

# **AGAINST TYPE E**

**Matthew McKeever**

**A Thesis Submitted for the Degree of PhD  
at the  
University of St Andrews**



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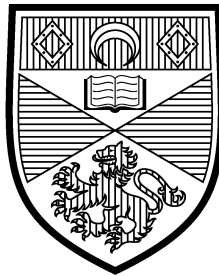
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# Against Type E

Matthew McKeever



University of  
St Andrews

This thesis is submitted in partial fulfilment for the degree of PhD  
at the  
University of St Andrews

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UNIVERSITY OF ST ANDREWS

## *Abstract*

School of Philosophical, Anthropological, and Film Studies  
Department of Philosophy

Doctor of Philosophy

### **Against Type E**

by Matthew MCKEEVER

It's generally assumed that a compositional semantic theory will have to recognise a semantic category of expressions which serve simply to pick out some one object: e-type expressions. Kripke's views about names (Kripke, 1980), Kaplan's about indexicals and demonstratives (Kaplan, 1989), the standard Tarskian semantics for bound variables (Tarski, 1956), Heim and Kratzer's Strawsonian view about definites (Heim and Kratzer, 1998), even an (admittedly unpopular) analysis of indefinites (namely Fodor and Sag, 1982), assume as much.

In this thesis, I argue that recent advances in the semantics of names and of quotation, and in metaphysics, give good reason to doubt there is such a category. I do so in two parts. In the first part, comprising about two thirds of the thesis, I combine two recent views, one from semantics and one from metaphysics. From semantics, I take over predicativism about names, the view, as one might have guessed, that names are predicates. From metaphysics, I take over the doctrine that there are temporal parts. Putting them together, I hold that the semantic contribution of the name 'Joan', in a sentence like 'Joan is a barrister', is a predicate, and in particular (and somewhat roughly) a predicate the extension of which is Joan's temporal parts. Names are not, as is commonly thought, e-type expressions.

In the second part, I further undermine the existence of a semantically important category of e-type expressions by arguing that a construction which might be thought to mandate such a category, namely *de re* attitude ascriptions, does not do so. I argue this by considering the nature of transparent occurrences of expressions in opaque contexts in general, and in particular by considering the phenomenon of mixed quotation. I present a new theory of it, and show how it can be parlayed into a new Fregean theory of *de re* attitude ascriptions. The thesis overall, then, will provide strong evidence against the semantic importance of e-type expressions either in language, or in (our reporting of) thought.



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<sup>1</sup>Anders Schoubye was de facto co-supervised by them a few years ago.



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*Dedicated to Liz*



# Chapter 1

## Introduction

### 1.1 Summary of the Thesis

That our best compositional semantic theory will recognise a category of e-type expressions is, to say the least, an extremely common view<sup>1</sup>. Kripke's views about names (Kripke, 1980), Kaplan's about indexicals and demonstratives (Kaplan, 1989), the standard Tarskian semantics for bound variables (Tarski, 1956), Heim and Kratzer's Strawsonian view about definites (Heim and Kratzer, 1998), even an (admittedly somewhat unpopular) analysis of indefinites (namely Fodor and Sag, 1982), assume as much.

Of course, there have been particular challenges offered to e-type analyses of many of these expressions. Most familiarly to the philosophical audience, the Russellian analysis of definites, brought up to date with the help of generalised quantifier theory and function-theoretic semantics, challenges the e-type interpretation of definites ((Russell, 1905), (Neale, 1990)). But we can also note King's quantificational account of complex demonstratives (King, 2003), the literature concerning descriptive indexicals (beginning with Nunberg, 1993), predicativism about names ((Burge, 1973), (Fara, 2015)), and various non-referential theories of specific indefinites (e.g. (Ludlow and Neale, 1991), (Schwarzschild, 2002)). Indeed, a recent book (Hawthorne and Manley, 2012) has made a whole-sale challenge by arguing that each of names, definites, indefinites and demonstratives are quantificational, and their referential uses are to be accommodated in terms of domain restriction. But even *that* view left untouched what Kaplan (Kaplan, 1977) called the paradigm of a directly referential term, namely a variable under an assignment.

The goal of this thesis is to present a new and more thoroughgoing challenge to the idea that we need e-type expressions, and to show how to do without them. I do so by means of two extended arguments.

In brief, the first one presents a new account of names, according to which not only are they not e-type (as the Millian would have it), or e-type when occurring in argument position of verbs (as contemporary predicativists would have it), or as good as e-type by denoting an existential quantifier with a singleton domain (à la Hawthorne and Manley), but they are  $\langle e, t \rangle$  type–predicates, *even when* occurring as a verb's argument, i.e. in such basic constructions as:

- (1) Joan is happy

In more detail, I will argue that semantically, the predicate 'Joan' denotes Joan's *temporal parts*, while syntactically, I'll assume a conjunction based view à la Pietroski (Pietroski, 2005), according to which syntactic concatenation encodes semantic predicate

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<sup>1</sup>Some notational conventions. I won't italicise foreign expressions. When it's not going to cause confusion, I'll sometimes be sloppy about use and mention, and will never use Quine quotes. Sometimes I'll use 'they' as a gender-unspecific third person pronoun.

conjunction, and there's an obligatory sentence level existential quantifier closing any free variables.

Assuming—without argument—that objects in some sense just are their temporal parts, I'll show that this view has semantic advantages over its four dimensional competitors, namely stage and worm theory, which each fall down when one tries to develop a compositional semantics for them. The upshot will be a more radical version of predicativism than has yet been seen, as well as a recognisably new four dimensional position which posits stages, but neither fusions thereof (as the worm theorist) nor a range of temporal counterpart relations (as the stage theorist) to account for problems concerning persistence and change. I call this view *Stage Semantics*.

The second argument concerns de re attitude ascriptions. Such ascriptions give a prima facie strong reason for positing expressions whose semantic function is exhausted by their standing for an object. In particular, the parties to the debate concerning a sentence like:

- (2) The president<sub>1</sub> is such that Quine believes he<sub>1</sub> is a spy.

Seemed to agree on at least the following data point: the pronoun is not well understood as contributing some descriptive content or sense as semantic content, as it would on Fregean or quantificational views, just because there doesn't seem to be any apt descriptive content. It can't, so the thought goes, be the sense or the description 'the president', just because such reports can be used—and, at least in philosophers' English, are solely used—when the reportee's belief doesn't seem well-glossed as that expressed by 'the president is a spy' (typically because the reportee doesn't know the object of her belief is a spy, and so wouldn't be disposed to utter things like 'the president is a spy'<sup>2</sup>).

A standard way to account for this is to say that 'he<sub>1</sub>' stands for an object under a variable assignment; the report is true provided there's a shifted variable assignment in which the president occupies the first position, such that relative to it, 'Quine believes he<sub>1</sub> is a spy' is true. And this is meant to capture well the fact that the sentence places no constraints on how Quine thinks of the object of his belief.

Having presented some problems with this picture, I will argue for an alternative analysis of such de re ascriptions, by considering the behaviour of opaque contexts in general. In particular, I will focus on arguably the clearest instance of an opaque context, namely quotation. I will suggest that the phenomenon of *mixed quotation* offers us a clue as to the behaviour of de re constructions.

An example of a mixed quotation is the following:

- (3) Quine said quotation 'has an anomalous character'

The claim I want to make is that we should understand this as essentially a de re style construction. Just as the report of Quine's belief in 2 doesn't place any constraints on how Quine *thinks* of the president, so the report of Quine's utterance in 3 doesn't place any constraints on how he *speaks* of quotation. In the former case, he could think of the president in a presidential way or guise or mode of presentation, or in a guy-I-know-from-the-beach way or guise or mode of presentation. In the latter case, he could speak of quotation by using either the word 'quotation' or 'citation' or 'metalinguistic discourse'.

Having reviewed recent accounts of mixed quotation, and found them wanting, I will present my own. According to it, as it occurs in the above, 'quotation' functions

<sup>2</sup>Unless she also independently believed the president was a spy, of course.

as a quantifier over words which mean quotation, with the result that the sentence is true provided there's some word meaning quotation (such as say 'quotation' itself, or 'citation') such that Quine said—roughly speaking—the result of concatenating it with 'has an anomalous character'. To occur seemingly extensionally in a hyperintensional context is just to function as a device of generality of subsentential hyperintensional entities.

I think this can be extended to de re belief reports. Consider—to take a case that's easy for me—the following:

- (4) Quine believes, of the president, that he's a war monger

I say that 'the president' here functions as a quantifier over Obama senses, and show how to compose that with the sense denoted by 'is a war monger' to yield the content of Quine's belief. This belief is one true provided there's some sense by which he thinks of Obama, and he thought that sense composed with the sense of 'is a war monger'.

The conclusion I'll draw from this is that we don't need e-type expressions to account for the semantics of de re reports, because these beliefs do not as has been assumed, serve to report that the believer stands in a direct, immediate relation to the object of the belief. Instead, it reports that the believer is related to a Fregean sense, but it doesn't say which one.

The two arguments together serve to cast serious doubt on the importance of such a semantic type, as well as presenting interesting new accounts of some important phenomena.

## 1.2 Plan For The Chapters

Let me now turn, in more detail, to the layout of the thesis. As I noted, it consists of two extended arguments, the first spanning chapters 2-6, and the second chapters 7-8.

### 1.2.1 Chapter 2: Names

This chapter reviews recent work on names. The goals are twofold: to present recent work on the semantics of names, and in particular two theories which have recently been the subject of a lot of attention, namely predicativism and variabilism. I'll set them out, see which is most faithful to all the data, and present some old and new problems for them. The conclusion I'll argue for is that the central claim of predicativism, that names are predicates, is worth upholding, even if its extant formulations fail.

### 1.2.2 Chapter 3: Metaphysics

The point of this chapter is to lay the groundwork for my positive proposal by reviewing the recent debate concerning persistence, certain ideas from which will play an important role in my semantic theory. In particular, I'll introduce perdurantism, endurantism, and stage theory, and consider some arguments in their favour and against them.



### 1.2.3 Chapter 4: On The Link Between Semantics And Metaphysics

This addresses a worry that may have occurred to the reader when reading the above summary: that I'm letting metaphysics intrude too much into my semantics; or alternatively, that I'm letting semantics drive my metaphysics too much. Here I defend myself against those charges. First I'll claim, by considering the practices of working semanticists, that it's fine to let semantics defeasibly guide one's metaphysical theorising, provided positing a particular item in one's ontology helps explain otherwise recalcitrant data. I present some familiar case studies, such as Davidson's event semantics. Second, I'll claim that it's fine to let one's metaphysical theorising guide one's semantics. I'll do so by defending what I call *metaphysical externalism*, the idea that just as we accept the idea that what a term like 'water' picks out depends on the chemical nature of the stuff in one's environment, so what 'Joan' picks out depends on Joan's metaphysical nature. If she is a stage, it picks out a stage; if she is an endurant, it picks out an endurant.

### 1.2.4 Chapter 5: Problems for Stage and Worm Theory

Part of the above establishes it's okay to make metaphysical posits provided there's data they can explain which can't be explained without them. In this section, I show there's data to be explained. In particular, I'll assume four dimensionalism is true, and also that our semantics should reflect this. Doing so, I'll show that trying to develop the central idea of either worm or stage theory into a compositional, categorematic natural language semantics fails. This will motivate a search for an alternative four dimensional semantics. Combining this insight with predicativism will lead to my positive proposal.

### 1.2.5 Chapter 6: Stage Semantics

Here, the culmination of the first argument, I present Stage Semantics, which I have already sketched above. In a bit more detail, I assume that the logical form of

(5) Joan laughs

Is:

- $\exists xx. \text{Joan}(xx) \ \& \ \text{laughs}(xx).$

Where the 'xx' is, importantly, a plural variable whose values are stages. The sentence is true provided there's some Joan stages, and they are laughing stages. This mimics the worm theory's idea that the possession of properties by an object is a question of some of its stages taken together possessing it. It differs in holding that what unifies unified stages is not mereologic fusion, but plural reference. It differs from stage theory in making no use of counterpart theory to account for predication, but agrees with its spirit to the extent that it tries to capture the facts of persistence with a stage only ontology. Finally, it's the purest form of predicativism, since even in argument position, a name just contributes a predicate (albeit one whose free variable gets mandatorily bound at sentence level). I show how to extend the view to cover more complicated constructions, such as quantification, where I make use of superplural reference.

### **1.2.6 Chapter 7: Belief and Belief Reports**

This chapter begins the extended argument concerning de re reports. First, I'll introduce the problem: what is the correct semantics for those sentences which appear to attribute to speakers beliefs directly about objects? First I'll consider a plausible and popular response. Then I'll review some problems with this response, before providing a sketch of the view I'll end up developing, which will turn crucially on the theory of quotation.

### **1.2.7 Chapter 8: Quotation (and more on belief)**

In this chapter, I'll present my positive theory of quotation, which I will make use of in giving an account of belief. Having reviewed the basic data points concerning the different varieties of quotation, I'll go on to consider in depth mixed quotation, reviewing extant theories of it in linguistics and philosophy, before presenting my own view, sketched above. Having replied to a range of objections, I'll return to belief and show that a relatively minor modification of my quotation semantics can account for de re belief reports in a new and interesting way.

### **1.2.8 Conclusion**

In the conclusion I review what we've seen, consider the prospects and some ways of extending Stage Semantics, and indicate certain directions from future research in areas other than language and metaphysics which my work indicates.



# Chapter 2

## Names

### 2.1 Introduction

In the introduction, I noted that my positive proposal would make use of two recent developments in metaphysics and semantics, namely four dimensionalism about objects and predicativism about names. The goal of this chapter is to consider the latter by reviewing and adding to the literature on names from the last twenty or so years. These years have seen two new positions to rival the Millian view which held sway in the latter half of the twentieth century: variabilism, which holds that names are variables, and predicativism, which holds that they are predicates. Each of these theories claims better fidelity to all the linguistic data concerning names, much of which has only been properly appreciated recently. Accordingly, I will start this chapter by considering data. In particular, because it's reasonable to translate variabilism into the claim that names are like demonstratives or personal pronouns, which have often been thought susceptible to an analysis as variables, and because it's reasonable to translate the most recent versions of predicativism into the claim that when they occur as arguments to verbs, names are like definite descriptions, I will begin by considering the behaviour first of demonstratives, indexicals, and definite descriptions, before comparing and contrasting it with the behaviour of names.

Having done so, and along the way presented what I think is an important problem for the standard predicativist view, and a new problem each faces, I will conclude that neither predicativism nor variabilism, when it comes to fidelity to data, conclusively wins out, although both have attractive features. I will, however, tentatively plump for predicativism, construed simply as a claim about syntax: names are syntactically predicates. In a later chapter, I will similarly tentatively plump for four dimensionalism. My hope is that showing how the combination of the two views leads to an interesting semantics which removes extant problems for each should raise our credence in that combination.

The plan for the chapter is as follows. In the next section, I provide a short history of twentieth century work on names, to situate the debate. Then I set to work assessing the recent views about names, by considering in detail the behaviour of referring expressions other than names, and then going on to see which sort of expression names behave most like. In so doing, I will present and evaluate variabilism, and then predicativism. The aim here is primarily, but not exclusively, expository. In particular, I want to add to the debate by presenting unappreciated data that centre around the fact that names sometimes behave like indefinite descriptions. I will argue that this undermines standard predicativist views, which hold that names, when occurring in argument position, behave like definites. I also present a new argument against both views.

## 2.2 A Potted History of Theorising about Names

A partial, potted, and historically inaccurate history of semantic theorising about names would go like this: Frege (Frege, 1892) argued that the reference of names is determined by a definite description associated with that name as its cognitive significance or sense. Russell (Russell, 1905) went further and argued that names are equivalent to definite descriptions in the sense that sentences containing names are, when properly analysed, equivalent to sentences containing definite descriptions, and in particular definite descriptions of a similar sort as those which Frege thought determined the reference of names. For example, the description associated with or equivalent to 'Aristotle' would be, perhaps, 'the teacher of Alexander the Great and student of Plato'. This view was refined and defended by Strawson in e.g. (Strawson, 1959) and Searle (Searle, 1958) in the fifties.

Kripke (Kripke, 1980) then came along and showed that this descriptivist picture of reference has big problems, by presenting arguments, which we will see later in this thesis, that showed that we get mispredictions if we assume the reference of names is either fixed by description, or that names have the same meaning as definite descriptions (which is a reasonable facsimile of Russell's original syncategorematic view).

Kripke himself went on to suggest the best explanation for the behaviour of names was that they were *Millian* (because it was the view supposedly espoused by John Stuart Mill): their semantic value was simply their bearer, and although sometimes their reference was determined by a description, normally it is done by causal reference transmission.

And then, the odd counterexample (Evans, 1973), opposing position (Dummett, 1973, Burge, 1973, Kroon, 1987), and refinement (Evans, 1982, Devitt, 1981, Soames, 2002, Chalmers, 2006) notwithstanding, all was quiet on the western semantic front. The Millian view, which balanced simplicity with impressive empirical coverage, led the day. There were concerns about cognitive significance, but these seemed far from insuperable, and, in the eyes of many, they were indeed supered by works such as Salmon, 1986, which suggested cognitive significance was not a semantic problem at all (we will discuss cognitive significance in the last two chapters of this thesis).

The past twenty or so years (and especially the last ten), however, have seen a resurgence in interest in the behavior of names, mainly due to people's attention being drawn to a range of important new linguistic data, which had previously been overlooked. It is this more recent work that forms the topic of this chapter, and in particular, two views opposing Millianism which have arisen: variabilism (Dever, 1998, Cumming, 2008, Schoubye, forthcoming) and predicativism (Elbourne, 2005, Matushansky, 2008, Fara, 2015). These views are easily summarised: the variabilist says that names are variables; the predicativist, they are predicates. Let's turn to look at the new data, by comparing and contrasting the behaviour of names with that of demonstratives, indexicals, and definite descriptions.

## 2.3 Data

In this section, I will review the data concerning names. I will initially do so obliquely by considering the relevant syntactic and semantic behaviour of other expressions often taken to be referring expressions: pure and complex demonstratives, indexicals, and definite descriptions. Because the variabilist takes names to be something akin to indexicals or demonstratives, and the predicativist takes bare occurrence of names to

be covertly definite descriptions, in so doing we'll be able to get a sense for how closely the behaviour of names matches that of those other expressions. I'll consider five criteria: two syntactic, and three semantic. The two syntactic criteria concern firstly what sort of phrase the expression is, and secondly whether it belongs to the open or closed class of expressions. The three semantic criteria concern respectively referential uses, attributive or descriptive uses, and binding.

### Pure Demonstratives

**Criterion 1.** Pure demonstratives are superficially syntactically simple. There are two ways one can make sense of this in terms of contemporary syntactic theory, if one wants to say that demonstratives are determiner phrases (that is, as a phrase with a determiner head and a noun phrase complement. For some discussion, see Carnie, 2013, chapter seven). One can say that the demonstrative itself is a noun phrase with a null determiner (one present at LF, but which doesn't get spelled out on the surface), as so<sup>1</sup>, and where DET stands for some determiner or other:

- [<sub>dp</sub> [<sub>d</sub> ∅ DET] [<sub>np</sub> that]]

Alternatively, one can say that there's an unpronounced noun phrase, and that demonstratives are actually a species of determiner, as so, and where NOUNP stands for some noun phrase or other:

- [<sub>dp</sub> [<sub>d</sub> that] [<sub>np</sub> ∅ NOUNP]]

There is data for and against each of these positions. In favour of the null determiner option, we can note that some expressions which behave sometimes as pure demonstratives can function seemingly as noun phrases in their own right, as evinced by, for example, occurring after a determiner in the following:

- (6) My cat is a she

On the other hand, we can note this isn't a very productive phenomenon. It's very hard to hear the following as ok, even in a favourable scenario:

- (7) Every she is in the hall

In favour of the second option, as noted by Elbourne (Elbourne, 2005, p58) who himself cites Paul Postal (Postal, 1966) we have the fact that sometimes pronouns appear to behave like determiners in preceding noun phrases, as for example:

- (8) You troops will embark but the other troops will remain

Moreover, Elbourne argues that if we go for this second option, then we can provide a neat analysis of the semantics of donkey pronouns. We can hold that, considering a discourse like:

- (9) A man walked in. He was whistling

There is a noun phrase accompanying the pronoun, inherited from the previous sentence, which is simply unpronounced. That is, we have:

<sup>1</sup>Note I use 'noun phrase' in such a way that a single common noun counts as a noun phrase, so that a phrase doesn't have to consist of more than one word.

- A man walked in. [<sub>dp</sub>[<sub>d</sub> He] [<sub>np</sub>∅ man]] was whistling.

If we then, as Elbourne shows, assume that 'He' is a definite determiner, and a situation semantics interprets such forms (as we will see later, in Chapter 4, section 2), we get the right predictions.

Another piece of evidence for the pronouns as determiners view comes from cross-linguistic data. We can note that in German, the definite article sometimes behaves like a pronoun. The first sentence below shows the determiner use, the second the pronominal use:

- (10) Der Handschuh ist schwarz.  
THE GLOVE IS BLACK
- (11) Kurt arbeitet in St Andrews. Der wohnt aber in Dundee.  
KURT WORKS IN ST ANDREWS. HE/THE LIVES BUT IN DUNDEE  
Kurt works in St Andrews. But he lives in Dundee.

**Criterion 2.** Pure demonstratives belong to the closed class of expressions. There are, in a given language, a number of expressions which function as pure demonstratives, and this doesn't change much, if at all, across time. In English, pure demonstratives are 'that', 'those', 'this', 'these', 'he', 'she', 'it', and 'they' and other dialectal expressions ('yon' and 'thon' for my dialect, just for your interest).

**Criterion 3.** Pure demonstratives have referential uses. There's a demand that the referent be salient (whatever that means) and either closer or further from the speaker (respectively for 'this' and 'that', and again whatever that means). Salience and closeness are not notions that are confined to the spatio-temporal world, or even, arguably, the world at all: one can demonstratively refer to abstract objects and perhaps to hallucinations.

- (12) That is a table
- (13) These are my pens
- (14) [After someone says direct reference is false] That's a good point
- (15) [Macbeth speaking to thin air] Is this a dagger I see before me?

To the extent that it's frequently assumed rigidity is a mark of referential uses, we can note that pure demonstratives appear to be rigid. Imagine we're sitting in on a bench in front of an oak tree; there's another bench beside it facing a beech tree. In a world in which we'd sat on the latter bench, we could have referred to the beech with 'that'. But we can't report this fact with the following:

- (16) That's an oak tree. Though if we'd sat on the other bench, that would have been a beech

The second 'that', despite being under a world shifting operator (assuming conditionals are such), keeps the value it gets from the context of use.

**Criterion 4.** Pure demonstratives also permit descriptive readings (Nunberg, 1993):

- (17) [pointing to the newly appointed German pope] He is usually Italian
- (18) [pointing to builders who arrived on time] That's surprising: they tend to keep you waiting

Descriptive readings of demonstratives (and indexicals, as we'll come to) are more or less well-glossed as sentences in which a pronoun appears to be going proxy for a definite description. For example, it makes no sense to say, of Ratzinger, that he is usually Italian. Rather, what we're saying is something like that the office of the pope is normally inhabited by an Italian. A reasonable gloss of 17 is thus: 'the pope is usually Italian', where 'the pope' is functioning non-referentially, such that relative to the times over which the adverb of quantification 'usually' quantifies, the pope relative to most such times is Italian (this is perhaps an inadequate account of how adverbs of quantification work, but it will suffice for our expositional purposes).

**Criterion 5.** Demonstrative pronouns are donkey bindable, personal pronouns are c-command bindable. In order to explain this, a brief excursus on binding is necessary. There are two sorts of binding (which I use as a descriptive, not technical term here) recognised in the literature: c-command, and donkey binding. The most familiar sort of binding is c-command binding. To get a definition of c-command, we first need the notion of domination:

**Domination.** An expression  $\alpha$  dominates an expression  $\beta$  provided  $\alpha$  is above  $\beta$  in the tree and one can trace a path from  $\alpha$  to  $\beta$  going only downwards.

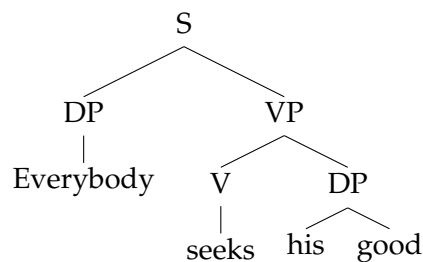
We can then define c-command as:

**C-command.** An expression  $\alpha$  c-commands an expression  $\beta$  provided the first branching node dominating  $\alpha$  dominates  $\beta$ .

Pronouns antecedent on universal quantifiers, if they are to be bound, require c-command, as the following minimal pair, followed by their phrase structure trees, evince:

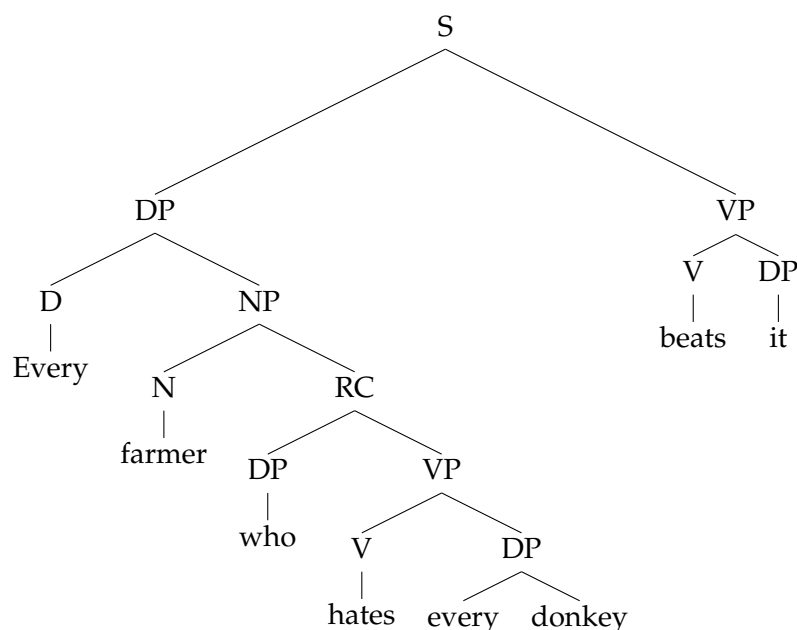
- (19) Everybody seeks his own good  
 (20) # Every farmer who hates every donkey beats it

Below we see that the first node above 'Everybody' dominates the whole vp, and thus the pronoun 'his':



By contrast, in the following, the first branching node dominating the dp 'Every donkey' is the vp node with it and 'hates' as children, and it clearly doesn't dominate the vp 'likes her', and so doesn't dominate 'it':





However, there's another sort of binding. Consider the following:

- (21) Every farmer who owns a donkey beats it.

This sentence is fine (unlike 20), and, intuitively speaking, the pronoun 'he' stands in the same sort of relation to its antecedent 'a farmer' as does 'his own good' to 'every man' in 236, giving good reason to call this a type of binding. But note that the expression 'a donkey' doesn't c-command 'he', so it's not c-command binding. Indeed, we can get binding operating beyond sentence boundaries, as witness:

- (22) If a man owns a donkey, he beats it.  
 (23) A man walked in. He was smiling.

It's a controversial question whether what I've called 'binding' permits of a single, unified analysis. Dynamic semanticists (a tradition started by Irene Heim (Heim, 1982) and independently, in a slightly different form, by Hans Kamp (Kamp, 1981). There's a nice overview in (Yalcin, 2012)) think the answer to this is yes, while others who favour static semantics think we can account for the non c-command binding by introducing new semantic, syntactic, or pragmatic resources to that semantics (representative examples here are, in addition to Elbourne, (Heim, 1990), (Neale, 1990), (Evans, 1977)). So note well that I use binding purely descriptively to pick out the surface phenomenon and do not seek to judge the underlying issue.

Demonstrative pronouns appear to exhibit neither c-command, nor non c-command binding. The following are both bad:

- (24) # Every trophy is resting on that's plinth  
 (25) # If the table in the corner is expensive, we should sell that

Personal pronouns, however, exhibit both c-command and donkey binding:

- (26) Every man thinks he's the best  
 (27) If a farmer likes cheese, he's portly

**Criterion 6.** Pure demonstratives have minimal descriptive content: their Kaplanian character is insufficient, given a context, to determine a referent. Something else is required to take one beyond that descriptive content to an object. What exactly that is is an open question (for some work on this issue see Kaplan, 1989, King, 2014, Stokke, 2010, Gauker, 2008).

### Complex Demonstratives

**Criterion 1** Complex demonstratives are syntactically complex. I assume it's uncontroversial that the logical form is something like<sup>2</sup>:

- [<sub>dp</sub>[<sub>d</sub> that] [<sub>np</sub> man]

**Criterion 2.** Complex demonstratives, containing a closed expression 'that' (or its inflected forms) and an open expression in the form of a noun phrase, are neither closed nor open, can occur in the singular or plural, with arbitrarily complex noun phrases, referring to things or to stuff:

- (28) That woman
- (29) That woman we met last night
- (30) This water
- (31) These women

**Criterion 3.** Complex demonstratives have referential uses, and can refer to the same sorts of things as pure demonstratives:

- (32) [possibly, although not necessarily, pointing to the tree] That tree is an oak
- (33) [talking about the number 7] That number is my favourite

**Criterion 4.** Complex demonstratives also permit descriptive readings

- (34) [pointing to a bus boy in a restaurant we've never been to before] That guy always gets stiffed when it comes to tips

**Criterion 5.** Complex demonstratives do not permit anaphora straightforwardly:

- (35) # Every player took that player's place

However, it has been argued we can get bound uses when we have a pronoun in the nominal, as so (King, 2001):

- (36) Every father regrets that moment when his daughter leaves for college

And we can also note that donkey anaphora seems just about ok with complex demonstratives:

- (37) (?) If a man walks into a bar screaming, that man tends to be angry
- (38) A guy came in screaming. That guy was angry!

<sup>2</sup>Actually, maybe it should be slightly controversial: after all, above we canvassed the idea that pronouns were noun phrases, in which case the form in the text wouldn't be correct unless we assumed that pronouns were ambiguous as between determiners and nps. Thanks to Matt Cameron for pointing this out.

**Criterion 6.** Complex demonstratives have richer descriptive content than pure demonstratives, but like them will often require something to supplement it to determine the referent. This will vary depending on what one thinks of the semantics of complex demonstratives: it could be whatever mechanism completes incomplete definites (see below), or it could be whatever takes us from occurrences of pure demonstratives to referents (for which see the references given above).

### Indexicals

**Criterion 1.** Indexicals appear to span syntactic categories. We could give a determiner phrase analysis for 'I' and 'you', but 'here' and 'now' are presumably adverbs (at least primarily; sometimes each can appear in the argument position of verbs).

**Criterion 2.** They belong to a closed category. They have few lexical realisations, limited to 'I', 'you' and other pronouns except the third person, 'tomorrow', 'today', 'here', 'now'.

**Criterion 3.** They have rigid, referential uses:

(39) I am eating soup

Consider the following, uttered by me and addressed to an Oslovian:

(40) If you had been talking, I would have been from Oslo

Doesn't have a true reading despite the fact that had my addressee been talking, *the person speaking* would have been from Oslo, and the descriptive meaning of 'I' is something like *the person speaking*. For some purportedly recalcitrant data, and ways of accommodating it, see the literature on monsters (for example: (Schlenker, 2003), (Santorio, 2012)).

**Criterion 4.** They also have descriptive uses:

(41) [at your door, which you opened quickly and wide, late at night] Be careful! I could have been an axe-murderer.

This has the sense of something like: *the person talking to you right now* (i.e. the one you opened the door to) could have been an axe-murderer.

**Criterion 5.** They don't appear to licence c-command anaphora:

(42) Every place I go has here's charms and perils

The above clearly doesn't get a bound reading, although the following reasonably similar sentence clearly does:

(43) Every place I go has its charms and perils

However, it has been argued that we do get bound indexicals, as the following example from Heim shows:

(44) Only I did my homework

The reason there's pressure to say this is bound is because it seems clear that it's truth-conditionally equivalent to: everybody was assigned homework: everybody but me didn't do the homework they were assigned, but I did to the homework I was assigned. Other examples include the following from Partee:

(45) If John comes, we'll play trios, otherwise we'll play chess

Concerning this example, it appears that the two occurrences of 'we' denote different things: the first denotes the group consisting of the speaker, the hearer, and John, while the second denotes the group consisting of just the speaker and hearer. That is to say, the first occurrence inherits content from the antecedent 'John' in what looks kind of like the way a bound element inherits content from its binder. Another possible example is the following. Imagine you find yourself tied up and blindfolded, and you're uncertain if your assailant is in the room, but if they are you want to let them know you won't stand for this nonsense. You could say:

(46) If there's someone there, you're going to be in trouble

It strikes me as somewhat plausible to say that the 'you' here is bound by the quantifier 'someone', so that the sentence comes out true even in case there is no one there, as it wouldn't were the indexical to get its value from context.

**Criterion 6.** Indexicals tend to have a rich descriptive content: indeed, one that, given a context, is sufficient to determine a referent. Thus the descriptive content of 'I' is something like *the person speaking* which given a Kaplanian context yields a referent. We get similar things with 'tomorrow' and 'today', although it should be noted that especially 'here' (and 'now') are importantly different. 'Here', for example, can refer to a room, a building, a town, a hemisphere, and so on.

### Definite Descriptions

**Criterion 1.** They are syntactically complex, with an LF like:

- [<sub>dp</sub> [<sub>d</sub> the] [<sub>np</sub> man]]

**Criterion 2.** They are the same as complex demonstratives, being neither closed nor open (or perhaps 'clopen', as mathematicians say):

- (47) The woman
- (48) The woman we met last night
- (49) The water
- (50) The numbers between one and ten

**Criterion 3.** They have referential uses as famously noted in (Donnellan, 1966):

- (51) The man drinking the martini looks interesting

This is a hugely controversial topic, but this certainly seems very close in meaning to 'he looks interesting' which I take it contains an expression occurring uncontroversially referentially. The exact nature of these referential uses is uncertain. In particular, it's uncertain whether a referential use implies a referential, e-type interpretation of the definite (Donnellan was notoriously cagey about this matter, but Kripke (Kripke, 1977)

famously argued it didn't). Moreover, definites are ubiquitously incomplete (for example, at the time of utterance, there were presumably several men in the world drinking martinis), and it's uncertain what accounts for their completeness. Some have argued that it's a semantic matter, coded in the syntax by covert domain restricting variables; others that it's semantic but not reflected in any part of syntax, while others think it's not reflected in the truth conditions (for the first, see (Stanley and Gendler Szabó, 2000). That paper also discusses the second view, an interesting version of which is in Dever's (Dever, 2004), while a different version uses the unarticulated constituents of (Perry, 1986). The third is most famously argued for by Kent Bach. See e.g. (Bach, 2000).

