Part of the motivation for accepting the criterion, Burke explains, is that otherwise there is no apparent way of making sense of the assertion that the later object, besides being qualitatively identical to the earlier, is identical to it numerically. For what would make the later object numerically the same (Burke, 1980, p.404)? Burke does have a response to this, but the key point to emphasise is that we can see clearly that these motivating intuitions about individuation are specific to ordinary persisting material objects, and kinds sidestep this worry neatly. In the case of kinds, what makes it the same kind being identified at a later time is the instantiation of the essential properties. No reference needs to be made to spatio-temporal continuity or unbroken existence, and the puzzle that Burke notes is not relevant.<sup>31</sup> The problem then is that an adherence to such a principle as NIE based on individuation criteria is unjustified for kinds. Though we may find ourselves prejudiced towards NIE, I believe that on inspection it should be treated in the same way that AL was treated: applicable to certain entities but not universally applicable, and in particular not applicable to kinds. NIE is nothing more than, as Peter Simons puts it, “a disposition based on our normal experience” (Simons, 1987, p.195). Though we may be disposed to accept NIE I argue that we have no reason to accept it. On the contrary, in the ontological category of kinds and the theory of kind physicalism we have reason to reject NIE. Some things do have intermittent existence.<sup>32</sup>

#### Objections to the Intermittent Existence of Artefacts

The second line of objection to my proposal challenges not intermittent existence *per se* but the claim that some entity or other in fact has intermittent existence in the way suggested. For example, the (supposedly) Aristotelian position that properties exist only when they have instances is often objected to not on the grounds that intermittent existence is impossible, but on the grounds that it is apparently demonstrable that properties exist even when nothing possesses them. Property existence is not intermittent, it is claimed, because their existence at a time does not depend on their being possessed at that time.

A similar argument may be levelled at the theory on offer here. Our intuitions about when repeatable artefacts exist, such as they are, do not neatly coincide with the existence of instances. In particular, we do not normally think of repeatables as temporarily going out of existence when they have no instances. Suppose that right now there is nowhere in the world where *THE NUTCRACKER* is being played or performed.

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<sup>30</sup>Quoted in Burke (1980, p.404).

<sup>31</sup>Burke defends his own theory against this challenge by appealing to the continuity of the parts of the table throughout the period during which the table itself does not exist.

<sup>32</sup>Indeed, if Burke is right, it is not only kinds that have intermittent existence.

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Could we really accept that if that was the case THE NUTCRACKER would not exist right now?

It cannot be denied that this appears problematic. However, I hope to show that giving up on it and accepting intermittent existence is not so serious a consequence as it first seems. To begin with, we need to consider carefully what exactly our intuitions are here and how our intuitions about existence should be handled. Our intuition seems to be that a song or a play exists at a time even if it has no instances at that time, but of course this is only relevant to our ontological account if we have reason to believe such an intuition. If ontological theory suggests one answer to the question of persistence and intuition suggests another, does the former show the latter to be mistaken or vice versa?

The obvious place to turn for help here is to the methodology of conceptual analysis defended in the first part of this thesis. If we can extract truths about the entities we are referring to by conceptual analysis, it might seem that we should take seriously our intuitions about the persistence of kinds. However, recall that in defending conceptual analysis I was careful not to endorse the view that ontology is beholden to *all* our pre-theoretical suspicions. We do not do ontology merely by listing all the things that seem pre-theoretically to be true about the entities of interest. Instead I argued that our basic dispositions concerning the application and co-application of terms are relevant in determining which entity we are referring to with that term.

However, little can be determined about the persistence of entities from these practices. We can recognise that if a kind is identified at time  $t$ , then the kind must exist at time  $t$  in order to be identified. This itself seems like an uncontroversial principle. However, this only provides a sufficient condition for existence at a time – one that does not conflict with the proposal that a kind exists when and only when it has instances, note – and little can be extrapolated in terms of broader principles. Nothing can be inferred about whether the kind comes into existence or goes out of existence, or under what circumstances this happens. The problem is that our dispositions to use a term are relatively opaque with respect to the persistence of the entity in question, the point just made about sufficient conditions notwithstanding. As such I think we ought to handle intuitions about when entities exist with caution.

An important reason for treating our intuitions about existence carefully is that there seems to be a number of different facts that we could be trying to express and that we may fear are being denied. Once these facts are made explicit in other terms, the temporary non-existence of the repeatable is much less worrying. We may find the claim that the artefact does not exist when it has no instances surprising, but what exactly are we reacting to here? Consider, for example, a song that is composed and played on Monday, but not rehearsed again until Wednesday. The claim being made here is that strictly speaking the song does not exist on Tuesday. But what truth about the world do we think this fails to capture?

It is still true that the artefact ‘exists’ in the loose sense that there have been instances and there will be instances, and things are such that instances can be produced from



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memory or from a score or other set of instructions. It is true that things on Tuesday are not as they were on the Sunday prior to the composition of the song, at which point the song was merely an unformed idea in the composer's head. We can even say the relevant difference is that something new has been added to the world. All an acceptance of intermittent existence means is that on Tuesday there is not at that time any entity that is identical with the song.

Furthermore, the artefact is still 'real' on Tuesday in that the name has a referent and we are in position to identify the artefact when it is instanced. For the sense of 'real' being appealed to here, we can contrast Beethoven's Fifth with Beethoven's Fifteenth. Even if, at a time  $t$ , Beethoven's Fifth is not being instanced anywhere there is still a significant difference at  $t$  between Beethoven's Fifth and Beethoven's Fifteenth. The former refers to a real (actual) repeatable artefact, where as the latter does not. Neither exists at  $t$ , according to the proposal set out here, but that does not put them on a par in other respects. Here we can draw a parallel with the distinction between a no longer living person and a fictional person: compare Julius Caesar with Hercules. Neither exist now, but 'Julius' refers to a real historical character where as 'Hercules' (we can assume) does not.

It is interesting to note our intuitions about substance kinds (masses) are generally more in keeping with a theory of intermittent existence. Suppose that all the aspirin in the world is used up and for a while no more is made. Does aspirin exist at that time? I think if our intuitions offer any answer here then they agree that when there are no instances of aspirin, aspirin does not exist. We might more normally say 'there is no aspirin', rather than 'aspirin does not exist', but it seems plausible that the latter sounds odd just because we don't mean to imply that there will never be any more aspirin, nor do we mean to imply that aspirin is not a 'real' substance.

What this discussion points to, then, is that intuitions about existence seem to be entangled with a number of other (related) beliefs about how things stand in the world at a particular time. Once these beliefs are untangled we face a choice. We can continue to insist that our initial gut response reflects the actual truth about the existence of such entities, perhaps claiming that this is just what 'exists' means in this context. Alternatively, we can allow a carefully articulated account of what these entities are like (developed, recall, in response to an analysis of our practices of identifying such entities) to indicate what should be said in response to this question. I find the latter approach more plausible here. If kinds are physical multiply located entities, then we should welcome the sharp clarity that this brings to an otherwise murky area.

#### **Creation and Existence**

With this account of the persistence of kinds set out, we are in a position to address the question of *creatability*: a particular focal point of debates in the ontology of art. Are repeatable artefacts created according to my account? The short answer is that if by 'created' we mean 'brought into existence', then yes, repeatable artefacts are created.

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Kinds exist when and where they have instances, on my view, so if there are no instances of the kind at a time, producing an instance will bring that kind into existence.

This might be taken as welcome news for those who think that works of music and the like are obviously created. However, some care needs to be taken here in understanding where my support lies. In ontological discussions, theorists such as Margolis (1980, p.22), Levinson (1980, 1987), and more recently Lamarque (2002, p.146) and Rohrbaugh (2003, p.190), have taken the creatability of the entities in question as a datum to be explained. Accordingly, that such things are created has been built into the subsequent ontological accounts, as we saw in §1.1 with Levinson's commitment to (Cre).<sup>33</sup>

I have argued on the contrary that we should be suspicious of building commitments to creatability into our theories, since that an entity, N, is created, does not seem to be part of our application and co-application conditions for the term 'N'. That a work of music is created is something that we could be wrong about, while still referring to that work of music.

Instead, that repeatable artefacts are created is, on my view, something that falls out of the more general metaphysical considerations discussed above.

Moreover, those committed to the creatability of works of music, for example, will be unsatisfied with my account of intermittent existence. On my view, even if a score has been written, the work itself does not come into existence until it has been performed. The unperformed symphony has yet to be brought into existence, and a minor symphony that is only performed or played occasionally only exists occasionally. Worse, am I to say that the occasionally performed work is created anew each time it is performed, and even then, not by the composer but by the orchestra and conductor? For some it will seem that my account gets out of the frying pan of eternal existence, and into the fire of something even more outlandish.

For example, Caplan and Matheson (2004) have argued that a view that holds that the entities (types, kinds) exist only when they are instanced (and here they discuss, briefly, an Armstrong-style immanent account of properties) allows for creation but violates what they call the *Persistence Requirement*, which they suggest is something else that we should intuitively be committed to for entities such as musical works:

Musical works must be such that they can exist uninterruptedly for a good stretch of time after the composer's compositional activity. (Caplan and Matheson, 2004, p.128)

They go on to say that, considering both the creation and the persistence requirements, "[s]atisfying one requirement should not come at the cost of satisfying another" (2004, p.128). However, no argument is given for this persistence requirement; it is just

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<sup>33</sup>Rohrbaugh speaks more broadly of the *temporality* of photographs: they both come into and go out of existence (Rohrbaugh, 2003, p.190). Note that the creationist view has also been defended on firmly metaphysical grounds. Howell (2002b) adopts a Levinsonian account of works of music as indicated types, and argues that the existence conditions for such types imply that they *cannot* pre-exist the acts of composition. Other defences are more conceptual. Nussbaum (2003) argues that musical works are created, rather than discovered, because, unlike mathematical proofs, "there is no timelessly valid deductive routes to specific musical works" (2003, p.284).

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offered as something as intuitive and unrevisable as the creation requirement. More importantly, for methodological reasons discussed above, I did not set out to satisfy the creation requirement *or* the persistence requirement, and so I will not be put out by failing to satisfy either. These kinds of objections from the creationists, then, will not bite.

There is, however, something else we can say about the notion of creation before ending the discussion. This is that it is far from obvious that our notion of *creation* should match exactly with the metaphysical notion of *coming into existence*. I very much share John Fisher's feeling that "creation seems to be just a place marker for our difficulty in conceptualising the relation of artists to their work" (Fisher, 1991, p.129). A metaphysical account that implies that a symphony comes into existence each time it is performed *need not* be committed to saying that it is created each time it is performed, because, it seems, being created may well be a *richer* notion than simply coming into existence. It is plausible that my account is compatible with artists and agents creating repeatable artefacts with authorial acts, in virtue of the fact that they are creatively responsible for all the subsequent instances being as they are (in a way that someone performing a work of music is not).

With that, I want to draw the discussion of creation to a close. A great deal more could be said along these lines, but it would principally involve further explication of our *concept* of creation, and that task is not something I am concerned with here. Metaphysically speaking, matters are clear. Repeatable artefacts exist when and where they have instances.

### Conclusion

The most important point to grasp about kind Physicalism is that kinds represent a unique way of identifying entities that occupy the physical world. Rather than being spatially and temporally bound entities, kinds are instanced wherever and whenever the essence of the kind is instanced. Thus while the description of kinds set out in Chapter 3 does not *entail* kind Physicalism (it is compatible with Platonism) the relationship is a natural one.

I have tried to overthrow the dominance of Platonism about kinds, but there is no doubt that Physicalism will still leave some metaphysicians feeling uneasy. This may be because the axiom of localisation, as understood in its limited application to ordinary objects, is deeply entrenched. Furthermore, it is hard to shift the intuition that, as David Wiggins put it, "material things have to compete for room in the world, and that they must tend to displace on another" (Wiggins, 1968, p.94). But the grip of this intuition should be loosened when it is emphasised that kinds and their instances belong to different ontological categories. Physicalism asks us to get used to the idea that out of the same basic stuff of the universe we can and do identify entities belonging to multiple ontological categories, and it is our very practices of identifying and individuating the entities around us that points to this. We are surrounded by and encounter inscriptions

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and utterances of words, copies of novels and performances of songs, but we are also surrounded by and encounter words, novels and songs themselves.

Part III.

Essences

## 5. Structuralism Rejected

At the heart of the theory of kinds being offered here is a commitment to the view that instances of a kind are instances of the same kind because they have something, or some things, in common. Instances of the same kind are not simply entities that happen to be called by the same name. To recap, for any kind, K, the properties, P, that every instance must have in order to be an instance of that kind can be called the essential properties of the kind, or more simply the essence of the kind. It is essential to being an instance of the kind K that an entity has properties P. Hence to hold that all the instances of a kind have something (or things) in common, and that having those common properties is what makes them instances of the same kind, is to hold that kinds have essences.

Now, of course, the all important question is this: If kinds have essences, and repeatable artefacts are kinds, what are the essences of repeatable artefacts? This isn't just a case of filling out the details. The challenges involved in providing suitable essences which we will meet have motivated some theorists to reject the whole framework. As such the plausibility of any kind theory (or suitably similar type theory) rides on the possibility of giving an acceptable account of the essences of the entities to which the theory is supposed to apply.<sup>1</sup>

I begin that task in this chapter by rejecting the 'default' view of the essences of repeatable artefacts, which assumes that artefacts have structural essences. In §5.1 I introduce structuralism as the view that kinds have structural essences. The discussion involves recognising the distinction between simple structuralism and what I have called modified structuralism. According to simple structuralism the essences of repeatable artefacts are purely structural. Modified structuralism is the view that the essences are partly structural and partly non-structural. While some theorists adopt simple structuralism, modified structuralism of some sort is far more widely accepted. I argue that at least in some cases, modified structuralism should be accepted over simple structuralism. Sections §§5.2 - 5.4 then deal with three challenges that any form of structuralism faces. I argue that the problem of *improperly formed instances* (§5.2), the

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<sup>1</sup>An important point of clarification is needed here regarding the scope of this discussion. If we consider kinds in general, we have no prima face reason to restrict the sorts of essences different kinds may have, thus any account of essences should be offered on a case by case basis. Restricting our enquiry to repeatable artefacts, we still have no clear reason to assume that *all* repeatable artefacts have essences of the same sort. As such, in what follows I will focus on paradigmatic examples such as works of music, novels, photographs and films. Hopefully, the extent to which these share a common problem and a common solution will become clear. A notable omission from this list is *words*. Though words provide a useful example of repeatability, they have had very little ontological attention in the literature (though see Kaplan (1990) and Cappelen (1999)). I think words raise a unique puzzle for essences, and I will not attempt to incorporate them into the account given here.

## 5. Structuralism Rejected

problem of *modal flexibility* (§5.3) and the problem of *temporal flexibility* (§5.4) jointly provide sufficient motivation to consider a rejection of structuralism entirely.

It should be made clear that none of these three issues taken individually constitute a conclusive rebuttal of structuralism. As with many other aspects of the ontological theory forwarded here, what we must engage with is a fairly subtle interplay between intuitions and theoretical virtues, and the defence of a given theory must be understood holistically. My aim then should be understood as that of building a case against structuralism. I will argue that the three issues mentioned give good reason, when considered together, to look elsewhere for an account of essences. The challenge then remains to offer a suitable replacement. If a kind theory is to remain plausible it must be demonstrated that a kind theory can be maintained despite abandoning structural essences. I take up that task in the final chapter, and whether one ultimately accepts the rejection of structuralism will depend significantly on the plausibility of the alternative. It is the comparative virtues of the alternative, as much as the inherent problems in structuralism, that will provide motivation for departing with tradition as far as essences go.

### 5.1. Structuralism Introduced

We both appreciate and recognise a piece of music, to a large extent, by attending to how it sounds. It is the structure of the sound that is particular to that piece of music that strikes us as being of primary importance, at least in many cases. The same can be said about the visual appearance of a photograph. A great deal of our aesthetic appreciation is directed towards visual structure. When we attend to a photograph we attend to how it looks. Again, with a poem or a novel we seem to be principally concerned with the word structure. The skill of the writer lies in her ability to choose which words to put where. The word order is surely not an accidental feature of the work but instead appears to be central in some way to what the work *is*.

In all these cases, structure is king. It is no surprise then that the obvious candidate, and for some the only candidate, for the essences of these entities has been structure. To be an instance of a novel it has been assumed that a copy must have the correct word structure. To be a performance of a work of music it has been assumed that the sound produced must have the correct sonic structure. To be an instance of a photograph it has been assumed that a print must have the correct visual structure. The essence of each repeatable artefact is to have just that particular sonic or visual or semantic structure. Charles Stevenson clearly made this assumption in his 1957 essay ‘On “What is a Poem?”’:

Having taken "What is a poem?" to be a way of asking what "poem" is to mean, I have given a partial answer to the question by saying that for an important sense, and subject to certain qualifications, "poem" refers to a sequence of words. (Stevenson, 1957, p.339)

## 5. *Structuralism Rejected*

Nicholas Wolterstorff also begins his account from a structuralist position when he considers the act of composing a work of music. The composer composes, he suggests, by “selecting a set of criteria for correctness of occurrence” (Wolterstorff, 1980, p.65). That is, the composer lays down the properties that instances of the kind must have to be instances of that kind. And because it is a work of music that is being composed the composer “must select a set of properties which sound-sequence-occurrences can exemplify – the property of being a piano sound of F pitch, the property of being a piano sound of A pitch, etc.” (Wolterstorff, 1980, p.62). Wolterstorff goes on to elaborate on this but the basic idea is that required properties for a musical work kind are sound-structure properties. And what could be more natural given that specifying this structure appears to be exactly what a composer is doing when writing a score?

The trend has been so strong that Guy Rohrbaugh, speaking of type and kind theorists, has recently remarked that

All agree that multiple works are individuated, at least in part, by the intrinsic qualitative and structural features of their occurrences. What two prints look like or what two performances sound like is clearly relevant to the question of whether they are prints or performances of the same work. (Rohrbaugh, 2003, p.5)

At this point we can usefully distinguish simple structuralism from modified structuralism. The theorist who favours simple structuralism holds that repeatable artefacts are individuated only by intrinsic qualitative and structural features. That is, in the language of the kind theory, they hold that the properties required of instances of artefact kinds are limited to structural properties. The modified structuralist holds that the required properties are structural “at least in part” but allows that kinds may also make certain other demands of their instances. It would be fair to say that simple structuralism, at least concerning the repeatable works of art discussed in the literature, is a minority position.<sup>2</sup> Julian Dodd favours simple structuralism for works of music, which he labels ‘sonicism’ (“what makes *In This House On This Morning* that work is that it sounds like that. Period.” (Dodd, 2000, p.425)), and is surely right to note that in defending his “unfashionable” position he is “swimming against a strong intellectual current” (Dodd, 2007, p.203).

In due course I will reject both simple and modified structuralism completely, but it is important to understand why simple structuralism has become so unpopular. We can start by looking again at Wolterstorff’s discussion of the act of composition. Wolterstorff claims, plausibly, that the composer of a musical work sets out the conditions for correct performance of his or her work. We have already noted that many of these conditions appear to be structural, but Wolterstorff also suggests that the composer may lay down conditions that go beyond this:

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<sup>2</sup>Note that there is no theoretical reason why one should not adopt a disjunctive approach, holding that some artefact kinds require purely structural properties of instances, while others require more complex combinations of internal structural and external properties.



## 5. Structuralism Rejected

The composer can also lay down as a condition for correctness that the sounds have the property of being produced in such-and-such a fashion on such-and-such instruments. (Wolterstorff, 1980, p.69)

That is, for a sound event to be an instance of a particular piece of music it may be the case that not only must it sound a certain way but that it must also be produced in the right manner – on the right instruments, for example.

This view – *instrumentalism* about works of music – has been championed in particular by Jerrold Levinson. Note the form of Levinson’s argument here:

Consider a sound event aurally indistinguishable from a typical performance of Beethoven’s Quintet Opus 16, but issuing from a versatile synthesizer... If performance means were not an integral aspect of a musical work, then there would be no question that this sound event constitutes a performance of Beethoven’s Quintet Opus 16. But there is indeed such a question... We can count something as a performance of Beethoven’s Quintet Opus 16 only if it involves the participation of the instruments for which the piece was written...(Levinson, 1980, p.16)

Here Levinson is making a claim about what counts as an instance of a particular work of music – only those sound events produced in the right manner, he claims – and so is making a claim about the properties required by sound events in order to *be* instances of certain works. Levinson does offer an argument for this (Levinson, 1980, pp.16-17).<sup>3</sup> His claim is that the full body of aesthetic and artistic properties of a work of music can only be transmitted via performances that are produced in the right kind of way. What we appreciate about a piece of music, Levinson claims, is not just how it sounds, but how it sounds in relation to the performance means.<sup>4</sup>

While this argument from appreciation seems to give a reasonable account of why we may include performance means in our practice of identifying what counts as an instance of a given work of music, what matters for us is the assertion that it is in fact the case that something counts as a performance of Beethoven’s Quintet Opus 16 if it produced in the right manner (i.e. using the performance means specified in the score). This assertion can (and has) been contested,<sup>5</sup> but if it is accepted then the apparent unavoidable conclusion is that modified structuralism should be preferred over simple structuralism, at least as far as works of music go.

Beyond instrumentalism for works of music there are a number of other arguments for modified structuralism that can be grouped under the name *contextualism*. If instrumentalism made demands on the immediate causal histories of music performances

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<sup>3</sup>Note here that I am ignoring a distinction, peculiar to certain types of repeatables, between a performance and an instance. Slightly counter-intuitively, I think, Levinson treats instances as subclass of performances (1980, p.26). I would prefer to see performances as a subclass of instances, being those instances produced by a performance action. However, for our purposes, the issue can be regarded as more terminological than substantial.

<sup>4</sup>“To assess that character correctly one must take cognizance not only of the qualitative nature of sounds heard but also of their source of origin” (Levinson, 1980, p.17).

<sup>5</sup>See e.g. Dodd (2007, pp.225-239).

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(‘performance means’), then contextualism can be understood as making demands on the long term causal histories of instances.

A very simple form of the contextualist argument can be made for photographs. To be an instance of a particular photograph, it is plausible to think, a print must not just look a certain way, but must also emanate from the right photo-taking action. For example, prints of your slightly blurred photograph of the Eiffel Tower against a blue sky may be visually indistinguishable from prints of my photograph of an accurate miniature model of the Eiffel Tower against a blue sky, but the two prints are not thereby prints of the same photograph. To be an instance of the photograph I took, a print must not just look a certain way, but also have a certain historical provenance connecting it to my photo-taking action. There are other examples involving more obviously evaluative aspects: a particular piece of photo-journalism may be admired for capturing a particularly significant moment of spontaneity. A visual duplicate produced by careful staging would be both a different photograph and far less admirable (or admirable for different reasons). Whether or not a print is a print of the former or latter photograph will depend not just on how the print looks (structural properties) but also on the historical properties of the print, or so it seems.

These intuitions about photographs no doubt arise from the fact that photographs are usually ‘of’ something or other. They are not just visual structures but represent actual objects. Regardless of how structurally similar two prints are they will be different photographs if they are of different things.<sup>6</sup> What a particular print is of will depend on the causal history of the print. However, the same phenomenon can be observed even when the relevance of the causal history is more subtle. Robert Howell offers the example of the “clockwise hooked cross used in pre-Columbian Amerindian cultures” which is visually indistinguishable from the Nazis’ swastika but the two “count as distinct symbols” in part because “[t]ribespeople will not count the Nazis’ mark as another example of their own symbol” (Howell, 2002b, p.120). Turning to the more fanciful realm of thought experiments, if space explorers discovered an alien race flying a white rectangular flag with a red cross on it, we would be unlikely to say that our new friends were flying St. George’s flag, despite the structural similarity. Plausibly, to be an instance of St. George’s flag something must not only look a certain way, but originate from England and other earlier examples of the flag. With photographs, swastikas and St. George’s flag we can be persuaded to move from simple structuralism to modified structuralism, accepting that to be an instance of the artefact an entity must have more than just the right structural properties.

Moving from simple structuralism to modified structuralism seems highly plausible at least in some cases. That is, at least for some repeatable artefacts the properties required of instances appear to be more than just structural properties. I do not claim to have covered all the bases in the arguments for some form of modified structuralism over pure structuralism, however. Enough energy has been spent on that elsewhere

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<sup>6</sup>Note that this specific line of thought will not be relevant to pure instrumental musical works as it is unlikely that they have representational properties (of the sort relevant here).

and, more importantly, my target here is not pure structuralism but structuralism in any of its forms. We will see that there are still significant problems for both simple and modified structuralism. In the next sections I will look in detail at these problems and suggest that they provide enough motivation to take seriously the idea of giving up on structuralism entirely.

## 5.2. The Problem of Improperly Formed Instances

One way to think about the move from simple to modified structuralism is as a problem for structuralism that was easily patched: allow for the addition of causal, historical and relational properties and our intuitions about what counts as an instance of this or that photograph can be accounted for. But the demand for plasters is not yet over. Three larger problems loom that cannot be so easily solved. The first is the seemingly widespread possibility of properly and, more importantly, improperly formed instances of repeatable artefacts. The second and third problems concern the possibility of repeatable artefacts being modally and temporally flexible: structuralism must deal with the intuition that as far as structure goes, we think that some repeatable artefacts could have been different, and in some cases may even change through time. I should emphasise that these problems for structuralism are not entirely conclusive. Responses can and have been given, and some will be discussed. However each problem does constitute part of the case against structuralism.

I will look first at the issue of properly and improperly formed instances. The problem for a structuralist kind theory is straightforward. According to a kind theory, the kind demands that its instances have certain properties in order to be instances of that kind. Anything possessing these properties is an instance of that kind, and anything lacking any of these properties is not an instance of that kind. However, our practices of identifying repeatable artefacts appear inconsistent with this if it is assumed that the essential properties are structural. For example, on this view a poem is such that copies of that poem should have certain structural features. But we allow that a copy of the poem can have some errors in it – deviations from the standard structure – and yet still be a copy of that poem. It will merely be an improperly formed copy of the poem. Similarly, a performance of a work of music with a few wrong notes is still a performance of the work, and a telling of a story that gets one or two details mixed up is still a telling of the story, albeit a bad one. How can a kind theory account for these improperly formed instances? According to the kind theory, do they not just fail to be instances of the artefacts in question in virtue of falling short on some of the required properties?

Here are two unacceptable solutions to this problem. The first ‘solution’ takes a leaf out of Nelson Goodman’s book (Goodman, 1968) and simply denies the existence of improperly formed instances. Goodman’s own (nominalist) theory of notational systems and the identity of works of music famously led him to the conclusion that a performance of a musical work must comply exactly with the score if it is to be a performance of

## 5. Structuralism Rejected

that work at all. Play a wrong note and what is played is not that piece of music but something else. Noting how much this conflicts with “ordinary usage”, Goodman is nevertheless willing to allow his theory to have the last word, otherwise “by a series of one note errors...we can go all the way from Beethoven’s Fifth Symphony to Three Blind Mice” and so “all performances whatsoever are of the same work” (Goodman, 1968, pp.186-187). The details of Goodman’s nominalism that result in this counter-intuitive conclusion can be safely passed over at this point, but might a kind theorist not want to adopt a similarly robust response to the possibility of incorrect instances? Why not say simply that while we *think* that there can be incorrect instances of poems or songs, we must accept our mistake in the clear light of philosophical enquiry? Surely philosophy should not be held hostage to our potentially error ridden pre-philosophical assumptions?