**Criterion 4** They have descriptive uses:

(52) The president is commander in chief

This seems to be about the role of presidency, just as 'he is normally Italian' appears to be about the role of pope. We can note that the problem of incompleteness affects descriptive just as well as referential uses, and the same solutions are available.

**Criterion 5.** They permit donkey, but not c-command anaphora

(53) If a farmer owns a donkey, the donkey is happy

(54) A man came in. The man was upset.

(55) # Every boy thinks the boy is happy

**Criterion 6.** They pattern with complex demonstratives, but are much less limited in their behaviour: they can be used to talk about things that aren't salient, and indeed things which weren't known by the hearer to exist prior to the utterance. This is the phenomenon of accommodation brought to prominence by (Lewis, 1979), and typified by an example like:

(56) I joined the neighbourhood watch in my area because I felt I should give something back

This is a fine thing to say, even if you know that your audience doesn't believe, prior to your utterance, that there is a neighbourhood watch in your area.

## Names

Let's now turn to our first crucial question: how do names behave with regard to this data?

**Criterion 1.** There is quite strong evidence to suggest that names are, contrary to surface appearances, syntactically predicates (common count nouns). We can begin by noting that they sometimes take an article in English<sup>3</sup>, as the following examples from (Fara, 2015.) show:

(57) The ever popular Michael is talking

(58) An Alfred from Princeton joined the club

<sup>3</sup>In particular, as Fara shows very well, they do so when followed by a restrictive relative clause, or preceded by either a non-restrictive or a restrictive modifier

(59) The childish Michael played while the sage Michael read

Compare with:

(60) The ever popular department head is talking

(61) A man from Princeton joined the club

(62) The childish boy played while the sage boy read

Moreover, they admit pluralisation, just like common nouns:

(63) Politicians from Alaska are normally brutal

(64) Sarahs from Alaska are normally brutal

Further, in other languages, they appear overtly with articles even in the absence of modifiers (for extensive cross-linguistic discussion, see (Matushansky, 2008)):

(65) La maria dorme

Where 'la' is a definite article and 'Maria' is a name. This has lead theorists to hold that we should posit a covert article for languages which lack an overt one, so that the logical form of a sentence like:

(66) Mary is sleeping

Is:

- [<sub>dp</sub>[<sub>d</sub> det] [<sub>np</sub> Mary]]

While the most common view is that the determiner is a definite, this will be questioned later.

On the other hand, opposing theorists (Schoubye, forthcoming) have noted that we appear to get uses of even pronouns accompanied by determiners, as in the following repeated from above:

(67) My cat is a she

And have argued on this basis that we needn't postulate hidden syntactic complexity.

**Criterion 2.** Names are noteworthy for their openness: there are many names, and new ones are frequently introduced. Exactly *how* open names are is an open question. For the Millian, given the ubiquity of names borne by more than one person, names are ambiguous: the English 'John' is many ways ambiguous, which we could mark with subscripts: 'John<sub>1</sub>' might stand for Keats, while 'John<sub>2</sub>' stands for Milton (just as we mark the disambiguations of 'bank' as 'bank<sub>1</sub>', 'bank<sub>2</sub>'). For a version of the ambiguity view, see e.g. (Soames, 2002)). So names are very open on this view. By contrast, for other views, there is simply one 'John' and we tell another story about how it is that a particular occurrence of 'John' manages to stand, as the case may be, for Milton and not Keats. But in either case it's notable we can freely add names to the language, be they of the form 'Moon Unit' or 'Moon Unit<sub>1</sub>'.

**Criterion 3.** Names clearly have referential uses, and it's hard to deny that to refer to an object is their primary goal:

(68) John is happy

To the extent that we can take rigidity as a criterion for being referring, then names *seem* like paradigm referring expressions. There's a famous and notable difference between the two sentences below, as Kripke noted:

(69) Nixon might have lost the election

(70) The winner of the election might have lost the election

While the definite can refer, relative to a counterfactual situation, to what satisfies its descriptive content in that situation, yielding the false reading of 70, names don't exhibit such behaviour: a name always refers to the same thing relative to every counterfactual situation. Or so it's widely thought.

**Criterion 4.** The consensus is that names don't have descriptive uses, apart from in special cases, where a name is explicitly introduced as standing for the same thing as a description. Evans's famous example is 'Julius', introduced stipulatively to stand for the inventor of the zip. One might think this isn't bona fide descriptive, any more than Kaplanian characters are descriptive, because the descriptive content appears immune to world shifting operators:

(71) If the zip had been invented in China, Julius would probably have been Chinese

Although exceedingly subtle, one might think 'Julius' here refers to the zip's actual inventor, and not the inventor in the closest world where the zip was invented in China, and that the sentence is thus false.

Accordingly, we should look for non-rigid examples. Arguably they are forthcoming. There is a set list of names for hurricanes, and as they occur during the hurricane season, suitably violent hurricanes are given a name, alphabetically, from the list (at least this is how things work in Ireland when it comes to naming storms.). Assume further, what is not the case, that each year the names are the same. And imagine 'Alex' is the first name on the list. Then the following seem like acceptable utterances prior to the start of 2016 hurricane season:

(72) Alex is always the first hurricane of the season

(73) It would be ironic if Alex hit Florida, because my aunt lives there and she's called Alex.

Arguably, we should classify these as descriptive uses because they appear non-rigid. Imagine this uttered before the hurricane season. Assuming an open future, 'Alex' doesn't refer to any particular hurricane, because there's been, as of yet, no process of baptising a particular hurricane with that name. Yet the sentence has a prominent (to my ears) true reading, glossable as something like: for all seasons, the hurricane named 'Alex' is the first hurricane of that season. Or again, imagine Alex has been used for a hurricane that's just been. We'd get the wrong result if the occurrence of 'Alex' in 72 referred to that hurricane, unless we have some very weird views about hurricane identity.

I am tempted to conclude, then, that in addition to the familiar phenomenon of Evans-esque descriptive names, there are also bona fide descriptive/attributive/non-rigid occurrences of names.

**Criterion 5.** Concerning binding, it has been argued that we get donkey anaphora, and that this is typified by the following examples, from Bart Geurts (Geurts, 1997) and Samuel Cumming (Cumming, 2008) respectively:

- (74) If they have a child called 'Bambi', Bambi will get sued by Disney  
 (75) There's a gentleman from Herdfordshire named 'Ernest'. Ernest like cats.

The thought is that the occurrences of the names in the second sentences are anaphoric on the indefinites in the first. Note this only works with singular indefinites:

- (76) # If they have two children and call them each 'Bambi', Bambis will resent them for their unoriginality

Moreover, to my ear, it's not great with definite antecedents:

- (77) # If the guy known as 'Slippery Pete' calls round, tell Slippery Pete I'm not here.

Although maybe the discourse case is better:

- (78) The criminal known as 'Slippery Pete' broke the lock. Slippery Pete is an expert burglar.

We seem quite clearly not to get c-command anaphora, however:

- (79) # Every Farquart thinks Farquart is well-named.<sup>4</sup>

Compare with the fine:

- (80) Every Farquart thinks he is well-named.

**Criterion 6.** Names appear to have a minimal descriptive content: a name like 'Joan' seems to encode the descriptive condition of *being called Joan* (for more on being called, see (Gray, 2014)). But this is a property possessed by many people, and also one which, given a context, as conceived by Kaplan, or even by any reasonably conservative extension of Kaplan's theory, doesn't determine a reference. As we will see later, names have an externalist character: one can refer to an object in substantial ignorance of what it's like. In particular, one doesn't need to know that it satisfies a description, and one doesn't need to stand in some perceptual or contextual relation to it for an utterance of a name to pick out an object. This is an important feature of names, which arguably differentiates them from the other referring expressions we have considered.

## 2.4 Variabilism

With the data on the table, we can now introduce and evaluate the two rival views. Variabilism ((Dever, 1998), (Schoubye, forthcoming), (Cumming, 2008)) says that names are variables. That is to say, they get their value provided by the assignment function. The underlying form of a typical simple sentence will be:

- John<sub>1</sub> is happy

<sup>4</sup>Correctly: the name has an interesting etymology.



Where the subscript is the same as the one posited to capture variable binding in the standard semantics of, say, Heim and Kratzer, 1998. Something, perhaps the speaker's intentions, will determine that the assignment function in play is one which maps 1 to the person the speaker wants to talk about.

Let's now consider some arguments in favour of variabilism. First is the capturing of the rigidity of names. As we've seen, this has been taken to be one of their defining characteristics. Moreover, it's also been widely taken to be a characteristic of variable-like elements, notably demonstratives. This is a strong point in favour of variabilism, and one which we should give sufficient weight to.

The second argument in favour of variabilism is its supposed ability to parry one of the predicativists' strongest points: the use of names as common nouns. The most recent version of variabilism, that of Schoubye, thinks it can capture this data by leaning on examples like:

(81) My cat is a she

Not only that, but Schoubye claims to be able to give an account of how we manage to get such readings. On the standard story, the predicative meaning of 'she' (namely *being female*) is not at the level of semantic content: rather, it's character level. One way to implement this is in terms of phi-features. This is a notion from syntax, albeit a heavily semantic one. It is assumed that expressions such as pronouns encode features such as number, case, and gender. For example, the phi feature of 'I' is first person singular, nominative, gender undefined. The phi features of 'he' are third person singular nominative, male. Schoubye's suggestion is that we can make sense of predicative uses of names provided we hold that the phi feature of a name is something like being called N, and if we make use of the operation, evidently in effect in 'My cat is a she' from turning an expression from one which has a given phi feature to one which has that phi feature as content.

I doubt this move. Names in post-determiner position are very productive, being almost always fine. This is not the case with variable-like elements:

(82) # Every she left

(83) #Most those are boring

(84) # Few its think so

Moreover, there's something dialectically weak about this move. In Schoubye (loc. cit.), a lot of attention is paid to the rigidity of names, as an argument against predicativism: he thinks the extreme difficulty of finding non-rigid names tells against the version of predicativism according to which they function as definite descriptions when in argument position. But I think the non-rigid examples I presented are about as convincing as the above predicative uses of pronouns. It strikes me that there's something of a deadlock: neither side fits the data perfectly, so one needs to be very careful when leaning on arguments from empirical coverage. Semanticists in glass houses shouldn't charge other semanticists with having empirically inadequate theories.

The third argument is from various sorts of parsimony. Most notably, the Millian has to posit at least two classes of referring expressions: names and variables<sup>5</sup>. The variabilist doesn't have to do this. This brings with it another advantage: by analysing names as variables, one can give an account of the lexicographic identity but semantic

<sup>5</sup>It would be a bold, albeit interesting, Millian who would try to analyse variables in terms of Millian names.

difference exhibited by name types. It's a mundane point that in the following sentence, 'Aristotle' can refer either to the Stagyrte or shipping magnate:

(85) Aristotle was rich

As we've seen, the Millian explanation here has tended to go by way of ambiguity, which isn't very appealing. However, it appears that the variabilist can do better. On this story, the 'ambiguity' is just that evinced in the fact that in different circumstances, different utterances of the sentence below may refer to different people:

(86) He was rich

Of course, we are not entirely without ambiguity on the predicativist story. The actual underlying form of the above will be as so:

(87) He<sub>1</sub> was rich

We'll get different referents in different contexts because the sentence will be evaluated with regard to different variable assignments. It's worth noting that theorists have thought that the 'ambiguity' introduced by numerical subscripts is noxious. Pauline Jacobsen, representing the variable free semantics tradition, makes this point (Jacobson, 1999), and more recently, Dever, Pickel and Rabern (Dever, Pickel, and Rabern, n.a.) have done so too. Moreover, I think there's good reason to be chary of any argument from parsimony, whether it be for a variabilist or a predicativist analysis. I shall make this point below.

The fourth argument, finally, is the argument from binding, which is intimated in (Dever, 1998) and stressed in (Cumming, 2008). Cumming especially wants to argue that in a case like:

(88) There's a gentleman from Herdfordshire named 'Ernest'. Ernest is well mannered

What we have is bona fide binding, and in particular this is just another case of discourse anaphora like:

(89) There's a gentleman from Herdfordshire named 'Ernest'. He is well mannered

How to give a unified account of c-command and donkey anaphora is of course a hard question, but it's also familiar that there are ways and means of doing so (such as dynamic semantics), to which the variabilist can help themselves.

There are two weak points to this sort of move. Firstly, it's very controversial that there is a unified semantic mechanism underlying binding. It could be that such readings as the above are generated by some extra syntactic, semantic, or pragmatic mechanism. For example, and as we've seen, at the syntactic level, it could be that an anaphoric pronoun functions as a copy of its antecedent indefinite noun phrase, resulting in a logical form like this, and assuming the plausible determiner analysis of pronouns:

- There's a gentleman from Herdfordshire named 'Ernest'. He [gentleman] is well mannered

On a semantic level, it could be that the content of the whole first sentence is bundled up and attached to the pronoun, either at character or at content level (respectively Evans and Neale). Finally, it could be simply that using the indefinite in the previous

sentence suffices to make the person the speaker is talking about salient, and thus eligible for demonstrative reference with the pronoun. Accordingly, we should be wary about moving from the surface appearance of binding to assuming that names are variable-like expressions. Secondly, the data, as we've already seen, doesn't fit well. If it could be shown that names have the same binding capabilities as pronouns, this would be a very strong result. But this can't be shown, because they don't. Again, the following minimal pair exhibits this:

- (90) # Some John thinks John is the best  
 (91) Some John thinks he is the best

All that we see is that we get non c-command anaphora with names. But we get exactly the same with definites!:

- (92) There's a gentleman in Hertfordshire named 'Ernest'. The gentleman likes figs.

Not only that, I think definite cases with plural indefinite antecedents are arguably better than their bare name equivalents, as compare:

- (93) If they have two children each named Bambi, Bambis will be angry  
 (94) If they have two children each named Bambi, the Bambis will be angry

So I think it's unclear that the binding data are really advantageous to the variabilist as against the predicativist.

In summary, my assessment of variabilism is as so: it does well with rigidity, and to some extent avoids the postulation of ambiguity, both of which are good things, but it struggles to explain convincingly the predicative uses, and the behaviour of names is different enough from standard variables that we should be wary of endorsing it. Moreover, it has no explanation for the fact that names are open class while variable-like elements are closed.

## 2.5 Predicativism

Let's now turn to predicativism. Predicativism is the view that names are predicates: common count nouns. A name such as 'Julia' applies to a person provided that person is called 'Julia' (see (Fara, 2015) and (Gray, 2014) for discussion of being called). As such, it can explain very easily the occurrences of names after a determiner which we saw above. It faces, however, a converse problem of explaining what's going on in very simple cases like the following:

- (95) John likes fish

The classic predicativist move is to argue that there's a covert article, so that we have a logical form as so, where *det* stands for some determiner, most commonly a definite:

- [<sub>s</sub>[<sub>dp</sub> [<sub>d</sub> det] [<sub>np</sub>John]] [<sub>vp</sub> likes fish]]

Let's now consider some arguments in favour of predicativism. Most obviously, there is a parsimony argument: it can explain the predicative uses freely, in a way that neither Millianism nor, arguably, variabilism can. As against this, we can note that what it gains in parsimony it must surrender in positing a covert determiner. Arguably this is not such a big deal: contemporary syntactic theory is rife with covert elements, after all. More worryingly, and telling against the parsimony move, are sentences like this:

- (96) He's no Jack Kennedy  
 (97) She's a real Einstein

The first of these uses can be true even if, as is admittedly unlikely, the person is called Jack Kennedy; the second, more clearly, can be true if the person isn't called Einstein. Rather, we're attributing to the person similar characteristics to the philandering world leader and the physics guy with the wavy hair. The predicativist can't capture these uses out of the box; so they'll have to appeal to something else to do so, and then there's the worry that having done so, that extra do-dah can be brought to bear to account for the more common predicativist uses. Moreover, as noted above, there's a general worry about parsimony which we'll get to.

Secondly, if one accepts the hurricane cases, then names do appear to have the semantic profile of descriptions, as permitting non-rigid readings. This is an important point. It's often taken to be uncontroversial that names *don't* have these descriptive, non-rigid readings: indeed, this forms the basis of Kripke's famous modal argument against Frege-Russell descriptivism. If this data point is incorrect, then predicativism instantly becomes considerably more plausible. Of course, an issue still remains: the non-rigid uses of names, if they exist, are very infrequent, while non-rigid descriptions are common. But it doesn't seem too far-fetched to think that some sort of pragmatic story could be brought to bear to explain this, although I don't have such a story at present.

Thirdly, we can note that names are open category expressions, just as common count nouns are. Moreover, their reference, on a given occasion of use, is not determined entirely by their associated meaning, the property of *being called X*. That is, they exhibit something like incompleteness, which as we noted is a central feature of definites. And finally, we saw that the binding data doesn't seem to tell against the predicativist: to the extent that it's there, it can be captured.

In short, I think the data, overall, is favourable to predicativism: it seems to do well with regards to criteria 1-6 in our list.

### 2.5.1 Which article?

I just suggested that predicativism does pretty well with the data; in the previous section, I argued that variabilism was less compelling. In this section, however, I want to present a problem for predicativism, concerning the nature of the covert article it posits.

I noted that the predicativist accounts for bare names by positing a covert article. It's fair to say that the standard view is that the determiner is a definite: let's call, using some terminology from Schoubye, anybody who holds that a 'the-predicativist'. I think the claim that the determiner is a definite has problems, and the aim of this section is to show them. They all turn on the fact that sometimes names behave as if they were fronted by a covert *indefinite* article.

Firstly, note that names have predicative uses in which they are accompanied by an overt indefinite article. We've already seen the following example:

- (98) An Alfred from Princeton joined the club

It does not seem to have been sufficiently realised, however, that names also have *referential* uses with overt indefinite articles. Imagine a newscaster beginning a broadcast with:

(99) A jubilant David Cameron last night lauded his party's success

This should *not* be assimilated to a predicative use: it's not true provided there's some David Cameron who lauded the Tories' success. Rather, it's true provided the David Cameron who leads the Tories lauded their success. This is a referential use of the name, but it doesn't have a definite article. So we see that names, just like indefinites, have both predicative and referential uses when accompanied by an overt indefinite article.

This is a problem for the the-predicativist. For a compelling reason in favour of the-predicativism is that we get referential uses of names with a definite article, both in certain constructions in English and cross-linguistically. But that reason is undermined by this data. It's now unmotivated to say that the covert article is a definite, given we know that indefinites, like definites, have referential uses, and that the indefinite article, like the definite article, can flank referential uses of names. Why not say the covert article is an indefinite? The the-predicativist owes an answer.

Secondly, I want to note that names sometimes behave like indefinites pragmatically. Let me begin by recalling that definites and indefinites both have referential uses (we have this above for the former; see e.g. (Ludlow and Neale, 1991) for a discussion of referential uses of indefinites). Here are mundane examples. Imagine first we went to a party last night, where we met a man and started talking. I could report on one of his utterances the next day by saying:

(100) The guy we met last night told me to avoid wheat

Next, imagine instead I went to the party alone. The following is appropriate:

(101) A guy I met last night told me to avoid wheat

Although definites and indefinites have referential uses, there is an important, if elusive, distinction between when it's appropriate to use them referentially. Typically, one uses an indefinite to talk about objects which one assumes to be unfamiliar to one's audience: I would use a sentence like 101 if I thought you didn't *know*, in the everyday, non-philosophical sense, the person I was talking about. There would be something weird and confusing about my using it to talk about a mutual acquaintance of ours. By contrast, definites are typically used to talk about objects that are in some sense already familiar to speaker and hearer. One way to see this is to note that the following variations on our above sentences are infelicitous in the context under consideration:

(102) # A guy we met last night told me to avoid wheat

(103) # The guy I met last night told me to avoid wheat

I don't have an account of the familiarity/unfamiliarity distinction to offer. But I do take there be some such distinction underlying the differential acceptability of the above, and aim to show that names, just like indefinites, have uses on which they introduce unfamiliar entities to the conversation. Here's a case. Imagine I'm a historian interested in the Crimean war. I could begin a lecture by saying:

(104) Today I'm going to tell you about the diary of a soldier in the Crimean war which I found in Sevastopol. He writes about the awful conditions he faced.

This is a paradigm example of introducing a new object into discussion by using an indefinite, in this case 'a soldier in the Crimean war'. Having been introduced, one can then go on to refer back anaphorically to him, as in the second sentence. With that in mind, note that the following is an equally fine way to begin the lecture:

- (105) Robert Russell was an obscure soldier in the Crimean war. I found his diary, and today I'm going to tell you about it.

I claim the name is playing exactly the same role as the indefinite: the speaker is introducing the hearer to someone previously unknown to him or her. But if this is so, then the the-predicativist has a problem. In these introductory cases, definites are bad. The lecturer couldn't have begun his lecture by:

- (106) # I found a diary and I'm going to tell you about it: it belonged to the soldier in the Crimean war. He writes about the awful conditions.

Similarly, were names definites, 105 would be predicted to be bad. But it isn't bad.

Let's consider some responses to this problem. Here's one thought: one could treat this as a case of presupposition accommodation. It's well known that one can use a definite felicitously even in case one's audience didn't know, prior to the utterance, that there was something which uniquely satisfied its nominal. Thus you may not know I have a cat, but I can say:

- (107) I couldn't come because my cat got sick.

Safe in the knowledge that you will come quietly hereafter to assume that I have a cat, even though you didn't before. If names are definites, one might think we can accommodate them as we can definite descriptions. The worry with this reply is that it's hard to see *what* is being accommodated. In the cat case, it comes to be common ground that the speaker has a cat. What could become common ground in this case? That there's someone called Robert Russell? That's clearly far too weak. 'Robert' is a common first name and 'Russell' is a common surname, so the hearer presumably already presupposed, if not explicitly, that there's someone called Robert Russell (this becomes even clearer if we change the name to, say, 'John Smith'). In light of this, one might think that one accommodates the whole sentence, namely that there's someone called Robert Russell who was an obscure soldier in the Crimean war. But this reply too has problems. Note that accommodation doesn't normally work this way: in hearing 107, the hearer doesn't come to presuppose that the speaker has a sick cat, just that the speaker has a cat. And we can see why: if that was how accommodation worked, then there really would be no interesting notion of accommodation: it would collapse into acceptance, by which I mean the taking for true by the hearer of what the speaker said.

So the the-predicativist can't really explain what's going on here in terms of an antecedently required notion of accommodation, but must introduce a new one, which substantially weakens the reply. Moreover, this new notion of accommodation is a bad one. In light of this, it's implausible to suggest that the the-predicativist can appeal to accommodation or an accommodation-like phenomenon to deal with this case.

Here's a second possible response. It's a familiar and perhaps the defining feature of names that typically, one comes to become competent in the use of a name by picking it up from someone else, who in turn picked it up from someone else. We can follow this chain back, the Kripkean story goes, until one comes upon a baptising: an event in which the name is bestowed upon its bearer. One might think that our case shouldn't count as an indefinite use of a name, because, having heard the sentence, the hearer becomes able to think of the object simply as Robert Russell. The hearer's epistemic impoverishment with regard to him would be no objection, any more than the typical hearer's impoverishment with regards to the physicist Gell-mann doesn't mean he or she can't speak about him as 'Gell-mann'. There's something special about names: one can make the unfamiliar familiar just by mentioning them by name.

This, one might think, explains the fact that a student can straightforwardly go on to ask:

(108) Where did you find Russell's diaries?

And in so doing use 'Russell' to refer to Russell.

While this response seems to have a *prima facie* plausibility to it, I don't know if we should allow the the-predicativist it. Surely if, as the the-predicativist claims, names occurring in argument position are definites, then we should be able to *explain* these Kripkean facts by appealing to some antecedently recognised feature of definites. Merely pointing out that names have this use isn't enough. But it's hard to think of what this can be: accommodation is the obvious thought, but I suggested above accommodation won't work here.

On the other hand, if we allow, as both the syntactic case and arguably the pragmatic behaviour here suggest, that names have an indefinite interpretation, then there's quite a nice explanation of the Kripkean facts. For we could say that names behave sometimes as definites and sometimes as indefinites. And, in particular, we can hold that the function played by 'Robert Russell' in 105 and that played by 'Russell' in 108 is different, and in particular that while the former is an indefinite use, the latter is definite.

And if we do that, then we can begin to explain the reference transmission facts. For it's familiar that a hearer, having been introduced to an object via an indefinite, can then go on to use a definite to refer back to the object meant by the indefinite. For an example:

(109) A: A man walked in.

B: Tell me about the man/him.

Given this, we can analyse reference transmission as an anaphoric phenomenon: the occurrence of 'Russell' in 108 can be understood as a definite whose anaphoric antecedent is the occurrence of 'Robert Russell' in 105, which is functioning as an indefinite. And we can say that that indefinite occurrence enables the hearer to be able, subsequently, to definitely refer to Russell using 'Russell', without having to say that that first occurrence is a definite. And if this is so, the second objection to treating it as an indefinite, from the Kripkean reference facts, is defanged. Firstly, it's insufficient merely to point out these facts, but the the-predicativist must explain them, and secondly, the the-predicativist can explain them in a principled matter, if only they allow that names sometimes behave like indefinites<sup>6</sup>

I just argued first that names sometimes behave *syntactically* like indefinites, and then that they are used with the same *pragmatic* purpose—to introduce new entities into discourse—as definites. Now I want to show that they behave interestingly *semantically*

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<sup>6</sup>Here's another worry with the idea that names are sometimes indefinites. Note that while 105 is fine, it seems that the environments in which one gets this is limited. For example, I can't say, out of the blue:

(1) Carol got a job

To talk about my friend Carol, whom you don't know. But one might think that if names can function as indefinites, it should be perfectly fine. I think I can explain why it's not fine. Names, on the story I'm developing, have two distinct types of uses: those on which they behave like indefinites, and the straightforwardly referential uses. Moreover, as matter of statistical fact, straightforwardly referential uses are the most prevalent. So there's a presumption, of any particular use, that it be referential. 1 is not good because the presumption isn't cancelled in any way. However, in 105 it is cancelled, by the use of a characterising predicate, and by the full name, and so it is good.

like indefinites, and in particular do so by having uses as existential quantifiers. Here's an example, inspired by Josh Dever (Dever, 1998, section 2.3.2.4.1.1):

**Nomination.** One nominates oneself for class president by writing one's name on a piece of paper and putting it in a box. We know there are several people called Sam in the class. The deadline has passed: I open the box, see two slips of paper both of which read 'Sam', read them, and remark: "Two Sams nominated themselves. I hope Sam and Sam get on."

Before going on to consider the problem, let me ward off an objection: one might feel the example is bad because it's infelicitous to conjoin two tokens of the same name, and that a more natural continuation is 'I hope the two Sams' or 'I hope the Sams' get on. While I agree it doesn't sound great, this is not a semantic issue. A simple google search reveals many instances of two tokens of the same name type being conjoined. Here's a sampling:

- (110) John and John, both Class of '83, lived in Dunne Hall their sophomore year (<http://www.scu.edu/scm/winter2006/learning.cfm>)
- (111) And at the feet of Jesus, and at the table where he eats, Mary and Mary both minister to Jesus (<http://drysdale.unitingchurch.org.au/wp-content/uploads/2013/02/17-Mar.pdf>)

With that said, let's see what the the-predicativist can say. If the sentence contains two definites, these definites are incomplete: the noun phrase which is the sister to the determiner, namely 'Sam', does not have a singleton extension. This is so because there are many people called Sam in the world. But now it's a condition on the use of incomplete definites that there be some way for the audience to contextually determine some singleton subset of the extension. So there must be some way of narrowing the domain of the first definite to a singleton, and some way of narrowing the second down to a singleton, but it cannot be the same way, because then we'd be conjoining the same thing twice, which would result in either anomaly or incorrect truth conditions.

I claim there is no such way to narrow the domains of two definites. Neither speaker nor hearer know anything to distinguish one Sam from the other; but in order to narrow the domain of each 'Sam' down to a different Sam, one would need to be able to distinguish them. The only properties one knows to hold of one Sam one knows to hold of the other. So, for example, the property of *having self-nominated* or *being one of the two who self-nominated* will not do, because that holds of both Sams, so neither definite would end up being complete. Given speaker and hearer's epistemic impoverishment with regard to the Sams, then, both definites must be incomplete, and so the computation of the semantic value of the sentence as a whole must crash. And so the the-predicativist incorrectly predicts the sentence is bad.

In fact, we can make the same point which the example above does more simply. Thus consider the following description of a podcast made by two people called Michael, Michael Ian Black and Michael Showalter:

- (112) In this segment, Michael and Michael announce the winners of the "Is This Art?" contest and determine whether or not the pictures that were chosen are art or not (<https://itunes.apple.com/us/podcast/topics/id662902268?mt=2>)

This, I take it, is perfectly natural. But it's plausible that, even though the writer has ways of distinguishing the Michaels, in writing the first 'Michael', he or she didn't



have either Michael in mind, and ditto for the second ‘Michael’. And so the context doesn’t appear to provide two different singleton restrictors, one for each occurrence of ‘Michael’. But intuitively, this doesn’t matter: it’s sufficient that the phrase as a whole stands for both Michaels, and it’s not necessary that the speaker make his or her mind up about which ‘Michael’ stands for which Michael. But for the definite theorist, it is necessary, as the domain of each occurrence of ‘Michael’ will have to have a singleton extension. So the the-predicativist places requirements on the speaker that just don’t seem to be there.

By contrast, if the covert determiner can have the semantics of an existential, as we know the indefinite determiner can, then these problems can be avoided. For we can say that each occurrence of the name simply contributes an existential quantifier. And then we can restrict the domain of the result of conjoining the two existential quantifiers. This might sound a bit strange, but we get this in natural language. For example, I can utter:

(113) An agent and a terrorist who met on a dissident website were in cahoots

Consider what the semantics of such a sentence must be like. The restrictive clause expresses a collective predicate, so it can’t distribute over the conjuncts of the noun phrase. It seems that what one must say is that first we compose the conjunctive noun phrase, and then compose the result with the restrictive clause. But then if we know that our language has such mechanisms, we can claim that they are in play here. Even if there is no explicit relative clause, we can supply one that holds collectively of the conjunction, namely *being the two people who self-nominated* and *being the stars of the Topics podcast* or rather, probably, rigidified versions thereof. The key fact we’re taking advantage of is that even if one has no way of, or no interest in, restricting either of the conjuncts, one still may have a way of, and/or interest in, restricting the conjunction as a whole. If names have an existential interpretation, we can explain this perfectly. And if names are indefinites, they have an existential interpretation, so we should hold that names sometimes function as indefinites.

The-predicativism fails to capture a range of important uses of names. What our cases show is that names often behave like indefinite noun phrases. Just like these, they occur with overt indefinite articles, both quantificationally and referentially. Moreover, even when bare, they have entity introducing referential uses, and existential uses. I thus conclude that as it stands, the-predicativism is insufficient as a theory of names. Moreover, I don’t think there’s a really neat fix to this problem. A first thought might be that the predicativist should hold that the covert article is ambiguous as between a definite and an indefinite determiner. But this isn’t too comfortable a position: most obviously, one of the main benefits of predicativism is meant to be its eschewing of ambiguity, but on the line suggested here it would crucially involve a claim about ambiguity. A more attractive position would be to adopt the view that one finds, for example, in (Ludlow and Segal, 2004), according to which there is no underlying semantic difference between the definite and indefinite article, and assume that the covert determiner has the semantic of the (in)definite but that it can be spelled out either as a definite or an indefinite. We could then hold, with Fara, that it only gets spelled out at all in certain syntactic configurations, such as when it has a preceding modifier, but add to this the claim that *which* article it gets spelled out as is determined by whether the use is familiar or novel. While this is more attractive, it’s still hardly without cost, as it requires the the-predicativist to take a stance on a substantial issue in the theory of definites and indefinites. I conclude that the findings of this sentence serve to cast

doubt on the-predicativism, and, to the extent that it's the most famous version of predicativism around, extant predicativisms in general. That said, the cross-linguistic data concerning how names behave syntactically like predicates remains impressive. One of the goals going forward will be to see if we can account for that data while avoiding the perils of extant predicativisms. And on the topic of perils...

## 2.6 A General Problem for non-Millian Theories

I noted above that a seeming benefit of both variabilism and predicativism is a certain parsimony: each manage to get rid of Millian names. To the extent that such a category of expression is problematic syntactically (we are forced to posit massive ambiguity in name types, despite the fact that names fail ambiguity tests), and semantically (after all, one could argue that the Kripkean 'picture' of causal reference transmission has never successfully gone further than being a picture), this might seem all to the good, and a good reason for being a variabilist or a predicativist.

I think this line of thought must be handled with extreme caution. In particular, here's an obvious point: if you get rid of a tool that plays a certain job, you better make sure you have a replacement for it, or that job won't get done. Now as we've noted, a feature of names—one of the roles the tool plays—is to enable a speaker to speak about objects which they don't know very well. For example, I can refer to Anaximander using 'Anaximander' despite the fact that I am not currently in perceptual contact with him, nor have been in the past, nor know a description which he uniquely satisfies. But perception, past perception, and description are, for the people who write about such things, three of the main ways we manage to come to know about objects in the world (e.g. (Recanati, 2012)).

The Millian has at least the beginnings of an explanation here. It doesn't matter that I'm so informationally impoverished with regards to Anaximander, because I can lean on the competency of others, such as the ancient philosophers two floors down. In particular, I belong to a community in which there's a name using practice which can be traced back, according to the Kripke line, to the man himself and his baptism in ancient Greece.

Now what can the predicativist or the variabilist say here? Consider first the predicativist. We'll have a logical form like:

- the Anaximander was from Greece

This definite, by virtue of there being more than one person called 'Anaximander', will be incomplete. So it will require completion. Now, without getting into exactly how completion is effected, we can say some general things. Consider the following examples of incomplete definites, and what it is, intuitively, that completes them:

- [Pointing to a table] The table is too heavy to carry
- [talking about a party last night] The guy with the Fedora was annoying
- [talking ignorantly about Canadian politics] What is the prime minister in charge of?

In the first case, what seems to take us from incomplete definite to completeness is the fact that speaker (and hearer, let's assume) are both presently perceiving the object. In

the second case, it seems to be past perception. While in the third, plausibly, it's further descriptive content, such as the property of being the prime minister of *Canada*.