At this point, however, we can appeal again to the method of conceptual analysis outlined in Chapters 1 and 2 of this thesis. In §1.3 I argued that to epistemically determine the reference of our kind terms we must appeal to our intuitions and practices about the application conditions for those terms. Thus an account of what we are referring to must allow for the fact that we do identify repeatable artefacts despite their being improperly formed. Moreover, we cannot use some of our intuitions about what counts as an instance of an artefact kind to decide on a form of structuralism, and then subsequently reject our further intuitions about the possibility of improperly formed instances. Structuralism, or at least modified structuralism, seemed plausible because of our assumptions about what does and does not count as an instance of a given kind. It would be ad-hoc to allow those assumptions to take us so far and no further. A Goodman-style bite-the-bullet denial of improperly formed instances is thus unjustified and would be methodologically inconsistent. By departing from our concepts in this crucial respect, it can no longer lay claim to being an account of what we ordinarily mean by these terms.

A second unacceptable solution goes as follows. In the face of improperly formed instances it might be thought that the structures required by the kinds are just less specific than previously thought. This way a few wrong notes ‘here or there’ does not annul a performance from being a performance of that kind because the kind in question does not require such specific properties of its instances. Rather than a musical work requiring an exact sound structure of instances, it might be that it only requires a ‘higher-level’ structure that allows for variation at the level of individual notes. This move isn’t very satisfactory, however. For a start, it is hard to envision exactly what kind of structure would do the required work of allowing for some variation but avoiding overly radical differences. Furthermore if we adopted this higher-level structure into our theory we would struggle to make sense of the claim that the performance had *wrong* notes. The difference between correct and incorrect instances would be mysterious. If the work of music is a kind and the kind is less prescriptive, the ‘wrong notes’ performance would in fact be a perfectly acceptable instance of the kind and there would be no sense in which there were really wrong notes.

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A more popular and initially more plausible solution, put forward by Nicholas Wolterstorff, is to appeal to *norm-kinds* (Wolterstorff, 1980, p.57). In short, norm-kinds are kinds that can have properly and improperly formed instances. The idea is to accept from the start that repeatable works of art (and by extension other repeatable artefacts) can have properly and improperly formed instances and that this is because while there are certain properties that it is *essential* for the instances to have to be instances of that kind, there are also properties that are *normative*. Instances must possess all the normative properties required by the kind if they are to be properly formed instances, but they can be improperly formed instances without these. According to Wolterstorff every essential property will be a normative property, but not every normative property is essential. Thus a performance with wrong notes will have all the essential properties required by the kind but only some (perhaps most) of the normative properties. Exactly how many of the normative properties are also essential properties would then be down to the kind in question.

However, the main worry with this view is that it appears to be a somewhat ad hoc amendment to the basic theory of kinds. Recall that two things are of the same kind when they share properties, and the kind that is instanced in both cases is individuated by the properties it requires of its instances. In that original characterisation there was no place for a normative/essential distinction and it is not clear that such a distinction can easily fit with the basic theory. If a kind, K, requires that its instances have properties P, anything with those properties will be an instance of the kind, and anything lacking those properties will fail to be an instance. To then amend this, so that there are normative as well as essential properties, appears to be a rather ‘purpose built’ solution to the difficulty of improperly formed instances.

Neither Wolterstorff, nor Dodd, who adopts Wolterstorff’s account, say very much about this (Dodd, 2007, p.32). Wolterstorff simply allows that some kinds can have improperly formed instances, and then builds that allowance into his definition by stipulating that some properties are ‘normative within’ a kind (Wolterstorff, 1980, p.58). Dodd adds that in the case of norm-kinds, the identity of the kind is then determined by the conditions something must meet to be a correctly formed instance (Dodd, 2007, p.32). Neither of these suggestions clear norm-kinds of their air of mystery. Dodd rebuts the charge of ‘ad-hocery’ by appealing to the *familiarity* of norm kinds. They are “common-or-garden entities” which are “part of the fabric of the universe” (Dodd, 2007, p.33). Not only are musical works and other art works norm-kinds, Dodd suggest, but so are words and many natural kinds (such as THE POLAR BEAR). Thus with an appeal to norm-kinds the theorist is “not guilty of plucking a notion out of thin air” (Dodd, 2007, p.33). I’m not sure that Dodd’s appeal to familiarity does the work that he needs here. What he shows is that it is common for us to see things as having properly and improperly formed instances (or more properly and less properly formed instances). However, this doesn’t count as evidence that such a practice is best analysed in terms of norm-kinds as described by Dodd and Wolterstorff. At best, what Dodd shows is that we might reasonably expect a common analysis of properly and improperly formed

instances in all these cases, and that finding a solution might have application beyond repeatable artworks.

Of course, this doesn't constitute a knock down argument against norm-kinds as a solution to the problem of improperly formed instances, but it should persuade us to be receptive to alternatives. The positive proposal I outline in Chapter 6 is just such an alternative. We will see there that it is possible to deal with properly and improperly formed instances by giving normativity a natural place in the overall account.

### 5.3. The Problem of Modal Flexibility

The problems of modal and temporal flexibility are raised for repeatable works of art by Guy Rohrbaugh, and the following account partially draws on his discussion (Rohrbaugh, 2003). We can begin by elaborating on the concept of modal flexibility. Roughly speaking, an entity is modally flexible if it could have been different in some respect. If we read 'some respect' broadly enough, it will seem that everything is modally flexible (unless a strict form of modal actualism were assumed – roughly the thesis that nothing could have been different in any way from how it actually is). As such, when discussing modal flexibility, it is useful to speak of entities being modally flexible in some *specified* respect.

For example, it seems plausible that the number seventeen is not modally flexible with respect to its status as a prime number – seventeen could not have been anything other than a prime number. Likewise, the number sixteen is not modally flexible with respect to its factors – it is not possible for sixteen to have or to have had any factors other than one, two, four and eight and sixteen. However, seventeen and sixteen are modally flexible with respect to other 'extrinsic' or 'relational' properties. Both numbers are used as examples in this paragraph, but they might not have been. They could have been different in that respect. Similarly, if Kripke is right about the necessity of origin, then because Barack Obama could not have had different parents, Obama is not modally flexible with respect to his parents (Kripke, 1980, pp.110-115). However, he *is* modally flexible with respect to his height – he could have been taller or smaller.<sup>7</sup>

We can note here that this notion is a *de re* modal notion (Rohrbaugh, 2003, p.181). To say that Obama could have been different in some respect is not a *de dicto* claim about possibly true sentences, but the claim that *that very person* could have been different. Furthermore, as we have seen, the issue is not one of simple logical possibility (concerning the avoidance of logical contradictions) but rather is about metaphysical possibility taking into account the very nature of the entity in question.

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<sup>7</sup>Rohrbaugh (2003, p.181) argues that what we are interested in is modal flexibility with respect to intrinsic properties: something is modally flexible if it could have been different "in and of itself". However, it seems to me that appealing to an intrinsic/extrinsic distinction fudges the real issue. We are interested, firstly, in whether or not an entity could have been different in some respect, and, secondly, whether or not our theory for those entities allows them to have been different in that respect. This appears to be a more precise way of handling the issue than bringing in an internal/external distinction.

## 5. Structuralism Rejected

How does all this relate to structural kinds? The apparent problem is that repeatable artefacts seem to be modally flexible with respect to certain structural properties; i.e. they could have been different structurally, at least to some degree. However, as stated in §3.3, kinds are modally inflexible with respect to the properties required of instances. Thus if repeatable artefacts are structural kinds, it would seem that they could not have been different structurally. First I will argue for the claim that repeatable artefacts are modally flexible in certain important respects, before going on to explain in more detail why this causes problems for structural kinds.

The first task is straightforward. Consider Austen's novel, *EMMA*. *EMMA* has a particular word sequence. Let's say that *EMMA* has word sequence *S*. But suppose that when writing the novel, Austen had chosen one word differently; perhaps one instance of 'very' is replaced with 'extremely'. Could she not have done this? And if so, would that very same novel *EMMA* not have had a very slightly different word sequence, *S\**? It seems straightforward to imagine that *EMMA* could have had word sequence *S\** instead of *S* and so, at least pre-theoretically, it is highly plausible that *EMMA* is modally flexible (could have been different) with respect to its word sequence. This doesn't mean that *EMMA* could have had any old word sequence. Too great a departure from *S* is surely unacceptable. *EMMA* could not have had the same word sequence as the rhyme *THREE BLIND MICE*. However, this kind of limited flexibility is commonplace for ordinary objects. Mt. Everest could have been a few metres taller, but it – that very mountain – couldn't have been a mole hill on Clapham Common.

Examples like *EMMA* can easily be multiplied. Beethoven's Fifth could have been a few bars longer it seems; the family photograph taken last winter could have had a slightly more realistic colour balance; Peter Jackson's film adaptation of *THE LORD OF THE RINGS* could have been shorter, and could have stayed closer to the original novel. If these statements are true then at least some repeatable artefacts are modally flexible with respect to structural properties.

To see why this is at odds with the thesis that repeatable artefacts are structural kinds, we need to recall the characterisation of kinds set out in §3.3. I argued there that the individuation principle for kinds, PIK, should be given a strong reading. Two kinds are the same if and only if they require the same properties of their instances, and this applies to both actual and possible kinds. Thus a kind could not have required different properties of its instances from those it actually does require. I noted in §3.3 that this condition for kinds (and a related condition for types) is widely assumed in the literature, but that it is actually quite difficult to show decisively why kinds must be understood like that. The argument I did give involves understanding the intuitive notion of two things being 'of the same kind' as applying across possible worlds as well as across times in the actual world. In other words, if two entities are instances of kind  $K_1$  in virtue of having properties  $P_1$ , then it is impossible that they could have been instances of  $K_1$  without possessing properties  $P_1$ , for had they failed to have properties  $P_1$ , they would not have been of the same kind as they actually are.

The consequences of this position for structuralism about repeatable artefacts is that

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if a structural kind actually demands that its instances have structure S (if the essence of the kind is to have structure S), then every actual and possible instance of that kind must have structure S. It is not possible for instances of that kind to have had different structural properties. Recall Austen's novel EMMA. We noted that it seems highly plausible that EMMA could have had a different word sequence, S\*, instead of the actual word sequence S. If this were the case then instances of EMMA would have had word sequence S\*. However, this is impossible if EMMA is a structural kind that demands that its instances have word sequence S. The entities in the hypothetical scenario with word sequence S\* could not possibly be instances of EMMA.

Consequently, either EMMA is not a structural kind, or, contrary to our intuitions, EMMA could not have had a different word sequence from the one it actually has. This is the problem of modal flexibility. We can generalise the problem in terms of an inconsistent triad:

1. A particular repeatable artefact, A, could have been different (is modally flexible) with respect to structural property P.
2. Artefact A is a structural kind and so property P is a property that A demands of its instances (in that an object must have property P in order to be an instance of A).
3. If A is a kind, A is modally inflexible with respect to the properties that it demands of its instances.

One cannot consistently hold all three of these, and so one or more must be rejected. (1) is based on our intuitions concerning ways that repeatable artefacts might have been; (2) follows from the fact that many of these intuitions concern structural features of the artefacts and from the assumption that the artefact is a structural kind; and (3) follows from the claim that kinds are strongly individuated by the properties required of instances.

It may be tempting to avoid the inconsistency by rejecting (3). After all, (3) relies on a claim about the nature of kinds for which no *conclusive* argument has been offered. Should we not re-think the nature of kinds to allow for modal flexibility? While I think going down that route would constitute an interesting line of research, it would involve a significant overhaul of our understanding of the 'same kind' relationship, and mark a break with the traditional understanding of kinds. As such, I will continue with commitment to modal inflexibility expressed in §3.2.<sup>8</sup>

What about rejecting (1)? To reject (1) is to reject our intuitions about what can and cannot be an instance of a particular kind. It is to say that despite our intuitions it is not possible for an entity with word sequence S\* to have been an accurate instance of EMMA. Thus to reject (1) is to just insist that when we say that Bruckner's Ninth

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<sup>8</sup>It is possible to view the conclusion of the next chapter as conditional on the modal inflexibility of kinds. That is, if kinds *are* modally inflexible in the way discussed, then firstly this raises a problem for structuralism and secondly that problem can be dealt with by appealing to a theory of copied kinds as set out in the next chapter.



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Symphony could have been finished, or that Austen could have chosen a few different words ‘here and there’, what we should really say is that Bruckner could have written a distinct but very similar longer work, and that Austen could have penned a numerically distinct but very similar novel. The structuralist can even say that this distinct but similar novel would have gone by the name ‘Emma’. According to our best theory, it might be claimed, we are shown to have been in error over the modal flexibility of works of art and other repeatable artefacts. So much the worse for those pre-theoretical intuitions.

Gregory Currie, discussing the relationship between a literary work and its text, has similarly argued that such talk of counterfactual variation is a “poor guide to the truth”. He goes on to say:

It’s up to the best theory of work identity to tell us which way of saying is correct, and our unreflective talk about counterfactual divergence between work and text cannot decide the issue. (Currie, 1991, p.327)

However, our intuitions regarding the application of kind terms cannot be so lightly dismissed. The reason is the same as the one already given in response to the ‘bite the bullet’ Goodman-style answer to improperly formed instances. That is, our intuitions regarding what a kind term would apply to in possible circumstances constitute an aspect of the application and co-application conditions associated with that term. That fact that we believe that instances of EMMA could have had a different word sequence from that which they actually do have says something about which kind we are picking out with the term ‘Emma’. As such, our intuitions which result in (1) should be taken seriously.

That leaves (2) to be rejected: the problem of modal flexibility gives us reason to think that repeatable artefacts are not structural kinds.

### 5.4. The Problem of Temporal Flexibility

The final component of the case against structuralism is the problem of temporal flexibility. If modal flexibility pivoted on the intuition that repeatable artefacts *could* have been different in certain structural respects, then temporal flexibility pivots on the intuition that some repeatable artefacts *can actually be* different from one time to the next in certain structural respects. However, if repeatable artefacts were structural kinds, this change would not be possible.

To begin with, we need to be clear about exactly what claim is being made regarding change. The claim here is not that individual instances can change structurally over time (though they may), but that successive instances of a kind can differ structurally from one another in a manner that appears to contradict the structuralist’s claims.

This last clarification is needed because the structuralist can allow for some structural variation among instances without any problem. Performances of a work of music may allow a certain amount of leeway for the creative interpretation of the performer without

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the performance thereby departing from the required structure, the structuralist will argue. Furthermore, some structural variations among instances are irrelevant to the identity of the artefact, even on the structuralist account. For example, the exact size of a copy of a novel and the colour of the cover are structural features of the instances that are irrelevant to whether or not they are instances of a given novel.

The kind of structural variation we are interested in, then, is variation in structural aspects that would otherwise be taken, by the structuralist, as being part of the essence of the kind. Suppose, for example, that a literary work has word structure *S*, so that on the structuralist account, having word structure *S* is part of the essence of that kind. However, it seems that in some cases an author can make amendments to the original word structure so that new instances of the work must have the new structure *S\**.

For example, consider an academic paper or thesis. When the work is sent away to the reviewer or the examiner we normally speak of the reviewer as suggesting structural changes to that very piece of work. Once the work is finalised and published, an editor may say that changes can no longer be made, but this could easily be seen as a practical limit imposed for the sake of the publication, rather than as a metaphysical claim. This thought is especially forceful if we consider electronic publications. A lenient editor might allow the author to make tweaks here and there after the initial publication, but should surely prevent this for the sake of the readership (just as an art curator should dissuade an artist from continually touching up their painting after the gallery is open to the public).<sup>9</sup>

Stories also seem to provide good evidence of this kind of structural change. Folk stories, passed on orally from generation to generation, seemingly get changed in small and sometimes large ways as details are added or removed and as emphasis shifts. The story that a veteran tells his granddaughter every time she visits may change over the years as pieces are remembered, forgotten or invented. Novels and other artefacts published serially also seem to undergo the change of getting longer as they are written. Arguably this applies to all novels, given that their production takes time, but the point is more obvious when the work is published along the way. In these cases we can identify the story by identifying the published parts (the newspaper column or TV drama episode) and then await the ‘rest of the story’, which will be the next part of that story. And it is no problem here that the next part of the story may not yet have been written. In something like the same way that adding new bricks makes a wall longer, as new instalments are written the story gets longer. In both cases the thing being added to undergoes qualitative change.<sup>10</sup>

One final example: digital post production makes easy the structural alteration of photographs, or so it seems. When I remove the red-eye from a holiday snap our intuitions tell us that I am making a structural change to that very photograph. Note

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<sup>9</sup>Roman Ingarden thought this sort of change uncontroversial: “No one, however, will dispute the fact that it is possible to change a literary work in the event that the author himself, or the publisher of a new edition, sees fit to delete this or that passage and introduce another” (Ingarden, 1973, p.11).

<sup>10</sup>Recalling the discussion in §4.3, we can note that the when talk of ‘parts’ of the story, we are talking in terms of kind parts.

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that when I do this digitally I am not just altering a photograph token, but the photograph type (the kind) itself. Any subsequent instances of the photograph will reflect the change. Similarly if I crop the photograph I am changing how it – *that* very photograph – looks.

The reasons that the phenomenon of temporal flexibility causes problems for kind structuralism are familiar from the discussion of modal flexibility. Firstly we need to note that all the apparent changes offered in the examples amount to changes in the ways that instances are structured. If an academic paper is changed in the light of suggestions then any new copies of the paper must reflect this change if they are to be accurate. Likewise with a photograph that is cropped or adjusted: after the changes any accurate print of the photograph will have to reflect the new features. Now recall that a structural kind requires that its instances have particular structural properties, and that entities are of the same structural kind if they have the same structural properties. There cannot then be a change in the structural properties required of the instances without a numerical change of structural kind. Thus if repeatable artefacts do change in the ways suggested by the examples above, repeatable artefacts cannot be structural kinds.

There are two lines of objection to this that I want to consider here. The first involves questioning the strength and validity of our intuitions regarding change. This objection can be brought out by noting that there is an ambiguity in our talk of change. If I say that I changed my bike I could mean either that the bike that I had before, and which I still have now, has undergone a change, perhaps of colour. I could also mean that I have bought a new bike and abandoned the old one. With bikes, the ambiguity is easy to resolve, but it might be thought that this is only because we have a relatively firm idea of the ontological categories involved. When ontological matters are more puzzling we might not be so sure of what we do mean. When the lawyer accuses the witness of changing their story, do they mean that the same story has undergone qualitative change or that the witness has offered a numerically different story?

These queries allow some room for supporters of kind structuralism (and related type theories) to resist the phenomenon of change as a pre-theoretical mistake. Dodd, for example, appeals to our understanding of different ‘versions’ of repeatable artefacts to downplay the appearance of qualitative change:

It is quite true, for example, that we commonly describe works as being ‘revised’, but such talk is unreflective, and in any case, it is not obvious that we should regard a revised work as a previous work that has changed, as opposed to being a distinct work whose composition was based upon an appreciation of the original one. (Dodd, 2007, p.87)

The argument here is straightforward. Although we might *think* that the photograph or the paper (or the work of music) has been changed, it does not take a great deal of conceptual revision to accept that each digital adjustment of the photograph (for example) actually creates a numerically new version of the photograph, and this conceptual

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shift is easy to make because in some cases we do speak of different versions as if they are numerically distinct. Where our intuitions are unstable, they can be resisted.

However, while the point about the ambiguity in our talk of change is surely right, it is our practices of re-applying the names for kinds in these situations that matter. Though we may not be able to draw any firm ontological conclusions from phrases such as ‘the novel has changed’, what we can take seriously is our tendency to re-apply the same name after some structural change has taken place.

As we have seen before, the structuralist will be tempted to bite the bullet on change and re-interpret all our claims of change as identifications of new repeatable artefacts. The paper or thesis that is returned with minor adjustments would not in fact be the same paper or thesis, but rather a new artefact merely based on the old one. The removal of red-eye would not be removal of red-eye from a photograph, but the replacement of the photograph with a new very similar one. The structuralist will say that our ordinary talk is ontologically opaque and, moreover, that practitioners are not interested in ontological distinctions.

But revising our practices or applying terms in this way is precisely what the method of conceptual analysis advises against. Modified structuralism seemed attractive because our practices at first glance suggest that we apply the term ‘Emma’ only if a book copy has the right word structure. Once the phenomenon of change is brought to light, so that what counts as ‘the right word structure’ can change in some cases, we should not stick dogmatically to our original assessment. If we want to give an accurate assessment of the essences of the entities we are referring to, we had better take all our understanding of application and co-application conditions seriously.

The second objection to consider questions the coherence of temporal change for repeatable entities. The argument, presented again by Dodd, hinges on the thought that if an entity undergoes genuine change, its previous state must be a thing of the past, resigned to the past. Dodd puts the argument as follows:

if a work were to undergo genuine change once it has been ‘revised’, it would no longer exist in its earlier state, a corollary plainly contradicted by the fact that an earlier version of the work may still be performable (if, for example, the original score is recoverable, or if someone remembers it). (Dodd, 2007, p.149)

The point can easily be made for digital photographs or word processed documents: ‘If the academic paper had really undergone change,’ Dodd would likely suggest, ‘it would not exist in an earlier state and so it would not be possible to print out the very same paper in its previous form.’

However, the reason that this argument is not conclusive against the structural change of repeatable artefacts is that it relies too heavily on our common sense notion of change. Dodd is perhaps right to point out that our ordinary notion of change raises a problem for repeatable entities, because our ordinary notion requires that for an entity to have genuinely changed at time  $t$  it must no longer exist after  $t$  in its pre-change state.

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However, the phenomenon we are trying to capture is the apparent temporal flexibility of repeatables as described above, and it may well be that this phenomenon does not mesh perfectly with our ordinary notion of change. What we do need to account for is the apparent fact that over time what counts as an accurate instance of a repeatable artefact can change. Whether one regards this as a case of genuine (ordinary) change for the artefact itself is, to some degree, beside the point.

### **Conclusion**

So far I have discussed a number of problems for the view that repeatable artefacts are kinds with structural essences. I suggested that simple structuralism, whereby repeatable artefacts have only structural essences, has been rejected for good reason, but also that modified structuralism faces problems from improperly formed instances, and modal and temporal flexibility. All of these arguments relied on taking our intuitions about the application of a term seriously, guided by the belief that these intuitions are indicative of precisely which entity is being picked out by a term. However, it will be no good insisting that our theory should agree with these intuitions if, in attempting to appease these intuitions, no coherent or remotely plausible theory can be produced. As such, the argument against structuralism will only be complete after a coherent and believable theory is produced that accords with these anti-structuralist intuitions.

With structuralism on the ropes, even in its modified form, kind theory faces its biggest challenge. It will seem to many that a rejection of structuralism is tantamount to a rejection of any kind or type framework.<sup>11</sup> However, a central argument of this thesis is that that is a mistake. With enough care and sensitivity to the evidence, I argue, an entirely non-structural kind theory can be shown to resolve many of the issues whilst maintaining both ontological clarity and solid explanation of repeatability. Moreover, jettisoning structure from the essences of repeatable artefacts opens the door to an unexpected consequence: many repeatable artefacts can be understood to have a high degree of naturalness. Developing and defending this view will be the task of the final chapter.

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<sup>11</sup>Similar arguments to those presented here have persuaded Rohrbaugh to suggest that repeatable entities belong to an entirely new ontological category of 'embodied individuals' (Rohrbaugh, 2003). I consider some of the difficulties facing Rohrbaugh's suggestion in §6.5.

## 6. Repeatable Artefacts as Copied Kinds

We have good reason to think that repeatable artefacts are not kinds with structural essences. Does this mean we have good reason to think repeatable artefacts are not kinds? No. In this chapter I will argue that many repeatable artefacts have non-structural essences. What is needed is a coherent and plausible account of these non-structural essences; an account that ties in with the basic kind framework and that does a better job of allowing for and explaining the apparent properties of repeatable artefacts than the structural proposal. If this can be provided – and I will argue that it can – the lack of structural essences does not provide a reason to reject the framework of kinds. I will argue that repeatable artefacts are *copied kinds*. The essence of a photograph or a novel or an academic paper – i.e. the group of properties required for an entity to be an instance of that artefact – is, on this view, purely extrinsic and relational. To be a print of a particular photograph, for example, the required property is not that it *look* a certain way, but that it be *related* to other entities in the right way.

Giving up on structural essences entirely requires something of a paradigm shift in how we think of essences. However, the move I am suggesting for repeatable artefacts is not unprecedented. The ontological challenge generated by the absence of structural essences has shaped debates about the nature of biological species over the last thirty years (although the absence of structural essences has been apparent for much longer). To prepare the ground for an account of repeatable artefacts as copied kinds, then, I begin this chapter by setting out the debate over the ontological nature of species (§6.1). Assuming that a lack of structural essences is a lack of essences simpliciter, there has been spirited and still popular move to deny that species are kinds at all. However, this can be seen as an over-reaction to the absence of structural essences. An alternative solution has also emerged: rather than throw out the baby of kinds with the bathwater of structural essences, we merely need to look for essence elsewhere. It has been argued that a kind theory can be retained once it is accepted that the essences of species are purely historical and relational.

Drawing on this response in the species debate, I then set out the proposal that repeatable artefacts are copied kinds (§6.2). Crucially, the story I give allows for structures to play the central role that they do play despite not playing the role of essences. In §6.3 I readdress the issues of modal and temporal flexibility and of improperly formed instances in the light of the new proposal. One of the most attractive features of the copied kinds theory is its ability to handle intuitions about ways that artefacts could have been and ways that they might be able to change through time. Moreover, copied kinds allow for improperly formed instances as a natural consequence of the theory,

rather than as a feature that must be shoehorned in once the bulk of the theory is in place.

In §6.4 I address some possible objections to this theory of copied kinds, before ending the chapter with a discussion of an alternative account proposed by Guy Rohrbaugh that views repeatable artefacts as embodied individuals (§6.5). I argue that although this view rightly rejects structural essences, it fails to stand up to scrutiny as an ontological proposal and should not be preferred over the theory of copied kinds.