Now I take it, in the Anaximander case, it can't be either past or current perception that does the completing. So one might think it ought to be a description. This doesn't necessarily mean disaster, for it doesn't have to be a Frege-Russell style description. It could be a causal description, in the style of e.g. (Kroon, 1987), like *the Anaximander who is the causal source of my use of 'Anaximander'*.

However, as a general matter, I don't think that this can be right. To see this, imagine that we'd been in contact with two Anaximanders, one from Greece and one from Turkey. And say we're talking about Turkish philosophers. Then we might want to interpret an occurrence of 'Anaximander' as 'the philosopher from Turkey who is a causal source of my use of 'Anaximander'.

But this is a problem. The problem is that this definite contains a name, which is to say a description. And this description is itself incomplete: there are quite a few places called Turkey (there's one in North Texas, for example). So then this description will have to be completed. But it could very easily happen that *this* description will also be incomplete. If it is, for example, 'the eurasian country bounded by the Black Sea', we'll run into problems because multiple things bear the name 'Black Sea'. It thus appears that the completion of names, if names are predicates, will often be a Hydra-like affair: completing one definite will just cause another incomplete definite to pop up, and one won't get anywhere, semantically. This, it strikes me, is a problem for predicativism.

It's also a problem for variabilism. Take perhaps the leading view about the metasemantics of variable-like elements: that it's the speaker's intentions which determine reference (I think similar remarks will hold for ideal interpreter theories à la Gauker, or mixed theories à la King). Problems arise here because of certain other features of our use of names. In order to intend to speak about a particular object (or for an ideal interpreter to understand an expression as picking out an object), one must surely have some way of picking that object out: this could be by perception, memory, description, or indeed by the possession of a Millian name. Now it's obvious that most of the names we use are not accompanied by perception, nor by memory of past perception. So then it seems like it will need to be descriptions that do the work. But then exactly the same problem as above will arrive: if we use a description containing a name, which will frequently be the case, then since names are variables typically requiring a description to take one to a referent, we will have the same hydra-like effect: to secure the reference of our 'Anaximander', we'll need to secure the reference of 'Turkey' in the philosopher from Turkey who is a causal source of my use of Anaximander'. And, in turn, we'll need to secure the reference of 'Turkey' in the same way.

I thus think these non-Millian theories of names struggle to accommodate, somewhat surprisingly, one of the main pieces of data which the Millian was concerned to capture. And this is quite a serious problem for both theories.

## 2.7 Conclusion

What, then, are we to make of the semantics of names? At this stage, it strikes me as very unclear. While each theory has something to say for it, each also suffers from quite clear problems. The most serious for the variabilist, I think, is the absence of clear (non-donkey) bound readings of names. The most serious for the the-predicativist is the fact that names don't seem always to behave like definites. And both suffer from the problem of the previous section.

However, I don't think all this has been in vain. I think the main lesson we should take from this chapter is the syntactic one that names very often behave like predicates. I will use this idea to present a new and to my mind more true to the name version of predicativism than has come before. However, before I can do this, I need to do some metaphysics, as my semantics will be informed by recent debates concerning persistence. The next three chapters turn thus to metaphysical matters.



## Chapter 3

# Metaphysics, Introduction

### 3.1 Introduction

The view I'll be defending, Stage Semantics, brings to bear recent work in metaphysics and recent work in semantics to present a new theory of how reference works. The previous chapter introduced the semantic work; this chapter is concerned with the metaphysics.

I'll begin by presenting the problem of persistence, subdividing it into two problems: the problem of persistence itself, which asks what it is for an object to exist at more than one time, and the problem of change, which asks what it is for an object to possess an (intrinsic) property at one time and fail to possess it at another.

I'll then consider the three extant answers to these questions, the perdurance view, the endurance view, and stage theory, considering some arguments for and problems with each theory. The modest conclusion I seek to extract from this discussion is that the existence of temporal parts – the sole metaphysical assumption I will later need – is well motivated, if not irrefragable.

### 3.2 The Problem

I existed this morning, when I was lying in bed. I exist now, as I'm typing this in the office, and I existed in between those times. How so? What is it for an object to exist continuously through a series of times? This is the problem of persistence. It was thus baptised, and the opposing positions laid out, in the following famous passage from Lewis, 1986:

Let us say that something *persists* iff, somehow or other, it exists at various times; this is the neutral word. Something *perdures* iff it persists by having different temporal parts, or stages, at different times, though no one part of it is wholly present at more than one time; whereas it *endures* iff it persists by being wholly present at more than one time. Perdurance corresponds to the way a road persists through space; part of it is here, and part of it is there, and no part is wholly present at two different places. Endurance corresponds to the way a universal, if there are such things, would be wholly present wherever and whenever it is instantiated. Endurance involves overlap: the content of two different times has the enduring thing as a common part. Perdurance does not. (Lewis, 1986, 202)

Sorting out precisely what the positions amount to, and, for example, what they commit one to has spawned a vast literature, with which I propose to engage only so far as is necessary to understand my eventual view<sup>1</sup>.

Before looking at responses to this problem, I want to consider a closely related problem, concerning change in intrinsic properties. While lying in bed my body was straight. Now, sitting in a chair, it's bent at the legs. How can it be that one and the same object, namely my body, possesses the incompatible properties of *being bent* and *being straight*? Call this the property problem. It seems that any decent theory of persistence should also be able to give an account of the property problem. Indeed, if we follow Lewis in holding 'the problem of persistence is the problem of change' (Lewis, 2002) then any decent theory of persistence *must* resolve the property problem.

Even if these are one and the same problem, I think things are helped, expositionally, by considering them as separate. Indeed, it strikes me that a reasonable way to view the debate between the perdurantists and the endurantists is precisely that they view what may be one and the same problem from these different angles. I'll accordingly consider them as separate. Let me first consider perdurance theory's answers.

### 3.3 Perdurance Theory

#### 3.3.1 The Persistence Problem

According to perdurance theory, an object persists through some times by having temporal parts at those times (the locus classicus here is ((Lewis, 1986), p204). To get a grip on the notion of temporal part, a helpful analogy is space: just as objects are spatially extended, and are so by having different spatial parts at different locations, so they are temporally extended, and are so by having different temporal parts at different times. Granting this, we can say that an object persists from time  $t$  to time  $t+n$  provided at each time it has a temporal part existing then, and we can follow Sider in defining a temporal part as so:

$x$  is a temporal part of  $y$  at  $t$  iff "(i)  $x$  exists at, but only at,  $t$ ; (ii)  $x$  is part of  $y$  at  $t$ ; (iii)  $x$  overlaps at  $t$  everything that is part of  $y$  at  $t$ ." (Sider, 2001,p60)

There are a battery of reasons familiar to the literature for adopting this theory of persistence. I don't intend to go through them all, mainly because I have nothing interesting to add to the debate, and it won't be necessary for what comes later in the thesis<sup>2</sup>.

That said, it will be helpful to review some of the arguments offered in its favour, as doing so will help get the view on the table. So let's consider perhaps the most famous argument for perdurantism in this section (in the next, we'll consider another): its account of problems of coincidence.

<sup>1</sup>Here, in particular, are some important issues I will not be discussing. Firstly, whether or not the debate is genuine or merely verbal. For an expression of the latter position see e.g. (Miller, 2005). Secondly, which further views one is, or is not, committed to by virtue of being a perdurantist (endurantist): for recent discussion of this matter, see Magidor, 2015. Thirdly, and somewhat relatedly, the metaphysics of time each view requires. A couple of references are: (Markosian, 1994), (Zimmerman, 1996). Fourthly, and still relatedly, I won't consider four dimensional views which do without temporal parts such as (Parsons, 2000): in my usage, four dimensionalism will be just the claim that there are temporal parts, and it is intended to be neutral between stage and worm theory.

<sup>2</sup>Some references: Lewis has a rather complicated argument in (Lewis, 1983); it is discussed in (Wasserman, Hawthorne, and Scala, 2004). Sider presents an argument from vagueness in the book already cited; discussion is, for example, (Koslicki, 2003). Sider provide a nice compendium and assessment of extant arguments in chapter four of his book.

There are a variety of forms which such problems, which concern what appear to be two material things which occupy the same space for part of their career, can take. The most famous example is from (Gibbard, 1975). Consider a lump of matter, Lump1, which exists for a while, before being formed into a statue, Goliath, which itself exists for a while, before being squashed. Goliath and Lump1 have different properties: Goliath has existed for less time than Lump1, for example. Or Lump1 could have survived squashing, while Goliath couldn't. Or, to consider an example that doesn't involve potentially problematic temporal or modal issues, Goliath is Rubenesque but Lump1 isn't (Fine, 2003). Given that they appear to have different properties, it appears they are different objects. But clearly they occupy exactly the same space. How can two different material objects occupy exactly the same space?

For the four dimensionalist, the argument goes, this is no problem. An analogy helps: two roads can overlap. For example Interstate 70 merges with Interstate 76 on the Pennsylvania turnpike. The same stretch of road is then in fact two roads: I70 and I76. Although they are different roads, as evidenced by the fact that they begin and end in different places, they physically overlap for a while, i.e. they share some spatial parts.

For the four dimensionalist, we have the same sort of thing, only temporally. The statue and the lump have some *temporal* parts in common: in particular, they have the parts from when the statue was formed to when it was destroyed in common. Despite this, they're not the same, just as the I70 and I76 aren't the same, because, for example, the lump is longer lasting than the statue.

There's plenty of controversial things about this argument. For one, we can note that things get troublesome when we consider cases of permanent coincidence (Gibbard, 1975): imagine a statue and a lump coming into being, and going out of being, at exactly the same time. We can't uphold the idea that we have two objects here by pointing out that they don't entirely overlap, because they do. But, one might think, if we have two objects in the original Lump1 and Goliath case, we have two here.

Some, such as Lewis, have appealed to counterpart theory here, saying that indeed the statue and the lump are one and the same, but the invocation of different counterpart relations can make sense of why a sentence such as 'the statue could not have survived squashing' is true while 'the lump could not have survived squashing' is false. The problem with this is that it can't easily extend to cases like Fine's Rubenesque: 'the statue is Rubenesque' doesn't appear to use any vocabulary that would invoke counterpart relations. Here an answer has been to just bite the bullet and say that the lump is indeed Rubenesque, albeit it's a misleading way of speaking in most cases (Frances, 2006).

For another issue with this argument, we can note that the endurance theorist has proposed alternative theories of what's going on which don't appeal to temporal parts. For example, Thompson (Thomson, 1998), among several others, defends the view that the statue and the lump are indeed different, so that there are two objects before one once the statue has been formed. And she appeals to the notion of constitution to make sense of this, and to allay any worries one may have about the two being unduly squashed together. The statue is constituted by the lump (but not vice versa), and this enables them to occupy the same space, but nevertheless constitution is a *bona fide* relation, so there are two things.

Here is not the place to evaluate all this. One might well be uncomfortable with the idea that there *are* two objects occupying the same space, as opposed to one shared part. On the other hand, one can note that this constitution idea works straightforwardly in the cases of permanent coincidence the perdurantist struggles with. In all, perhaps one



should say it's a dialectical tie. Let's move on and consider another reason for believing in perdurance theory. Doing so will enable us to present the perdurantists' answer to the property problem.

### 3.3.2 The Property Problem

Lewis famously argues that problem of change itself supports four dimensionalism: that is, he thinks that the perdurance theorists' answer to the property question is a good reason to believe in it. Let's see what that answer is.

The key idea behind the perdurantist's response to the property problem is that it's parts of objects that bear temporary intrinsics. For example, just as I have a spatial part where my heart is, and also another, larger one where my thorax is, so, for the perdurantist, I have a temporal part spanning from my first birthday to my second, and another larger one spanning from my birth to the day I got my tonsils out when I was eight.

Now consider a sometimes bent, sometimes straight stick. Assume it came in to being on Sunday and was bent all day, and then at the stroke of midnight became straight, and remained so til Tuesday. Then the stick has a temporal part spanning from its coming to being to Sunday night, and another from then til Tuesday. The first part bears the property of being bent simpliciter, while the second bears the property of being straight simpliciter. Thus—by having parts which possess them—can the stick itself, the whole thing with all its temporal parts, possess the seemingly incompatible properties of being bent and being straight simpliciter, *and* it can do so without making the assumption, beyond the pale for Lewis (as we will see), that properties are time relative.

This isn't hugely dialectically satisfying. As we will see, Lewis rests a lot of weight on the intuition that intrinsic properties just aren't relational. But here's another intuition, one might think: it's everyday objects like you or me that possess properties. So it's such everyday objects, if any, that possess intrinsic properties. But that's not so for him: everyday objects are those persisting things which have temporal parts—the mereological fusion of all of them. Such things are not plain straight, nor are they plain bent. Rather, the things which are plain straight or bent are parts of such objects. But it seems we've gone considerably past intuition in saying this (that a lot here seems just to turn on a clash of intuitions is noted by Sider, loc. cit. p96).

However, if it's not pretheoretically compelling, it doesn't mean that it's a bad picture overall. Indeed, it seems coherent, and even if it's not intuitive, arguably that doesn't matter: why should metaphysics be intuitive? Although Lewis seems to care a lot about intuition, that's no reason for us to do so. Let's now see how endurance theory can do.

## 3.4 Endurance Theory

It will be helpful to start with the endurantists' answer to the property problem.

### 3.4.1 The Property Problem

Recall the property problem: I was straight this morning and am bent now, but an object can't both be bent and straight. How can this be?

It seems clear that time is playing an important role here. One is tempted to say that there's no problem here because there's nothing inconsistent with an object's possessing a property now, and a contrary property at some time later. The rub, however, is explaining more precisely what is involved in saying this.

It turns out there are several different ways to do so. Perhaps the most obvious suggestion, and one put forward, if certainly not endorsed, by Lewis (Lewis, 1986, p204), would suggest that some properties which we thought were monadic were actually relational, and in particular, they involved a relation between a particular and a time.

So, in the case under question, I would possess the property straight-at-7am and the property of bent-at-2pm. But that would mean that what we think of as a monadic property, namely shape, is actually relational. In fact, it's worse than that: for one might think that shape is an *intrinsic property* of me, where an intrinsic property is – roughly – something the possession of which turns only on how things stand with me (for an overview of the literature on the idea of intrinsic properties, see Weatherson and Marshall, 2014). If the relationiser is right, then it seems as if it's not intrinsic, but relational<sup>3</sup>. For Lewis, this is 'simply incredible...If we know what shape is, we know that it is a property, not a relation' (Lewis, 1986, p204).

The problem with this move is that it really doesn't seem that incredible. If 'we' are normal people, it's arguable we don't have too settled opinions about what 'properties' are: they're not the sort of things we care about. I imagine we could elicit from the man in the street, with suitable prodding, the claim both that colours, for example, are monadic, or that they are relational. We might think that our knowledge (shared by the man in the street) that there must be *something* to do with time going on here trumps any conceptions we might have of monadicity.

A slightly better sounding line against this view comes from Lewis, 2002. There he argues that there are surely *some* intrinsic monadic properties, even if there are also temporally relational versions of them too (which he is always happy to acknowledge). The example he gives is of living three score and ten. It does seem like Socrates (let's assume, as may be correct, that he died at 70) possesses that property but that he doesn't possess the property of having lived 70 years at *t*, for any *t*. At the very least, it sounds rebarbative to say that Socrates lived for seventy years in 2014, or even in 399bc or in 430bc, in a way that it doesn't sound rebarbative to say the stick was bent in 2014 and straight in 2015.

However it's unclear that we should be overly nonplussed by this rebarbateness. One thing we can note, along with Wasserman ((Wasserman, 2003, who attributes the idea to John Hawthorne), is that the relationalist appears to have some way of cashing out this notion of monadicity (or simpliciterness, as Lewis uses in the just cited paper): we could say that an object is bent simpliciter provided it possesses the property of being bent at *t*, for all *t*. This would deal nicely with the other example Lewis gives of a surely monadic property, namely the bentness of a instantaneous stick. The thought is that surely we don't need relationalised properties here, and the momentary stick is simply bent simpliciter. But we can say this: it's bent simpliciter because for all times *t* it exists, it possesses the property of being bent at *t*. Accordingly, the relativised move seems in reasonably good shape, and I don't think we should discount it (for further discussion in this vein, see the Wasserman paper just mentioned).

A similar, but slightly different view, is the adverbialism defended by, inter alia, Haslanger and Lowe ((Haslanger, 1989), (Lowe, 1988)). Rather than relationising shape,

<sup>3</sup>This is perhaps a bad example; maybe on account of special relativity we shouldn't say shape is intrinsic, but rather relative to a reference frame. But there surely are some plausibly intrinsic properties in Lewis's sense, and since shape is discussed in the literature, we'll stick with it for expository convenience

and turning it into a relation to time, this move suggests that it's the instantiation relation which is time-relative. A standard idea is that the instantiation relation is two-place, relating an object and a property just in case the object has that property. Adverbialism suggests that it is, in fact, triadic, taking an object, a property, and a time to true provided the object possesses the property at the time. What's going on, when a banana is yellow on Tuesday the 19th of February 2016, is that it stands in the instantiation relation both to the property of being yellow and the time Tuesday the 19th of February 2016.

The virtue of this view is that it enables one to keep the Lewisian intuition to the effect that shape is a property, and not a relation, while making sense of the idea of change. For an object to change from being F to not so being is for there to be two times such that the object bears the instantiation relation relative to one to F, while it fails to bear the instantiation relation relative to the other to F. The important thing is that in each case it bears the relativised instantiation relation to F, not to F-at-t. It's thus that the monadicity is upheld.

It strikes me that these views more or less stand and fall together. If one is particularly moved by the thought that there is such a property as being bent simpliciter, one may well be equally moved by the thought that there's such a relation as having or instantiating dyadically, and that it's an incredible travesty to suggest that having is actually triadic. And if one isn't offended by triadic having, you probably shouldn't be offended by triadic bentness. I think one shouldn't be respectively moved or offended, so I think both the time relativisation strategy and the adverbial move are decent answers to the property problem. I will later briefly suggest that either can be built upon to provide a definition of endurantism that doesn't make use of the unclear notion of being wholly present. However, to make this point it's necessary to consider how the endurantist responds to the persistence question.

### 3.4.2 The Persistence Problem

Notoriously, people have worried about the definition of endurance. A natural enough preliminary thought is that, if perdurantism and endurantism are to be contradictory theories, and perdurance theory says that an object exists at a time merely by having a temporal part at that time, then endurance theory should deny this. And that is plausibly to say that if an object exists at a time, it exists completely at that time, or, in the more familiar idiom, it is completely or wholly present at that time.

A familiar worry with this is that it's obscure. This is nicely expressed by Sider (*loc. cit.*, p67). At first, he suggests trying to cash the notion of being wholly present in terms of mereology. The reasonable dual of the perdurantist claim that an object exists at a time by having *a* part at that time then becomes it exists by having *all* of its parts at that time. So we'd have: an object exists at t by virtue of all its parts existing at t. The problem with this is for the endurantist, the notion of x's having a part will be temporally relative: x's having a part at t. We then seem to get claims like that an object exists at t by virtue of all its parts at t existing at t. But that, as Sider notes, is a triviality which the perdurantist will be happy to accept.

So that can't be right, and Sider convincingly shows that other such formulations in the vicinity won't do the trick either. He then goes on to ask whether the endurantist should give up trying to give a mereological account, and seek some other way. But he quickly dismisses it, saying such views 'tend towards the obscure' (p68), and challenges the endurantist to give some other account.

While I don't intend to rise to the challenge, I would like to suggest that the endurantist shouldn't be overly disturbed about their dialectical position. Following Wasserman, 2016, we can suggest that endurantism is just the denial of perdurantism, and doesn't require a notion of 'being wholly present'. He suggests that we should perhaps look in other directions for a positive characterisation of endurance. He notes in passing, for example, that we could appeal to haecceities.

Let me just say a word about that. If we have haecceities to hand, we can say that an object endures through  $t_1, t_2 \dots t_n$  by virtue of its haecceity being instantiated at each of those times. I don't intend to defend this view in any depth. We may note in passing that provided we allow that something can instantiate more than one haecceity, which is non-standard but hardly absurd, we could give an account of statue and lump cases. In the permanent coincidence case, for example, we say that for each time when the statue's haecceity is instantiated the lump's is, and vice versa.

But what I would like to note is that arguably this is the *sort* of answer we should be looking for, given the nature of the puzzle. And it kind of makes sense of what is going on dialectically: the problem of persistence is really two closely related puzzles, or a problem with two different vantage points. You can start from the persistence vantage point, giving an answer to that problem, and then going on to the property vantage point. This is what Lewis seems to do. Or you can start from the property vantage point. If one does so, then it seems completely natural that one would try and answer the property question along the lines Wasserman indicates, that is to say in terms of resources available in the theory of properties.

Accordingly, I don't really think we should worry about the problems concerning the notion of being wholly present: if we want a proper definition, we should look elsewhere. But if we don't, that's fine: we have a decent enough grasp, I think, of what endurantism is to be even if we can't give necessary and sufficient conditions for an object to be an endurant.

### 3.5 Stage Theory

So far, we've been assuming it's a two horse race, between endurantism and perdurantism. But actually four dimensionalism subdivides into two different theories: the worm theory, which we've so far been considering, and the stage theory. The theory I'll eventually opt for is something in between worm and stage theory, so it is necessary to consider the latter (stage theory is defended by Sider in the book already cited and by Hawley, 2001).

The worm theory and stage theory differ as to what objects are. For the worm theory, objects, such as bananas and tennis balls, are temporally extended, four dimensional space-time worms. For stage theory, on the other hand, objects are the instantaneous temporal parts themselves.

Of course, this immediately raises our persistence problem: how do we make sense of the fact that objects persist through time? The stage theorist says, in a certain sense, they don't. Persistence through time is a question of having certain temporal counterparts.

Recall Lewis's counterpart theory of modality, first unveiled in (Lewis, 1968). For an object to be possibly F is for it to have a counterpart in a possible world that is F. For two objects to be counterparts is for them to be similar. Of course, there are a variety of different ways for two objects to be similar, so the counterpart relation is flexible:

in one conversational context, an object  $o$  in  $w'$  may be a counterpart of me, while in another, it may not.

Notoriously, of course, people have disagreed as to whether this is good enough to capture what we mean in using *de re* modal predications. Amidst much banging of feet and stamping of tables, Kripke (Kripke, 1980) said no, because the objects were strictly not the same, while Lewis thought this was no problem. At the very least there's a good sense in which an object's bearing a modal property turns on how some other object, elsewhere in modal space, is. The same thing goes for stage theory and time.

Famously, tense logic has been treated as a modal logic, which suggests the possibility of temporal counterpart relations. Just as I can be possibly sad by having a counterpart that is sad, so, the counterpart theorist says, I *was* sad by virtue of the stage currently picked out by 'I's having a counterpart at some past time that is sad. And more generally, we can say that an object persists through times  $t_1..t_n$  by virtue of having counterparts at each of those times. An object satisfies a tensed predicate in just the same way as an object satisfies a modal predicate..

We can extend this thought to cover cases in which it seems to be the case that for a predicate to be satisfied, a series of stages must possess a property. For example, an instantaneous temporal stage cannot eat a banana: that takes time. Similarly, an instantaneous stage can't put on weight. Thus consider:

(114) John put on weight

For the stage theorist, this will be true provided John stands in the counterpart relation to each of a connected series of stages  $t_1..t_n$  such that  $t_n$  is heavier than  $t_1$  (roughly; perhaps the satisfaction conditions of 'put on weight' are slightly different). The question of how to derive these truth conditions compositionally are not broached in extant stage theories; we'll later see it's not an easy task.

With this in place, we can put forward a positive argument for stage theory: it gets certain semantic facts concerning the crucial coincidence cases right in a way that the worm view doesn't. When presented with the clay formed into a statue, and asked how many objects there are before one, one will tend to answer 'one'. However, this is not what the worm view implies. For one is looking at a common part of two different mereological sums, one of which is the sum of all the clay stages, and the other of which is the sum of only the clay- formed-to-statue stages. So one should say "two".

However, the stage theorist doesn't say this. For statues and lumps are themselves simply stages. When one is presented with a formed statue, there is, by common consent, only one stage in front of one. So the stage theorist will answer one, which is, Sider says, the right answer. We can account for the fact that this one stage can be both statue and lump by appealing to temporal counterpart relations. In particular, consider:

(115) That lump will survive squashing

(116) That statue will not survive squashing.

For the stage theorist, the two complex demonstratives stand for the same thing. Yet both sentences can, as they should, come out true because they each make salient different counterpart relations: the former makes salient lump-y counterpart relations, while the second makes salient statue-y counterpart relations. So for it to be true, we look for future stages which are similar, as far as statues, to the current stage; if no such stage survives squashing, then the sentence is true. By contrast, for the first sentence, we look for things that stand in the lump-counterpart relation.

In this way, the stage theorist says, we can get the right result in the counting cases, while accounting for how, in a certain sense, there is both a statue and a lump before one: there are, so to speak, statue and lump counterparts before one, which determine statuesque and lumpesque histories for the object.

The stage theorist is not free, however, from counting problems. Consider the sentence: 'over a trillion human beings have existed'. This sentence is false; but if human beings are instantaneous stages, it comes out true. Sider's response is to hold that sometimes we count worms (loc. cit., p222). But needless to say this ambiguity is a weak spot for the stage theorist.

### 3.6 Conclusion

Each of these theories has something going for it, and none has come out the clear winner of this brief discussion. Let me end by saying what my view will end up looking like, some of which we are now in a position to understand. The theory I'll go for is recognisably four dimensional, but something of a hybrid of stage and worm theory.

With the stage theorist, I eschew fusions of stages (or at least their use in accounting for persistence). Rather, I hold that all there are are stages. On the other hand, with the worm theorist, I hold that the satisfaction of lingering and tensed predicates goes not by a single stage standing in a series of counterpart relations to other related stages. Rather, the satisfaction of such predicates involves some stages taken together. On my view, some stages satisfy predicates *collectively*: predication is irreducibly a plural phenomenon. And I can now present one reason for thinking this: contemporary semantics gives good reason to think that names are predicates, and predicates have extensions, i.e. stand –typically–for more than one thing. The things they stand for, on my view, will be the stages. 'Joan', for example, once disambiguated, will stand for Joan's stages.

But there are other reasons. I think there are also technical semantic reasons for doubting both the viability of the stage and the worm theory's view of predication, which I will present in chapter four.

So I'm going to be resting quite a lot of weight on semantic considerations to draw metaphysical conclusions. One might worry about this, thinking that semantic considerations shouldn't bear on metaphysical theorising, and vice versa. The goal of the next chapter, accordingly, is to dispel this worry.



## Chapter 4

# On The Link Between Semantics and Metaphysics

### 4.1 Introduction

In the previous chapter we saw the basic space of metaphysical possibilities concerning persistence. In the introduction, I noted that I am going to be combining one view about persistence—namely the existence of stages—with a view about the semantics of names, predicativism, in the hope of producing a package that resolves some outstanding problems which both views face.

Not only that, but the problems for the metaphysics will themselves be, as we'll see in the next chapter, semantic problems. For me, then, semantic theorising is both guided by and guides metaphysical theorising (although of course I don't endorse the claim that it is guided solely by, or solely guides).

This idea is best brought out by an example. Consider, as is the purpose of this thesis, referring expressions. And consider the different metaphysics of objects we introduced in the last chapter. I'll argue that metaphysics should guide semantics in the following sense: if objects are stages, then the compositional semantic value of a referring expression should be a stage; while if they are endurants, it should be an endurant (or rather, perhaps, a constant function from worlds to such). Moreover, as we've seen, the worm theorist holds that properties are not time relative (at least intrinsic properties). So, as far as temporal relativity is concerned, predicates should be constant functions from times to extensions. On the other hand, the endurantist typically holds that properties are temporally relativised, and so should say that predicates denote non-constant functions from times to extensions.

But semantics should also guide metaphysics, to some extent. If we think objects are stages, but we find that we can't embed the claim that the compositional semantic value of a referring expression is a stage (or a function from worlds to stage), then we should lower our credence in the proposition that objects are stages.

As I said, the following chapter will show that we can't readily embed the claim that the compositional semantic value of a name is a worm in our theory. Nor can we do so with stages. This chapter, however, has a different goal. It is to defend against the claim that this is the wrong way of doing semantics and/or of doing metaphysics. Semantic theorising and metaphysical theorising should be kept separate, one might think: the semanticist shouldn't care how an object persists through time, and the metaphysician shouldn't care about the compositional semantic value of referring expressions.

Ted Sider holds something like this. In his (Sider, 2011) he contemplates the possibility that mereological nihilism is true, and wonders what effect it should have on linguistic semantics. He notes that if it were so, then a sentence like 'there are hydrogen atoms' would be true just in case the following existential quantification were true:



- $\exists x.\exists y.(\text{Electron}(x) \ \& \ \text{Proton}(y) \ \& \ \text{Orbits}(x,y))$

However, he doesn't think that *this* should be the truth condition assigned by a compositional semantic theory. For he thinks

it would be inappropriate to require linguists to warp semantics around metaphysical scruples about molecules of helium, or tables or chair, or events.  
(Sider, 2011,p123)

Rather, he thinks that we can draw a distinction between different sorts of truth conditions: there's the linguists' truth conditions and the metaphysical truth conditions. The former can, but need not be, be homophonic: the linguists' truth condition for the above would be 'there exists a hydrogen atom'. But the linguists' truth conditions for 'necessarily, there exists a hydrogen atom' may be the non-homophonic 'for all possible worlds  $w$ , at  $w$  there exists a hydrogen atom'. Linguists are free to introduce seemingly metaphysical talk if it helps them to capture linguistic data; the thing to realise is that such talk isn't to be taken as telling us what *really* exists. That's the job of the metaphysical truth conditions, and is exhibited by the quantificational formula above.

In this chapter I want to argue that it's not inappropriate to require linguists to warp semantics around metaphysical scruples. Semantics should both, to some extent, guide metaphysics, and also metaphysics, to some extent, should guide semantics. I have an argument for each of these claims. Firstly, concerning semantics guiding metaphysics, I appeal to the practice of semantics: the goal of semantic theorising is to capture data, and sometimes it's necessary to make metaphysical posits to capture data. Think, for example, of the Davidsonian semantics for events, which remains popular to this day, which argues that their existence can best account for certain inference patterns involving adverbial modification. Or think again about situations, as used by people like (Heim, 1990) and (Elbourne, 2005) to give a syntactically sophisticated account of donkey anaphora. The point is that sometimes—indeed, frequently—there's data which call for a particular metaphysics. The point of the next chapter is, assuming four dimensionalism, to present some problematic data for worm and stage theories of objects. The eventual upshot will be that this data can be accounted for if you opt for my view, according to which names are predicates with stages in their extension, and what we think of as an object is just some stages taken together (as opposed to a fusion of stages, as the worm theorist says, and as opposed to a single stage, as the stage theorist says).

Now this first argument is nothing that Sider should have trouble with, as it doesn't involve any claim about fundamentality. Indeed, one might think that the situation semanticists—for example—are not doing metaphysics in *any* sense, but are rather making some sort of claim about modelling or useful fictions or something like that. And, in fact, I think for the purposes of this thesis, this somewhat weak interaction between semantics and metaphysics is enough for me: if the reader is so minded, she can read all talk of stages and such like in the same way as she reads talk of situations in situation semantics.

But, in fact, I think there's a stronger claim to be made. This concerns how metaphysics should guide semantics. To begin to see this, note that there's an important disanalogy between my use of metaphysics and some of the others mentioned above, namely the following: in a lot of these other cases, one came by the metaphysics from the semantics. Kripke noticed the duality between necessity and possibility like that between existential and universal quantification and posited something for the adverbs 'necessarily' and 'possibly' to quantify over; Davidson noticed that adjectives behave

like predicates and posited something for them to be predicates of; Heim and Elbourne noticed that conditionals seem to carry a quantificational force and, like Kripke, posited something for them to quantify over.

But in this thesis, to a large extent, we're going the other way round: we're starting with extant work in metaphysics, and trying to build our theory around that. Moreover, one might think this work, on the persistence debate, concerns fundamental ontology, and so we would be trying to warp our semantics around fundamental metaphysics. And one might find this objectionable even if one were happy about the other sort of interaction between metaphysics and semantics defended in the first argument.

However, I don't think it is objectionable, and the second argument aims to explain why. Basically the thought is that we should extend the doctrine of semantic externalism to metaphysics. Just as natural kind externalism tells us that what a term like 'gold' refers to depends on the nature of the shiny ductile metal in our environment, so metaphysical externalism says that what a name like 'Joan' refers to depends on the *metaphysical* nature of Joan, and in particular depends on whether she is—say—a stage or an endurant. I'll present reasons for thinking if one is externalist about natural kind facts, so one should be about metaphysical facts. The end result will be that, because of very general facts about metasemantics, our linguistic semantics, if it is to be accurate, must be guided by metaphysics.

The plan for the chapter is as follows: first I'll make the semantics guides metaphysics argument, then the metaphysics guides semantics one.

## 4.2 Semantics Guides Metaphysics

Let me begin the semantic argument by presenting some familiar instances where we use semantics to constrain ontology. A famous case is Davidson's event semantics (Davidson, 1967). Recall the basic line of thought here. Consider the following inference:

- John buttered the toast slowly, so John buttered the toast.

This seems to be an inference good in virtue of its form. But if we were to try and portray its form using the standard sort of translations into first order logic, we'd have to formalise 'buttered...slowly' as an atomic predicate, yielding:

- Buttered-slowly(John), so Buttered(John)

And that's not an inference good in virtue of its form. However, Davidson noticed that if instead we were to interpret the sentence as a quantifier over events, we could make sense of it. For then we could have something like:

- $\exists e. \text{Agent}(\text{John}, e) \ \& \ \text{Action}(\text{buttering}, e) \ \& \ \text{Way}(\text{slowly}, e)$ , therefore  $\exists e. \text{Agent}(\text{John}, e) \ \& \ \text{Action}(\text{buttering}, e)$

And this inference *is* good in virtue of form, in just the same way the inference from "Someone is happy and tall" to "Someone is happy" is. So if we assume the existence of events, in addition to individuals, then we can make sense of the inference, and that supplies us with a (defeasible) reason to believe in events.

Here's another example. Elbourne, leaning on work by Heim (both cited above), argues that we can best make sense of donkey anaphora if we introduce the idea of

situations. One can think of a situation as a part of a world. For example, part of the world is what's going on with me: it's a situation consisting of a man typing, with a grey Norwegian day outside the window.