## 6.1. The Ontological Status of Species

Biological species are commonly used as paradigm examples of natural kinds in philosophy. We rely on scientists to tell us what species there are, and, for any particular biological organism, which species it belongs to. Scientists, we presume, can also tell us what the essence of a species is, in the same way that they have told us that water is  $H_2O$ . However, this familiar view of species as natural kinds with discoverable essences has suffered sustained criticism from both biologists and philosophers of biology. The challenge to orthodoxy over the nature of species began perhaps most forcefully with a series of publications by Michael Ghiselin (1966; 1969; 1974).<sup>1</sup> According to this line of thought, the modern biologist's concept of a species suggests that species are better understood not as natural kinds, but as individuals. In what follows I will assess the arguments for this revisionary conclusion, but show that rejecting a kind theory for species can be seen as an over-reaction to the fact the species do not have structural essences. We will see that a kind theory can be retained if the essences are understood to be historical and relational.<sup>2</sup>

### The Rejection of Species as Kinds

The concept of a kind at play in this debate is that of an abstract entity that lays down necessary and sufficient conditions for its members. Though in this thesis I defend a view of kinds as physically located entities, there is enough common ground in the view that kinds have essences to make the debate over species entirely relevant. The most influential reason for the rejection of species as kinds, and the one most important for us, is that many take post-Darwinian evolutionary theory to have shown that species *do not have structural essences*. Being a member of a biological species is not a matter of possessing a set of necessary and sufficient characteristics, it is argued, as no such set can be found that is possessed by all and only the members of that species, even if one looks as far down as the genetic code.<sup>3</sup> As Samir Okasha has forcefully put it:

The idea that species can somehow be “defined in terms of their DNA” has

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<sup>1</sup>See also Hull (1976).

<sup>2</sup>Here I draw out only the key points in the debate, emphasising that which is most relevant to the ontology of repeatable artefacts. For a recent treatment of the issue and thorough bibliography, see Crane (2004) and Rieppel (2007).

<sup>3</sup>Ghiselin (1974, pp.537 -540), Hull (1976, p.176), and Okasha (2002, pp.196-197).

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no basis in biological fact, despite what many non-biologists appear to think.  
(Okasha, 2002, p.197)

It is important to see that the absence of structural essences is not being posited because those essences have yet to be found, otherwise we might think that problem is just that the microscopes are not powerful enough, so to speak. Rather the absence of such essences seems to be built into the modern biologist's concept of a species. Decisions among practising biologists about whether a particular organism belongs to a given species do not appeal to facts about the internal structure of the organism. When biologists name a species, and apply that name to organisms, it seems that what they are emphatically not doing (to the extent that there is a consistent shared practice) is applying the name because of some shared set of structural properties that constitute necessary and sufficient conditions for membership in that species. Rather, modern biology recognises species in terms of genealogical groups and evolutionary branches (clades).<sup>4</sup>

Because being a member of a species is not understood in terms of an organism satisfying some necessary and sufficient structural properties it has been assumed that species lack essences entirely. In other words, it has been assumed that there are no necessary and sufficient conditions for belonging to a given species. Since they apparently lack essences, species cannot therefore be kinds.

This lack of structural essences also relates to two further characteristics of species that, it has been argued, sets them apart from kinds. Firstly, species are things that can evolve, which is to undergo change. They display what we have called temporal flexibility. On traditional understandings of kinds and their essences (where the essences are understood to be structural) no such change would be possible. The motivations for thinking this are just those we discussed in the previous chapter. Evolution represents a change in structure of the member organisms and can occur at any structural level. Thus if species had structural essences, member organisms at some later evolutionary stage could not be instances of the same species as member organisms at an earlier evolutionary stage. Taking the traditional view of kinds as having essences that cannot change (the view defended in this thesis), and (crucially) assuming that if species are kinds they have structural essences, it has been argued that species cannot therefore be kinds.<sup>5</sup>

Secondly, it is argued that species supposedly differ from kinds because species are spatio-temporally restricted whereas kinds (it is claimed) are not (Hull, 1976, p.176).<sup>6</sup> The notion of spatio-temporal restriction being appealed to here is best spelled out using examples. The claim is that the kind gold is not spatio-temporally restricted because something can be a sample of that kind irrespective of when and where it

<sup>4</sup>Roughly speaking, that is. There are currently around twenty proposed species concepts, each of which individuates species according to different criteria. See Griffiths (1999, p.222).

<sup>5</sup>"If species were not individuals, they could not evolve. Indeed, they could not do anything whatsoever. Classes are immutable, only their constituent individuals can change" (Ghiselin, 1987, p.129). See also Wilson (1995, p.340).

<sup>6</sup>See also Hull (1978).



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exists. By contrast, species are spatio-temporally restricted because for an organism to be a member of a species it must satisfy certain spatial and temporal restrictions. For example, for something to be a member of one of our Earth species it must be born into that species: “No matter how similar to our terrestrial horses Alpha Centaurian organisms may be, they are not members of the horse species” (LaPorte, 2004, p.10). This is because when an organism is assigned to a species it is not the structure of the organism that matters, but rather the historical origins of the organism. Hull writes that:

If a new species evolved that was identical to a species of extinct pterodactyl save origin, it would still be a new, distinct species. (Hull, 1978, p.349)

Being a member of a species is not a matter of being a certain way structurally, but instead depends on an entity’s place in a genealogical chain. The assumption behind this argument then seems to be that kinds do not have essences that involve restrictions on when and where the instance exists. Since species membership does involve restrictions on when and where the organism exists, species cannot be kinds.

### Species as Individuals

Given that it appears that species lack essences, are spatio temporally restricted, and can change through time, it has been claimed that they more naturally fall into the ontological category of individuals. On this view of species as individuals, they are “chunks of the genealogical nexus” or something similar - large ‘superorganisms’ that are the units of evolution and have the particular organisms with which we are more familiar as their parts. The tiger in London Zoo is a member of its species not because it is an instance of its species but because it is a *part* of its species. Likewise, you and I are *parts* of HOMO SAPIENS.<sup>7</sup> The proposal can be seen to be equivalent to the scattered object hypothesis for repeatable artefacts encountered in §3.2.

Viewing species as individuals neatly explains the lack of necessary and sufficient structural conditions for members of the species, since in general entities are not parts of a whole in virtue of certain intrinsic properties had by the parts. As individuals, species can also undergo change and will be spatio-temporally restricted by their very nature.<sup>8</sup>

Unlike the counterpart hypothesis for repeatable artefacts, or for masses such as water or gold, I will be more hesitant in rejecting the species-as-individuals hypothesis. The names for species as used by biologists are, to a degree, theoretical terms and must find a place within wider evolutionary theories. It may be that the entities most interesting to broad evolutionary theories, and which receive names such as *Equus Caballus* (a horse species) within those theories, can be understood as large scattered individuals. We can

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<sup>7</sup>Ghiselin (1974, p.536), Hull (1976, p.174).

<sup>8</sup>Note that the view of species as individuals discussed here is neutral between the view that they are ‘three dimensional’ (or spatially continuous) entities and the view that they are ‘four-dimensional’ (spatially and temporally bound) entities. See Crane (2004) for a discussion.

note that it is not so counter intuitive to think of a member of a species as a part of that species, as it is to think of a print of a photograph as *part* of that photograph. Moreover, we sometimes think of species as being *spread out* over certain geographical regions.

However, my aim is not take sides on the species debate. What is most interesting to us is that one *need not* accept the conclusion that species are individuals simply because they lack structural essences. Instead such a move can be seen as an over-reaction that failed to recognise perfectly viable alternative non-structural essences. If we re-conceive what we assume the essences of a natural kind can be, the original theory of species as kinds can be retained.<sup>9</sup>

### Kinds With Historical Essences

Of the species concepts currently in favour, phylogenetic or cladistic concepts are dominant. That is, species are understood in terms of reproductively connected organisms that form evolutionary branches in the genealogical tree. Defenders of the view set out above, that species are individuals, regard the names of species as referring to those branches as single scattered objects with reproductively connected parts. However, it has been suggested instead that phylogenetic concepts point in fact to kinds with phylogenetic rather than structural essences. In other words, rather than doing away with essences, it can be argued that phylogenetic species concepts merely redefine essences in terms of relationships and historical origins.<sup>10</sup> de Queiroz, for example, writes that

[i]n contrast with the situation under traditional definitions, the possession of particular organismal traits [structural essences] is neither necessary nor sufficient for an organism to be considered part of a taxon. *What is both necessary and sufficient is being descended from a particular ancestor.* (de Queiroz, 1992, p.300)

So for every species we can recognise an essence, but the essence will make no reference to the internal structure of member organisms. Instead it will be purely relational. On this view there *are* species-specific essences but they consist of historical and relational properties. Darwinian evolutionary theory, focusing on chains of decent and interbreeding populations, has not ruled out species as kinds, but just forced a re-evaluation of what species qua kinds require of their members:

These accounts do answer the question “in virtue of what is my pet dog Rover a member of *Canis Familiaris*?”, but the answer does not cite intrinsic aspects of Rover’s genotype or phenotype, but rather his relations to other organisms and/or to the environment. (Okasha, 2002, p.199)

<sup>9</sup>Here I agree with the conclusion of LaPorte who argues that “[e]ven if it is granted in this way that there is an individual whose parts are the organisms of a species, it is nevertheless the case that there is a *kind* here, as well” (2004, p.17). In other words, though one might be justified in identifying a scattered individual, it does not follow, as we will see, that there is no species-as-kind.

<sup>10</sup>See, e.g., de Queiroz (1992), de Queiroz (1995), Griffiths (1999), Millikan (1999), Okasha (2002), and LaPorte (2004).

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By re-conceiving essences as historical and relational rather than structural, a theory of species as natural kinds can be retained. What is more, this view can also make sense of species evolving and being spatio-temporally restricted. Consider the worry about species evolving. On the structuralist view, the problem is that members of species may undergo structural change as they evolve, and so risk losing supposedly essential structural properties in the process. But if the essential properties are not structural at all then the successive members can change with respect to these structural properties without ceasing to be members of that species. This is because, though successive members may be structurally different from one another, they all possess the necessary and sufficient relational properties needed for being instances of that kind.

It might still be claimed that even understood with historical essences, *strictly speaking* species as kinds can't undergo change and so contra biological theory species can't (strictly speaking) evolve (since it is only the individual members that change). We will meet this challenge with respect to the temporal flexibility of repeatable artefacts later in the chapter, but for now we can note that one way to respond to this worry is just to say, as Joseph LaPorte does, that this "fails for taking idioms too seriously." When we say that species evolve, all we need to commit to is that "successive members of a kind gradually become different from their ancestors" (LaPorte, 2004, p.10). The evolution of species is, on this view, the gradual change of successive organisms that fall under that species kind, not a change in the essential properties of the kind itself. For LaPorte, it doesn't matter that the kind itself is not strictly speaking changing, because all the data can be explained perfectly well in terms of the successive differences in member organisms.

The worry about species being spatio-temporally restricted also turns out not to be pressing. The fact that the Alpha Centaurian organism is not a horse merely follows from the fact that it will fail to possess the essential relational characteristics. Being 'restricted' in this way is simply a consequence of relational essences. In other words, it was a mistake to think that kinds cannot place spatial and temporal restrictions on their instances. There is certainly no theoretical reason why a kind could not be restricted in this way; consider the kind which requires that its instances are people living on Earth between 1900 and 1950. Unnatural as this kind may be, it is certainly spatially and temporally restricted.

The most important result to be gleaned from this foray into the debate over the ontological nature of species is already clear. Given the absence of structural essences, theorists have recognised that viable kinds can be had from purely historical and relational essences. In the previous chapter I argued that repeatable artefacts do not have structural essences – in the next part of this chapter I will propose that a kind theory can be maintained for repeatable artefacts by recognising purely historical and relational essences.

## 6.2. The Proposal: Repeatable Artefacts as Copied Kinds

The relationships that are relevant to the historical essences of species are almost exclusively reproductive in nature. While repeatable artefacts do not reproduce in the same biological sense, earlier instances of repeatable artefacts are nevertheless causally responsible for later instances in an important respect and the language of reproduction is still appropriate. The importance of this was brought to my attention in particular by Charles Nussbaum (2003). Employing a notion from Ruth Millikan (Millikan, 1984, p.23), Nussbaum has described instances of the same musical work (and by extension, instances of other repeatable artefacts) as forming a *reproductively established family* (Nussbaum, 2003, p.274). Entities belong to a single reproductively established family just when they derive from a causal process of reproduction in virtue of which they share reproduced characteristics. The claim that instances of a repeatable artefact form a reproductively established family is, as Nussbaum stresses, neutral as to the ontological nature of the repeatable artefact itself (Nussbaum, 2003, pp.274-275). However, the plausibility of the claim points to the similarity between biological relationships within a species on the one hand and the connections between instances of a single repeatable artefact on the other. The connections between instances of a repeatable artefact can be thought of as a non-biological form of reproduction. More specifically, we can note that the relationships between instances of a repeatable artefact can be described as ones of agent initiated *copying*.

This suggests the following proposal: the essences of many familiar repeatable artefacts are entirely non-structural and instead involve being historically related via a *suitable copying relationship* to other entities. I will call this the claim that repeatable artefacts are *copied kinds*.

### Copied Kinds Described

Recall that all the entities that are instances of the same kind are the same in some way – they have something in common; something *because of which* they are all instances of that kind. If the entities are all instances of the same *copied kind*, so the proposal goes in its most general form, then they all have the property of being related in the right way to certain other entities, where this suitable relational property will be rooted in the notion of *causal history*. What is more, how ‘related in the right way’ should be cashed out will vary from one sort of repeatable artefact to another.

The most straightforward example to begin with is perhaps the photograph. Recall that naïve structuralism is the view that to be an instance of a particular photograph, a print must just look a certain way. The modified structuralist view combines structural properties with historical properties – the print must look a certain way and originate from the right photo-taking action. The copied kind view takes this a step further by taking structural properties entirely out of the essence of the kind. What we will see, however, is that this doesn’t involve a rejection of the importance of structure, nor a rejection of the need for structural similarity.

## 6. Repeatable Artefacts as Copied Kinds

For example, for two or more prints to be prints of the same photograph, the essential property is not that they look the same but that they are suitably produced from the same causal origin. In this case the ‘suitably produced’ will mean copied via an acceptable photographic copying process – one that accurately and reliably reproduces the photographic image (i.e. reproduces structure). Hence even though the possessing of certain structural properties is not essential, it will be a *consequence* of the essential relational properties that instances actually do possess, to varying degrees of accuracy, the same structural properties. Thus structural properties are still very important to whether or not a print is an instance of a given photograph, though a specified set of structural properties no longer constitute the essence of the kind.

When asked if two prints are prints of the same photograph the natural response is certainly to look at the prints carefully to see how closely their visual properties match, but it follows from the copied kind view, and not implausibly, that in doing this we are looking for evidence that the prints originate (in the right way) from the same negative (or the same chunk of digital data). And it is this that decides whether or not they are prints of the same photograph. Of course, in this case what it means to originate ‘in the right way’ will involve standards of structural reproduction, and thus the historical and relational properties that constitute the essence of the kind are not entirely independent of structural properties. Structure does matter, but its importance *derives* from the essence, rather than the structure itself being the essence.

A similar story can be told for text-based artefacts, although here what counts as a suitable copying process will be different. To set out the position in as much detail as possible, I will begin the account from the initial process of authoring: A poet types a sequence of words, makes a few adjustments, and then declares the poem finished. Let’s suppose that she gives the poem the title ‘Poem No. 1’. The instance of the poem on the computer screen is then part of a chain of copies which is so far very short. Being the first instance of the poem there are no other entities to which it can be suitably related, which is what we would expect given that no other entity is an instance of that poem kind. It is not the same poem as anything else yet. Suppose the poet then both emails the text to a friend and prints a copy for herself. Because the email and printing mechanisms are designed to generate word-sequence accurate copies, the text displayed on the friend’s computer and the poet’s own printout will both be suitably accurate copies of the original instance, and so they will both be instances of the same poem. Furthermore, they are both instances of POEM NO. 1 because that was the name given to that copied kind in the naming process. From then on, for anything to be an instance of POEM NO. 1, an entity must have the property of being suitably copied from previous instances. As before, the notion of being a ‘suitable copy’ is a notion that appeals to structural similarities and thus might be understood as itself being a structural notion. However, the crucial point is that the essential property is not itself a specific structure.

Beyond photographs and poems, we need to be a little more careful. If we consider works of music, for example, we can draw a distinction between instances that involve

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reproduction in the more ordinary sense of copying, and those that involve *interpretation* or *realisation*. For though there will often be cases where what matters is a process of copying normally understood (performing a song from memory, or producing sound events via CD or mp3 recordings, for example) there will also be instances that involve an element of interpretation.

This is especially so for scored works of classical music, and scripted plays. Here, generating an instance of the artefact can involve performers working hard to *avoid* simply copying previous instances. Though previous instances will no doubt influence their own performance, it would not be right to merely regard the performance as an act of copying.

Do these cases of interpreted repeatables lie outside the copied kind theory? I think not, if we are willing to interpret the notion of a copied kind broadly enough. What matters here, and what justifies including these interpreted repeatables under the same analysis, is the role of causal history in conjunction with structural similarity. A performance of Beethoven's Fifth may strive to bring something new to the piece, and as such this will not be an exercise in *mere* copying, but it is still a performance that follows a copied score and it must be an acceptable interpretation of that score, structurally speaking, if it is to count as an instance. I would argue that the notion of 'copying' still makes sense in this context, even if it is not *mere* copying.

It is important to note that the claim that structural properties are not the essential properties of a photograph or a poem is nevertheless compatible with the possibility that the structural properties are the most important properties in *other* respects. We may still value a photograph (to return to that example) principally for how it looks, and the aesthetic appreciation of the visual properties may even be the primary motivation for making further copies. Having a non-structural essence does not interfere with structural value. However, it is also worth noting that we often (perhaps nearly always) appreciate a photograph for properties that are not purely structural. Award winning photographs are appreciated for a wide variety of reasons including such non-structural properties as the technical skill required by the photographer and the significance of the subject matter. In both the case of non-structural and of structural appreciation, it is a mistake to assume that what is valuable about a photograph must correlate with what is ontologically essential. We may value a chicken for the eggs it lays, but it need not be (and is not) the case that an ability to lay eggs is essential to being a chicken.

A further important point to make is that the copied kind theory allows for the fact that so often an entity can fail to be an instance of a particular repeatable artefact simply by failing to possess the required structural properties. This occurs, according to the copied kind theory, because in failing to possess certain structural properties, an entity can fail to be related in the right way to the relevant other entities. A musical performance can fail to be an instance of the intended work of music if the majority of the notes are wrong, not because the essence of the work is to have those notes, but because in producing the wrong notes the performance will not be suitably related to

previous instances of that musical work.<sup>11</sup>

This account of the essences of copied kinds is admittedly quite rough. There are two challenges that we face if we attempt to be more precise. Firstly, though a general relational account of essences may cover a great many repeatable artefacts, there is no reason to think there won't be significant variation in the precise details. What counts as a suitable copy for one kind may not carry over to a different kind, and so we should not expect to be able to provide a precise and general account. Secondly we must take into account the points made in §3.3: the essences of specific kinds may be vague in the way described, meaning that it may not always be straightforward to say when a given actual or hypothetical example satisfies the essence of the kind; and there may be a degree of indeterminacy within a linguistic community as to exactly which kind is being referred by a particular term.<sup>12</sup>

### 6.3. Overcoming Structuralist Problems

Having outlined the copied kinds theory, we can now return to three specific issues that motivated the rejection of structuralism. The problems of modal flexibility, temporal flexibility and improperly formed instances can be dealt with neatly by a copied kind theory and this suggests that a theory of relational essences coheres well with our intuitions regarding the use of names for repeatable artefacts.

#### Modal Flexibility

Taking first the issue of modal flexibility, recall that the conflict between modal flexibility and kind structuralism arose out of the intuition that there are repeatable artefacts that are such that they could have been different with respect to certain structural properties. Because of the nature of kinds, however, a kind cannot be modally flexible with respect to the properties it requires of its instances. Thus if a repeatable artefact is a kind, and if these required properties are structural (as structuralism assumes) then

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<sup>11</sup>Note that in one sense these 'amount to the same thing' as far as the performance goes. However, the distinction lies in the properties that constitute the essence. For example, contrast the property 'being a year older than Jill' where Jill is 45, with the distinct property 'being 46 years old'.

<sup>12</sup>It should be noted that the notion of a 'copied kind' appears previously in the work of Crawford Elder (1996; 2007). Elder's concept of a copied kind is similar to the one being employed here. In particular, Elder is recognising kinds, including kinds of artefacts, with historical essences centred around processes of copying. However, Elder's account differs both in application and in detail. Elder's concern is not with the ontological status of the kind itself, but with the implications copied kinds have for the ontological status of instances of the kind. He is concerned with defending the view that individual screwdrivers and individual tables are real objects. He argues that many (though not all) of our ordinary artefacts are members of copied kinds and that copied kinds are sufficiently natural to justify the claim that these ordinary artefacts are real objects. Elder seems to be operating on the premise that for an entity to exist it must have essential properties that it has in virtue of being a member of a suitably natural kind (Elder, 2007, p.39). By contrast, my use of copied kinds aims merely to isolate an interesting class of kinds with historical essences. I do not share Elder's worries about the reality of instances of kinds. Furthermore, Elder's notion of a copied kind is more restrictive than the one being employed here, making demands on instances of copied kinds (that they share a 'proper function' and 'historically proper placement') that are not relevant to our project (Elder, 2007, p.38).

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the repeatable artefact could not be modally flexible with respect to those structural properties.

The copied kinds theory thus needs to handle our intuitions about modal flexibility without falling foul of this basic constraint on the modal inflexibility of the essence of the kind. It does so because our intuitions concern the structural properties of artefacts but according to the copied kind theory, the essential properties of repeatable artefacts are not structural properties. As such, Austen's novel EMMA could have had a different word sequence,  $S^*$ , instead of the actual word sequence  $S$ , because having word sequence  $S$  is not part of the essence of the kind. 'But', it will be objected, 'having word sequence  $S$  is not *optional* for copies of EMMA. How then can it be optional for possible copies of Emma?' To answer this I want to consider two examples of relational kinds. By using hypothetical examples we can state quite precisely how the modal flexibility of structure relates to the non-flexible relational essence of the kind.

Consider the rather unnatural kind AS TALL AS THE EIFFEL TOWER. By stipulation, let us say that this is the kind that requires that all its instances have the purely relational property of being as tall as the Eiffel Tower. Consequently, in the actual world an object must be 325m tall to have the property of being as tall as the Eiffel Tower (and hence to be an instance of that kind).

However, the Eiffel Tower is only contingently 325m tall. It could have been 330m tall, for example (and it actually used to be 312m tall). Hence, in the actual world, today, an object must have the property of being 325m tall to be an instance of the kind AS TALL AS THE EIFFEL TOWER. Anything that is not that tall will not be as tall as the Eiffel Tower and will not count as an instance. Nevertheless, because the tower itself could have been 330m tall, the kind AS TALL AS THE EIFFEL TOWER *could* have had instances that were 330m tall, while still being *that kind*.

The key point is that the property of 'being as tall as the Eiffel Tower' is not a rigid designator of the height '325m' (to have the property 'as tall as the Eiffel Tower' in every possible world is not just to have the property of being 325m tall in every possible world). Rather 'as tall as the Eiffel Tower' is intended as a relational property – it pertains to a height-wise relationship with the Eiffel Tower, rather than a height above sea level.

Here is another similar example more closely analogous to repeatable artefacts. Consider the kind  $K_{\text{Eiffel}}$  which is individuated by the requirement that its instances be Eiffel Tower miniature facsimiles. Given the (contingent) fact that the Eiffel Tower looks the way it does, say it has structural properties  $S$ , all of the instances of  $K_{\text{Eiffel}}$  in the actual world must also look a certain way – have structural properties  $S$  – if they are to count as properly formed instances. However, it is not part of the essence of the kind that its instances have structural properties  $S$ , but only that they have the *same* structure as the Eiffel Tower. It is a *contingent* fact that in the actual world having the property of being a facsimile of the Eiffel Tower entails having structural properties  $S$ . Hence it is possible for instances of  $K_{\text{Eiffel}}$  to have had *different* structural properties (if the Eiffel Tower itself had had different structural properties) even though in the actual



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world, given how the tower actually is, they must have structural properties S.

It is true that Austen could have chosen a few different words here and there when writing EMMA, and it is true that EMMA could have had a slightly different word sequence (and that every copy of the novel EMMA could have had a slightly different word sequence), because the essence of the kind EMMA is not to have a particular word sequence but to bear a suitable copying relationship (one that maintains word order accuracy, in this case) to previous instances of EMMA. Hence if the original final draft penned by Austen had had a slightly different word order (as it might have done had she chosen differently), every subsequent copy would also have had that word order. Crucially both actual and hypothetical copies are of the same kind (are instances of the same novel) because they all have the same property of being causally related to the same initial manuscript.

An important consequence of this is that the degree of modal flexibility allowed remains unsettled. The theory does not offer an answer to question of *how* different the novel EMMA might have been. It seems plausible that some of the words could have been different and that it could have been a little longer or shorter, but not plausible that every word could have been different, or that it could have been a haiku. According to the copied kind theory, the vagueness of these intuitions is a direct result of the uncertainties in our intuitions about the modal flexibility of ordinary objects. When Austen wrote the manuscript for EMMA, she produced an entity that was the first in a branching chain of copies. By linking the essence of the novel to the status of ordinary non-repeatable objects, the question of how different a novel could be becomes effectively the same as the question of how different a non-repeatable object, such as a painting, could have been while still being that same object. Matisse's painting THE DANCE perhaps could have had one extra character in it, but it probably could not have been a still life of an apple. Similarly, we judge that EMMA couldn't have had the same word sequence as the rhyme THREE BLIND MICE because a chain of copies starting with a manuscript with that word sequence written on it would not count as the same chain of copies with the same origin as that which we actually have.<sup>13</sup>

A related question pertains to the necessity of authorship. Could EMMA have been written by someone other than Austen? While there have been attempts to answer this question for works of art in general,<sup>14</sup> the important point here is that problem about the authorship of repeatable artefacts can be reduced to the problem as it applies to ordinary non-repeatable objects. The question of whether EMMA could have had a different author reduces to the question of whether the chain of copies, beginning with the original manuscript, could have been produced by someone else while still being those same entities. The result then is that in many respects, the ways that repeatable

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<sup>13</sup>We can note that these kinds of modal considerations may well be influenced by value judgements, so that how different an artefact could have been may depend on what we value about that artefact. The view defended here – that the essences of repeatable artefacts are relational and historical, also sits well with the idea that such artefacts are inherently historical and contextual – see, e.g. Levinson (2007). Copied kinds are no less 'historically embedded' than ordinary physical objects.

<sup>14</sup>See, for instance, Rohrbaugh (2005).

artefacts could have been different depends entirely on the ways that non-repeatable particulars could have been different.

### Temporal Flexibility

The issue of temporal flexibility is handled in a very similar manner. The problem, recall, was that in certain cases we have the intuition that repeatable artefacts can change through time, but it is also the case that if artefacts are structural kinds there can be no change in structure without a numerical (rather than merely qualitative) change in kind. However, because copied kinds do not have structural essence there can be structural change without a change of essence. When an academic paper is altered following the suggestions of a reviewer there are structural changes to the text but there is no change in the properties that are required for an entity to be an instance of that paper – before and after the structural change, it is still the same relational property that matters.<sup>15</sup>

The fact that such change is possible tells us something important about that relational property because it is clear to see that the modified version of the paper will not be a word order duplicate of a previous instance (since some of the words will have been changed). Built into the requirements of a ‘suitable copy’, then, must be the allowance that certain people can, in appropriate circumstances, make structural changes and still produce an entity suitably related to previous instances. Often this privilege will be granted to the author or authors only, but changes are also made by editors (who, in some technical sense at least, share in the authorship). In cases of unauthored artefacts, such as folk songs and folk stories, small changes may be acceptably made by anyone who produces an instance of that artefact.