Situations can help us with anaphora as follows. Without getting into the details (some of which we saw earlier, in the chapter on names) too much, we can note that there's some independent reason for thinking a sentence like:

(117) If a farmer owns a donkey, he beats it

Has an underlying syntactic logical form something like:

(118) If a farmer owns a donkey, the farmer beats the donkey.

Given this, we can make sense of what's going on if we say that the if-clause functions as a quantifier over situations, and in particular, it quantifies over situations in which there's a farmer who owns a donkey. The sentence is true provided every such situation can be extended to one in which the consequent holds. So, in this example, we take each antecedent situation, that is each situation consisting of a farmer who owns a donkey, and try to extend to one in which the farmer in the situation beats the donkey in the situation. If we can, then the sentence is true. The advantage of using situations is that they're small enough that we can secure uniqueness for the definites in the consequent relative to each situation we quantify over, something we couldn't get with worlds. Again, it seems reasonable to say that the fact that this semantics works, if it works, provides us with a defeasible reason to believe in the existence of situations.

They key thing to note here is that these sorts of arguments seem relatively uncontroversial and an established way of doing semantics. Provided a particular metaphysics explains some data better than another, and that metaphysics is not otherwise outlandish (for example: it's quite plausible that we see events and situations, talk about them using nominalisations, evoke them in physics, and so on), we should feel free to adopt it. Of course, what it means to 'adopt' it is a massive question, which could be taken in heavier or lighter senses. As I said above, for this argument I'm happy to accept the lightest possible sense: whatever you think semanticists are doing when they do this, that's what I'm doing.

I think to some extent saying that should be enough for the purposes of this thesis. If there is some data best explained by the positing of a certain metaphysics, then one has reason to adopt that metaphysics. I'll later argue that assuming the existence of temporal parts, one particular metaphysics of them best captures certain data.

However, the reader might have qualms. For she might think that I've already let metaphysics intrude too much even by thinking that a semantics should talk about temporal parts, even granting they exist. Should not semantics be neutral?

Sider, in the passage quoted in the introduction, certainly seems to think so. He continues to say:

The advantage of this approach is that it allows linguists..to be guided by concerns internal to linguistics (Sider, 2011,p123)

One might well agree that the existence or not of temporal parts is not a concern internal to linguistics. The next argument purports to show that it is. The reason for this is that semantic externalism is a concern internal to linguistics and if semantic externalism is true, then if stage theory is true, referring expressions stand for stages, while if endurantism is true, they stand for enduring objects. And this second conditional has

consequences for formal semantics. The goal of the next section is to argue for the truth of the whole conditional. However, prior to doing so, I need to review some (familiar) material concerning externalism.

### 4.3 Metaphysics Guides Semantics

#### 4.3.1 Kripke, Putnam, Burge

In *Naming and Necessity*, having disposed of Frege-Russell descriptivism about names, Kripke needed something to stand in its stead. In particular, he needed to answer the question of *how it is that names refer to what they refer to*. The Frege-Russell view is simple here: a name refers to whatever satisfies its associated description. But that leads to familiar mispredictions. If the associated description is the meaning of the name, then, were, say, the description associated with 'Alexander the Great' *the teacher of Aristotle*, the following would be false, or at least has a false reading:

(119) Aristotle might have never got into teaching.

Even if the description just determined the referent, we would still get bad results. Imagine Gödel hadn't discovered incompleteness, but Schmidt had, and imagine the description associated with 'Gödel' is *the discoverer of incompleteness*. Then 'Gödel' would refer to Schmidt, which seems like the wrong result (though see below for dissent.).

As an alternative, Kripke famously proposes a picture about how our naming practices work:

Someone, let's say, a baby, is born; his parents call him by a certain name. They talk about him to their friends, Other people meet him. Through various sorts of talk the name is spread from link to link as if by a chain. A speaker who is on the far end of this chain, who has heard about, say Richard Feynman, in the market place or elsewhere, may be referring to Richard Feynman even though he can't remember from whom he first heard of Feynman or from whom he ever heard of Feynman. (Kripke (1980, p. 91))

He then goes on to make some necessary conditions a bit more explicit: an initial *baptism* takes place (which could be by ostension or by description), and then the name 'is passed from link to link', where a necessary condition of this passing is that the hearer and subsequent user intends to use the name with the same reference as the person he picked it up from: if I tell you about Napoleon, and on that basis you go on to refer to your pet aardvark with that name, you don't get initiated in the usage (Kripke, 1980, p96).

To spell that out a bit more, think about the circumstances that attended my friend Kitty being named: her parents said, at some point, 'let's call her Kitty' (or something like that), and thus baptised her. Then they told their best friends, the X's who straight-away could start asking 'how's Kitty today?', clearly using the name to refer to Kitty. These friends could go on to mention my friend's parents had a child called Kitty to others (the Y's), and then *they* in turn, could start using 'Kitty' to refer to Kitty. What enables these Y's to refer to Kitty is that they're connected, in the right way, to her: they learnt her name from someone who learnt her name from someone who bestowed her name upon her. But this is not only an epistemic point about learning, but also a semantic point: the Y's utterance of 'Kitty' *means* Kitty because they became competent

in the word's use from someone who was connected to the bestowal of the name in the baptism.

We can see why, then, that it doesn't matter whether it was Gödel or Schmidt who discovered incompleteness, since what determines whether a given speaker refers to Gödel is not her beliefs, but her standing in the appropriate chain of communication. Provided her use of 'Gödel' eventually leads back to Kurt, as it presumably does, she refers to Gödel, regardless of what he did (or didn't do). This is externalism about the content of names.

Next, let's turn to Putnam (Putnam, 1975), who proposes externalism about natural kind terms. His argument goes by his famous twin earth thought experiment. In our world, the clear potable liquid in streams and the like is H<sub>2</sub>O. He asks us to imagine a world in which the clear, potable liquid were instead some other chemical element, say, XYZ. Let's call that world Twin Earth. Apart from that difference, Twin Earth and Earth are the same in every other respect: in particular, there's an identical copy of me now writing this chapter on Twin Earth. Now consider the sentence:

(120) Water is H<sub>2</sub>O

If I utter that sentence, then I have said something true; on the other hand, if my twin, TwinMatt, utters the sentence, he utters something false. For TwinMatt, 'water' refers to XYZ, not to H<sub>2</sub>O. Of course, I know, and thus my twin knows, the relevant factors about chemistry, and one might think that this prevents us from being identical copies. But we can easily get around this by changing the thought experiment. Imagine going back to some time prior to 1750 when the relevant chemical factors were not known, and consider two identical speakers, one on earth and the other on twin earth. Even then, Putnam says, they 'understood the term "water" differently in 1750 *although they were in the same psychological state*' (Putnam (1975, p. 141)). If the reference of an expression were determined by factors internal to the speaker, then two duplicates would refer to the same thing. But the preceding thought experiment is meant to establish this as false.

Finally, let's turn to Burge's famous discussion (Burge, 1979). He extends the Putnam line of argument even further. Consider Alf, who has a sore thigh, and falsely believes that arthritis occurs in the thigh. When he utters

(121) Arthritis occurs in the thigh

He says something false. But imagine a world in which doctors used 'arthritis' to refer to a different ailment, say tharthritus, which is a condition marked by pain in the thigh or joints. Moreover, imagine there were an identical ailing Alf in that world, who utters that sentence. Then, Burge says, he says something true, and refers to tharthritus in using 'arthritis'. But again, Alf and Twin Alf are internally identical: the both associate with 'arthritis' something like *the ailment causing pain in the thighs and joints*. If reference were determined by description, 'arthritis' would mean the same in Alf and in Twin Alf's mouths. The key point here is that a word like 'arthritis' expresses a *deferential* concept: what it means depends on what the relevant experts say it means (Burge thinks externalism extends beyond such concepts, but I won't consider that here, as taking us too far off track). And so if one varies what the experts mean, without making any internal changes to a given language user, what that average user like Alf will mean will change too.

These arguments, of course, are not uncontroversial. It would take us too far afield to consider and respond to the various objections and replies one finds in the literature<sup>1</sup>. If you're already an externalist, then I'll urge below that you accept externalism about metaphysical matters. If you're not, I urge that you accept the conditional that if one is an externalist, then one should be an externalist about metaphysical matters.

#### 4.4 Extending Externalism To Metaphysics

Here I want to argue that concerns about metaphysics are internal to linguistics, because different metaphysics lead to different views concerning the embedding behaviour of referring expressions under temporal expressions, and the study of embedding behaviour is a fundamental part of formal semantics<sup>2</sup>. Assume stage theory. Then my claim is that the semantic content of a name is a modally constant function from worlds to the stage currently picked out by a name. A consequence of this is that, relative to a non-actual world in which endurance theory is true (and in addition there are no temporal parts), 'Joan' doesn't pick out anything.

This, I fully realise, can be challenged. One might think that 'Joan' relative to this world picks out the stage; relative to the endurance world it picks out the endurant, and so on. But I don't think this is ultimately a happy position, because it doesn't make sense of externalism. Externalism about natural kinds tells us that there are chemical facts out there, facts about, say, the nature or essence of the clear potable liquid around us. And it says that that's what our semantics latches on to. But then there are metaphysical facts out there, so why shouldn't externalism say that they are what our semantics latches on to?

Firstly, we can note that from the point of view of language users, there doesn't seem to be any great difference between chemistry and metaphysics. The average speaker knows about as little, and cares about as little, about the chemistry facts and the metaphysical facts (admittedly, we might say the patient cares rather a great deal about arthritis, but really it's generally what's hurting him that he cares about; moreover, sometimes we do care about metaphysical matters, as the vacillating believer does about the existence of God). Moreover, if we go back in time to prior to the discovery of organic chemistry, just as the speaker's dispositions to behave will be the same on Twin Earth world, so, assuming just for the sake of argument stage theory is true, they will be the same in an endurantist world.

Secondly, I take it it's not too outlandish to say that metaphysics and chemistry lie on a continuum of investigation. In particular, I take it that most people who take analytic metaphysics seriously will assume that there is some fact of the matter about

<sup>1</sup>For some pertinent references: counterexamples are suggested by Evans's Madagascar case (Evans, 1973), and slightly more recondite ones to natural kind terms by Unger (Unger, 1984). Some experimental philosophers (e.g. Machery et al., 2004) claim to have shown judgements concerning these thought experiments are culturally relative; for some dissenting discussion see (Ichikawa, Maitra, and Weatherson, 2012). Other responses involve beefing up one's ontology to recognise, alongside H<sub>2</sub>O and XYZ, substances whose chemical composition is modally variable, being H<sub>2</sub>O on earth and XYZ on Twin Earth, or again to recognise diseases such as tharthritis in addition to arthritis (Unger, 1984, Crane, 1991). And yet others are somewhat concessionary, trying to uphold the central externalist idea without giving up completely on internalism: among these I would count causal descriptivism (defended inter alia in (Lewis, 1997) and (Kroon, 1987), as well as two dimensional semantics (see, e.g. (Chalmers, 2006).

<sup>2</sup>Examples could be presented ad nauseam, but think of: Tarski's semantics for the behaviour of variables under co-indexed quantifiers, the realisation that indefinites exhibited odd behaviour in scope islands, the Frege-Geach problem, the problem of de re attitude reports, the behaviour of epistemic modals under 'suppose' etc.

whether or not objects are endurants or are stages, just as everybody will take it seriously that there's some fact underlying whether a particular sample of a shiny metal is actually gold, or fake gold (not everyone, of course: some may take metaphysics seriously yet still think the debate concerning persistence is merely verbal). Given that we've recognised our semantics is sensitive to these latter facts, we should say the same about the former facts. In short, from the perspective both of the speakers of language (they are ignorant of chemistry and metaphysics) and of the world (it contains chemical and metaphysical facts), chemistry and metaphysics are on a par. Since we accept externalism concerning the latter, there is no reason not to accept to concerning the former.

Moreover, it's important to realise that this has important semantic consequences: it's not something which semanticists can just safely ignore. In particular, given externalism about metaphysics facts, which metaphysics is correct has consequences for whether names are temporally rigid (Varzi, 2003). For we can note that stage theory would seem to imply that names *aren't* temporally rigid. In particular, it's sensible to say that a name like 'Barack Obama', relative to now, refers to Barack's current stage, while relative to six minutes ago, it referred to the stage that existed then. Since stages are instantaneous, these must be different stages, and so we have temporal non-rigidity. By contrast, if names are endurants, we can easily say that 'Barack Obama' refers to the same thing at both times.

And this has important technical semantic consequences. As we'll saw, one of the ways for classifying determiner phrases as referring or not is whether or not they are rigid. In particular, it's often been taken to be an important distinction between names and definite descriptions that the former are *de jure* rigid, while the latter are not. But if stage theory is true, then this is not so. A semanticist who believes in externalism, then, would be well advised to keep an eye on how the persistence debate turns out.

## 4.5 Conclusion

I've argued that semantics guides metaphysics, and vice versa. Both of these claims will be used going forward. Assuming the truth of four dimensionalism, that metaphysics guides semantics means that our language must talk about temporal parts, because temporal parts are what's out there, and our language talks about what's out there. That semantics guides metaphysics will be used to decide between different views about temporal parts: in the next chapter, I'll make the paradigm move of showing that there's certain data which can't be accommodated under extant four dimensionalisms, in the same way Davidson showed there's data which can't be accommodated under extant semantics of adverbs. Then later I'll show that a new sort of four dimensionalism can account for this data, and thus that we are given some reason to believe in it.

## Chapter 5

# Problems For Stage and Worm Theory

### 5.1 Introduction

We can quite naturally understand both stage and worm theory as making certain semantic claims. The stage theorist says that a referring expression such as a name stands, at a time, for the stage which exists at that time. For example, an utterance of 'Obama' at  $t_1$  stands for the Obama stage which exists at  $t_1$ . The worm theorist, by contrast, since she thinks that Obama is a worm, should say that 'Obama' always stands for that worm. In this chapter, I'll show that neither of these approaches are tenable: once one tries to develop either idea into a workable semantic theory, problems soon arise. The next chapter will present a semantics that takes four dimensionalism seriously and doesn't fall prey to these problems.

### 5.2 The Problem For Stage Theory

Existing defenses of stage theory by Katherine Hawley and Ted Sider quite explicitly include certain semantic claims. Thus Sider says that stages are

the referents of ordinary terms, members of ordinary domains of quantification, subjects of ordinary predications, and so on. (Sider (2001, p60))

In the same vein, Hawley says:

The two accounts [perdurantism and stage theory] differ over what we talk about when we use phrases like 'the tennis ball', and about which objects satisfy sortal predicates like 'is a tennis ball'... according to stage theory, it is stages themselves which are tennis balls (or bananas, or human beings, as the case may be).(Hawley (2001, p42))

Not only that, both agree, in broad outline, about the stage-theoretic truth conditions of sentences. A sentence like:

(122) John was happy

Will be true for the stage theorist provided there's some stage which is suitably related to the stage picked out, at the time of utterance, by 'John', such that it is prior to the time of utterance, and it possesses the property of being happy (Sider (2001, p193), Hawley (2001, p54-57)). About the nature of suitable relatedness Sider and Hawley disagree; it is unimportant for this chapter.



My aim here is to examine the cogency of these claims, in light of what our best theories of the compositional semantics of natural language, and in particular tense, tell us. Having briefly reviewed the theory of tense, I will present a very simple sentence which I claim the stage theorist will not be able to deal with and show that this failure arises from a deep mismatch between what the stage theorists need of a semantics of tense and what natural language supplies. The conclusion will be that there's good reason to doubt that the stage theorist will be able to make good on their semantic claims, and thus, to the extent that these claims are an important part of stage theory, good reason to think stage theory false.

### 5.2.1 The Semantics of Tense

Initially, things look good for the stage theorist. I'll begin by showing how a simple modification of perhaps the most plausible current theory of the semantics of tense can capture a sentence like 122 from a stage-theoretic perspective. The theory of tense I adopt is the quantificational one, according to which expressions which carry tense features, like the past tense ending '-ed' or 'was' function as quantifiers over times, and temporally sensitive expressions are accompanied in the syntax by variables which can be bound by these quantifiers. This theory has empirical advantages over the more philosophically well-known operator theory, although it is also familiar that with sufficient complexification of the latter theory the two can capture the same data (for some discussion, the former more philosophical and the latter more empirical, see: (King, 2003), (Kusumoto, 2005)).

On the quantificational view, the truth conditions for 122 will look like this (I use the notation of first order logic for familiarity):<sup>1</sup>

- $\exists t < u. \text{Happy}(t, \text{John})$ .

The quantifier here ranges over times, binds the temporal variable  $t$ , and has its domain restricted to those times prior to the time of utterance,  $u$ . 'Happy' denotes a relation mapping a time and an object to true provided the object is one of the happy things at that time. If  $g$  is the variable assignment with respect to which the whole sentence is evaluated, the whole will be true provided the open formula 'Happy( $t$ , John)' is true with respect to an assignment variant  $g'$  differing from  $g$  at most in that it assigns a time prior to  $u$  to  $t$ . The end result is that the sentence will be true provided there's a time prior to the utterance such that the John is one of the happy things at that time, and that's just to say provided John was happy.

Now let's see how to modify this in the spirit of the stage theorist. The key thought behind stage theory is that a name like 'John' denotes different stages at different times. On the other hand, the stage theorist doesn't say that predicates like 'happy' have different extensions over time: they don't need to, and can just stand for all the happy stages, past, present and future. So an obvious thought is to say that it's names which,

<sup>1</sup>To get a compositional, categorematic treatment of the sentence, we assume a logical form roughly like so:  $[_{tp} \text{PAST} [\lambda 1. [_{vp} [_v \text{Happy}(t1)] [_{dp} \text{John}]]]]$ , where  $t_1$  is a trace, a syntactic object which behaves like a variable, standing for something (in this case a time) only relative to an assignment function, the lambda binder  $\lambda 1$  turns the sentence it scopes over from something which has a truth value only relative to an assignment function (whose values are times) into a function from times to truth values, and PAST stands for an existential quantifier over times, which is to say, in generalised quantifier-ese, a function from properties of times to truth values defined as so  $\lambda P <_{m, t} >$ . There's a time  $m$  earlier than  $u$  such that  $P(m) = \text{true}$ . Applying the composition rules of Heim and Kratzer, 1998 to the above sentence will yield the correct truth conditions, although I won't show that here. With this said, I'll continue to use first order logic in the body of the logic, to aid readability.

contrary to appearances, have a place for a variable: they denote, not objects, but *functions* from times to stages. We'll have:

- $\exists t < u. \text{Happy}(\text{John}(t))$ .

Our sentence will be true provided there's some time prior to the time of utterance, such that applying it to the function denoted by 'John' gives a stage which belongs in the set denoted by 'happy'. And this captures the correct truth conditions: the function applied to a past time will yield a past stage of John as is required, and the sentence will be true if that stage possesses the property of happiness.

The stage theorist can appeal to a simple and well motivated modification of the semantics of tense to get the right results for basic tensed sentences. Next we'll see, however, that this modification can't account for even slightly more difficult sentences.

## 5.2.2 A Problem

Consider the following sentence:

(123) Kant was born and died in Koenigsburg

The first observation to make about this sentence is that the two verbs, roughly, talk about different times: we're not asserting that there's some time such that Kant was born and died then. Rather, what we're saying is that there's some time in the past when we was born, and some other time in the past when he died.

In stage-theory talk, that is to say that there's some past stage of Kant which was born, and some different past stage of Kant which died. So the sentence is really talking about two past stages. My claim is that the stage theorist can't account for this, and for a very simple reason: there's only one occurrence of 'Kant' in the sentence, so the sentence can only talk about one stage. Slightly more technically, because there's only one occurrence of 'Kant', there's only one unbound temporal variable in the sentence, which can only be bound by one of the two past tense quantifiers. But that means the one which doesn't do any binding will be semantically inert, and so the sentence will not be able to talk, as it needs to, about two different Kant stages.

And this reflects a fundamental way in which the truth conditions demanded by the metaphysics of stage theory doesn't fit with the semantics of natural language. For the stage theorist, the semantic point of tensed elements is to allow us to talk about past and future stages, and not, as common sense might first suggest, past and future events or states. But this is just not reflected in the syntax, in which tensed elements form syntactic constituents with adjectives and verbs, as in the adjectival phrase 'was happy' or the verb phrase 'lived', and do not form syntactic constituents with names (there's no such word as 'Kanted'). A consequence of this is that every finite verb must co-occur with some tense expression, and so when we conjoin two finite verbs, we must have two tensed elements. And this is where the problem arises, because there may be only one name for it to semantically modify.

Let me first note that there's no problem for the (unmodified) theory of tense I've given. We'll have the syntactic form, omitting brackets and labels to aid parsing:

- PAST  $\lambda 1$ .PAST  $\lambda 2[[_{dp} \text{John}][_{vp} \text{Live}(t1) \text{ and } \text{Die}(t2)]]$

But now consider the stage theorist. The logical form must be something like:

- PAST  $\lambda 1$ .PAST  $\lambda 2[[_{dp} \text{John}(?)][_{vp} \text{Live and Die}]$

The question mark must be replaced with a trace (either  $t_1$  or  $t_2$ ), but it's clear that whatever quantifier we index it to, we'll get the wrong result. Neither of the two following truth conditions (assuming our first order language has been supplemented with a predicate conjunction operator):

- $\exists t_1, t_2 < u. \text{Born-and-Died}(\text{Kant}(t_1))$
- $\exists t_1, t_2 < u. \text{Born-and-Died}(\text{Kant}(t_2))$

Will do: they both are true just in case the same stage of Kant both possesses the property of being born in Koenigsburg and of dying in Koenigsburg. This is not the result we were looking for.

### 5.2.3 An Attempted Solution

The problem arises because we don't have enough names to go around: we need two, which can be saturated by two different temporal variables, which can then each be bound by a quantifier. A natural thought is that we could resolve this issue by positing that there was another referring expression in the syntax. Modern syntactic theory posits a range of such silent expressions, so this isn't an outlandish idea. The most natural thought is that it's a *trace*, a variable-like element which could semantically depend on the name, and in particular which could inherit the function the name denotes. That is, we could posit a syntactic form like so:

- PAST  $\lambda t_1$  PAST  $\lambda t_2$  [[Kant( $t_1$ ) be born] and [ $t_2$ ) die in Koenigsburg]]

where the trace  $t$  anaphorically refers back to the function denoted by 'Kant'. This would yield truth conditions equivalent to:

- $\exists t_1, t_2 < u. \text{Born}(\text{Kant}(t_1)) \ \& \ \text{Died}(\text{Kant}(t_2))$

Which would secure the correct result. But there are solid syntactic and semantic reasons to doubt that this approach can work. From the point of view of syntax, it's a familiar point that one can conjoin expressions of any syntactic category, as the following list indicates for names, adjectives, auxiliary verbs, and adjectives:

- (124) Kant and Hegel were philosophers
- (125) The table was heavy and blue
- (126) You can and must do it.
- (127) He danced skillfully and gracefully

There is thus good reason to take the surface form of 123 seriously and hold that it is indeed a conjunction of tensed verbs. It would be surprising if one could not conjoin them, as the stage theorist seems to demand. In the same vein, it seems that we must hold this to be a case of verb conjunction to make sense of cases like:

- (128) Kant caught and ate a deer

The reason there is pressure to hold that we have a conjunctive verb phrase rather than a sentential conjunction is that both the verbs 'caught' and 'ate' are intransitive and thus look for an object. But the object of 'caught', on this view, would be in a completely different sentence, and so the sentence should be as anomalous as:

(129) #Kant caught. He ate a deer.

But it isn't. Before going on, let me ward off an objection. The above sentence is bad because the syntactic object of 'caught' is not close enough for its denotation to semantically interact with 'caught''s denotation. But it's familiar to syntacticians that there are constructions like this but which are nevertheless acceptable: this is the phenomenon of *right node raising* introduced into the literature by Postal, 1974, and exemplified by:

(130) John hates, but Mary loves, model theoretic semantics

We here have an intransitive verb the object of which 'model theoretic semantics' is in the wrong place, but the sentence is ok. One might think that the fact that such constructions are ok gives the stage theorist an out: they could say that what's going on here is something like right-node raising. But this isn't hugely convincing: most notably, it doesn't *seem* like a case of right-node raising. Our example 128 doesn't have the commas or intonation pattern distinctive of such constructions.

But even if that were an acceptable response to this syntactic issue, there still remains a more serious, semantic problem with the stage theorist's position: the strategy invoked here will get quantificational cases wrong. To see this, consider:

(131) Everybody arrived and left safely.

In fact, to make things simpler, let's consider a tenseless example (or rather pretend that this is a tenseless example):

(132) Everybody grows and shrinks

On the view we're considering, this is equivalent to the following, where the subscripts indicate binding:

- Everybody<sub>1</sub> grows and t<sub>1</sub> shrinks

But there's a problem here. In order to get the reading which we want, it must be the case that the quantifier 'everybody' binds the trace element t<sub>1</sub>. But – at least according to orthodoxy – a universal quantifier in one sentence cannot bind a variable in another. The following is no good:

- #Every girl arrived. Then she left.

If the covert trace strategy were correct, then we would also predict 131 to be no good. Since we don't, that's a reason to doubt the covert trace strategy, and thus the viability of the stage theorist's response to our problem.

Here's a response to the worry I just raised. There *are* some purported examples of cross-sentential binding of a pronoun by a universal quantifier. Here's one, from Barbara Partee via (Roberts, 1987):

(133) Every player choses a pawn. He puts it on square one.

One might try to argue that since such sentences are ok, we shouldn't overly worry about the fact that we're forced to posit cross-sentential binding. My response to this is to say that I doubt such sentences *are* ok—certainly they're much more marked and infrequent than more the more mundane cases of binding. And I think that's sufficient to make it doubtful that the stage theorist can appeal to such cases to motivate her

view: if cross sentential binding of pronouns by universal quantifiers is infrequent and marked, then sentences like 131 should be infrequent and marked, but they are neither: they are both common and very natural.

Here's a different sort of objection which may have occurred to the reader. My problem, as explicitly stated, is a problem for the conjunction of the quantifier theory of tense and stage theory. But of course there is also the operator theory of tense. Although I think the trend in the literature has been to move away from it, it's hardly a dead option, and so one might think that aid will come to the stage theorist from that direction.

I very much struggle, however, to see how that is going to work. The problem, essentially, is that operators, as typically conceived, operate on sentences. And what we'd need is that there are two operators, to pick out the two different times. But then we'd need two sentences, but we've just seen above that the two sentence approach to predicate conjunction is a no-go.

In a bit more detail, we may note that we can get a rough translation of our sentence in using an operator approach, provided we help ourselves to Vlach's K operator (Vlach, 1988), which keeps track of the time introduced by an operator, enabling another operator (N), no matter where embedded, to refer to that time. Thus we could have, where 'P' stands for in the past, and F for in the future:

- P K Kant live and F N Kant die

This would get us that there's a past time when Kant lived, and in the future of that time, Kant dies. Which is roughly speaking correct. If we then made the claim that the intension of 'Kant' was a non-constant function from times to Kant stages, while the extensions of the predicates were temporally rigid, we would have the correct result. However, the problem with this is just what we've already seen: it makes of a predicate conjunction sentential conjunction, against the syntactic evidence. Accordingly, it seems that the operator account is not going to come to our aid here.

Let me end this section by briefly considering another objection. If we were to treat the predicate 'died' as denoting the property *has a past counterpart who dies*, and 'Kant' as always picking out the present counterpart of Kant, we would secure the right result for the case at hand: that Kant is such that there's a past counterpart of him that was born in Königsburg, and a past counterpart of him that died there, with no requirement that the times be the same. But I don't think this view generalises. Consider the following pair, when we've been talking about the year 1984.

(134) The president was a child then

(135) The president was Reagan then

These sentences have different readings: the first talks about the current president, and the second the president in 1984. The proposed solution can deal with the first reading. What about the second?

We can clearly get:

- The president has a past counterpart who was Reagan.

But that's false: Obama has no such counterpart. We need 'the president', somehow, to talk about Reagan. But then in such a sentence, we would need not to be looking at past counterparts of Obama, but instead past holders of the office of the president. Now we know how to do that: say that 'was' functions as a quantifier over times that

affects the interpretation of 'the president'. But on the view we are considering, that wouldn't be the function of the past tense morphology: its function would be to look to past counterparts, not past times. Accordingly, the view here would either postulate ambiguity in tense morphology, or would be unable to deal with all the readings it needs to, which I think is reason to reject the view.

The stage theorist, then, can't deal with our simple sentence. Of course, this has been a rather brief discussion, and one might think with sufficient ingenuity a stage theorist could come up with a semantics that would avoid this problem. But I think there's quite good reason to doubt this. For stage theory's inability to deal with our sentence derives from a deep and fundamental fact about language, namely, and as we've seen, that tense elements modify verbs and not determiner phrases. But the stage theorist is compelled to say that such elements semantically work on determiner phrases, to enable us to talk about past and future stages of objects. This mismatch between what stage theorists require of the semantics of tense, and what natural language actually delivers, renders stage theory vulnerable to problems like the one I've suggested, and supplies a good reason to disbelieve it.

### 5.3 The Problem For Worm Theory

One might think, given the above, that the tables have turned in favour of the worm theorist. This is not so, however, for worm theory fails quite quickly too, once one looks at the details.

The key features of worm theory are that referring expressions pick out worms, and worms possess properties by virtue of their parts possessing them. This motivates the following two examples of semantic values, where ' $<$ ' stands for the parthood relation:

- $\llbracket \text{Joan} \rrbracket = \text{Joan}$
- $\llbracket \text{laughs} \rrbracket = \lambda x. \exists y y < x \text{ and } y \text{ laughs.}$

The entry for names is simple: a name simply refers to the whole worm which the object is. The entry for predicates is slightly more involved: it takes a worm, and returns true provided that worm has a part which has the property in question. This existentiality in the semantic value, we will see, can be used to derive a problem for the worm theorist.

For tensed cases, it appears that a straightforward quantificational treatment will work, initially. We'll posit covert temporal variables, and amend the semantic value as so:

- $\llbracket \text{laugh} \rrbracket = \lambda t. \lambda x. \exists y y < x \text{ and } y \text{ exists at } t \text{ and } y \text{ laughs.}$

We can note that it will sail through our problematic sentence above:

- $\text{PAST}_{t_1} \text{ PAST}_{t_2} \text{ Kant lived}(t_1) \text{ and died}(t_2)$

For Kant to satisfy the first predicate is for him to have a part existing at  $t_1$  which possesses the property living, and for him to satisfy the second is for him to have a part existing at  $t_2$  which possesses the property dying. If we assume the domains of the quantifiers are restricted in such a way that the domain of the second quantifier contains only later moments than that of the first, we'll get the right result.

So things look good. But the worm theorist will fall to a problem almost the opposite of that which afflicts the stage theorist. Imagine we're talking about the night of Kant's death. Then the following is bad, and indeed necessarily bad:

(136) Kant was both alive and dead

And the reason it's necessarily bad is that we're attributing incompatible properties to one and the same thing. But its worm theoretic logical form will be:

- $PAST_{t1}$  Kant be both alive( $t1$ ) and dead( $t1$ )

And for it to be true there has to be a time in the salient past—the night of Kant's death—such that he satisfies both predicates. And to satisfy the first is to have a living part existing then, while to satisfy the second is to have a dead part existing then. But the following scenario verifies this: on the night of his death, Kant is alive from 7pm-11pm, and dead from 11.01pm to 12.00am. So the sentence, which sounds necessarily false, is possibly true for the worm theorist. And that's a misprediction. And it's almost a converse problem in the following sense: for the stage theorist, we have problems when we wish to speak about two different stages, but have only one name around to speak about them. By contrast, the worm theorist's problem is that sometimes sentences are bad because they attribute to one and the same thing inconsistent properties. But because the semantic value for predicates for the worm theory involves existentially quantifying over parts, we don't get this guarantee: one part can verify one of the predicates, while another can verify the other. One of the goals, then, of a four dimensional semantics will be to navigate the Scylla and Charybdis we've just seen.

## 5.4 Conclusion

So much the worse, one might think, for stage theory and for worm theory, and thus for four dimensionalism. Should we stick with three dimensionalism? I think that would be overly hasty. The next section, building on all that has come before, shows how to develop a new sort of four dimensional semantics.

## Chapter 6

# Stage Semantics

### 6.1 Introduction

Let's recap what we've seen so far. We have seen some good, if far from conclusive, reasons to opt for four dimensionalism, construed broadly in the sense that persistence is a matter of temporal parts, in the debate about the metaphysics of persistence. And we've also seen so good, if far from conclusive, reasons to opt for predicativism in the debate about the semantics of names.

However, we have seen that both of these theories face problems. The stage and worm theorist have problems with semantics, while the predicativist's postulation of a covert definite is empirically inadequate in several ways. My aim in this chapter is to solve these problems in one swoop by developing a predicativist semantics that doesn't require the positing of any covert article, and which takes seriously the existence of stages by holding that the extension of a disambiguated name is a predicate which has stages in its extension; 'Barack Obama' stands for all of Barack's stages.

The former part of the view requires a different understanding of the syntax/semantics interface, and I'll adopt a version of Pietroski's (Pietroski, 2005) conjunctivism, according to which a sentence like 'Joan swims' is a conjunction of predicates with variables bound by an obligatory covert sentence-level existential quantifier. However, this is not a *singular* quantifier. The reason for this is that many, indeed probably most, predicates are what Hawley calls 'lingering': they are satisfied not by a single stage, but by some stages taken together. For example, a single stage can't satisfy 'eat a banana': that takes the co-operation of a series of stages. To capture this, I will hold that the sentence-level existential is a *plural* quantifier.

Moreover, this in turn requires, or at least suggests, that to treat natural language quantified noun phrases like 'every book' will require something more than singular quantification, and indeed I will suggest that this is so: natural language quantification crucially involves higher levels of reference and quantification, of the sort called *superplural*.

There are several reasons why one should be interested in such a project; let me remind the reader of, or introduce them to, them. Firstly, as we have seen in chapter four, with regard to four dimensional, and two with regard to predicativism, existing theories have problems and I claim this view can solve them. Secondly, in the spirit of pluralism, one may as well let many semantic flowers bloom, and try to work out the details of non-standard formal semantics, to see what interesting properties they have. Thirdly, it's interesting to note that most semantic theorising in the tradition of quantified modal logic has a notably three dimensional flavour: objects exist at different times (and worlds), and predicates' extensions are time (and world) relative (Lewis's counterpart theoretic semantics is to some extent a counterexample to this: this is more clear with regard to worlds). It's thus an oversight that alternative systems haven't



been considered, especially given the importance the notion of rigidity has played in guiding our theorising about reference, and the different verdicts stage theory and endurantism give about temporal rigidity (as has already been seen in chapters two and four). Fourthly, if one bought the stuff about metaphysical externalism in chapter three, and one believes in four dimensionalism, then a four dimensional semantics is something one ought not only merely explore, but which one has to try to pursue, because if stages are in our environment, they must show up in the semantic facts.