It is clear to see that different sorts of artefacts will warrant quite different standards of the copy-relationship, and which standards are adopted in any given situation will depend on various social and cultural factors. The task of spelling out all of the different standards and practices would be a long one, and I am not going to undertake it here. All we need to accept for the account to be plausible is that for any given repeatable artefact, what sort of relational essence is relevant can be teased out by careful scrutiny of the kind in question.

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<sup>15</sup>One might object here that even though the same words are used to describe the conditions required for instances before and after the change, the actual conditions themselves *have* changed. However, such an objection could be mounted only if the existence of relational properties in general was being denied. Recall the example of the property ‘being a year older than Jill’. If Jill is 45, then a person will have that property if they are 46 years old. However, it does not follow that ‘being a year older than Jill’ and ‘being 46’ name the same property. If a person has the property of being a year older than Jill they will have that property in three years time, though they will no longer have the property of being 46 years old. If one began with a principled rejection of relational properties, they may insist that ‘being a year older than Jill’ is a phrase that picks out different properties each year. However, short of an argument against relational properties, we have no reason to interpret ‘being a year older than Jill’ in that way. The more natural reading holds that the phrase picks out the same property each year, though the age one must be to possess the property clearly changes as Jill gets older. Similarly, short of a rejection of relational properties, we have no reason to think that the relational essence of the kind changes before and after the structural change to the paper.

## 6. Repeatable Artefacts as Copied Kinds

Some clarification is needed at this point about exactly what is meant by the claim that the kinds themselves can undergo change and that they could have been different. On the more traditional view of kinds as abstract entities – a view that I rejected in Chapter 4 – kinds are often understood to be unchanging and modally fixed because they are abstract. They are, in Joseph LaPorte’s words, “abstract objects with immutable essences” (LaPorte, 2004, p.9). Abstract kinds undergo neither modal or temporal variation. A supporter of that view must say that even if artefact kinds have relational essences the kinds themselves are not modally or temporally flexible. Instead, the fact that the essences are relational rather than structural allows for the fact that successive instances of kinds can vary structurally, and that possible instances of kinds could have different structures. The variation is at the level of instances only. Accordingly, talk of modal and temporal flexibility is explained away rather than strictly speaking allowed for. We say things like ‘EMMA could have been different’ but all we can really mean here is that instances of EMMA could have been different. This is the line that LaPorte takes towards the evolution of species, as discussed in §6.1. However, since I have argued for kind physicalism I am not restrained by the supposition that kinds, as abstract objects, cannot undergo modal or temporal change. All that I am committed to in this respect is the view that kinds cannot undergo modal or temporal change of their essences. That is, they cannot change (and could not be different with respect to) the properties that they require of their instances. However, because kinds on my view are co-present with their instances, and because the instances can be different from one another in structural respects, and also could have been different from how they actually are, there is an important sense in which the kind itself is liable to modal and temporal variation. The kind itself, which is multiply located at every point at which the kind is instanced, will show variation across times and places in (potentially) all but its essential properties. As such, the kind theory defended here, combined with the thesis that many artefact kinds have historical essences, does not explain away modal or temporal flexibility but embraces it. Repeatable artefacts really do change, and really could have been different.

### **Improperly Formed Instances**

In §5.2 I discussed possible ways that a kind structuralist could deal with the apparent phenomenon of kinds with improperly formed instances. The norm-kind solution worked by dividing the structural properties into normative and essential properties. All instances must have the essential properties, but only properly formed instances have the normative properties. I argued that this view faces the challenge of accounting for a distinction between normative and essential properties, and also feels like a somewhat purpose built and ad hoc amendment to a basic kind theory.

The copied kind theory allows for an alternative and simpler take on the issue. Before considering repeatable artefacts, it is interesting that Wolterstorff and Dodd both appeal to biological species to give examples of kinds with improperly formed instances (Wolterstorff, 1980, p.57; Dodd, 2007, p.33). But if we accept the view that species

are kinds with historical essences then the idea of an improperly formed instance of an animal species can be seen in a whole new light. We are inclined to say that a cat with three legs is in some sense ‘improperly formed’, and on the structuralist kind view this was deemed to be because it lacks some of the properties required to be a cat. It somehow ‘falls short’ of ideal cat-hood. However, if species are kinds with historical essences then the number of legs that the animal has has nothing to do with its being a cat or not – that is something decided by lineage. The sense in which the cat is improperly formed then becomes entirely about reproductive norms and expectations. Because cats are normally born with four legs, a cat with three legs will represent a divergence from this standard and so it will strike us that there is something wrong, or merely out of the ordinary, about it.

The story for repeatable artefacts is similar. To be an improperly formed instance of a repeatable artefact an entity will be copied from a previous instance but will break certain of the norms of copying and thus be a (more or less) poor copy.<sup>16</sup> However, the difference here between artefacts and species is that the norm is not set by independent biological processes and our subsequent expectations, but is directed by our desire to reproduce structural properties with varying degrees of accuracy. Suppose that we print off a number of copies of the same photograph, but in one of the prints the colour is unbalanced because the red ink in the printer has run dry. The off-colour print will be an instance of the photograph, but it will be a poorly formed instance because it will not be as good a copy as we normally expect and intend for colour photographs.

Thus viewing repeatable artefacts as copied kinds not only allows for modal and temporal flexibility, but makes sense of the possibility of better and worse formed instances of the kind without resort to ad hoc divisions in the essence of the kind. Normativity on this view ‘comes for free’ because normativity is built into the relational property of being a suitable copy.

## 6.4. Objections

The theory of copied kinds has been outlined and I have described how it is able to handle the key difficulties faced by a structuralist account. What remains is to defend the account from some possible objections, before moving on to a more specific challenge laid down by Guy Rohrbaugh. The first possible objection is a technical one. It might be thought that the proposal I have made falls foul of one of the basic axioms of S5 modal logic. The unique axiom of the S5 system of modal logic states that if it is possible that  $p$  it is necessarily possible that  $p$ . This is logically equivalent to the claim that if it is possibly necessary that  $p$ , it is necessary that  $p$ . However, it might appear that my claim is that, as it happens, it is necessary that copies of EMMA have structure  $S$ , but

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<sup>16</sup>Of course, the evaluative terms ‘wrong’ and ‘poor’ will not always be appropriate for instances that diverge from the norm. A performance of a work of music, for example, may be deliberately produced so that it diverges from the norm. In this case we wouldn’t describe the instance as being ‘wrong’ or ‘poor’ in any way, and may commend a performer for producing an aesthetically valuable or interesting inaccurate copy.

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that it is possible that they need not. In other words my claim could be interpreted as implying that it is contingently (possibly) necessary that copies of Emma have structure S. But according to the above axiom of S5 modal logic, if it is possibly necessary then it is necessary simpliciter, and if it is necessary simpliciter then copies of EMMA could not have had a different structure. Does my claim about how EMMA could have been therefore contradict S5 modal logic?

To see that it does not, we need to distinguish between a strong, or universal, sense of modality, and a weak, or restricted, sense.<sup>17</sup> In the universal sense, if it is necessary that p then there is no world in which p is not the case. In the restricted sense, it can be ‘necessary’ that p where p is the case just if some *other* (not universally necessary) facts obtain. For example, we can identify one sort of restricted necessity as *physical* necessity. Something is physically necessary only if it must be the case in every world at which the actual laws of physics hold. Though it is physically necessary that nothing travels faster than the speed of light, it is presumably not a universal necessity. When we say, then, that copies of Emma ‘must’ have structure S, we mean this only in a restricted sense as described. It is necessary given Austen’s manuscript that subsequent copies are as they are. It can therefore be possible in the unrestricted sense that they have a different structure, because it is possible that the original manuscript and subsequent copies are different. S5 is not violated because if it is unrestrictedly possible that something is restrictedly necessary, it does not follow according to S5 that it is unrestrictedly necessary.

The second objection I want to consider derives from the thought that the properties considered here as constituting the essence of the kind – such as ‘having the same structure as Austen’s original manuscript’ – are not genuine intrinsic properties and so cannot be essential properties of the instances. There are two relevant issues that we can separate. The first is that it might be thought that kinds can only require intrinsic properties of their instances. However, on the basic kind/instance theory set out here, this thought is unmotivated. Why should it be the case that kinds can only make requirements on the intrinsic properties of the instances? If kinds are individuated by the properties that their instances must have to be instances of that kind there is no conceptual or theoretical reason to put a restriction on what those properties may be. Thus there is no barrier to regarding repeatable artefacts as kinds that demand that their instances have only relational properties.

The second worry here is that the instances *themselves* cannot have relational properties as essential properties. However it is no part of kind theory that instances of kinds are instances of that kind essentially or necessarily. Consider my physical copy of EMMA. That is an instance of the kind EMMA, but (plausibly) it might not have been, and it may not be in the future if it undergoes some sort of serious damage. Instances need not be instances of their kind essentially, and so the properties they must have in order to be instances of that kind can be any kind of property whatever.

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<sup>17</sup>See Sherratt (2001, p.381) for a discussion of restricted modality.

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The third objection I want to consider is based on Peter Kivy's argument from the TRISTAN CHORD (Kivy, 1993, p.46). Kivy argues that the TRISTAN CHORD is a certain structure of pitches (a sound structure), and that there is no principled difference between the chord and TRISTAN AND ISOLDE itself. Hence TRISTAN AND ISOLDE cannot be anything more than a more complicated sound structure – that is, it must be a kind that demands only of its instances that they sound a certain way. But this argument is not persuasive simply because there *is* a significant difference between the chord as kind and the musical work as kind. Kinds are individuated by the properties they require of their instances and so the proof of the differences between a chord and a work of music lies in the different kinds of properties that each requires of its respective instances. Kivy's argument involves assessing this for a chord, and then extrapolating up to the whole work of music, but that extrapolation is unjustified. When we identify the chord it is very plausible that we are identifying a kind of sound event that requires its instances have a certain sound structure and nothing more. However, when we identify the work itself we seem to be identifying something quite different, as evidenced by the fact that we think the work could have had a slightly different structure. Certainly there *is* a kind such that it requires that its instances necessarily have only the sound structure of the actual TRISTAN AND ISOLDE, but why think that this is what we are actually referring to when we talk about TRISTAN AND ISOLDE? Our referential practices, including our intuitions that TRISTAN AND ISOLDE could have been different and could have gone through a number of small changes in its early life, suggest that we are not referring to a sound structure (or even a sound structure with a number of historical and relational properties thrown in, as modified structuralism suggests).

### 6.5. Embodied Individuals Rejected

In the final part of this chapter I want to consider and reject an alternative account of the nature of repeatable artefacts presented by Guy Rohrbaugh (2003). The reason that Rohrbaugh's proposal deserves particular attention is that, like the copied kind theory, it is motivated by an insistence that repeatable artefacts are modally and temporally flexible. What is more, before presenting his positive proposal, Rohrbaugh considers and dismisses a view very similar to the copied kind theory defended here. In what follows I will argue firstly that Rohrbaugh's reasons for rejecting a copied kind theory are flawed, and secondly that his own positive proposal compares badly with the copied kind theory.

Rohrbaugh's consideration of a position much like the one proposed here comes under the heading 'Neo-Type Theories'. Using the terminology of types rather than kinds, Rohrbaugh suggested that a position may be forwarded according to which "the types to be identified with artworks have identity conditions that make reference only to extrinsic, causal historical features of the tokens" (Rohrbaugh, 2003, p.195). He goes on to say that

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what would remain is the claim that photographs are types, but types whose identity conditions are given almost exclusively in terms of the shared history of the prints, their coming from a particular picture-taking in an appropriate fashion. (Rohrbaugh, 2003, p.196)

Rohrbaugh then seems to have a number of issues with this move that I will address in turn. Firstly Rohrbaugh claims that for this theorist it would be “difficult ...to deny that structure, at least low level structure, is even partially constitutive of a work...” (Rohrbaugh, 2003, p.195). However, we can see that on the copied kinds theory, all that is being claimed is that structure is not part of the *essence* of the kind. The visual structure of a photograph, or the textual structure of a novel, for example, can still be very important in other respects. The structure present in *EMMA* is still a highly important feature of that kind, and structure understood generally is a constitutive feature of the kind in that it is necessary that instances have the same structure as Austin’s final draft. So structure has not been ejected altogether from our concept of a repeatable artefact. All that Rohrbaugh is expressing here is the unfounded assumption that specific structure ought to be ontologically essential to repeatable artefacts.

Further down Rohrbaugh writes that “to give such an account is still, I think, to have missed the point”, and this because he “argued that photographs are subjects of change and of certain modal potentialities, but causal-historical types are not subjects of change or modal potentiality; they are types that are unchanging and necessarily generous about what they count as their tokens” (Rohrbaugh, 2003, p.196). However, this charge is only effective, if it is effective at all, against a theory of kinds as abstract entities. Rohrbaugh is assuming that a kind or type theory does not really allow for modal and temporal flexibility, but instead “merely simulate[s] these phenomena” (Rohrbaugh, 2003, p.196). Yet this is not the case according to kind physicalism. As I argued at the end of §6.3, kind physicalism implies that kinds really are modally and temporally flexible in everything but essence, and thus Rohrbaugh’s charge is not relevant.