So that's the rough shape of the view, and some motivation for exploring it. The plan for the chapter is as follows. In the next section, I'll expand a bit on the précis given above, introducing the idea of conjunctivist semantics, and showing the need for the default quantifier to be plural. Following that, I will review some reasons for thinking that plural and superplural reference are bona fide semantic phenomena. Along the way I will present a new argument defending the legitimacy of the latter. Having done so, I will be able to present my account of quantification and various other constructions. The section following that will present some interesting consequences of the view which we have landed on, and the also consider and reply to some objections.

## 6.2 Stage Semantics, The Basics

Names are predicates the extensions of which contain stages. For example, on one of its disambiguations, 'Obama' has as extension all the Barack stages. This is the first step towards taking four dimensionalism semantically seriously. Moreover, having bitten the bullet of ambiguity, I am free to respond to the problem earlier noted for non-Millian theories of names, namely that they can't account for the familiar reference-transmission facts (chapter two, section six), by taking over Kripke's causal picture of reference. Of course, it needs minor adjustments. In particular, I'll need to rethink the phenomenon of baptism: it's not the case that when a child is baptised, a name is bestowed upon just the current stage (resulting in a predicate with a singleton extension); rather, the baptism must somehow stretch into the future to comprehend (what we think of as; for more on this qualification see consequence three below) successive stages of the entity. This isn't overly worrying: the process of introducing a new predicate by ostending a satisfier of it, as when one discovers a new type of frog and says: 'that is a *rana exemplagratiensis*', already involves going beyond the particular one is faced with. I leave exactly how such predicative baptisms work for other work, however.

The next important step is to consider the problems which beset the extant versions of predicativism: that was, recall, that names not only don't always pattern with definite descriptions, but also they sometimes *do* pattern with indefinite descriptions. I suggested that moving to an ambiguity position here wasn't particularly attractive, and the response to this data which I want to consider is the somewhat extreme one of saying that when they occur in argument position, they are accompanied by *no* determiner.

That is to say, the underlying logical form of:

(137) Joan swam

Is not:

- [<sub>s</sub> [<sub>dp</sub> [<sub>d</sub> DET] [<sub>np</sub> Joan] ] [<sub>vp</sub> swam] ]

Where 'DET' stands for some determiner or other. Instead, it's (to a very rough first approximation):

- [<sub>np</sub> Joan] [<sub>vp</sub> swam]

How can this be? An intriguing clue comes, not from syntax, but from semantics. If we were to give a semantics for such a logical form, it would have to be, perforce, a conjunctivist semantics, not a function-application theoretic one. Because we'd be dealing with two syntactically concatenated expressions of type  $\langle e,t \rangle$ , we wouldn't have a function-argument structure. However, we could compose them by  $\langle e,t \rangle$  composition, yielding the complex property  $\lambda x. \text{Joan}(x) \text{ and } \text{swam}(x)$ . If we then assume a mandatory existential closure, we'd get the result that it's true provided there's *some* thing which is Joan and which swam. And that doesn't seem like a bad result, and can be modified to account for existence of stages, as I will shortly show.

But I'm being overly hasty. Let me take a step back to explain the basic idea behind conjunctivist semantic theories. Conjunctivism (Pietroski, 2005) is the view that syntactic concatenation expresses conjunction, and not function application.

Let's recall the distinction, as it would be treated in a standard textbook:

(138) Jan saw the Polish man

According to Heim and Kratzer (Heim and Kratzer, 1998), following the tradition initiated by Frege (see e.g. (Frege, 1951), (Frege, 1956)) and developed by Lewis (Lewis, 1970) and Montague (Montague, 1973), we should understand the meaning of the sentence to be built up by repeated applications of applying functions to arguments (which themselves may be functions), each of which are supplied as the semantic value of subsentential expressions. Firstly, we assume that 'Polish man' has as semantic value  $\lambda x. \text{Polish}(x) \ \& \ \text{Man}(x)$ . and apply it to the function denoted by 'the', which is a function that takes a property and returns the sole member of its associated set, provided it has a sole member, and returns nothing otherwise<sup>1</sup>. Then we supply the result of that application, an individual, to the function denoted by 'saw', which denotes a function taking an object to a function taking an object to a truth value. And then, in turn, we apply the output of that function to the denotation of 'Jan', which is simply an object, resulting in a sentence true iff Jan saw the Polish man. So we see that by successively applying functions to arguments, we eventually get from the meanings of subsentential expressions to that of sentence meanings (on the Fregean assumptions that such meanings are functions, objects, and truth values).

But of course there was a lacuna in my derivation: I assumed that 'Polish man' denoted a function, but I didn't tell you how to derive that meaning from its component parts. It's here where conjunction comes in. We can think of a function also as a set: thus we can think of the function 'man', which maps an object to true provided it's a man, as a set of ordered pairs containing an object and either true, if the object is a man, or false otherwise. We can then think of phrases like 'Polish man', where we conjoin two  $\langle e,t \rangle$  functions as the intersection of the sets associated with each  $\langle e,t \rangle$ : that is, all and only those ordered pairs that are in both the set associated with 'Polish' and that associated with 'man'. This is the set theoretic version of the function  $\lambda x. \text{Polish}(x) \ \& \ \text{Man}(x)$ , as was desired. We see here that in the case of adjectival modification, syntactic concatenation encodes something like conjunction: something satisfies 'Polish man'

<sup>1</sup>This is of course presupposing a Frege-Strawson view of definites, but merely here for expository purposes.

if it's both Polish and a man<sup>2</sup>. So it turns out it doesn't seem like we can say that concatenation always expresses function application.

This exposes a possibility for the rogue semanticist: how much can we do if we assume that concatenation never expresses function application, but always expresses conjunction? Are there any advantages to doing so?

The literature replies: a lot, and yes. Regarding the first, Pietroski (Pietroski, 2005) shows how one can account for a wide range of constructions, from simple sentences like the above to quantification and modality, in terms of a conjunctivist semantics. His theory, although I take inspiration from it in several ways, is complicated and would require somewhat extensive exposition, so let me present a very rough sketch of the sort of view he likes by considering an answer to our second question.

One advantage of a conjunctivist semantics is that it promises to make sense of certain inferences that traditional semantics doesn't, and in particular those that appear to involve *events* (Davidson, 1967). Davidson argued that we could best make sense of such inferences as:

- John buttered the toast slowly, so, John buttered the toast

If we assumed that the underlying logical form of our sentences were existential quantifications over events. That is, he proposed that the following was the form:

- $\exists e. \text{Actor}(\text{John}, e) \ \& \ \text{Action}(\text{Buttering}, e) \ \& \ \text{Patient}(\text{toast}, e) \ \& \ \text{Manner}(\text{slow}, e)$

We could then say that the goodness of the inference is just the purely formal goodness of any inference of the form  $\exists x.Fx$  and  $Gx$  to  $\exists x.Fx$ .

The key thing to note is that on this view, concatenation doesn't encode function application, but rather something in the vicinity of conjunction. Pietroski's view, though of necessity considerably more complicated, follows in the Davidsonian tradition.

Now the conjunctivist semantics I favour does not involve events<sup>3</sup>. Our first pass logical form was as so, where the text level quantifier is over objects:

- $\exists x. \text{John}(x) \ \& \ \text{swims}(x)$ .

Let me just note that it's not a strictly mandatory feature of my view that there be a covert existential quantifier, as there are, familiarly, different ways to encode tacit semantic information, and indeed tacit existential quantification. We could think of it, in the spirit of Perry (Perry, 1986) *inter alia*, as an unarticulated constituent. On another, we could think of it as part of the semantic interpretation procedure, in roughly the same way that DRT achieves a quantificational force for indefinites while treating them as simply predicates (Kamp, 1981). Or, we could simply say that it's an articulated, but covert component, which is what we did above, and shall do. One might fear, on this latter approach, that we're veering too close to the standard predicativism I argue was problematic. I can see why one might think this: I ask the reader to wait until the view is on the table, at which point it should be clear that it is markedly different.

Given this, then I think we have the cleanest form of predicates to date. Names are predicates. Punkt. There's an existential quantifier, sure, but it's not encoded in

<sup>2</sup>Notoriously this won't work for all cases of adjectival modification, like for example 'fake Rolex', but that's a complexity we can ignore.

<sup>3</sup>That's not to say I have anything against events, or even event semantics. It could well be that a completely adequate semantics of verbs requires event semantics. But I'm concentrating on giving a semantics of names, so that question is not one which I have the space (or indeed the knowledge) to consider.

a covert determiner. Now here's a question: can we get a stage semantics by simply replacing the objectual quantifier with a quantifier over stages? Using 's' as a variable for stages, does this following work?:

- $\exists s$ . John(s) and swims(s).

The answer is no. To see this, consider what Hawley calls a 'lingering' predicate. Here is a sentence containing a lingering predicate:

(139) John ate a banana

Single stages don't eat bananas: rather, it's something that requires the cooperation of a series of stages. So a mere existential quantification over stages won't get the right results: it's not the case that there's some John stage such that it ate a banana. The important move to make is to agree with this, and to hold that the existential quantification is not singular, but plural. That is to say, we have (ignoring tense, which we'll get to):

- $\exists ss$ . John(ss) & Ate-a-banana(ss)

What this says is that there are some stages and they *together* ate a banana. Importantly, this requires thinking of the predicate, and indeed all predicates, as non-distributive: for all F, F(ss) iff the ss's together F; F(ss) does not imply that for any s which is one of the ss's (henceforth notated:  $s \ll ss$ ), F(s).

Three features make up the core of my view: names are simply predicates, their extensions are stages, and free variables associated with the predicates are plural, and quantified over by a sentence level plural existential quantifier. Let's now consider some more complicated constructions.

In order for my view to be even a candidate, I must show that it can be extended to cover larger fragments of natural language. To do that completely, of course, would require a book in itself. My goal is more modest: I'll show, in some detail, how to get quantification to come out right, and in lesser detail, tense. I hope this will convince the reader that the view can be extended.

So let's begin to think about quantification. On the view I'm developing, as we've just seen, a sentence like 139 will express a plural existential quantification. A natural question to ask is what about English sentences that look like they're quantificational, as in, for example:

(140) Some woman laughed

Or, to make matters more tricky, sentences that are overtly plurally quantificational, as in the Geach-Kaplan sentence:

(141) Some critics admire only each other

I propose to begin the work of giving these semantics by considering singular quantification in detail (the further application to plural quantification shouldn't be too difficult, I think). The eventual response I will land on will make crucial use of *superplural reference*. Roughly, as a first pass, superplural reference is what stands to plural reference as plural reference stands to singular reference. In the next section I introduce and defend the legitimacy of superplural reference, and present a semantics for it to be found in the literature, and which I will take up, with certain tweaks, for my theory.

### 6.3 Interlude: On Plural Reference, and Superplural Reference

In order to get to my treatment of quantification I need to introduce some technical material concerning plural and superplural quantification. Along the way I will also present a new argument for the legitimacy of superplural quantification. The best way to begin to do this is to consider something I've already used, without much argument or explanation, namely plural quantification.

#### 6.3.1 Plural Reference and Quantification

Many think that in addition to singular reference and quantification there is another sort, plural reference and quantification. It's exhibited by plural definite descriptions, plural demonstrative and personal pronouns, and conjunctions of names on the referential side, and by quantified noun phrases with plural determiner phrases binding plural pronouns:

- (142) The boys came to dinner
- (143) They washed and dried the dishes afterwards
- (144) Those dishes and these ones, I think.
- (145) Some boys thought they could lift that piano but it was too heavy

It seems plausible that just as one can refer to a single boy with 'the boy', so one can refer to several boys with 'the boys'. In addition to reference to singular things, there is also a different type of reference, plural reference. Plural reference relates an expression (or perhaps sometimes a speaker or thinker) to several objects.

One particularly good reason for thinking this is the existence of collective predicates. Consider 'they lifted the piano'. One might think, on the most natural reading, one is ascribing something to some people: they *together* lifted the piano. Not all predicates are like this: 'they are English' doesn't mean they together are English but *each* of them is English. In the absence of collective predication, one might think, one could treat a purportedly referring expression like the boys as referring to *each* of the boys, and not to the boys together. But because we have such collective predicates, it seems we can't always say that.

This isn't completely uncontroversial. It's been a perennial temptation for philosophers and linguists who encounter such constructions to attempt to say that purported plural reference to several objects is actually just singular reference to a special type of object. There are two familiar views about this object. A famous paper by Link (Link, 1983) which is the starting point for many in the linguistics literature argues that plurals denote mereological fusions: plural objects which however also belong in the domain of individuals. On the other hand, philosophers' attention has focused on the idea that they denote sets (for an extensive list of people who hold either of these views, see Oliver and Smiley, 2013, p42). Against both these views there is, to my mind, quite decisive objections. A quick argument against the former option is that a sentence such as the following:

- (146) The cup and the bottle are one

Is false, yet without some further stipulation, would be predicted true (for a helpful discussion of responses and counterresponses to this move see Oliver and Smiley, 2013, p37ff).

A quick argument against the latter view comes from Boolos (Boolos, 1984) and turns on set theoretical paradoxes. We know that there is no set containing all sets. Now consider:

(147) The sets are abstract

If 'the sets' referred to a set, then it would refer to the set of (all) the sets. But there is no such thing, so the term would be empty and the sentence truthvalueless or false. But the sentence seems true. By contrast, if we say that 'the sets' refers plurally to the sets, we don't thereby commit ourselves to some collectivising set which has all the sets in it, and can avoid the problem. That seems like a good reason to posit a distinctive form of plural reference.

If one likes these arguments, one should accept that locutions such as:

- $\exists xx. \text{John}(xx) \ \& \ \text{Ate-a-banana}(xx)$

Which involve plural reference, are legitimate. However, that's not enough for my purposes. For consider a sentence like:

(148) Every woman swam

Intuitively speaking, 'Every woman' generalises about women. And it's natural to say that what 'Joan' denotes is a women, so it generalises about the sort of things names like 'Joan' denote. But 'Joan' denotes the Joan stages. And, for example, 'Mary' denotes the Mary stages. So 'every woman' will seem to have to generalise about—to speak in a way that will be clarified below—several different pluralities of stages. The next section is concerned with the legitimacy and nature of this sort of generalisation: of superplural reference and quantification.

### 6.3.2 Superplural Reference

So let's grant that there is plural reference. Is there anything else, something which stands to plural reference as singular reference stands to plural reference? One strategy to try to find such expressions is to look for collective predicates which seem to hold only of conjunctions of plurally referring expressions. Here is an example, inspired by Linnebo and Nicholas (Linnebo and Nicolas, 2008):

(149) Those people, the Smiths, and we are facing off in the three-legged race.

Consider the predicate 'are facing off in the three-legged race'. This is not a distributive plural predication. If it were, then we would get the result that it entails that 'those people are facing off in the three-legged race'. But that's not right: for those people to be facing off, they would have to be in competition, which they're not. Nor is it equivalent to any conjunction of distributive predications (even if it were, it would be baffling how, from the point of view of compositional semantics, such a conjunction could be derived from the sentence's parts and the way they are combined).

But now recall when we found a non-distributive singular (i.e. one that can take several singular arguments) predication, as in 'lifted the piano', then we were moved to posit plural reference. But now that we've found a non-distributive plural (one which can take several plural arguments) predication, we should make the same move and posit something above plural reference: superplural reference.

One might be tempted to say that in fact this is just an instance of straightforward plural reference: that each of the three conjoined determiner phrases stand for singular things, teams. There are two things to say here: firstly, as note Linnebo and Nicolas note, this just seems super implausible as a matter of natural language semantics. We've already granted that 'those people' sometimes refers plurally. The natural thing to say is that it always does. Evidence for this comes from sentences like:

- (150) Those people and Smith and Jones faced off in the race, then each went to their respective houses.

Here, the relevant information is that 'faced off in the race' is a non-distributive predicate, while 'each went to their respective houses' is distributive. The team analysis clearly can't work here: predicating 'each went to their respective houses' of a single entity, a team, makes no sense. To wheel out teams just when the going gets tough certainly seems like special pleading.

The second thing to say is that we can quite readily explain the thought that somehow teams are involved. In particular, we can note that it's perfectly natural to think of the duos as teams, in this context. But how one thinks of them is really of no *semantic* import. Consider an analogous case. One might think that when I utter:

- (151) Obama is commander in chief

I think of Obama as the president. But we obviously don't want to say that the semantic content of that occurrence of 'Obama' is the president: it's simply Obama<sup>4</sup>. In the same vein, we can say that perhaps our *grasp* of what we say, in these sentences, goes by means of collectivising sortals like team, but it doesn't by any means follow that what we actually say does.

Accordingly, there appear to be superplural constructions in natural language. That, one might think, is not particularly relevant to the current topic. After all, I'm going to argue that even expressions that don't *appear* to be superplurally referential are in fact so. But I think it does help some. For it shows that the human language faculty has the capacity to deal in superplural reference, so it shouldn't be too hard to take when I say that it does so in the unobvious ways which I will claim it does.

All that being said, however, I would like to briefly consider a new argument for the legitimacy of superplural reference that doesn't go by way of language, but by way of thought. However, it's somewhat of a distraction from the main thread of the argument, so if the reader is already on board with superplural reference, they might want to skip it.

### 6.3.3 Superplurality in thought

We have, plausibly, singular thoughts about objects: thoughts about particular objects. It also seems plausible that we have plural thoughts about objects. For example, my thought that those are differently sized, while attending to the phone and the one euro coin on my desk, is an example. If one likes the above examples given in the previous, then one can just take the thoughts they express to be examples of superplural thoughts.

But those examples are undoubtedly somewhat hard to analyse: they are sufficiently *recherché* that I wouldn't blame the reader if she lacked strong feelings one

<sup>4</sup>I'm obviously not speaking in my own voice here, but the same point applies to my semantics: the semantically encoded predicate is 'Obama(ss)' not 'President(ss)'.

way or another about them. So I'd like to present another argument in favour of the existence of superplural reference in thought, which turns on our capacity to refer to something despite being mistaken about it.

In particular, imagine the following scenario. We are faced with what looks like an alien: a mess of tentacles and such like. We think: ugh, that's gross, and go about our daily business. In fact, it's *two* creatures. My claim is that in such a scenario, one has plurally referred, even though one doesn't know that one has.

Here's the argument. It's widely held that knowing the sortal under which an object falls isn't necessary for being able to think about that object. That would place too many constraints on typical thinkers. It would be unfair, for example, to say that a child (or indeed an uninformed adult) can't think thoughts about the venus flytrap in their garden because they're uncertain whether it's a plant or a somewhat stationary animal, or perhaps a postmodern sculpture.

So one doesn't need to know a sortal. But knowing the sortal is to know the criterion for sameness and difference of two purportedly different objects. So it's not necessary to know that criterion for reference. But now the only reason I can think for saying that one hasn't managed plurally to refer, given the fact that one is faced with several things, is because one is mistaken about the number. But to be mistaken about the number is to be mistaken about the criterion of identity and difference of the objects before one. But that, I've just suggested, is not a mistake which makes any difference to one's referential capacities. So the thing to say is one has managed to plurally refer, despite one's thinking one is singularly referring, just as it's possible to refer to a plant despite thinking it's an animal.

But if this is so, then a very minor modification of the case will get us to one that involves superplural reference. One day, another similar looking mess of tentacles and such comes by, and comes to be located by the first mess. You perceptually judge that they're of unequal heights, thinking that there's two thing before you. In fact, there are four. It strikes me that if you're happy to say in the original case one has unbeknown plural reference, so in this one it should be unbeknown superplural reference. This is because in the first case, there is one case of unbeknown plural reference going on, but in the second, there is two cases.

One could also argue for this, although I grant it's controversial, from truth conditions. For one could argue that one can only get the right truth conditions provided the predicate has a superplural interpretation. In particular, it can't have a distributive reading: it's nonsense to say of each creature that it's unequal height. It also can't be a collective plural predication: it's false, plausibly, that the whole group are of unequal heights, since each pair, which one incorrectly thinks of as a single creature, is the same height. It seems that it should be a collective plural predication: one which takes several plurals as argument and holds of them, if it holds, together.

How might one reply to this? Here's one thought. One might think that what one should do is attribute to a speaker grasp of a non-standard concept, roughly like team. Let's call the actual creatures (of whom there are four in the example we are considering) *squirms*. And let's introduce the concept *squirmy* with the following stipulation: something is a squirmy iff it is two closely located squirms. We then attribute to the speaker the concept of squirmies, rather than squirms. In the original case, her judgement would be that that squirmy is gross; in the latter, that those squirmies are of unequal height. Plausibly these are both true judgements.

I think the reply to this is just a variation on an externalist theme we encountered in chapter three. People before the existence of chemistry believed that water is H<sub>2</sub>O, because water is H<sub>2</sub>O, and they believed water is water. What they referred to didn't



change when they became chemically enlightened. Arthritic Alf had false beliefs about arthritis, but he still picked out arthritis. When he comes to be corrected, he doesn't acquire a new concept. In the same way, once our thinker learns that there are two things before her, and thus that she's plurally referring, the content of her belief doesn't change. In each case, the content of our belief is just what's there. If we have to attribute to her the false belief that what is before her is one thing, then so be it: it is a false belief. We shouldn't change that by giving her deviant concepts. In short, I propose number externalism, in addition to my earlier posit of metaphysical externalism. And again, the underlying justification for this is just the same: that's how metasemantics works.

### 6.3.4 Semantics For Superplural Reference

In this section, I want to lay out Rayo's (Rayo, 2006) semantics for a language containing super and higher plural reference and quantification. The reason for this is that I will eventually borrow some ideas from it, turning it into a conjunctivist categorematic natural language semantics. But it will be nice to see the basic ideas in a slightly simpler format.

Rayo begins by noting the advantages of doing semantics without sets, such as, as we've seen above, the avoidance of set theoretical paradoxes, and proceeds to show how we give a semantics for a language containing plural and superplural expressions without having recourse to them. He begins with plural terms, like 'the elephants'. Rather than denoting the set of elephants, it refers to the elephants themselves. More formally, we have the following reference condition:

- $\exists xx.(\forall y \ll xx \text{ iff Elephant}(y) \ \& \ \text{Refers}('the \text{ elephants}', xx))$

Which he glosses:

there are some things—the xxs—such that: (a) for every y, y is one of the xxs iff y is an elephant, and (b) 'the elephants' refers to the xxs

He also holds, interestingly, that *predicates* function in the same way. For example, the predicate 'is an elephant' itself just stands for the elephants like the plural definite description, and not, as in standard theories, for a function or a set.

He then moves on to consider non-distributive predicates like 'are scattered' or 'surround the castle'. He suggests we can think of them as super-plurally referring, with the following reference condition:

- $\exists xxx.(\forall yy(yy \ll xxx \text{ iff Scattered}(yy)) \ \& \ \text{Refers}('are \text{ scattered}', xxx))$

Superplural variables are notated with *three* letters, while plural variables are notated with two, and singular with one. To understand what this means, Rayo says something worth quoting at length:

The reference of ". . . are scattered on the table", for example, is the super-plurality to which all and only pluralities scattered on the table belong. But it is important to be clear that apparently singular quantification over "super-pluralities" is a syntactic abbreviation for super-plural quantification over individuals. Super-plural quantification is not singular (first-order) quantification over a new kind of "item" ("super-plurality"), nor is it plural quantification over a new kind of "item" ("plurality"). *Super-plural quantification is a new kind of quantification altogether.* And like its singular and plural

counterparts, it is quantification over individuals, which are the only kind of "item" there is. I would like to insist that thinking of super-plural quantification as an iterated form of plural quantification—plural quantification over pluralities—would be a serious mistake. Plural quantification over pluralities can only make sense if pluralities are taken to be "items" of some kind or other. And a plurality is not an "item": apparently singular quantification over pluralities is a syntactic abbreviation for plural quantification over individuals. (Rayo, 2006, emphasis added)

I'd like to emphasise something about this. Rayo uses superplural reference to give an account of a somewhat tricky semantic phenomenon, namely non-distributive predication. There are other ways, of course, one can go. To see this, consider a case where we have a predicate that typically, but not always, gets a collective reading:

(152) The men lifted a car, then pulled it to the finish line (in a bid to be crowned world's strongest man).

Here the predicate 'lifted a car' is functioning distributively, although typically it is non-distributive. And we can capture this fact in several ways. For one, we can say that the predicate is ambiguous. It has, semantically, two distinct meanings, one mapping an object to a truth value, and the other mapping some objects to a truth value. Or, we can say that the sentence contains a covert distributivity marker: a hidden 'each' (or 'together', as the case may be) which takes one from a collective to a distributive predicate, or vice versa. Or, finally, one can say that the crucial difference is that there's two different types of reference which 'lifted three goodyear tires' can exhibit.

The point I want to make is that we should think of the different types of reference as another direction of freedom available to us, alongside the positing of more (and more complex) semantic values, or of hidden syntactic complexity, to account for various tricky semantic phenomenon. Another way to put this is that there are three places we can look when faced with a problematic sentence: to the sentence itself; to the world, or rather to the semantic values the world makes available; or to the speaker or thinker and their acts of reference. The first two options are well understood across a range of domains, such as, for example, quantifiers in object position, or donkey pronouns, or domain restriction. The latter is less so, and is the approach I (following Rayo) propose to take.

That said, let's return to our discussion of Rayo. Now he thinks there are no super-plurally referring subject terms in English (his paper predates Nicholas and Linnebo's), but if we grant he's wrong about this, we can give the reference condition for our 'Those people, the Smiths, and we' similar to the one we gave for 'are scattered'. For the sake of explicitness:

- $\exists xxx. (\forall yy (yy \ll xxx \text{ iff } \text{Those People}(yy) \text{ or } \text{The Smiths}(yy) \text{ or } \text{We}(yy)) \ \& \ \text{Refers}('Those \text{ people, the Smiths, and we'}, xxx))$

We can then suggest truth conditions for simple sentences that look as so:

- The marbles are scattered iff  $\ll$ the marbles $\gg \ll$ are scattered $\gg$

One final detail will be useful, as I think it helps bring out what exactly is going on when we appeal to these superlative plurals, and that's the account of intransitive verbs, like 'loves'. Although Rayo doesn't describe it in much detail, he notes two

ways to capture such things. Either one can say that it refers to a plurality of ordered pairs, or one can say that it superduperplurally refers. This is another *sui generis* type of reference. Unfortunately it's more or less impossible to express what it involves without nominalising devices like those Rayo mentions. But if we heed his warning not to take them ontologically seriously, we can say that 'loves' refers to a superduperplurality, consisting of all those superpluralities which consist of pluralities consisting of lover and lovee, and (assuming singles are limiting case of plurals) lover.

We're here making use of a technical trick of encoding the subject of the verb by having the subject occur twice: once in a plurality consisting of lover and lovee, and again in a limiting plurality consisting solely of the lover. Assuming Romeo loves Juliet, but not vice versa, and Charles loves Emma, but not vice versa, we could represent the semantic value as so:

| | |Romeo, Juliet| , |Romeo| | | |Charles, Emma| , |Charles| | |

Now the reader might start getting impatient at this stage. Even if you're happy with plurals, and maybe with superplurals, you might think that supersuperplurals are beyond the pale: *they*, one might think, have no natural analogue, and would require a complexity of attention that most humans probably aren't capable of. However, I think this worry is misplaced, as I think the fact that to account for intransitives may require going up the superlative hierarchy actually tells us a bit about how we should think of that hierarchy.

In particular, and returning to the point I made above, I think we should think of these different types of reference as just alternative ways of packaging the complexity any compositional semantics will need. After all, recall the standard treatment of intransitive verbs, according to which they are  $\langle e, \langle e, t \rangle \rangle$ . While perhaps not as mentally taxing as superduperplurality reference, nevertheless in saying that these are the denotations of intransitives we have gone a long way from intuition. Not only that, but some views don't stop there. Consider *in situ* treatments of quantifiers in object position, which posit a type raising operation that takes the intransitive to the massively complicated  $\langle e, \langle e, t \rangle \rangle, \langle e, t \rangle$ . The only way standard theories manage to avoid such positing is by committing to not uncontroversial positions in syntax concerning movement and covert elements. The point is that semantics for natural language requires a certain degree of complexity. One can put it into the semantic types, or one can put it into the syntax, or one can, with superlative reference, put it into the relation of referring. But one must put it somewhere, and so complexity alone is not sufficient reason to reject approaches like the one we are considering.

Let me say one more thing about this. I think it's instructive to compare what we're doing when we assume these different types of reference, on the one hand, and when we posit higher order semantic types, on the other. Indeed, I think we should view the two activities as similar: we should think of these types of reference just as we think of the types of denotation. Some of them, like singular and plural and—in my view—superplurality reference reflect an important fact about speaker's and thinker's understanding of language. In that way, they are like the basic *e* type expressions assigned as the denotation of names, or perhaps the idea of intensions as functions from worlds to truth values (if one thinks that the core notion of content is that which distinguishes between possibilities). On the other hand, there are higher order functions and higher level reference relations, which don't reflect an aspect of speaker's understanding, but are posited because they get the job done.

With that apologia for superplurality reference out of the way, let's see how to make use of it in the semantics I am developing.

## 6.4 Details

### 6.4.1 Simple Sentences

I am going to assume, following Rayo, that nouns, verbs, and names are each plurally or superplurally referring expressions. But what they'll refer to will be stages. In fact, although I used 'ss' as a variable for stages earlier, I think it will be somewhat neater if I switch to 'xx': so note well, variables now range over stages, be that plurally or superplurally. So we'll have things like:

- $\llbracket \text{Joan} \rrbracket =$  the xx: xx are the Joan stages.
- $\llbracket \text{laughs} \rrbracket =$  the xx: xx are the laughing stages.
- $\llbracket \text{man} \rrbracket =$  the xxx. for all  $yy \ll xxx$ ,  $\text{Man}'(yy)$ .

Note that  $\text{Man}'$ , here, is a predicate holding only of, so to speak, those stages which together comprise the life of a man. That is to say, we don't have that if  $\text{Man}'(xx)$  and  $yy \ll xx$  then  $\text{Man}'(yy)$ . Also note that I've given different treatments of verbs and common nouns: while the former are merely plurally referring expressions, the latter are superplurally referring expressions. One could do it another way: one could say that 'laughs' is superplurally referring. That is, using the nominalisation, it refers to all the laughing pluralities (i.e. stages which collectively laugh). For example, I laughed last night from 9:12pm to 9:15pm. On this analysis, the plurality of stages of me from 9:12pm-9:15pm would be one of the things superplurally referred to. By contrast, on the current analysis, 'laughs' refers to all the laughing stages: it refers to my 9:12pm stage and my 9:13pm stage and so on. I must confess to being uncertain as to which of these analyses is superior<sup>5</sup>.

We know what basic subject predicate sentences will look like:

- $\exists xx. \text{Joan}(xx) \text{ and laughs}(xx)$

Semantically, we'll interpret predication as being one of-ness, with the result that we'll get as truth conditions:

- $\exists xx. xx \ll \llbracket \text{Joan} \rrbracket \text{ and } xx \ll \llbracket \text{laughs} \rrbracket$

A note on ' $\ll$ '. I have said things like ' $xx \ll \llbracket \text{Joan} \rrbracket$ ', which is to say that the xxs are among the Joan stages. I've also said things like  $xx \ll xxx$ , which is to say the xxs are among the superplural xxxs. These are importantly different, and should be marked as such. I'll retain ' $\ll$ ' for the relation which takes a plural expression (or, theoretically, although this will never occur, a singular expression) on its left and a plural expression on its right, and maps them to true provided what the former refers to are (or is) among what the latter refers to. I'll use ' $\ll^2$ ' for the relation which takes a plural expression on the left and a superplural expression on the right. This is the higher level analogue of

<sup>5</sup>Note also one could treat *names* in the same way, although this would require a slightly more radical departure. We could say that a name N superplurally refers to those pluralities of stages called 'N'. We could then introduce some domain restriction story to ensure that a given occurrence of a name had a restricted singleton domain (or rather, that it referred to a sole plurality). This would bring the view close to standard the-predicativism. Again, I'm somewhat agnostic about the merits of this approach (it'll run straight into the problem for non-Millian theories, on the one hand, but one might think it's closer to other predicativisms in attributing the same *type* of semantic value to names and to common nouns, on the other), but I think it's quite nice that it's available with me.

the first level being among relation: as an example,  $\llbracket \text{Joan} \rrbracket \ll^2 \llbracket \text{woman} \rrbracket$ . There's another salient relation, call it ' $\ll^*$ '. This relation would be such that for any plurality  $p$  of objects among  $\llbracket \text{Joan} \rrbracket$ ,  $p \ll^* \llbracket \text{woman} \rrbracket$ . For example, take the plurality consisting of the stages of Joan on each of her birthdays. This discontinuous plurality  $\ll^* \llbracket \text{woman} \rrbracket$ , but it doesn't  $\ll^2 \llbracket \text{woman} \rrbracket$ .

Let's consider the question of how we compositionally derive these truth conditions. I suggest the best way to do is to steal as much as possible from familiar theories, making changes only where necessary. So let's review two basic features of the treatment in (Heim and Kratzer, 1998): the treatment of variables and quantifiers. Firstly, variables. In Heim and Kratzer, repackaging the work of Tarski, they treat variable-like elements such as pronouns and traces as sensitive to a variable assignment, where this is a function from numbers to objects.

For us, this will have to be modified to account for stages: we'll need to treat the assignment function as one which maps a number not to some one object, but to some objects<sup>6</sup>. Thus, if we use subscripts to number a person's stages, we might have as an example:

- $1 \rightarrow \text{Hume}_1, \text{Hume}_2, \text{Hume}_3$
- $2 \rightarrow \text{Kant}_1, \text{Kant}_2, \text{Kant}_3$
- $3 \rightarrow \dots$

If we were following Heim and Kratzer, then we'd say the constituent "Joan(xx)" is true, relative to assignment  $g$ , iff  $g(xx)$  has the property denoted by "Joan". But that's not entirely right. We're following Rayo in treating predicates as plurally referring expressions. What we want is for predication to be the is-one-of relation. That is to say, we want:

- $\llbracket \text{Joan}(xx) \rrbracket^g = 1$  iff  $g(xx) \ll \llbracket \text{Joan} \rrbracket$

Turning now to our default existential quantification, and still in the interests of clinging to the familiar, we'll separate the work of quantification and variable binding. However, we don't want the binder to be a familiar lambda binder, taking a sentence relative to an assignment function to a function from individuals to truth values. The reason for this is that our semantics makes no use of such functions: where the standard theory has them, we have rather plural or superplural referring expressions. What we really need is something to take us from truth values, relative to an assignment function, to stages in the plural (it's a bit clearer metalinguistically: from sentence meanings to plurally referring expression meanings). For this, we will introduce what I'll call an *iota* binder. This takes an open sentence to the things which satisfy it. Here is an example of first an open sentence, second its lambda bound variant, and third its new iota bound variant, and their denotations:

- $\llbracket \text{Joan}(xx) \rrbracket^g = \text{Joan}(g(xx))$
- $\llbracket \lambda xx \text{ Joan}(xx) \rrbracket^g = \lambda xx. \text{Joan}(xx)$
- $\llbracket \iota xx \text{ Joan}(xx) \rrbracket^g = \text{the } xx: \text{Joan}(xx)$ .

<sup>6</sup>This will require going up the superplural hierarchy to deal with bound plural pronouns as in 'Some guys out there think they could carry that piano down the stairs'. But any theory is going to have to do *something* new to account for such pronouns, so I don't think this is a strike against my view.

The semantic value of the latter, then, is the things that are Joan stages. I assume there's a maximality condition built in: it refers to *all* of Joan's stages. Moreover, let me note that I will often use  $\iota$  in the object language (just as one uses  $\lambda$  both as (an unpronounced) part of English and of the metalanguage of semantics). With that said, we have the following logical form for our sentence:

- $\exists xx.\iota_{xx}. \text{Joan}(xx) \text{ and laughs}(xx)$ .

The iota phrase here will stand for those stages that are both Joan stages and laughing stages. We can then interpret the existential quantifier as checking whether the referring expression it applies to is empty or not. This is the referential analogue of the function theoretic behaviour of the existential quantifier, which is to see if the property it applies to has an instance or not. In the metalanguage, I'll use  $\text{EXISTS}_{xx}$  as a translation of  $\exists xx$ .

The final thing to do, at this stage, is to give a rule which captures the semantic effect of natural language syntactic concatenation. For Heim and Kratzer, this is the function application rule. For me, it will be the conjunction rule. It will go like:

If  $\alpha$  is a branching node with  $\beta$  and  $\gamma$  as its daughters, then  $\llbracket \alpha \rrbracket^g = \llbracket \beta \rrbracket^g \text{ and } \llbracket \gamma \rrbracket^g$ .

So far, the changes have been, while somewhat extensive, nevertheless kind of conservative. We need to get slightly less conservative once we come to consider more complicated constructions. Consider first transitive verbs, as in:

(153) Joan loves John

A reasonably safe looking logical form, extrapolating from what we did above, is as so:

(154)  $\exists xx.\iota_{xx}. \exists yy.\iota_{yy}. \text{Joan}(xx) \text{ and loves}(xx,yy) \text{ and John}(yy)$ .

Now the question is: what is the semantic value of 'loves'? As Rayo's discussion indicates, there are two ways to go about it. We could go for an ordered pair strategy: 'loves' would plurally refer to those ordered pairs each of whose members were pluralities such that the first plurality loved the second plurality. If that were so, of course, it would require a new conception of the now misnomer 'ordered pair', because what stands in the ordered pair relation aren't individuals, but rather pluralities: we would have to say that it in fact there are a multitude of ordered pair relations. There would be the ordered pair relation which relates single objects to each other, and pluralities of objects to each other, and also superpluralities of objects to each other. Alternatively, we could say that it's a question of superduperplural reference. I propose to go with the former option, mainly as I think it's somewhat simpler:

- $\llbracket \text{hugs} \rrbracket = \text{the } xx: \forall yy.\forall zz. \text{if hugs}(yy,zz) \text{ then the } x.x = \langle yy,zz \rangle \ll xx$ .

That is to say, 'hugs' stands for all those ordered pairs of pluralities such that the first member of the ordered pair hugs the second. We'd then want the following rule, the analogue of the one we gave above for monadic predicates:

- $\llbracket \text{hugs}(xx,yy) \rrbracket^g = 1 \text{ iff } \langle xx,yy \rangle \ll \llbracket \text{hugs} \rrbracket$

To get this, we can introduce the following schematic rule for commas and brackets:

$$(155) \quad \llbracket (xx,yy) \rrbracket^g = \iota x. x = \langle g(xx), g(yy) \rangle$$

What this says is that the semantic value of an expression of the form '(xx,yy)' is the ordered pair consisting of what  $g$  assigns  $xx$  and what  $g$  assigns  $yy$ .

Running through a derivation will perhaps help the reader see how things are working. Here we gradually move up the tree:

- $\llbracket \text{John}(yy) \rrbracket^g = 1$  iff  $g(yy) \ll \llbracket \text{John} \rrbracket$
- $\llbracket \text{loves}(xx,yy) \text{ and } \text{John}(yy) \rrbracket^g = 1$  iff  $\{ \text{the } x. x = \langle g(xx), g(yy) \rangle \ll \llbracket \text{loves} \rrbracket \} \& g(yy) \ll \llbracket \text{John} \rrbracket$
- $\llbracket \text{Joan}(xx) \text{ and } \text{loves}(xx,yy) \text{ and } \text{John}(yy) \rrbracket^g = 1$  iff  $g(xx) \ll \llbracket \text{Joan} \rrbracket$  & the  $x. \{ x = \langle g(xx), g(yy) \rangle \ll \llbracket \text{loves} \rrbracket \} \& g(yy) \ll \llbracket \text{John} \rrbracket$
- $\llbracket \iota yy. \text{Joan}(xx) \text{ and } \text{loves}(xx,yy) \text{ and } \text{John}(yy) \rrbracket^g = \text{the } yy. g(xx) \ll \llbracket \text{Joan} \rrbracket$  & the  $x. x = \langle g(xx), yy \rangle \ll \llbracket \text{loves} \rrbracket$  &  $yy \ll \llbracket \text{John} \rrbracket$

At this stage, our expression refers to all those stages which are among the John stages, and which stand in the loved by relation to those stages which are Joan stages and are assigned by  $g$  to  $xx$ , provided there are some. The next stage sees if there are some by quantifying:

- $\llbracket \exists yy. \iota yy. \text{Joan}(xx) \text{ and } \text{loves}(xx,yy) \text{ and } \text{John}(yy) \rrbracket^g = \text{EXISTS}_{yy}(\text{the } yy. \{ g(xx) \ll \llbracket \text{Joan} \rrbracket$   
and the  $x. \{ x = \langle g(xx), yy \rangle \ll \llbracket \text{loves} \rrbracket \}$  and  $yy \ll \llbracket \text{John} \rrbracket \})$

This expression doesn't refer. Rather it yields true provided the iota phrase isn't empty. In the next step, we turn it into a referring expression again by iota binding the  $xx$ :

- $\llbracket \iota xx. \exists yy. \iota yy. \text{Joan}(xx) \text{ and } \text{loves}(xx,yy) \text{ and } \text{John}(yy) \rrbracket^g = \iota xx. \text{EXISTS}_{yy}(\text{the } yy. \text{xx} \ll \llbracket \text{Joan} \rrbracket$   
and the  $x. x = \langle xx, yy \rangle \ll \llbracket \text{loves} \rrbracket$  and  $yy \ll \llbracket \text{John} \rrbracket \})$

At this step, all assignment sensitivity has been got rid of, as can be seen by the lack of 'g's on the right hand side. The next step is:

- $\llbracket \exists xx. \iota xx. \exists yy. \iota yy. \text{Joan}(xx) \text{ and } \text{loves}(xx,yy) \text{ and } \text{John}(yy) \rrbracket = \text{EXISTS}_{xx}(\iota xx. \text{EXISTS}_{yy}(\text{the } yy. \text{xx} \ll \llbracket \text{Joan} \rrbracket$   
and the  $x. x = \langle xx, yy \rangle \ll \llbracket \text{loves} \rrbracket$  and  $yy \ll \llbracket \text{John} \rrbracket \})$

So our sentence is true provided the right hand side is true. And the right hand side is true provided a particularly complicated definite is non-empty. This definite refers to those stages such that they are Joan stages and they are loving stages, and they are loved by the denotation of the non-empty definite which refers to stages that are John stages and loved stages.

## 6.4.2 Natural Language Quantification

Now let's turn to ordinary language quantification. What are we to say about a sentence like:

$$(156) \quad \text{Every man swims}$$

My thought is that we treat this as relational. That is, 'every' stands for a relation between the denotation of (the logical form of) its noun phrase sister and the denotation of (the logical form of) its nuclear scope. In particular, it takes a superplurally referring expression S (in this case 'man'), and a plurally referring expression P (in this case 'swims'), and returns true provided a plurality which S superplurally refers to has in it some stages which are in P, which is to say if everything which S superplurally refers to is P<sup>7</sup>.

Since on the way we're doing things, relations stand for ordered pairs, its denotation would then be:

- $\llbracket \text{Every} \rrbracket = \iota xx. \forall xxx. \forall uu. \text{ if (for all } ww \text{ if } ww \ll^2 xxx \text{ then } \exists zz \text{ } zz \ll ww \text{ and } zz \ll uu) \text{ then } \iota x. x = \langle xxx, uu \rangle \ll xx.$

In English this says that for any superplurality (such as that referred to by 'man') and plurality (such as the denotation of 'swims'), if an object is in the superplurality and some of its stages are in the plurality, then the ordered pair containing them is one of the things denoted by 'every'. Here are the entries for some other determiners:

- $\llbracket \text{some} \rrbracket = \iota xx. \forall xxx. \forall uu. \text{ if there's some } ww \ll^2 xxx \text{ and } \exists zz \text{ } zz \ll ww \text{ and } zz \ll uu \text{ then } \iota x. x = \langle xxx, uu \rangle \ll xx.$
- $\llbracket \text{no} \rrbracket = \iota xx. \forall xxx. \forall uu. \text{ if there's no } ww \text{ such that } ww \ll^2 xxx \text{ and } \exists zz \text{ } zz \ll ww \text{ and } zz \ll uu \text{ then } \iota x. x = \langle xxx, uu \rangle \ll xx$

The truth conditions of our sentence will be:

- Every man swims iff  $\llbracket \text{Every} \rrbracket \llbracket (\text{man}, \text{swims}) \rrbracket$
- Every man swims iff  $= \llbracket \text{man}, \text{swims} \rrbracket \ll \llbracket \text{Every} \rrbracket$ .
- Every man swims iff  $\langle \llbracket \text{man} \rrbracket, \llbracket \text{swims} \rrbracket \rangle \ll \llbracket \text{Every} \rrbracket$

And that last will hold just in case for all  $xx \ll^2 \llbracket \text{man} \rrbracket$ , i.e. for all men, there's some  $zz \ll xx$  that are in  $\llbracket \text{swims} \rrbracket$ , i.e. if all men have some swimming stages, which is the correct result.

However, we need to consider what the logical form will look like. A first thought, taking our lead from above, would be as so:

- Every (man(xx),swims(xx))

As it stands, the arguments provided to 'Every' aren't respectively a superplurality and a plurality, but a pair of open sentences. So we really need to convert these open sentences by binding operators. In the case of the second argument, things are easy:

- Every (man(xx),  $\iota xx. \text{swims}(xx)$ )

<sup>7</sup>Obviously, this raises questions about sentences like 'Every man is a human', where it's arguable that the nuclear scope stands for a superplurality. There are a couple of possible ways to go: either take this as evidence we should treat common nouns and verbs as on a par, and amend the semantics accordingly (which would be straightforward) or say that the contribution of 'is a human' is just an expression referring plurally to all the human stages, by assuming the function of the copula is, as it were, to dearticulate a superplurally referring expression into a plurally referring one.



However, if we were to take the same tack for the first argument, we'd get the wrong result. For we want the first argument to be a superplurally referring expression. That is, we want it to be such that there are some  $xx$ s which are  $\ll^2$  the denotation of the first argument. But our simple iota binder would merely give us  $\iota xx. \text{man}(xx)$ , i.e.  $\iota xx.xx \ll \ll [\text{man}]$ , that is to say all the men stages. I think the solution is to introduce a new iota operator, call it double iota, which looks as so:  $\iota\iota$  with the following rule for trees containing it:

**Double Iota Rule.** If  $\alpha$  is of the form  $[\iota\iota xx [\iota_{vp} F(xx)]]$  then  $[[\iota\iota xx F(xx)]] = \iota\iota xxx$ .  
 $\forall xx: xx \ll \ll^2 xxx$  iff  $F(xx)$ .

What the double iota does is take an open sentence and converts it into a superplurally referring expression such that anything which is  $\ll^2$  it satisfies the open sentence. We then have a final logical form like so:

- Every  $(\iota\iota xx.\text{man}(xx), \iota\iota xx.\text{swims}(xx))$

Let's go on to consider cases of multiple generality, as evinced by:

(157) Every man loves some woman

One might initially have fears about how to analyse this: it'll seem somehow to involve a relation inside another relation, and one's brain might start to hurt at the thought. But in fact a little reflection reveals that the problem is not really any harder than the traditional problem, and doesn't require (overly) different resources.

In particular recall that for Heim and Kratzer we account for such constructions by means of traces, movement, and lambda binders, so that our sentence would look as so:

- $[_s [_{dp} \text{Every man}] [_{vp} \lambda 1. \text{Some woman. } \lambda 2. 1 \text{ loves } 2.]]$

I can more or less take this over, with one change: I make the lambda binders into iota operators (and change the variables from singular to plural, of course). That is, as a first stab:

- Every  $(\text{man}, \iota\iota xx. \text{Some woman. } \iota\iota yy. \text{loves}(xx, yy))$

This isn't quite right, since, as we've seen, we actually have:

- Every  $(\text{man}(xx), \iota\iota xx. \text{Some } (\text{woman}(yy), \iota\iota yy. \text{loves}(xx, yy)))$

Or rather, since we need to bind the variables in the determiners' sisters, we get:

- Every  $(\iota\iota \text{man}(xx), \iota\iota xx. \text{Some } (\iota\iota yy \text{ woman}(yy), \iota\iota yy. \text{loves}(xx, yy)))$

I grant the reader this isn't the most pulcritudinous logical form ever proposed, but, for example, the massively complicated functions posited by the variable free semanticists are hardly exactly pretty either. The wide scope reading will be derived in the same way as normal, but permuting the order of the quantifiers at LF. Let's run through a derivation of the narrow scope existential reading to keep us honest. The second argument of the lower quantifier will be computed as so:

- $[[\text{loves}(xx, yy)]]^g = 1$  iff  $\iota x. x = \langle g(xx), g(yy) \rangle \ll \ll [[\text{loves}]]$

- $\llbracket \iota yy. \text{loves}(xx,yy) \rrbracket = 1$  iff  $\iota x. x = \langle g(xx), yy \rangle \ll \llbracket \text{loves} \rrbracket$

The same procedure will apply to the first argument. We can then say:

- $\llbracket (\iota yy. \text{woman}(yy), \iota yy. \text{loves}(xx,yy)) \rrbracket = \iota x. x = \langle \iota xxx. \forall yy. yy \ll^2 xxx \text{ iff } \text{woman}(yy), \{ \iota yy. \iota x. x = \langle g(xx), yy \rangle \ll \llbracket \text{loves} \rrbracket \} \rangle$

That is to say, the bracketed phrase will stand for the ordered pair the first element of which is the superplurality consisting of the women, and the second of which is the yys such that yy are loved by g(xx). Next, applying 'some':

- $\llbracket \text{Some}(\iota yy. \text{woman}(yy), \iota yy. \text{loves}(xx,yy)) \rrbracket^g = 1$  iff  $\iota x. x = \langle \iota xxx. \forall zz. zz \ll^2 xxx \text{ iff } \text{woman}(zz), \{ \iota yy. \iota x. x = \langle g(xx), yy \rangle \text{ and } x \ll \llbracket \text{loves} \rrbracket \} \rangle \ll \llbracket \text{some} \rrbracket$

That is to say, we have a sentence true relative to g provided the ordered pair consisting of the woman superplurality and the plurality of things loved by g(xx) belong to the denotation of 'some'. Next, we bind the currently free xx variable with the iota binder, to turn the whole second argument of the quantifier into a referring expression:

- $\llbracket \iota xx. \text{Some}(\iota yy. \text{woman}(yy), \iota yy. \text{loves}(xx,yy)) \rrbracket^g = \iota xx. [\iota x. x = \langle \iota xxx. \forall zz. zz \ll^2 xxx \text{ iff } \text{woman}(zz), \{ \iota yy. \iota x. x = \langle xx, yy \rangle \ll \llbracket \text{loves} \rrbracket \} \rangle] \ll \llbracket \text{some} \rrbracket$

Although very complicated to parse, what this refers to is the xxs, for which the following condition obtains: there is an ordered pair the first element of which is the women and the second of which is the stages which are loved by the xxs, and this ordered pair is in the denotation of some. And for that to be the case, some woman must have some stages among her that are loved by the xxs. And that's to say that the xxs refer to the stages loved by some woman. And that is what we want, intuitively speaking, it to denote.

The remainder of the derivation works in the same way: we look at the men, and provided *all* of them have some stages which are in the referring expression just parsed, i.e. all of them have some stages loved by some woman, we get a true sentence.

While obviously there is more to be done (for example: the treatment of English language plural quantifiers, and of pronouns), I hope this sketch suffices to show that my Stage Semantics has the resources to capture quantification, while both respecting the core of predicativism, and the existence of stages.

### 6.4.3 Tense

Let's now consider how to cash out tense. This is a big topic, and my aim here is somewhat modest: to sketch a way I think things could go that will work. I don't intend to give an exhaustive treatment. I think the thing to do is hold that tensed elements function as properties of stages. For example, a sentence like:

(158) John swam

Will have a logical form like so:

(159)  $\exists xx. \iota xx. \text{John}(xx) \ \& \ \text{PAST}(xx) \ \& \ \text{swam}(xx).$

That is, there are some John stages, which are past stages, and which are swimming stages. We can and perhaps should allow that what PAST means varies between different occasions of use. For example, if we had been talking about what people did yesterday, and uttered the above, one might wish to say that PAST stood for the stages that were present yesterday. By contrast, if we're just talking about the things which John at some time previously did (say as part of a list: John used to be so active: he swam, he fished, he golfed etc.), then it can stand for all the stages that were around over some much longer time. In this way we can account for the referential features of tense noted by Barbara Partee (Partee, 1973).

Moreover, as far as I can tell, this stance on tense can account, with a little massaging, for the sort of scope ambiguity readings one gets when one has descriptions or quantifiers as subjects. For example, the following familiarly has two readings:

(160) The president was a child

On one, it can be used to say the true thing that Barack was a child (almost always there will be some contextually salient reference time: an out of the blue utterance of the above will provoke confusion). When so it will have the following syntactic logical form:

- The ( $\iota$ . president(xx),  $\iota$ xx.PAST(xx) & be a child(xx) )

The sentence will have relational truth conditions: it will be true provided the sole member of the superplurality denoted by the subject term has some stages among it that are both past stages and child stages. Of course, in this particular example, the denotation of 'president' does not yield a superplurality with a sole member: for a start, there is the president of France, Argentina, etc. Moreover, even if we had a more complete definite, such as 'the president of the U.S.' that still wouldn't get us completeness, for it will refer to all past present and future presidents, while we're looking to talk about the present president.

Incompleteness is of course a well explored terrain, and not one I need worry about: whatever the correct story should be about different country induced incompleteness (whether that be that there are covert syntactic components which get values to narrow the domain, or some semantic mechanism, or some pragmatic mechanism), I will take it over to account for different time induced incompleteness. It can also be used to say, again relative to some salient reference time, that the president then was a child. And our theory can capture this, if we say that the logical form is:

- The ( $\iota$ xx.[PAST(xx) & President(xx)],  $\iota$ xx.be a child(xx) ).

The semantic value of the double iota phrase will be:

- $\iota$ xxx.  $\forall$ yy: yy«xxx iff PAST(yy) & President(yy).

'President' will superplurally refer to those pluralities which collect up presidential parts of people: all of Bush's stages from the time he was inaugurated until the time he ceased; all Barack's stages from and to the same times; ditto (hopefully) Hillary or Bernie. The function of 'PAST', on this view, will be to get rid of some stages: certainly the Bernie or Hillary stages, and possibly the Barack stages. It will definitely contain Bush stages, Bill Clinton stages, Reagan stages, and so on. Of course, on this reading the sentence is obviously false, since none of these pluralities of stages have the property of being a child.

How many stages the definite will stand for depends on what value PAST gets from context. If it gets a very specific one, say, *existing in 1984*, then the definite will be complete. Otherwise, it will be incomplete. We can then give a standard clause for the definite, in line with the above:

- $\iota xx. \forall xxx. \forall uu. \text{ if the sole } ww \ll^2 xxx \text{ is such } \exists zz. zz \ll uu \text{ then } x = \langle xxx, uu \rangle \ll xx.$

In English, this refers plurally to all those ordered pairs consisting of a superplurality and a plurality such that the superplurality has a sole member and among it are some stages which are in the plurality. So, for example it refers to  $\langle \ll[\text{president now}], [\text{Democrat}] \rangle$ .

Again, more needs to be said. But again, I hope what has been said should suffice as a proof of concept for my Stage Semantics.

#### 6.4.4 Other Phenomena

Thus far we have given treatments of noun phrases, both what are traditionally thought of as e-type and quantificational, transitive and intransitive verbs, and tense. I hope that suffices to show the reader that the semantics is powerful enough to rival other more traditional theories. In this section, I want to reinforce that impression by sketching how the theory could be modified, rather straightforwardly, to account for other phenomenon.

Firstly, context sensitivity. This strikes me as causing no particular problems. On the assumption that there are no monsters, there's no new embedding phenomena to worry about, so if we just make the Kaplanian move of adding another layer of meaning, character, to the denotations we have given, that should be enough. Here, for example, would be the Kaplanian character for 'I':

- $\lambda c. \iota xx. xx \text{ are the stages of the speaker in } c.$

And here is a logical form:

- $\exists xx. \iota xx. I(xx) \ \& \ \text{swim}(xx).$

Secondly, the binding of pronouns. Again, this needn't cause any undue problems. Note that we've already made use of traces, variable assignments, and binders, and we can do so again. But just to rehash: we'll have assignment functions be functions from numbers to stages. Personal pronouns will plurally stand for different stages relative to different assignment functions. An iota binder coindexed with a pronoun will abstract away from this relativity, turning what it scopes over from a sentence, i.e. something which has a truth value relative to an assignment function, to a referring expression.

Third, English language plural quantification, as in 'Some men carried the piano'. I don't propose to treat of this in great detail: suffice it to say that 'some men' will supersuperplurally refer to those superpluralities consisting of several men which themselves are, familiarly by now, pluralities. To express this without recourse to the nominalising 'superpluralities' is unfortunately beyond me (and perhaps beyond the English language), but I hope if you understand superpluralities, you'll understand supersuperpluralities. And again, I would like to argue that if you're not offended by functions from functions to functions to functions to truth values of the sort standard semantic theories posit, you shouldn't be overly offended by supersuperplural reference.

## 6.5 Consequences

Let's now turn to a (hopefully inexhaustive) list of Stage Semantics's good features. In successive sections I'll point out four: its new take on predicativism, its new take on persistence, the light it can shed on the debate concerning acquaintance, and its ability to overcome the semantic problems of stage and worm theory.

### 6.5.1 Consequence 1: Pure Predicativism.

I claim for my view that it is the purest manifestation of predicativism. Predicativism says that names are predicates. Previous theories have tried to overcome the problem thus posed by names in argument position by positing a covert determiner, but that has been problematic. However, it only *is* a problem on a certain view of the architecture of semantics and its syntactic interface. But this view is far from irrefragable: the above has been an attempt to show how to frag it.

However, the attentive reader won't fail to have noticed something slightly odd about my form of predicativism. The oddity is that I am not able to deal, out of the box, with the data which primarily inspires the predicativist, namely the use of names following determiners and when pluralised:

(161) Every Joan is cruel

(162) Peters are nice

For the classical predicativist, there is a single predicate, whose extension is those things called Joan. For me, however, there are a range of predicates, one for each Joan, which contain the Joan in question's stages. Accordingly, for me the above, taken literally, would be first ambiguous, and once disambiguated would make a claim that the sentence doesn't report.

I admit this might seem *prima facie* odd, but I'm not hugely worried by this fact. We should understand predicativism as a syntactic thesis, and no more. Names are predicates. That's it. It doesn't, in and of itself, tell us anything more than where they can occur syntactically, and what type of meaning they have. What those particular meanings *are* is a separate question.

Indeed, it's worth noting that the more standard predicativist isn't in a completely happy position here, either. For there are predicative uses, as we've seen, which the the-predicativist cannot account for:

(163) She's a real Einstein

(164) You're no Ted Kennedy

Here the semantic contribution of the predicate is surely not the property of *being called Einstein* or *being called Ted Kennedy*. A sign of this is the felicity of the following sort of use. Imagine one is addressing someone called Jesus who is exhibiting less than great moral behaviour:

(165) You may be called 'Jesus' but you're no Jesus.

The point is, *every* theory will have to recognise at least three different uses of names: straightforward referential ones, standard predicativist ones where the predicate seems well-glossed as *being called X*, and non-standard predicative uses like the ones above. A view suffices to be called predicativism provided it makes the crucial claim that in

each such use, the name contributes a predicate to the semantic composition procedure. It's then a separate question how to derive these three types of meaning: for the standard predicativist, the referential uses are got by positing a covert element and the non-standard predicative uses, perhaps, by means of some pragmatic mechanism. By contrast, for me, the referential uses come for free, and then I need to say something about the standard and the non-standard predicative uses. While I don't have a story yet, I feel that's something that can wait for later work. The key point is there's no reason to think that whatever story I end up with will have to forego the central claim that names are predicates, and so no reason to doubt that my view deserves to be called a version of predicativism.

### 6.5.2 Consequence 2: Stages Without Fusions of Stages

Another interesting feature of my view is that it can reap the benefits provided by worms without requiring unrestricted mereological composition. As such, my view can be seen as an analogue of the debate concerning overt plural quantification in natural language. It has been remarked in several places that four dimensionalism often comes bundled up with universal fusion, but at the same time, it's been realised that they are different views. My position provides a clear realisation of a view that is four dimensional while avoiding universal composition.

Let me expand a bit on what sort of metaphysics my view supports. Any account must have some story to tell about how different stages which are temporal parts of, in some sense, the same object, are related. For the worm theorist, it is by being a part, ultimately, of a complete worm. For the stage theorist, it is by standing in the counterpart relation to other stages. For me, it is by falling under the same predicate.

Of course, and familiarly, that is to leave much unsaid. In particular, it leaves unsaid what, if any, relation two objects must bear to one another if they are to fall under the same predicate. This is, from my point of view, a nice position of freedom to have. For my view is compatible both with a view that maintains everyday objects have some sort of unity, and with one which denies this. Firstly, the unity: I can say that the predicates in question are natural ones, in the sense that they express joint-carving relations. On this idea, we could say that our thought latches onto not enduring objects, but the natural properties instantiated collectively by stages, like *being Joan*. In this way, we could respond to a criticism which would say that we haven't saved the phenomenon of change. In particular, we could make the claim that we have saved the phenomenon pretty much as well as the stage theorist has. The stage theorist, recall, allows for sameness across time counterpart theoretically. Now, table banging aside, this means that an object's sameness is, strictly, a question of there being two different objects. I say the same thing here, only the glue that knits them together need not be the counterpart relation, but whatever relation it is which unites some stages into (what we're inclined to call) one call object. Indeed, it could be the counterpart relation, albeit if it were, this would have no effect on the semantics. Indeed, strictly speaking, it could be the belonging to the same maximal fusion relation, although that would be, I grant, somewhat quixotic.

On the other hand, we don't have to say this. Speaking personally, I'm attracted to a view like Chisholm's *entia successiva* or (arguably) the Buddhist view of impermanence, according to which the unity of the disparate stages is a mind-generated fiction that gets expressed in language because it helps us categorise the world, even though it doesn't get at any vein of reality. This latter view, by placing the unity of the stages not in fusions or counterparts, may be looked on with fondness by those who are keen on

stages but wary of the ontological and ideological baggage that extant theories seem to bring along with them, and I think it's a point in my favour that my view can appeal to such people.

To sum up, by holding the minimal position that the source of unity is merely that of co-membership, I give myself a lot of ontological freedom. Indeed, I don't really make any ontological commitments at all (apart from to stages), so my view can be espoused by people no matter what commitments they actually have.

### 6.5.3 Consequence 3: Acquaintance

The third consequence I want to consider concerns the notion of acquaintance. We will discuss acquaintance in more detail in the following chapter, so I will be somewhat brief. The idea, introduced by Russell (Russell, 1912) but taken over and modified by many subsequent thinkers, is that in order to understand certain expressions, one must stand in particular sorts of relations to the things the expressions stand for. The literature generally recognises several such relations, such as perception, memory of previous perception and testimony. The crucial one, however, has always been perception.

However, it's also long been realised that there are some problem cases: for example, imagine being in a small wood bounded on all sides by wall, and knowing there's a bear in it. Is there such a sharp distinction between catching a fleeting glimpse of the bear up and to the left, and seeing freshly made tracks pointing in the same direction? It seems not: in each case, the thought one expresses and one's hearer will understand by 'the bear/he is over there' would seem to be the same, which means either that perception isn't sufficient for acquaintance (if the thought expressed is general) or it isn't necessary (if the thought expressed is singular).

We'll see further problem cases later, but I want to note that Stage Semantics can maybe help us say what's going on here. In particular, I think I can effect something of a rapprochement between the pro and the anti acquaintance people. The key to doing so is to realise that, to repeat myself, names are predicates. If names are predicates, then we should expect, at least *prima facie*, that the epistemology of names is like the epistemology of predicates. Now, disappointingly, I don't have a worked out epistemology of predicates. But I think even an unworked out one leads to some interesting thoughts. For the following seems not implausible: that perception is not a sufficient condition for understanding even what we might call perceptual predicates, examples of which might include, at the least, colour words. It's plausible that it's insufficient, for me to understand the word 'red' or to have a grasp of redness, that I have merely perceived some red thing. That seems far too undemanding: one might think, for example, that we must be able to go beyond the one instance and have the general purpose ability to recognise when presented with us, red things, or failing that to have some sort of general knowledge about red things (I couldn't recognise fuschia, but it's arguable that I have a grasp of 'fuschia' by virtue of knowing it's a type of purple and the leaves of the fuschia plant are so coloured). If that is so, then it'll also be so that it's insufficient, to understand a disambiguated 'Joan', that one merely have perceived some Joan stage. Again, something going beyond that will be necessary.

This can be viewed as an anti-acquaintance point: I'm saying that what has been taken to be the paradigm of acquaintance is insufficient for the understanding of names. But I think that would be a bit hasty. It's only right provided there is no acquaintance constraint for predicates, *punkt*. And that I haven't argued, and indeed wouldn't want

to. Indeed, if one accepts the view that, for example, understanding a predicate involves a capacity to recognise its instances, or have some general knowledge about it, then since this will hold for names, then we'll have a new conception of the epistemology of names that might be in the spirit of the pro-acquaintance people. Of course, to make this case requires much more investigation, but I think the foregoing is at least suggestive, and so I think a nice feature of Stage Semantics is that it may offer a new perspective on the old and long-argued about question of acquaintance.

#### 6.5.4 Consequence 4: Overcoming Four Dimensionalism's Semantic Problems

Let's now turn to see how my view does against the problems I raised for stage theory and worm theory. Consider worm theory first. It struggled with the following:

(166) Kant was both alive and dead

The problem was that a worm satisfied a predicate provided it had a part satisfying the predicate. It's then easy to think of scenarios in which Kant satisfies each of a conjunction of predicates by having one part satisfying one predicate and another satisfying another. The scenario the above sentence reports furnishes one, when uttered in a context in which the night of Kant's death was salient. The logical form of the sentence, for me, will be (omitting the iota binder):

- $\exists xx. \text{Kant}(xx) \ \& \ \text{Past}(xx) \ \& \ \text{alive}(xx)\text{-and-}\text{dead}(xx)$

We see, then, that it's the *same* stages, on this view, that must be alive and dead, and since no stages can be simultaneously alive and dead, I correctly predict our sentence is bad.

However, this makes pressing the stage theorist's problem, which was to account for the fineness of:

(167) Kant lived and died in Koenigsburg

Our logical form will look like:

- $\exists xx. \text{Kant}(xx) \ \& \ \text{PAST}(xx) \ \& \ \text{lived-and-died}(xx)$

But now that should be predicted to be *equally* bad as the first, yet it isn't. This doesn't seem great for my view. Thankfully, I think there's a reply available here. Recall on my view that every predicate expresses a collective predicate which holds of stages in the plural. For example, 'swims' holds of some stages provided they together swim. Now, as a matter of fact, when one has a plurally referring expression as subject and a conjoined predicate as predicate, one sometimes gets interesting readings. Consider the following sentence:

(168) The boys washed and dried the dishes.

Imagine the boys are b1..b6. Then they can satisfy the conjoined predicate in the following way: b1..b3 washed the dishes, and b4..b6 dried the dishes. That is, it's not necessary that each boy both wash and dry. One way to put the thing, roughly, is that in such cases, the conjunction behaves as if it were a disjunction.

Now, I don't have an account for what generates such readings. But they seem there. And given that, I propose to make use of this fact to account for our sentence.



In particular, I'll say that our sentence is true because there are indeed some stages of Kant which live and die, and thus which witness the plural existential. The plural witness will consist of all Kant's living stages and all Kant's dying stages. Now the living stages don't die, and vice versa. But this is just like how the boys who wash don't dry, and vice versa. Just like in the dishes sentence, I would propose to analyse the Kant sentence as if it roughly expressed disjunction. And, while I grant this isn't a perfect explanation, since I have no account of why conjunction sometimes expresses something like disjunction, I nevertheless think it's reasonably principled and not too ad hoc.

## 6.6 Objections

### 6.6.1 Objection 1: Why Should I Believe This?

Let me first consider the following obvious but still quite powerful objection: Stage Semantics rests on a *lot* of assumptions, many of which are contested. These include: predicativism about names, conjunctivism about semantic composition, the existence of stages, and the the legitimacy and indeed centrality of superplural and higher forms of reference.