Finally Rohrbaugh suggests that a theory of historical essences such as that defended here ceases to have any appeal as a theory of artworks now that the essence is no longer given in qualitative terms. He writes that

If we give up the task of attempting to provide a qualitative essence for a work of art, what point is there in continuing to assume that it is a type of thing for which we are giving identity conditions and not an individual in its own right?” (Rohrbaugh, 2003, p.196)

Rohrbaugh seems to be insisting that unless we understand the essences of a kind or a type in structural (qualitative) terms, there is no point in regarding the entity in question as a kind (or type) rather than a what he calls ‘an individual in its own right.’ Leaving aside what is meant by ‘an individual in its own right’, we can respond here by recalling that the kind theory was originally posited to explain repeatability. The kind theory explains how the same word can appear twice in one display, how the same

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film can be seen, in its entirety, at multiple times during the day, and how the same photograph can be both on my fridge and on my parents' mantelpiece at the same time. The kind theory provides us with an ontological category that allows for this, regardless of what the essences of the kinds turn out to be. The lack of structural essence does nothing to remove the central motivation for favouring a kind theory.

Rohrbaugh's brief reasons for rejecting a theory of kinds (or types) with historical essences are unconvincing. However, to see what is meant by the claim that a repeatable work of art is 'an individual in its own right', we need to consider Rohrbaugh's own positive proposal. I will argue that his account is ontologically obscure and crucially makes no useful advances on a copied kind theory. Any moves we make to clarify the position leave us with a theory that is in no useful way distinct from a copied kind theory.<sup>18</sup>

The first point to make is that now that the theory of copied kinds has been articulated and defended in response to the lack of structural essences of repeatable artefacts, Rohrbaugh's own account is unmotivated. Rohrbaugh argues that because entities such as photographs are temporally and modally flexible, they must not be types or kinds. As such he proposes that they are what he calls 'historical individuals'. But given that a plausible kind theory can be offered which allows for such flexibility, as I have argued it can, then one need not look elsewhere for ontological answers.

Being unmotivated, the obscurities of Rohrbaugh's proposal make it doubly unattractive. Firstly, Rohrbaugh articulates his position by stressing that the relation of a photograph to its copies should not be of the form 'Fa', which he suggests is the relation of instantiation, but should rather be of the form 'Rab' where the photograph is related to its print by the relation of 'print of' where this is a specific case of the more general 'occurrence of' relation. However, other than in choice of terminology, this does not set his view apart from a kind theory. According to the copied kind theory, the kind (an entity) is related to its instances (also entities), and thus we also represent the relation as 'Rab' rather than 'Fa'. It is not clear how the 'occurrence of' relation that Rohrbaugh favours is in this respect any different from the instantiation relation of a mature kind theory.

Rohrbaugh insists that the 'occurrence of' relation is a special case of the relationship of 'embodiment'. The embodiments of a repeatable entity (the prints, performances, copies) are "those things on which [the repeatable entity] ontologically depends for its continued existence" (Rohrbaugh, 2003, p.198). The notion of ontological dependence being appealed to is far from clear, but we need not enter into a discussion of its complexities here.<sup>19</sup> This is because merely stating that an entity ontologically depends on its 'embodiments' brings us no closer to understanding the relationship between a repeatable artefact and its instances. Rohrbaugh's historical individuals seem to occupy

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<sup>18</sup>Rohrbaugh's proposal receives a thorough critique in Dodd (2007, pp.143-166). As my positive theory undercuts the central motivations for Rohrbaugh's account, we need only draw out the central weaknesses of that view in what follows.

<sup>19</sup>For a recent survey of the topic of ontological dependence, see Correia (2008). Dodd discusses this notion in the context of Rohrbaugh's proposal in Dodd (2007, pp.162-166).



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a hazy ontological middle ground between the scattered object hypothesis and a kind theory that holds that kinds are abstract. Presumably Rohrbaugh rejects the scattered object hypothesis because he finds it counter intuitive to say that the ‘occurrences’ of a photograph are parts of the photograph. Instead he prefers abstract entities that are ‘sustained’ by numerous physical objects that count as their ‘occurrences’. However, we can ask here: *in virtue of what* is it the case that a particular entity is an embodiment of a particular work of art? If the question is answered by citing historical and relational properties, then it seems we do have a kind theory after all. If such properties are not cited, the relationship remains mysterious.

Rohrbaugh further demarcates his view by stating that his ‘historical individuals’ are in the class of real entities. Recognising that ‘real’ is a widely used term he attempts to be more precise:

I think there is a common, possibly pre-philosophical, set of intuitions of reality which attends the bulk of what one might call our ordinary ontology. Photographs, species, words, rocks, tables, and persons are, in some primary sense, equally real and I would trace their intuitive reality to their being genuinely historical objects. Not only do these things exist in time, but they all come into existence at some point in history and cease to exist at a later one. They are more than merely temporal, for each has what you might call ‘a life story’. They are all subject to change over time, and all, had their life stories gone differently, could have been somewhat different than they in fact are. Further, while not all such objects must be particular, physical, concrete, or basic for explanatory purposes, the existence of all such items is rooted in the physical world. (Rohrbaugh, 2003, p.199)

This is all very well, but notice that this description does not exclude the physical kinds defended here. Such entities fit neatly into Rohrbaugh’s understanding of the ‘real’ and account for all that Rohrbaugh insists we want to say about repeatable artefacts without needing to posit notions of ‘embodiment’ or ‘higher level’ objects (Rohrbaugh, 2003, p.199).

In short, then, with a theory of physical copied kinds on the table, Rohrbaugh’s offering is both unmotivated and unclear. To explain the nature of repeatable entities we need not posit a new and ontologically hazy category of things. Rohrbaugh was right to reject structuralism but ultimately wrong to move from this to a full rejection of kind or type theories.

## Conclusion

The challenge of this chapter has been to overcome the difficulties faced by structuralism while still holding on to a kind theory. This has been achieved, I argue, by recognising the role of historical and relational essences. The copied kind theory conforms with our intuitions about the application and co-application of the names for many key repeatable

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artefacts, and shows how these can nevertheless be genuine kinds with essences. The chapter began with a discussion of the ontological nature of biological species, and one of the outcomes of this chapter is that novels, films, plays and photographs (as well as the less ‘artistic’ logos, adverts, jingles, and essays) have more in common with biological species than we might have thought. Insofar as one agrees that species are kinds, both have essences that are historical and relational and involve mechanisms of reproduction that ‘copy’ characteristics from one instance to the next (bearing in mind the role of interpretation for performed repeatables). The precise structure of these entities is therefore contingent. It could have been somewhat different, and it can change through time. It has been suggested that these intuitions concerning modal and temporal flexibility are mistaken: that we are just wrong to think that such entities could have been different and can change through time. However, now that a theory is available that allows for this flexibility, there is no good motivation for regarding such intuitions as in error.<sup>20</sup>

I have also argued that a significant feature of the copied kind theory is that it still allows for structure to play a central role in the nature of these artefacts. The essences of these kinds require that the instances have certain aspects of their structure copied from previous instances. We can now take this point further and note that it is *because* structure matters for the appreciation, reproduction and identification of these artefacts that they can be copied kinds at all. Just as the passing on of structural traits is an important part of reproduction and evolution in biology, so it is the structure – sonic, visual or textual (or a combination of these) – that we wish to copy and that thus allows for relational essences for these kinds. A care for structure and the copied kind theory go hand in hand, for if we did not care for structure, we would not have the social and cultural mechanisms in place to produce copies. This is an important observation because it points towards a principle for collecting together those repeatable artefacts that are copied kinds. So far I have given only indicative lists: we have been looking at novels, photographs, works of music, etc. Now we can say that what these have in common is the primary importance of structure. To be a copy is to be a structural copy.

### The Naturalness of Copied Kinds: An Endnote

An answer to our opening question has been offered. When we recognise the phenomenon of repeatability, I have argued, we are contrasting the identity of the kind with the distinctness of numerous instances of that kind. To recognise and identify a novel is to identify a physical kind that can be multiply located, and is found wherever and whenever the essence of the kind is satisfied.

In looking to give a broad account of those essences, at least for paradigmatic cases of repeatable artefacts, I have drawn parallels with biological species. As structural traits

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<sup>20</sup>This suggests a further shortcoming of such error theories: they lack any explanation for why we held these beliefs. Why think that many repeatable artefacts are modally and temporally flexible, if in fact they are not? The copied kinds theory has quick answer: we think they are modally and temporally flexible because they are modally and temporally flexible.

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of biological species are transferred via causal-historical mechanisms of reproduction, so are the sounds, words and images, that we create and value, passed on via causal-historical mechanisms of artefact reproduction. Moreover, in each case we can identify kinds with essences that appeal directly to those causal histories. All that remains is to point out a potentially surprising consequence of this view.

In §3.4 I defended the view that kinds can be understood as being more or less natural and that naturalness comes in degrees. I said very little at that point about specific criteria employed in judging naturalness. However, one intriguing consequence of the account of copied kinds defended here is that repeatable artefacts turn out to be significantly natural.

Naturalness is commonly understood in terms of the processes of explanation and induction. Explanation and induction allow us to know about, and understand, the world we inhabit, and they rely on our ability to pick out projectable properties and processes. We often aim to describe the world in terms of casual structures that ground reliable inferences.<sup>21</sup> Appealing to the work of Ian Hacking (1991) and Richard Boyd (1991), Ruth Millikan has claimed that “the term “natural kind” has commonly been used to characterise kinds over which numerous reliable inductive generalisations can be made” (Millikan, 1999, p.49). Similarly Paul Griffiths notes that “[a] kind is minimally natural if it is possible to make better than chance predictions about the properties of its instances” (Griffiths, 1999, p.216).

Typical examples of kinds that fulfil this criteria are the kinds of physics and chemistry. The essences of these kinds correlate with law-like generalisations that hold irrespective of place and time. The essence of gold, we can assume, is to have the atomic number 79, and it is argued that this essence is causally responsible, in accordance with laws of nature, for the other properties that gold reliably has. Gold is a highly natural kind, then, because if we know that some sample is gold – that it satisfies the essence of gold – we can make reliable predictions about many of the other properties that the sample will have. Because the causal laws that ground these inference are deemed to hold irrespective of time and place, Millikan has called kinds such as these “eternal natural kinds” (Millikan, 1999, p.50).

However, the kinds of physics and chemistry are not the only kinds that allow for such generalisations and reliable inferences. Millikan contrasts *eternal* natural kinds with *historical* natural kinds. Historical kinds – and here Millikan cites biological species – are just those kinds whose essences refer to historical relationships among instances rather than internal structure. Crucially, though, in historical natural kinds the relational essences play the same role as the internal structural essences in grounding reliable inferences to other properties of members of the kind. Moreover, the relational essences allow us to explain the properties of instances, in the same way that knowing that a sample is a sample of gold allows us to explain the fact that it has the melting point that it does have. Millikan writes that for historical natural kinds:

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<sup>21</sup>See, e.g., Boyd (1991, p.139), also Griffiths (1999) and Hacking (1991).

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Inductions made from one member of the kind to another are grounded because there is a certain historical link between members of the kind that causes members to be like one another. (Millikan, 1999, p.55)

Thus we have story to tell about how the processes of biological reproduction that underpin the essence of species allow for species to be natural kinds. The relational essences provide a causal mechanism – through the transfer of genes and taught behaviour – that grounds reliable inductions and explanations about other (what are sometimes called ‘surface level’) traits in members of the species. The essence of the POLAR BEAR, for example, is to be a product of a reproductive chain involving other polar bears, and it is because of this essence that polar bears share a great many other properties (LaPorte, 2004, p.19). In identifying kinds with such essences we are therefore identifying kinds that match the causal structure of the world and allow for induction and explanation: we are identifying natural kinds.

We are now in a position to draw the crucial parallel with those repeatable artefacts that are understood to be copied kinds. The key point here is that the causal mechanisms found in historical essences that allow for reliable induction need not be limited to human-independent mechanisms. Paul Griffiths has argued that understanding the role historical essences can have in providing reliable causal grounds for other co-occurring properties “breaks down the traditional distinction between natural kinds and kinds generated by human agency” (Griffiths, 1999, p.218). The claim being forwarded, then, is that our social and cultural practices can play the same role for many artefact kinds as biological reproduction plays for species.

The essence of a copied kind, we have said, is to be historically related via a suitable copying process to the relevant chain of previous instances. We can see, though, that our social practices of desiring and producing suitable copies of instances will licence reliable inferences to a great many other properties that instances of the same kinds will possess. The copying process that we facilitate acts as a real causal mechanism that grounds reliable inductions and explanations. If you go to see a film that I have seen already, I can tell you what to expect just because the copying processes that underpin the essence of the film allow us to make highly reliable inferences from one instance of the film to another. The underlying relationships of copying are effectively the ‘hidden’ essence of copied kinds that are causally responsible for the many shared ‘surface level’ properties.

Many repeatable artefacts, if they are indeed copied kinds, are kinds that allow reliable induction and explanation from essences to other surface level properties of instances of the kinds. They are, in an important respect, significantly natural kinds.<sup>22</sup> It does not follow that all repeatable artefacts are similarly natural, since it cannot be

<sup>22</sup>It might be objected that the terms ‘natural’ and ‘artefactual’ are mutually exclusive. However, the notion of an artefact in use here was never clarified in opposition to naturalness, and other than by *stipulation* of the meanings of those terms, I see no reason not to understand those paradigmatic examples of repeatable artefacts as being natural, as that term is understood here. After all, we are part of the natural world. As Crawford Elder has remarked, “we ourselves, with our intelligence and our agency, are items which nature produced” (Elder, 2007, p.40).

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assumed that all repeatable artefacts are copied kinds. However, by exploring the possibility of non-structural essences and by comparing repeatable artefacts with biological kinds, we have reached a rather unexpected conclusion, one that seems to me to suggest an interesting avenue of further research.

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