None of these, I am fully aware, are uncontroversial. These can and presumably have or will be written on each. Moreover, in my discussion I haven't presented anything close to knockdown arguments for *any* of the claims. Indeed, I've suggested that for at least predicativism and the existence of stages, there exist strong viable alternatives. So why should the reader go along with a view which rests on the incredibly controversial conjunction of four controversial claims?

My answer to this is that any semantic theory rests on a series of assumptions. The standard theory works within a framework that takes from predicate logic the idea that the function of a name is solely to pick out its bearer. But the good arguments for predicativism, based on extensive cross-linguistic data, call into question whether names should be so analysed. Moreover, much semantic theory works within a framework familiar from standard quantified modal logic according to which the extensions of predicates vary with regard to time and world, while names don't: a name, at any time it exists, stands for the same thing. But this seems pretty close to being a semanticised version of three dimensionalism: in particular, it seems to make the central claim that objects exist sans phrase at different times, and the popular claim (inimical to Lewis, for example) that the possession of properties is time relative. But the good arguments for four dimensionalism call into question whether we should accept this picture so readily (of course, this relies on the further assumption that our semantics should in some sense reflect metaphysical reality, which I defended in 4). Similar things hold, albeit less neatly, in the case of conjunctivism and hyperplural reference. Thus although working semanticists will often assume something like the standard function theoretic semantics, this is far from obligatory. Pietroskis's book-long defense of conjunctivism shows there to be a viable alternative, and thus not something to be dismissed unless for reasons better than unfamiliarity. As for the importance of higher level reference, that's the least defended and perhaps the least defensible of my assumptions. But there are nevertheless good arguments out there and relatively well understood semantic theories for it.

The point is that at least three of my assumptions are well-supported alternatives to the more standard views. To the extent that in semantic theorising one must start somewhere, and that it's not obligatory that one start in a function-theoretic version of

quantified modal logic, I see nothing wrong with starting in a slightly different place and seeing how far we can get. And I hope this chapter has shown one can get reasonably far.

### 6.6.2 Objection 2: It's Really Complicated!

A related reason for dispreferring my view is that it's very complicated: the logical forms and semantic values I posit are unfamiliar and hard to get one's head around, especially compared with the simplicity of the standard Heim and Kratzer approach. One might think simplicity is one of the symptoms that a semantic theory is right, and conversely that complexity shows it's not right.

I have two sorts of response to this line of thought. The first, which we've already to some extent seen, points out that semantics is hard. Heim and Kratzer's theory is as simple as it is because it allows itself movement, lambda abstraction, traces, indexing, variable assignments and expressions from high up the type hierarchy. That is, it is simple because it makes free use of a range of syntactic and semantic assumptions, and thus sort of distributes the complexity involved between syntax and semantics. By contrast, a theory like Jacobsen's variable free semantics (recently given a textbook presentation in (Jacobson, 2014)) eschews adding to the syntax, but at the cost of having some extremely complicated semantic values. Put it where you like, the complexity has to show up somewhere. For me, admittedly, I have complexity both at the level of syntax, with all the binders, and at the level of semantics, with the higher order reference. But that complexity gets me things: it lets me respect the syntactic data concerning names being predicates, and it lets me be non-committal with regard to ontology (apart from the positing of temporal parts). I think that's a reasonable trade.

The second thing I want to say is that for the most part it's familiar complexity. For the most part, I'm just taking stuff from Heim and Kratzer, and from Rayo, and modifying it slightly. So even if it is complex, it's well understood. For the most part, if you accept the legitimacy of covert binders and of plural reference, as most do, you shouldn't balk at the apparatus I invoke, because you're already committed to it.

### 6.6.3 Objection 3: Is This Really Predicativism? (redux)

I've lauded my view, repeatedly, as being the clearest realisation of the idea that names are predicates. But there are two ways one could deny this. We've already seen one: that I don't capture what tends to be treated by the predicativist as the main explananda, namely the occurrence of names post-determiner and with bare plurals. Here's another. Common nouns on the one hand, and names on the other, don't function semantically in the same way: one is a superplurally referring expression, while the other is a plurally referring expression. Is this a problem?

I don't really think so. Consider an analogy: on any standard semantics, one gives a different treatment to 'man' and to 'men'<sup>8</sup>. There are obviously different ways of cashing this out, but one is to introduce the idea of a ground level of atomic individuals, and another level which can consist of more than one individual, and then say that the extension of man can contain only atomic individuals, but the extension of 'men' ranges over the second level (in addition, perhaps, to the atoms). But this semantic difference doesn't lead one to hold that we are here not dealing with predicates, so nor should the semantic difference between plural and superplural expressions.

<sup>8</sup>At least as they occur post-determiner; 'Men' can of course also have a bare plural use.

## 6.7 Conclusion

I've here presented my Stage Semantics: names plurally refer to stages, and they share this function with predicates: it's thus fair to say that names are predicates. But my view is markedly different from extant versions of predicativism: I don't posit a covert article, which as we've seen is a source of difficulty for extant predicativisms. Moreover, I can avoid the problem which arises for theories which try to analyse names in argument position as descriptions or variables, namely the fact that we can refer to objects about which we know little with names. I do so by taking over the Kripkean picture about reference transmission and by saying that although names are predicates, they are ambiguous ones. My theory can also avoid the problems faced by stage theory and worm theory. All in all, then, I think my theory has some good features and that it deserves the attention of both semanticists and metaphysicians.

## Chapter 7

# Belief and Belief Reports

### 7.1 Introduction

So far I've argued that names, which have often been thought to be e-type, are in fact not so. The aim of the next, final two chapters is to extend the attack against type e expressions further by considering another construction which has been thought to mandate them, namely de re attitude reports.

A classic example of a de re attitude report, from Quine (Quine, 1956), is exhibited by the ambiguous English sentence:

(169) Quine believes someone is a spy

This has two readings: on one it asserts that there's some person such that Quine believes he or she is a spy. On the other, it asserts that Quine believes that there are spies. For the former, it seems that there's some particular object which Quine's belief is about, while for the latter, this is not the case.

The problem of de re attitudes, as I'll use this phrase, is to give an account of what's going on in such constructions. In fact, there is a popular and rather neat story available. On this story, the sentence is scopally ambiguous, having two logical forms representable as so:

- $\text{Someone}_1 \lambda 1. \text{Quine believes } t1 \text{ is a spy}$
- $\text{Quine believes } \text{Someone}_1 \lambda 1. t1 \text{ is a spy}$

We account for the former, object-involving reading by scoping the indefinite over 'Quine' and leaving a bound pronoun in the attitude report. If we then assume the standard semantics for bound pronouns, according to which they denote an object relative to an assignment, we see a reflection in the semantics of the intuitive contrast: the sentence has an object-involving reading because on one disambiguation the content of the belief is specified using a pronoun and the semantic function of a pronoun is simply to stand for an object.

So the contrast seems technically accountable for (certain challenges for this account will be noted later). Moreover, it seems to reflect something about the nature of belief. Starting with Russell (Russell, 1912), a popular idea has been that there's a distinction between two sorts of belief: singular and general. The former are, in a sense to be explained, directly about objects while the latter are not. Again for a canonical example, my belief, formed on the basis of general reasoning that there are spies, and one of them is taller than any other, that the tallest spy is a spy does not seem to be as directly about the tallest spy, whoever he or she may be, as my belief, concerning the bottle on my desk that I'm currently looking at, that it is blue. We could then say

that the object-involving reading reports a singular, direct belief, while the other reading merely reports a general belief. So not only is the distinction between the readings technically accounted for, but it seems to correspond to an antecedently existing distinction between different types of belief.

Overall, this is a remarkably neat picture. However, I want to argue against it, and present a new theory of what's going on. Arguing against it, I'll present some new arguments and rehash some old ones against the picture of the semantic functioning of pronouns and the purported distinction between singular and general belief. To get my new, alternative view on the table, I need to set the scene. I think *de re* reports are in fact just one species of a genus, and that looking at another species of the same genus can help us understand them. In order to see this, let's change example.

Imagine Quine's at a dinner party with the queen, but he doesn't know she is the queen. Because of this, he didn't bow when he introduced himself. We can then say:

(170) Quine met the queen but didn't bow because he didn't know she was the queen

Compare that with:

(171) Quine met the queen but didn't bow because he didn't know the queen was the queen

While the first of these is perfectly fine, the second is somewhat odd. But this is puzzling: it seems the only difference between the two sentences is that we've replaced the pronoun 'she' with its anaphoric antecedent, and one might think that shouldn't make any difference to acceptability (one might reply here that there is no difference in acceptability: note if one stresses 'was' 171 improves markedly. However, it's known that stress can effect semantic content, so the puzzle would remain).

A plausible explanation is that the second places the demand on Quine that he believed something with the same content as 'the queen was the queen'. Alternatively, we could say that the second suggests that he is disposed to accept the sentence 'the queen is the queen'. By contrast, we can view 170 as leaving open exactly what the content of Quine's belief was, or which sentence(s) he was disposed to accept. It could be something like 'the old lady at the party was the queen', or 'the lady wearing the crown was the queen', or 'the one who ate all the guacamole was the queen'. And we could explain this by saying that 170 reports a *de re* belief directly about the object, while the latter reports a *de dicto* general belief about the property of being the queen.

My leading thought is that this phenomenon is not confined to belief. I think we see it also with speech reports, in the phenomenon of mixed quotation (first noted by Davidson, 1979):

(172) Quine said that quotation 'has an anomalous character'

The notable thing about this is that just as 170 seems to place no constraints on how Quine thinks of the queen, so 172 places no constraints on what word Quine uses to speak of the queen: the above is true provided he said 'citation has an anomalous character', 'quotation has an anomalous character', 'metalinguistic discourse has an anomalous character' (assuming, falsely, that 'metalinguistic discourse' and 'quotation' mean the same thing). This is notably in contrast with what we, following the literature, will call direct quotation, as typified by:

(173) Quine said 'quotation has an anomalous character'

This sentence *does* place constraints on what word Quine uses to speak of quotation: he must use the word 'quotation' (certain caveats to this will be noted later).

Given this similarity—that de re belief seems similar to mixed quotation and de dicto belief to direct quotation—it strikes me as plausible to say that the two phenomena are in fact manifestations of one and the same feature of language: different species of one genus, as I put it above. And given that, it's plausible to think that an adequate theory of mixed quotation could help us develop an adequate theory of de re attitude reports. And, indeed, I think this is so, and propose to show it: in the chapter following this, I will critique extant theories of mixed quotation, and present a new theory of it, and show how a relatively small modification of it can lead us to a new theory of de re attitude reports. A consequence of this will be that there's no need for e-type expressions to account for the data concerning de re attitude reports: we can account for their behaviour in a unified, compositional way, from within a broadly Fregean theory of the attitudes. Thus this part of the dissertation furnishes another reason not to posit the existence of e-type expressions.

The plan going forward is as follows. In the remainder of this chapter I'll flesh out the problem of de re reports in more detail, presenting some new and old problems for the notion of singular thought and the semantics of bound pronouns. I'll then end with a preview of my position. The next chapter will introduce quotation, present problems for extant theories, and develop my own view, before returning to de re attitudes.

## 7.2 A neoRussellian view

Here are two venerable, plausible, and popular views, which together furnish an explanation of what's going on in de re reports. The former comes from Tarski (Tarski, 1956) and the latter from Russell (Russell, 1912):

**Semantic For Pronouns.** Pronouns are e-type, which is to say pronouns refer to objects. However, unlike constants in logic, they do so only relative to an assignment sequence (or function).

**Singular and General Belief.** There is a distinction between singular beliefs, beliefs directly about some one object, and general beliefs, which are not directly about some one object.

We've already seen these views elsewhere in the thesis, but just to rehash: the syntactic and the semantic properties of variable-like elements seem to be in tension with one another. Let's look at this in the context of first order logic; the points transfer over to the treatment of pronouns in natural language. Semantically, it seems like variables are devices of generality. In the first order formula:

- $\exists x.Fx$

The variable occurring after 'F' doesn't seem to be standing for anything particular. Indeed, it sort of looks like it's just functioning to mark which argument position of the predicate gets controlled by the quantifier (admittedly, this function is kind of redundant for monadic predicates), which is a means of generalising. On the other hand, syntactically it occurs in the same position as a constant, and so the neatest syntax/semantics interface would treat it as belonging to the same semantic type as a constant, which is to say e-type. So there's a tension: they seem singular from the point of view of syntax, but general from the point of view of semantics.

Tarski thinks we can have our semantic cake and eat it syntactically too. We say that variables are indeed e-type, but only relative to a parameter, an assignment sequence or function. This can be conceived of either as a sequence of objects, or a function from variables to objects. I'll use the latter. For example, we could have:  $x \rightarrow \text{John}$ ,  $y \rightarrow \text{Mary}$ ,  $z \rightarrow \text{Alex}$ , and so on. This semantic relativity bubbles up to sentences in which the variables occur. We can then say that quantifier serves to generalise over sequences: our sentence is true relative to an assignment  $g$  provided there's some possibly different assignment  $g'$  such that  $F(g'(x))$ .

The important point is that variables are e-type, and moreover e-type expressions which—it's been thought—serve clearly to simply point to objects. If this is so, then provided variables and descriptions behave differently, we can begin explain the intuitive contrast between the two readings of : the de re reading places few constraints on Quine's belief because its content encodes simply the object, and no descriptive way of thinking about it, while the de dicto belief requires he believe something with a content like that expressed by 'there are spies'.

Famously, Quine himself, in the paper which introduced this topic, thought there was something problematic about this way of understanding things. Say the person whom Quine believes to be a spy is Orcutt: he believes that he's a spy because he saw Orcutt furtively taking pictures on the beach. But he also knows Orcutt as a local politician, and doesn't realise the politician and the beach photographer are the same. Then, if we assume that belief reports involving names are de re, we'll be committed to the truth of both 'Quine believes Orcutt is a spy' and 'Quine believes Orcutt isn't a spy', which doesn't sound like a great commitment.

Quine's response to this was to hold that 'believes' is ambiguous: in de dicto beliefs it's a dyadic relation, relating a speaker to a sentential intension (not that Quine liked intensions), while in de re reports it expresses a triadic relation between a thinker, an object, and a predicate intension. However, this is not unproblematic: 'believe' fails standard ambiguity tests.

A better solution is to put the difference in the objects of belief: there are different types of objects of belief, to each of which one can be related by the belief relation. On this picture, associated with Russell and later followers, the objects of belief come in two kinds, singular and general.

In order to see this, consider the following cases:

- I see a bowl of cereal on my bed, and form the belief that it's muesli
- I saw a bowl of cereal on my bed this morning: I think about it, and form the belief that it was muesli
- I have the belief that *I* am not hungry
- You tell me about your cousin Liam, and I form the believe that he sounds interesting
- I conclude the tallest spy is a spy from the premises that there are spies, for any two people  $a$  and  $b$ , probably either  $a$  is bigger than  $b$ , or vice versa, and spies are spies

There does seem to be notable difference between the last listed belief and the four which precede it. It seems that one's epistemic relation to the tallest spy is weaker in the last one than in the others. We could note, for example, that each of the other beliefs involve a channel of information (perception, previous perception, proprioception, and

testimony) which one could go on to use to inform oneself further about the object, while the latter does not. The latter seems, so to speak, considerably less epistemically intimate.

The idea that beliefs can be typed by means of the epistemic relation the believer bears to the object of the belief is present in a large amount of the most influential work on reference and description, such as, for example, Strawson, 1959, Donnellan, 1966, Kripke, 1977, Evans, 1982, Neale, 1990. More recent work is in the anthology Jeshion, 2010a). It has most recently received a book length defense from François Recanati (Recanati, 2012), who holds that to have a singular thought about an object it's necessary to stand in an *epistemically rewarding* or ER relation to it. The epistemically rewarding relations which he countenances correspond to the first four beliefs above. If one fails to stand in such a relation to an object, as in the fifth case, the best one can do is a general belief.

We can use this distinction to explain, at the level of belief, what's going on in Quine's original case. In the *de re* case, Quine has a singular thought about the person whom he takes to be a spy by virtue of having perceived him looking suspicious at the beach. By contrast, in the *de dicto* case, where he comes to hold that spies exist by virtue of general reasoning, he has only a general belief.

Overall, I think this is quite a nice package. My own view will be very different from it, however, and so first I want to present some problems for the package to motivate my looking for a new theory.

## 7.3 Problems for the neoRussellian view

### 7.3.1 Problems For The Singular And General Belief Distinction

The first big problem I want to consider concerns the demarcation between singular and general thoughts<sup>1</sup>. What is it that makes it the case that when I have a belief about the tallest spy the object of my belief is a congeries of properties, while when I have certain other beliefs, the object is an object? The idea of acquaintance is tempting but ultimately, I think, too unfirm a foundation to ground semantics on.

While there is something *prima facie* attractive about this view, as we already saw in 6.5.3, it has problems. It seems that the notion of epistemic intimacy which it's concerned with is inherently vague. For example, consider the following two cases (similar in theme to one in Jeshion, 2010b): in the first, you catch a fleeting glimpse of a bear in the woods, before he runs off. In the second, you see fresh bear footprints heading towards a forest you know bounded by walls. It's arguable that in the latter case you stand in a more epistemically intimate relation to the bear than in the former. Here's another particularly neat illustration of this issue from Mark Sainsbury:

Consider the following example of the way in which knowledge by 'by description' might modulate into knowledge 'by acquaintance'. You notice that the cheese in your larder is disappearing overnight. You hypothesise that this is caused by a mouse. You imagine this mouse in some detail, which you reinforce by experiment: you leave sand near the cheese and confirm that it is a mouse by its tracks; by similar methods, you discover

<sup>1</sup>Recall I already argued, based on my Stage Semantics, that perception is insufficient for reference. Here, in the spirit of not putting all one's eggs in a single basket, I present alternative routes to that conclusion.



where it lives when not raiding your larder. Your behaviour becomes a little obsessive: you dignify your mouse by the name 'Freddie'; you speculate, correctly in fact, that Freddie is the head of a large family of mice, and that he does not eat all the chesse he steals but takes some back as paternal investment in a recent brood; you get a camera and in the morning you replay scenes of Freddie's activities the previous evening. Then one evening you actually see Freddie eating your cheese (Sainsbury, 2005, p21)

Sainsbury asks at what point in this story do we come to be acquainted with the mouse, and surely he's right to say that there doesn't seem to be a sharp cut off point. Moreover, it seems that we should want to say that before one gets to the final stage where one actually sees Freddie, one has enough information to count as acquainted with him. But in such a circumstance, one has not perceived him (nor remembered perceiving him, nor heard about him, nor been him).

What cases like this seem to show is that actual perception isn't necessary for singular thought (for an interesting discussion of why we might think perception is not necessary in this case, which ties the notion of acquaintance to a speaker's interests, see the Jeshion paper cited above). Not only that, I think one can find, somewhat surprisingly, cases in which perception isn't *sufficient* for reference. In particular, consider cases of masses which are rapidly losing parts. Imagine one is faced with a melting pile of snow, and one thinks to oneself: it's white and it's melting. I take it there's some pressure to say that, as far as the subject terms go, these two beliefs should have the same content, that is to say (the mental analogues of) the two 'it's should refer to the same. But if these two beliefs are object involving, they will not do so, on the following plausible assumption:

**Mereological Essentialism For Masses.** If  $x$  is a mass of  $K$ , then, for every  $y$  such that  $y$  is a mass of  $K$ ,  $y$  is a part of  $x$  if and only if it always was and always will be the case that, if  $x$  exists, then  $y$  is a part of  $x$  (Zimmerman, 1995, p79)

Because the snow has melted and lost parts between the two beliefs, if the beliefs are singular, they are about two different masses<sup>2</sup>. But then they don't have the same content, which was our intuitive desiderata. So the thing to say, I think, is that these beliefs are actually descriptive beliefs, ones which could be expressed by 'the snow is white' and 'the snow is melting'. This secures that the two beliefs have the same content (even though they will denote—in the Russellian sense of being uniquely satisfied by—different masses). If this is so, then to the extent that the two beliefs are clearly perceptual, we have a counterexample to the claim that perception is sufficient for knowledge (I discuss such cases at more length in McKeever, n.d.(a)). So it seems that perception is either sufficient nor necessary for the having of singular thought. But to the extent that it's always been thought that perception furnished the clearest means by which one could come to have singular thoughts, this serves to undermine the distinction.

Not only that, but there are arguably cases in which clearly general beliefs enjoy the epistemic closeness typical of singular beliefs. Thus consider the following case:

<sup>2</sup>One might try to deny this by taking the second thought 'it' to be anaphoric on the first. If that were so, in a scenario in which the snow first dazzles (at  $t_1$ ) because it's illuminated by the sun, and then ceases to (at  $t_2$ ), two consecutive thoughts to the effect that 'it's dazzling' and 'it's not dazzling' wouldn't come out true, which seems like the wrong result. Similar things apply in the case where we have only one thought at  $t_2$ , to the effect that it was dazzling. If the pronoun is functioning to pick out the snow at  $t_2$ , then the sentence reports something false, as the mass around at  $t_2$  has just come in to being, and so there's no past time such that it dazzles then.

**Penguins.** I'm an experienced penguinologist, who has just taken into my care 10 penguins. They are dotted around my penguin sanctuary, and I've been going round meeting them. I meet nine, thereby coming to be able to have singular thoughts about them. The only area I haven't yet visited is the penguin sauna. I form the belief: the penguin I haven't met yet must be in the penguin sauna.

This seems like a paradigm instance of general belief. But in a certain respect my cognitive position with regard to the penguin I haven't met is similar to the one with regard to those I have met. In particular, note that I have the capacity to recognise, when presented with it, the last penguin: it's the penguin who doesn't look like one of the penguins I've already met. But one of Recanati's ideas is that this capacity to recognise is a mark of acquaintance. So it seems like this should count as an acquaintance thought. Of course, Recanati will reply that in this case, there fails to be an ER relation between the speaker and the object, but I think we should just take this as a case which shows that ER relations aren't needed.

Arguably, then, not only is perception neither necessary nor sufficient for acquaintance, but it also seems that arriving at a definite description belief, in certain particular circumstances, is sufficient for an acquaintance thought. I conclude that the notion of acquaintance, despite being *prima facie* attractive, should play no crucial role in semantic theorising, and in particular is insufficient to support a contrast between singular and general belief.

### 7.3.2 Problems For The Theory Of Pronouns

Not only that, the theory of pronouns made use of is questionable. It's long been familiar that certain uses of pronouns can't be accounted for in the standard Tarskian way. Here's a typical example (Neale, 1990):

(174) The president is a democrat. He used to be a republican

On the more plausible reading of the second sentence, it is equivalent to 'the president used to be a republican'. That is to say, it seems as if the pronoun must be functioning descriptively, expressing the same content as 'the president'. We can't derive the reading we're after if we assume that pronouns simply stand for an object.

What hasn't been realised is that we get a similar phenomenon, on certain plausible assumptions, with indexicals. In particular, the assumption we need is the one, important to Kaplan, that there is a semantically important notion of indexical validity: of sentences which are true at every context (which is to say relative to every context express a proposition true with regard to the world and time of the context). This is typified by the following canonical (but flawed: see Predelli, 1998 and the huge literature it spawned) example:

(175) I am here now

Compared with:

(176) Matt is in Oslo 11/04/16

In Kaplan's famous words:

Intuitively, [175] is deeply, and, in some sense, which we will shortly make precise, universally, true. One need only understand the meaning of [175] to know that it cannot be uttered falsely. No such guarantee applies to [176]. A Logic of Indexicals which does not reflect this intuitive difference between [175] and [176] has bypassed something essential to the logic of indexicals (Kaplan, 1977, p509)

My claim is that we get apriori truths involving anaphoric pronouns, and such truths cannot be accounted for within a standard semantics for pronouns. Consider:

(177) Yesterday is in the past and it's never to return

This is true relative to every context: relative to every context, the day before the day of the context will indeed be in the past, and will indeed be never to return. But think about what the character of the pronoun must be. The natural thought is that the character of such pronouns serves to pick out the linguistic antecedent of the expression. There are several ways to cash this out. It could be that this is effected by means of the variable assignment. We can assume that antecedence is marked in the syntax by co-indexing, as so:

(178) Yesterday<sub>1</sub> is in the past and it<sub>1</sub>'s never to return

We could then suggest that the effect an utterance of a referring expression  $r_i$  has on the variable assignment is to put the object referred to in the  $i$ th place of the assignment. We would then have a character like:

- $It_1 = \lambda c. \lambda w.$  the object assigned by the variable assignment of  $c$  to 1.

Alternatively, another thought is that such referential anaphora goes by salience. 'It' refers to the day in question because it has been rendered salient by just being mentioned. We would then have:

- $It_1 = \lambda c. \lambda w.$  the salient mentioned object in  $c$ .

However, a little reflection reveals that neither of these will yield the correct result. Consider a context  $c^*$  in which the following has just been uttered:

(179) Tomorrow<sub>1</sub> is going to be great.

Then relative to that  $c^*$ , on either possible character for 'it<sub>1</sub>' mentioned above, 'it<sub>1</sub>' will stand for the day after the day of  $c^*$ . So the sentence will be true relative to  $c^*$  iff the day before the day of the context is in the past, and the day after the day of  $c^*$  is never to return. So the second sentence—assuming the falsity of eternal recurrence—is false relative to the context, and thus so is the conjunction, and so the sentence is not indexically valid. But the sentence *is* indexically valid, so something is wrong with our purported characters for 'it<sub>1</sub>'.

Not only that, it seems quite clear that there's an independently motivated fix available. We noted above that sometimes pronouns inherit the *content* of descriptions on which they are anaphoric. Our case suggests sometimes they inherit the *character* of indexicals on which they are anaphoric. Not only this, but when we combine it with the Elbournian idea that pronouns are syntactically covert descriptions, then we have the option to say that in both these cases, we should think of anaphoric pronouns as just copies of their antecedent which gets spelled out as explicitly as pronouns for some reason. That is, we would have:

(180) The president is a democrat. He [president] used to be a republican

(181) Yesterday is over and it [yesterday] is not coming back

If this is so, we have a good reason to doubt that pronouns function as the standard theory has it, and to the extent that the standard theory is an important part of the position concerning de re belief we're considering, we have good reason to doubt that theory (for more on indexical cases like the one above, see McKeever, n.d.(b)).

Let me make a couple of comments before going on. Firstly, the copy theory I just bruited is of limited application. It would clearly get the wrong result in the following case, for example:

(182) Every father loves his child

This does not report that every father loves every father's child. So the copy theory has its limits. But it nevertheless shows us that at least as concerns constructions like:

(183) Quine met Elizabeth but embarrassed himself because he didn't know she was Elizabeth

The account given above can't be right, and we need an explanation for how it is that this sentence doesn't seem to claim that Quine doesn't know that Elizabeth is Elizabeth.

Moreover, my criticism is, and is intended to be, limited. We can note that we get de re readings in the absence of pronouns. Thus imagine that Quine is at a party with the Queen, except he doesn't know she is the queen, and so doesn't bow in front of her. The following then has a true reading:

(184) Quine embarrassed himself because he didn't know the queen *was* the queen

To make sense of this, we should say that the first occurrence of 'the queen' is functioning just to pick out the person at the party. That is, we get de re readings not involving pronouns, so my criticism of the standard account of pronouns doesn't extend to them.

Let me just say a word more about this. One might think that such a case *does* in fact involve pronominal elements, albeit at the level of logical form. In particular, a not implausible thing to say is that the stress on *was* forces the first occurrence of 'the queen' to scope over the whole sentence, and leave behind a trace. That is, we'd have:

- The queen  $\lambda 1$ . Quine embarrassed himself because he didn't know  $t_1$  was the queen

So this would be a case of scope after all. However, this is no good: it's a familiar point that scoping can't account for all de re/ de dicto ambiguities. Here's about as clear a case as one gets (adapted from (Bäuerle, 1983)):

(185) George believes that a woman from Liverpool loves every member of Manchester United

And consider the following scenario: we're in Liverpool. George sees a bunch of famous, rich looking people on a bus. He assumes they are celebrities or something, and knowing how people like celebrities, he comes to believe there's somebody who loves them all, although he doesn't know who. Now as it so happens, the men on the bus are the Man United football team.

The sentence seems like an accurate report of George's belief. But no permuting of scope will give us the reading we are after. The salient facts are that because George

doesn't have some particular woman in mind, 'a woman from Liverpool' must scope under 'believes'. Moreover, because there's some one woman who loves them all, 'a woman from Liverpool' must scope above 'every member of Manchester United'. But because George doesn't know the people he sees are Manchester United players, 'every member of Manchester United' must scope above 'believes'. But these, a little thought reveals, are inconsistent requirements.

The standard response to this has been to assume that attitude verbs and other modal expressions are in fact quantifiers which can bind covert world variables (for a textbook presentation, see (Von Stechow and Heim, 2002), chapter eight). If we then say that the variable associated with the indefinite gets bound by, and thus covaries with, the universal quantifier over doxastically accessible worlds supplied by 'believes', while the world variable associated with the universal quantifier is free and gets the world of the context as its value, then we'll get the right result. And if that is so, then our queen cases could receive the same treatment. We could claim that in the good reading of 236, the world variable associated with the first occurrence of 'the queen' is free and thus gets its value from the actual world.

If this were the case, however, 'the queen' would be functioning just like an e-type expression: notably, it would be rigid, picking out the actual queen relative to all doxastically accessible worlds, and it wouldn't give rise to the scope interactions which are one of the defining features of quantified noun phrases. So the more sophisticated and empirically adequate theory more or less relies on e-type expressions too. While I don't have any particular criticisms of it, the positive theory I'll present will, I think, have a certain advantage in terms of theoretical unity, as it will give more or less the same treatment to both speech and belief reports, which I claim is desirable.

## 7.4 A Preview of my Fregean Account

What I think this shows is that what I called above a neat account of *de re* constructions arguably isn't so neat. This should hopefully cause in the reader a desire to find some better solution, and that's what I propose to do now, by considering new evidence from an arguably unexpected corner, namely the theory of quotation. I'll argue that some data which has only really become the object of serious study quite recently favours a Fregean account of attitude ascriptions, which takes its lead from (Frege, 1892) and some later neoFregeans.

A simple Fregean account holds that expressions have both sense and reference. The reference is the semantic value: what enters into the composition procedure, while the sense is a way of thinking about the semantic value. However, when we deal with attitude verbs, the reference of an expression shifts, so that its semantic value comes to be its sense. This enables us to explain the contrast between a pair like:

(186) Lois believes that Clark Kent is puny

(187) Lois believes that Superman is puny

While the first seems true, the second seems false (this is of course contested. The locus classicus here is (Salmon, 1986), who resorts to pragmatics to account for how the two sentences are co-extensional yet appear not to be. I have nothing interesting to say against this line, except that the data seems reasonably strong in this case).

This is puzzling because 'Clark Kent' and 'Superman' are co-referential, and so, one might think, supply the same value to the composition procedure which, given the sentences are otherwise identical, should return the same truth value for both sentences.

For the Fregean, we can say that the first sentence is true, and the second sentence is false, and we can get that straight from the semantics: because the occurrence of 'Clark Kent' has as semantic value the sense of 'Clark Kent', which one could gloss descriptively as something like *the nerdy guy who works for the newspaper* while 'Superman' has as semantic value the 'Superman' sense, something perhaps like *the guy who saves everybody but is allergic to Kryptonite*, there's no expectation that the sentences should have the same truth value. We can then use this to give a metaphysics of belief: belief is a relation to the senses expressed by whole sentences. Frege called the sense of a whole sentence a *thought*, and I shall occasionally do so too.

I'm going to defend pretty much this position. In particular, I'm going to defend what is near enough the view that when occurring in belief contexts, an expression has as semantic value, roughly, a sense rather than its customary reference or content. However, there is one way I propose to update Frege's view in light of recent work. Frege took senses to be abstract objects, belonging to the unfortunately named *Drittes Reich* (the first two *Reiche* were ideas and ordinary physical objects), as he wanted to secure both their mind independence and their intersubjectivity. But he didn't say much more about that they were. Frege lived, of course, before the era of intensional logic, and was thus deprived of one of semantics's great tools. Contemporary Fregeans not thus impoverished have made use of the apparatus of possible worlds to make sense of sense. For example (Chalmers, 2006) has argued that we can think of senses just as we think of contents: as functions from worlds to extensions. The only difference is that the worlds in question are not metaphysically possible worlds, but epistemically possible worlds. I propose to lift this apparatus from Chalmers and use it. As we will see, it's a reasonably natural move to make, given the theory of quotation I end up with, and which I'll present in the next chapter.



## Chapter 8

# Quotation (and more on belief)

### 8.1 Quotation: Introduction

Without further ado, let's now turn to quotation. At the heart of my thinking about quotation lies the phenomenon of mixed quotation. Mixed quotation has been a topic of investigation by philosophers and linguists for about 40 years (on the philosophical side, see among others Davidson, 1979, Recanati, 2000, Cappelen and Lepore, 2007, and García-Carpintero, 2012. Recent work in linguistics includes among others Potts, 2007, Shan, 2010, and Maier, 2014). Neutrally and provisionally described, a mixed quotation is a complement to a speech verb which includes both quoted and unquoted expressions<sup>1</sup>.

Here's the famous example:

(188) Quine said that quotation 'has an anomalous feature'

The truth conditions seem clear: a smiley face below indicates a circumstance in which it's true, an 'x' falsity, and I assume, incorrectly, that 'quotation', 'citation', and 'metalinguistic discourse' are synonyms for the sake of the example:

- ☺ Quine said 'quotation has an anomalous feature'
- ☺ Quine said 'citation has an anomalous feature'
- ☺ Quine said 'metalinguistic discourse has an anomalous feature'
- × Quine said 'quotation has a strange characteristic'

For a mixed quotation to be true it's necessary that the reportee say what's quoted verbatim (this notion of verbatimness will be elucidated below), say some words which mean what the unquoted words mean, and say them in the same order as the complement sentence does. What's unclear is how to derive these truth conditions compositionally, and in particular how to compose (seemingly) used and mentioned expressions.

There have been several accounts which attempt to capture these truth conditions<sup>2</sup>. However, each such attempt works with an assumption which is, as I'll show, very problematic, namely that quoted expressions in mixed quotations are used. This goes all the way back to Davidson, and to the following passage:

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<sup>1</sup>A word about notation: I'll try to stick to single quotes throughout in examples of quoted speech. I'll sometimes use italics to talk about words in the body of the text, and when necessary, will use double quotes to talk about quoted expressions in examples.

<sup>2</sup>Some, e.g. (Stainton, 1999) think it's not a semantic but a pragmatic phenomenon. Discussion is beyond the scope of this chapter, and I'll simply assume it's semantic.



Are the words [‘has an anomalous character’] used or mentioned? Obviously mentioned, since the words are Quine’s own, and I want to mark the fact. But equally obvious is the fact the words are used; if they were not, what follows the word ‘quotation’ would be a singular term, and this cannot be if I have produced a grammatical sentence. (Davidson, 1979, p29)

Subsequent literature has realised that this requires qualification. This can be easily seen if we consider the felicity of mixed quoting context sensitive and dialectical expressions:

(189) Hilary said Biden ‘has always looked out for me’

(190) Meagan said she put out the trash ‘on accident’

There’s a clear sense in which the quoted words here aren’t being used: they aren’t supplying the semantic value which they have, relative to the context of utterance of the report, to the semantic composition procedure. If they were, we would get the wrong result: that Hilary said Biden has always looked out for me, the author of the thesis you’re reading. Similarly, in the dialect of the current writer, ‘accident’ takes the preposition ‘by’, not ‘on’. So relative to the context of utterance of the report, the string ‘she put out the trash on accident’ is semantically ill-formed.

To account for such cases, we need to qualify Davidson’s remarks somewhat. Rather than being used simpliciter, we want that quoted words be used to mean what the speaker meant in uttering them. And indeed, this is what one finds in the recent literature, more or less explicitly in Cappelen and Lepore, Shan, and very explicitly in Maier. Let’s call this the Use Assumption (it will require a slight qualification later):

**The Use Assumption.** Quoting expressions in mixed quotations are used, and mean what the reportee meant in using the expression.

My first and critical aim is to suggest The Use Assumption is untenable. There are two serious problems with it. First, it elides the distinction between semantic meaning and speaker meaning. Imagine that Sam is colour blind, and mistakes red and green, so that he calls red things ‘green’ and vice versa. He looks at a traffic light, which is red, and says ‘the light is green’. The following sentence is false:

(191) Sam said the light was ‘green’ and he was right

Yet it’s predicted, by the Use Assumption, to be true. This is a bad result, and suggests The Use Assumption is false: further such cases will help confirm this. Second, according to most theorists who work on imperatives, one cannot indirect quote imperatives<sup>3</sup>. The following is bad:

(192) \* Mary said stop annoying her.

However, it appears that one can mixed quote imperatives. Here’s an example. Imagine a mother and her daughter went hiking earlier, and at a certain point the daughter made to jump across a wide stream. Seeing this, the mother says:

(193) ‘Don’t even think about doing that, missy!’

Later on, the daughter is complaining about her mother’s strictness, and says:

<sup>3</sup>For discussion of some data, and a dissenting voice, see Kaufmann, 2011, chapter six.

- (194) You're always preventing me doing stuff I want to! You won't let me watch Game of Thrones and earlier when we were hiking you said 'don't even think about' jumping across that tiny stream.

The second conjunct of the daughter's utterance seems felicitous. For the proponent of the Use Assumption, its truth conditions will be derived by composing the semantic value of 'don't even think about jumping across that stream' with 'said'. But such a composition should crash, in the same way that the attempt to indirectly quote an imperative above does. For another slightly different sort of example, consider the title of a chapter of MacAskill, 2015:

- Don't 'follow your dream'

We can imagine MacAskill giving a talk based on this chapter and saying:

- (195) I say to you, don't 'follow your dream', if you want to help people in the most efficient way

Again, this seems like a fine utterance, but if the use theorist is right, then the complement clause is an imperative, and so the whole should be as bad as 192.

My main constructive aim in this paper is to present an adequate semantics for mixed quotation which does without the Use Assumption: as we shall see, this requires giving a new theory of direct quotation, and will yield as a corollary a newish theory of indirect quotation. Before presenting a sketch of my view, let me note one important thing: I am *not* going to take into account the earlier section of the thesis, and in particular, I'm going to assume a standard Heim and Kratzer function theoretic semantics. There are several reasons for this: it will be easier for the reader to assess the view if, as they go along, they don't have to be asking which novelty belongs to the theory of quotation and which to that of names. Moreover, as far as I can tell, to combine the two views shouldn't be particularly difficult, although it would be complicated and produce some spectacularly ugly logical forms.

A natural thought, given the failure of The Use Assumption, which attempts to assimilate mixed to indirect quotation, is whether we should think of it as a species of *direct* quotation. An example of a direct quotation is:

- (196) Quine said 'quotation has an anomalous feature'

If we were to treat mixed quotations as direct quotations, then the 'said' which occurs in them should, like the above, denote a sentence. This might seem to be an obvious non-starter. After all, for our 188 to be true, Quine could have said *any* of the quoted sentences in the smiley examples above: mixed quotations aren't specific in the way that direct quotations are, so it's implausible that there's any one sentence denoted by a mixed quotation's complement. But that needn't be fatal to the analysis just bruted. After all, the verb *loves* always takes some thing as object (after all, it's an  $\langle e, \langle e, t \rangle \rangle$ ), but the following is completely fine:

- (197) Joan loves somebody

Even though the sentence isn't specific as to the object of Joan's love, and 'somebody' doesn't denote an object. So even if the mixed quotation doesn't denote some one sentence, it doesn't yet follow that *said* doesn't always denote a relation between speakers and sentences. For mixed quotations could be quantificational. And indeed, that's my

view. Unquoted words in mixed quotation complements denote existential quantifiers over words. In our leading example, 'quotation' denotes a quantifier whose values are words which mean quotation, like 'quotation', 'citation', 'Zitat', and so on. Let me briefly sketch how this will go.

Quantifiers, according to the standard treatment of Heim and Kratzer, 1998, scope out and leave a variable-like element called a trace. The following is a plausible logical form, where  $w_1$  is a trace over words (and where these are, importantly, not conceived of as type  $e$  expressions, but rather of a higher type, as I'll expand on later), and QUOT is a covert quotation operator which shifts a word from its standard meaning to a quantifier over words which have that meaning:

- [QUOT quotation]  $\lambda_1$  Quine said  $w_1$  'has an anomalous feature'

I'll present a semantics such that the composition of  $w_1$  and "has an anomalous feature" goes just like the quotation internal composition of, say, the occurrences of 'anomalous' and 'feature' in the above. The end result will be that the LF constituent [ $w_1$  'has an anomalous feature'], relative to an assignment function  $h$ , will denote a sentence. The quantifier denoted by the raised [QUOT quotation] will look to assignment variants  $h'$  of  $h$  such that  $h'(1)$  is a word which means quotation. If there is one, then the whole will be true if the composition of  $h'(1)$ , in the way to be specified, with 'has an anomalous feature', is a sentence which Quine said. But since  $h'(1)$ , according to the meaning of the quantifier [QUOT quotation]' must be a word meaning quotation, the semantics will correctly predict the sentence is true, without needing the false assumption that a mixed quotation is used when it occurs. One can then extend the theory to indirect quotation simply by taking indirect quotation to be the limiting case of mixed quotation in which the whole that-clause denotes a quantifier over sentences which mean what the sentence means. For example:

(198) Quine said that quotation has an anomalous feature

Will be true provided there's some sentence which means what 'quotation has an anomalous feature' means, such that Quine said it. We thus end up with a theory of indirect quotation roughly similar to Davidson's paratactic account of indirect quotation (Davidson, 1968), but now we have an independent motivation for it: it falls out as a consequence of an empirically adequate theory of mixed quotation.

The big picture lesson to draw from this is that an expression which we've thought to look for intensional entities in fact looks for hyperintensional entities. It is this lesson that will enable us, with some tinkering, to turn the theory presented here into a Fregean theory of attitude ascriptions: roughly, I'll claim 'believe' is just like 'say': the only difference is that the that-clauses function as quantifiers over *senses*, conceived of as functions from epistemically possible worlds to extensions.

The plan for the following few sections is the following. In the next section, I'll consider the data pertaining to mixed quotation. Then I'll consider previous approaches to the problem.

## 8.2 The Varieties of Quotation

I want to begin by presenting the different linguistic phenomena which are, in some sense or other, quotational. It's important to do this because, as we'll see, there are different ways to taxonomise the data which in turn lead to different adequacy conditions

on a theory of mixed quotation, and the taxonomy I adopt is importantly different from the standard one.

The literature recognises four varieties of quotation: pure, direct, indirect, and mixed quotation (terminology is from Cappelen and Lepore, 2007). Let's begin just by giving examples. Examples of pure quotations are:

- (199) "cats" is a word
- (200) "cats" has four letters
- (201) "ahahsd" is nonsense
- (202) "a" is a letter
- (203) "Jackie Onassis Kennedy" refers to Jackie Onassis Kennedy

Here one talks about, in some sense, what I'll call (following Cappelen and Lepore, 2007) a quotable item in order to say something about it. A lot has been written about pure quotation, both because of its intrinsic interest and its connection to issues in the theory of meaning exemplified by the final sentence above. People have argued both about what pure quotations stand for—some think words, some symbols, some concepts (for some discussion, see e.g. García-Carpintero, 1994). They have also argued about how they do so—some think by naming (e.g. in Quine, 1960), some demonstration (Davidson loc. cit.), and some description (Geach, 1957).

Direct quotation is exemplified by:

- (204) Quine said 'quotation has an anomalous feature'

Direct quotation is useful if one wants to report, verbatim, the words a speaker used, and as such plays a fundamental role in the reporting of news, history, literary criticism and so on. A quite natural thought is that direct quotation is simply an instance of pure quotation: one might think that *being said by so and so* is a property of quotable items in just the same way that *having four letters*, or *being nonsense* is. If one thought that, then it's to be expected that the theory of pure quotation can handle direct quotation without any problems.

This is a natural thought which I want to challenge, and I can do so by considering the next item in our taxonomy, *indirect quotation*:

- (205) Quine said that quotation has an anomalous feature

There are both continuities and discontinuities between direct and indirect quotation. The obvious continuity is that they are both concerned with *saying*: this gives one a prima facie reason for thinking that they are instances of the same phenomenon. The obvious discontinuity is that indirect quotations typically contain no quotable item. Indeed, it seems like it's precisely the point of indirect quotation that it not commit the reportee to having said any particular sentence.

So we seem to be pushed in opposing directions: there's reason to think that a decent theory of pure quotation should furnish a decent theory of direct quotation (because both phenomena appear to involve the predication of properties of quotable items). On the other hand, there's reason to think that a decent theory of direct quotation should furnish a decent theory of indirect quotation (because they both involve the notion of saying and in particular speech reporting). But it's also the case, I take it, that there's little reason to expect a decent theory of pure quotation to furnish a decent theory of indirect quotation: the two phenomena seem sufficiently dissimilar that the prospects for such a theory are poor.

This last point suggests that quotation as a whole will require two different theories: one to cover (at least) pure quotation, and one to cover (at least) indirect quotation. Given that we'll need a theory to account for direct quotation, it doesn't yet say which (if either) of the two theories that should be. There are two possible views: we have a theory which deals with pure and direct quotation together, and with indirect alone, or a theory which deals with direct and indirect together, and pure alone.

Once we then add mixed quotation to the mix, another choice point arises: do we account for mixed quotation using the resources of one of our theories, or will we require both? It's plausible that if we adopt the first view mentioned, we will need both, because we'll need to appeal to features of both direct and indirect quotation, and these dealt with by different theories. On the other hand, if one goes for the second view, one has the option of saying that mixed quotation is dealt with by the theory which covers direct and indirect quotation alone.

Most of the previous literature has, I think, worked under the assumption that view one is correct, and thus that mixed quotation is indeed a bona fide mix, requiring the resources of two theories. I will be going for view two, however, giving a unified theory of mixed, direct, and indirect quotation. I then have the separate duty to give an independent theory of pure quotation (which is not one which I carry out in this chapter). The position I defend, thus, involves a somewhat different taxonomy of the phenomenon of quotation than is normal, and should be judged with that in mind.

### 8.3 Data

In this section, I'll set out the linguistic data concerning mixed quotation. I lean heavily on previous expositions, such as Maier, 2014 and Cappelen and Lepore, 2007.

**Verbatimness, other languages and cleaning up.** Perhaps the defining feature of *direct* quotation is that it a true report requires the reportee to have said the sentence which is the complement of 'said'. Mixed quotation shares this feature with direct quotation, and as one would expect their behaviour is markedly similar. As Maier notes, sentences like the following are bad:

(206) # Quine said that quotation 'has an anomalous feature', but he didn't use those words.

However, the verbatimness requirement doesn't extend to the unquoted words. The following is fine:

(207) Quine said that quotation 'has an anomalous feature', but he called it 'citation'.

However, there's a range of ways in which it appears that the verbatimness requirement can be flouted. It's been noted that one can felicitously use a direct quotation of a sentence belonging to one language to report a speaker's saying a translation of it in another language. Thus we can say:

(208) Descartes said 'I think therefore I am'

This despite the fact that he didn't speak English and actually said 'cogito ergo sum' and 'je pense donc je suis'. We get the same thing with mixed quotations, as in:

(209) Kant said intuitions without concepts 'are blind'

Similarly, as Cappelen and Lepore note, we often ‘clean up’ direct quotations by removing pauses, mumbles, and false starts. The following is felicitous:

(210) Bush said ‘that position is preposterous’.

Even if he had actually uttered ‘well, umm that position, it’s perpostrous’. One can do the same with mixed quotation.

However, although one can, it’s not necessary that one clean up either direct or mixed quotations. One can use a direct or mixed quote to point out a speaker’s mis-speaking, or their use of a word belonging to their idiolect, even if it’s not part of the speaker’s idiolect:

(211) Bush spoke of his ‘ekullectic’ reading list.

(212) Meagan said she took out the bins ‘on accident’.

‘Ekullectic’ is not a word of English, but it can nevertheless be mixed quoted (and direct quoted). Similarly, ‘accident’, in my dialect, takes the preposition ‘by’, but in others it takes ‘on’ and I can use a mixed quotation to convey this. Note that indirectly quoted both are anomalous:

(213) # Bush spoke of his ekullectic reading list

(214) #Meagan said she took out the bins on accident

**Indexicals.** It’s a notable feature of direct quotation that indexicals in them don’t get their value from the context of utterance, leading some (for example Cappelen and Lepore) to label them Kaplanian monsters. If I say

(215) John said ‘nobody likes me’

I have not thereby said anything about myself: the ‘me’ doesn’t refer to the speaker of the utterance. We get the same thing with mixed quotation:

(216) Hillary Clinton said Biden ‘has always looked out for me’

**Dequotation**<sup>4</sup>(Shan, 2010). We can suspend direct quotations by putting things in brackets. This is seen most clearly in certain indexical shift cases, as for example:

(217) Joan said ‘it annoys [her] when you say that’

This can be used to report Joan’s utterance of ‘it annoys me when you say that’. Recent theorist have attached quite a lot of importance to dequotation. Maier calls it ‘the dual’ of mixed quotation, but it seems to me rather that it’s simply the same phenomenon, and so an adequate theory of the former must be able to capture it.

**Syntactic agreement.** An interesting more recently observed data point concerns morphological agreement in languages more inflected than English: it’s sometimes necessary that one adjust quoted material to agree with unquoted material. Here’s an example from Shan. If Ken says:

(218) Gli uomini italiani mi sembrano molto carini

Since ‘uomini’ is m.pl, so is the adjective ‘carini’. But if one wanted to mixed quote Ken, using a different word than ‘uomini’, that different word would have to agree:

<sup>4</sup>Maier calls this ‘unquotation’ but I prefer to use ‘unquote’ and its cognates for different purposes.

- (219) Ken ha detto che le persone italiane “mi sembrano molto  
 KEN HAS SAID THAT THE PL PEOPLE FPL ITALIAN ME SEEM VERY  
 carine”  
 CUTE FPL  
 Ken said Italians “seem very cute to me”

Maier has shown something similar for Dutch. While main clauses in Dutch are SVO, subordinate clauses are SOV. Thus we have the main clause:

- (220) Ik zal die idioot een koekje van egen deeg geven  
 I SHALL THE IDIOT A COOKIE OF OWN DOUGH GIVE  
 I’ll give that idiot a taste of his own medicine

To report this indirectly, we say

- (221) Ken zegt dat hij die idioot een koekje van egen deeg zal geven  
 KEN SAID THAT HE THE IDIOT A COOKIE OF OWN DOUGH SHALL GIVE  
 Ken said he’ll give that idiot a taste of his own medicine

And not:

- (222) Ken zegt dat hij zal die idioot een koekje van egen deeg geven

Maier reports that mixed quotation follows the same pattern. We must say:

- (223) Ken zegt dat hij "die idioot een koekje van egen deeg zal geven"

And indeed, one cannot say quote verbatim what the speaker said, on pain of ungrammaticality.

**Going Beyond ‘Said’.** There are cases in which we get mixed quotation-like behaviour either in the absence of the verb *said*, or in its presence but when the person whose speech is being reported didn’t say the quotable item. Here’s an example of the first phenomenon from Maier:

- (224) Quine was interested in quotation, the ‘anomalous feature’ of which intrigued him.

And here’s an example of the second from (Potts, 2007):

- (225) A: The Godfather 2 was a total snooze  
 B: Well Pauline Kael said this ‘total snooze’ was one of the defining moments in American cinema

**Summary** An adequate theory, then, must account for the verbatimness requirement of mixed quotation. In so doing, it will get the behaviour of indexicals for free. But it also, if possible, must say something about the various ways in which the verbatimness requirement is flouted, such as cleaning up, translating, and morphological agreement. It would also be desirable to have an account of dequotation and the cases not involving said. The theory I’ll present can, I think, capture most of this (with the exception of translation which all theories struggle with).

## 8.4 Extant Theories assume The Use Assumption

The aim of this section is to show either by direct quotation, or by describing their theory, that the six theorists mentioned in the introduction, whom I take to be a suitably representative sample, have been committed to the Davidsonian idea that quoted words in mixed quotations supply their unquoted semantic value (in the mouth and from the context of the reportee) to the composition procedure, which is to say they've been committed to the Use Assumption.

We've already seen Davidson say that the quoted words are obviously used. Next we have (Recanati, 2000), who says:

In mixed quotation the demonstrated material is, at the same time, in active use, instead of being issued for the sole purposes of the demonstration (p. 199)

If we allow that being in active use implies supplying its unquoted semantic value, then my problem will be one for Recanati too. Turning now to (Cappelen and Lepore, 2007), we see the same thing. On their story, a sentence like our leading one is true provided Quine said the quotable item 'has an anomalous feature', and predicated 'what corresponds to [it]' (their phrase: p135) of quotation. While they don't explicitly spell it out, it seems clear that what corresponds to "has an anomalous character" is simply the property denoted by its unquoted constituent, in this case  $\lambda x. x$  has an anomalous character'. Moreover, if they are to account for indexicals, they will need to say that what corresponds to an utterance by Quine of "is strange to me" is  $\lambda x. x$  is strange to Quine. So we can add Cappelen and Lepore to the list.

For (Shan, 2010), we don't get any neat quotes, so let me spell out the basic features of his theory. He is, for the most part, concerned to get dialectical uses, which we've already encountered above, right. Here's his leading example:

(226) Bush spoke of his 'ekullectic' reading list.

His view is that quoted words denote functions from languages to meanings. So, for example, the word *ekullectic* relative to Bush English stands for  $\lambda x. x$  is eclectic. He gives a semantic rule for quoting expressions which takes the quoted expression, plugs in a contextually salient language, and then composes the content with the rest of the sentence. For example, in the above sentence, one will supply Bush English to  $[[ekullectic]]$ , and then compose that with  $[[reading list]]$  by predicate modification in the standard way. It's quite clear, then, that the quoting expression supplies a standard semantic content to the sentence, once one has resolved the context sensitivity induced by the fact that quoted expressions need a contextually salient language parameter. So for Shan, too, with a slight wrinkle, a quoted expression is used to mean what the speaker meant by it.

Turning now to (García-Carpintero, 2012), things are a bit more nuanced. On his view, it is indeed the case that the main content of a mixed quoted sentence is the result of the composition of the semantic values of the used expressions with the rest of the sentence. He holds that the metalinguistic flavour of mixed quotation is rather conventionally implicated (here he is influenced by the work mainly on scare quotes in Predelli, 2003). In fact, he holds that there is two levels of implicated content. An utterance of one of our example sentences will put the following contents on the table

(227) Hilary said Biden 'has always looked out for me'



**Asserted Content.** Hilary said Biden has always looked out for the author of this paper

**Pragmatically-Enriched conventionally implicated Content.** Hilary uttered the words 'has always looked out for me'

**Conventionally Implicated Content.** Some contextually salient speaker uttered the words of which 'has always looked out for me' is a contextually adequate version

The thought is that we can account for the felt fact that in uttering our sentence, the speaker is *not* talking about me by appealing to the other levels of content: when we realise the asserted content is implausible, we appeal to the other levels to patch it up (I'm here paraphrasing García-Carpintero (2012, p222-223) which isn't maximally perspicuous, although the guiding idea is clear enough). It seems fair enough, then, to attribute to García-Carpintero the Use Assumption.

The most worked out theory to of mixed quotation to date, Maier, 2014, is also the most explicit. Maier holds that mixed quotation is a presuppositional affair. The presupposition of a sentence containing a mixed quotation is that someone salient (normally, but not necessarily, the subject of a speech verb) said the quoted words, while the at issue content of the quoting expression is what that person meant by the quoting expression. So, for example, it will give the result, for our leading sentence

**Presupposed:** there's a property P such that Quine meant P by 'has an anomalous feature'

**At issue:** Quine said that quotation is P.

It is obvious, from the above sketch, that Maier assumes The Use Assumption.

## 8.5 Problems For The Use Assumption

### 8.5.1 Problem 1: Speaker Meaning and Semantic Meaning

The Use Assumption has been popular. In this section I want to argue that it is problematic: assuming that the compositional semantic value of a quoting expression in a mixed quotation is what the reportee (or some other salient speaker) meant by it leads to mispredictions.

The basic problem is easy to state: it's important to draw a distinction between what a speaker said on an occasion, and what she intended to say, but the Use Assumption obliterates this distinction. To see this, let's consider some cases. Imagine Joan is colourblind, and mistakes green for red: she calls green things 'red' and red things 'green'. She looks at a clearly green traffic light, and utters 'the light is definitely red'. Now consider the following:

(228) Joan said the light was 'definitely red', and she spoke truthfully.<sup>5</sup>

This sentence, pretheoretically speaking, is false. But the Use Assumption predicts that it's *true*. After all, what "red" means is what the speaker meant by red, and Joan means green by 'red'. And the light *was* green.

Let's consider a reply to this problem. It seems we could fix the problem quite easily by changing the Use Assumption so that it doesn't make explicit reference to the

<sup>5</sup>I thank Josh Dever for giving me examples of this form

speaker, but rather, say, the speaker's community: quoting expressions in mixed quotations mean what the community in which the speaker is embedded would have meant by an utterance, at the same time as the speaker's utterance, of the quoted material. The problem with this reply is that it fails to accommodate one of the main data points concerning mixed quotation, namely the possibility of quoting expressions from other dialectics, strange portmanteaus, and misspeakings, as for example:

- (229) Palin said she 'refudiated' the claim  
 (230) Bush spoke of his 'ekullectic' reading list

The problem here is that we're using mixed quotation precisely to draw attention to the reportee's using an expression which *doesn't* belong to their community's dialect. Accordingly, the patched up version of the Use Assumption again leads to anomaly in very basic cases: since there's nothing Palin's community means by 'refudiated' (at least before her saying it was publicised), the quoting expression would supply no semantic content to the sentence, and its composition would crash.

Here's another problem. Occasionally one says things without meaning anything by them. Imagine I'm an English teacher, teaching children new words. I list the words at the start of the day, then use them throughout lessons, and give points to children who notice them. Occasionally, I introduce a made up word, such that if a child claims to notice it as a word I listed, he or she loses points. Accordingly, I say:

- (231) The class is barremious

Knowing that 'barremious' isn't a word, and meaning nothing by it. It's perfectly fine for a student later to report:

- (232) The teacher said the class was 'barremious', but I knew it wasn't a word.

But proponents of the Use Assumption must predict it to be semantically anomalous, since I didn't mean anything by the word, and it will supply nothing to the semantic composition procedure. This again is a bad prediction, as the sentence is clearly true.

The Use Assumption must tie the content of a quoting expression too closely to what the speaker means, and thus mispredicts when the speaker is either mistaken about what the quoted expression means, or when the speaker doesn't mean anything by it. We should thus explore theories which don't make the assumption.

## 8.5.2 Problem 2: Mixed Quoted Imperatives

One can't indirect quote imperatives. Recall from the introduction the following, reporting a mother's injunction that her child not climb a big mountain:

- (233) \*Your mum said don't even try to do that!<sup>6</sup>

We can plausibly understand the claim that one can't indirect quote imperatives as saying that the semantic value of an imperative, concerning which I can remain completely

<sup>6</sup>Of course this improves markedly if we add a 'to' before 'shut', but it seems reasonable to claim that such infinitival clauses aren't imperatives.

agnostic<sup>7</sup>, can't enter into the composition of the semantic value of the complement of a speech verb. It's equally uncontroversial that we can direct quote imperatives:

(234) Your mum said 'don't even try to do that'

But it's not been sufficiently realised that we can mixed quote imperatives. Here's an example. Imagine it said later on, when the family had returned from the mountain, and the child was trying to deny they'd been banned from climbing:

(235) I remember well: your mum said 'don't even try' to climb the mountain

This, I take it, is a fine utterance, with clear truth conditions, given below:

- ☺ The mother said 'don't even try to climb the mountain'
- ☺ The mother said 'don't even try to do that', in a context where *doing that* will be understood as *climbing the mountain*
- × The mother said 'don't think about climbing the mountain'
- × The mother said 'you shouldn't climb the mountain'

This shows The Use Assumption to be incorrect, and thus any theory which rests upon it. For were it true, then the imperative would be used, and would enter into semantic composition with the rest of the complement clause. And so 236 should be anomalous, just as 233 is. But it isn't.

Let's consider some possible responses to this data. A quite natural thought could be that this is a mistranscribed direct quote, and is correctly represented as so:

(236) I remember well: your mum said 'don't even try to climb the mountain'

This would have the unfortunate effect of making the sentence go false, since the mother's original utterance was 'don't even try to do that'. And this is problematic just because the original sentence seems straightforwardly true. However, one could perhaps reply that the speaker is being sloppy (despite her initial asseveration), and that contrary to what it seems the original is in fact just false.

This strategy becomes more implausible once one considers other cases. Consider the following: it's the day of a big campus protest, and the dean had warned us by email last week not to attend. We address him:

(237) Last week, you said 'don't even attempt' to come into college today (but we have a right to be here and so we are.)

Attempting the fix-up strategy mentioned above would yield:

(238) Last week, you said 'don't even attempt to come into college today' (but we have a right to be here and so we are.)

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<sup>7</sup>Actually, this isn't strictly true. A view like Kaufmann's, on which imperatives are a sort of modal and so simple sentences containing them denote propositions, won't have a problem with this data. On the other hand, such theories are faced with a sort of converse problem, namely explaining why indirect quoted imperatives are bad. For further criticisms of Kaufmann's position, see Starr, na.

Because the indexical 'today' is inside the quotes, it can only pick out the day, last week, of the dean's admonition. This reports a completely different warning on the part of the dean: it's a bad feature of a semantic theory to pull apart actual and felt truth conditions in such a violent way as the fixing up strategy advises, and so we should reject that strategy.

Let's turn to another worry. One might have concerns the fact that imperatives in mixed quotations don't show up with much frequency, and often sound awkward when one tries to manufacture them. For example, one might think:

(239) Quine told Davidson: 'Study mixed quotation' or he'd dump him as a supervisor.

Sounds slightly strained (although from discussing this in seminars, people are generally OK with it). However, even if it is strained, I think there's a way of explaining its strainedness. Note that it's plausible that there's a covert expression denoting the addressee in the case of imperatives. We might have, then:

(240) Quine told Davidson: '[you] study mixed quotation' or he'd dump him as a supervisor.

But now note the following with an explicit 'you':

(241) My supervisor said 'you are banned' from contacting her on the weekend.

It strikes me that this is about as awkward as 240, but it contains no imperative. And there's a plausible explanation for why such sentences are to be avoided: working out the referent of all the shifting pronouns takes a bit of mental effort, and speakers try to minimise the mental effort required to understand their utterances. Accordingly, we have an explanation for the strainedness and accompanying infrequency.

## 8.6 My Proposal

As announced in the introduction, I shall analyse mixed quotation, and indeed indirect quotation, in terms of direct quotation. The first step is to say how I understand the semantics of quoting expressions.

But first I want to reiterate something I said in a previous section. There I noted that it was a matter of controversy both how and to what quoting expressions referred. These I take to be primarily problems for the theory of *pure* quotation. Now if one subscribed to the view according to which mixed quotation was a mixture of direct and indirect quotation, and direct quotation was a species of pure quotation, such questions would require an answer before one could treat mixed quotation. But as I said earlier, I do not subscribe to such a view. Rather, I suggest that mixed, direct, and indirect quotation be given one treatment

This allows me some freedom: I do not have to answer how it is that quoting expressions come to have the semantic values they do: whether they name, demonstrate, or describe them. Moreover, I need not assign them semantic values which will make sense of the pure quotational cases, since on my view pure quotation is a separate phenomenon requiring a separate treatment.

With that said, then, let me say what I think quoting expressions denote. I follow Shan, 2010 in thinking that quotation is a language-sensitive phenomenon. Just as, when we're doing intensional semantics, we relativise semantic values to possible

world and time, so I think that the semantic value of a quoting expression is relative to a language. An intuitive justification for this claim would be sentences like:

(242) In English 'queue' picks out queues, whereas in French it picks out tails

The thought would be that we could think of 'In English' as something like a modal operator which shifts the language under consideration, just like various expressions (tenses, auxiliary verbs, certain adverbs, and conditionals) shift the world or time. If this is so, then the semantic value of a quoting expression should encode enough information about what the quoted word means relative to different languages. So, for example, relative to English, "queue" means  $\lambda x.x$  is a queue, while relative to French it means  $\lambda x.x$  is a tail. Relative to every other language, it will map to nothing.

The simplest way to capture this formally is to have quoting expressions simply denote functions from languages to semantic values (in a more complicated treatment, it would be a function from languages to Kaplanian characters, but I'll suppress that complexification for the most part. Note also that for the purposes of exposition, I'll ignore world and time sensitivity.). For example, we say that quoted 'quotation has an anomalous feature' maps a language to what 'quotation has an anomalous feature' means in that language. If we then say that quoted 'quotation' maps a language to what 'quotation' means in that language, then we can deduce in a familiar way that quoted 'has an anomalous character' should be a function of type  $\langle l, e \rangle, \langle l, t \rangle$  defined as so  $\lambda c_{\langle l, e \rangle} . \lambda l. [c(l)$  is what 'has an anomalous character' means in  $l]$ . Of course, this means that strictly speaking, such an expression isn't a function from a language to a content (which would be of type  $\langle l, e, t \rangle$ ) but it's close enough, and it allows us to tell a neat compositional story.

If we make this move, then we could suggest a simple intraquotation composition rule like the following:

- $[[ \text{"} \sigma \tau \text{"} ] ] = [ [ \text{"} \sigma \text{"} ] ] ( [ [ \text{"} \tau \text{"} ] ] )$

That is, so to speak, we distribute the quotation marks of a complex quoted expression across its simpler parts. This seems reasonable, albeit as a first step: note that I haven't said anything informative about the syntax of quotation (for example, does the above rule indicate that at LF, a complex quotation is just a series of simple quotations?). Nor, in this thesis, do I intend to; I leave that for later work, or more capable syntacticians.

This, then, is what we should say about quotations, both simple and complex. The problem of mixed quotation is now acute, however. We need to give an account of how quoted and non-quoted expressions interact. Consider our original example, repeated:

(243) Quine said that quotation 'has an anomalous feature'

Here is my view: unquoted words in mixed quotations are existential quantifiers over words, and in particular a word like 'quotation' is a quantifier whose values are words which mean quotation, like 'quotation', 'citation', and so on. So, 'quotation' really has two meanings: we have a violation of semantic innocence. I'll account for this formally by assuming an operator in the syntax QUOT which maps normal meanings to quantifier-over-word meanings. We'll have schematic logical forms like:

- $[S [DP \text{ Quine}] [VP [V \text{ said}] [S[DP \text{ QUOT quotation}] [VP 'has an anomalous feature']]]]$

I'll assume the analysis of quantifiers provided by generalised quantifier theory, according to which a quantifier is a set of properties: for example, the quantifier denoted by 'a dog' is the set of properties possessed by at least one dog. In this case, it will be the set of properties possessed by at least one word which means quotation. In symbols, where  $P$  is a variable which stands for properties of words:

- $\llbracket \text{QUOT quotation} \rrbracket = \lambda P_{\langle \langle l, e \rangle, t \rangle}$ . There's a word  $w$  and language  $l$  such that  $w(l) = \llbracket \text{quotation} \rrbracket$  and  $P(w)$ .

Were we to attempt semantically to compose this with 'has an anomalous feature', a familiar type mismatch would result: 'has an anomalous feature', a  $\langle l, e \rangle, \langle l, t \rangle$  will look for a  $\langle l, e \rangle$  to compose with, but find instead an  $\langle \langle l, e \rangle, t \rangle, t \rangle$ . This mismatch will result, following the standard theory of Heim and Kratzer, 1998, in movement and the leaving behind of a trace and a co-indexed lambda binder, as so (where I omit the syntactic parsing to aid readability):

- $\llbracket \text{QUOT quotation} \rrbracket [\lambda 1. \text{Quine said } w_1 \text{ 'has an anomalous feature'}]$

The final bit of information needed at the moment is the semantic value of 'said'. It will be a function from sentence meanings, that is functions from languages to truth values, and speakers to truth values. It is as follows<sup>8</sup>:

- $\llbracket \text{said} \rrbracket^h = \lambda S_{\langle l, t \rangle} . \lambda x. x \text{ uttered } S$ .

With all this in place, things go smoothly: we compose the quoting expression with the trace, giving an expression which, relative to an assignment, denotes a sentence. It in turn composes with 'said' and then 'Quine'. We remove the assignment sensitivity by lambda abstracting, yielding a property of words which maps a word  $w$  to true if Quine said 'has an anomalous feature'( $w$ ). We then apply this property to the quantifier over words denoted by  $\llbracket \text{QUOT quotation} \rrbracket$ . A more rigorous derivation is consigned to an appendix, which also deals with a couple of more tricky cases.

However, before ending this section, I want to consider a natural and interesting generalisation of the above theory. It is that we can treat indirect quotation as a limiting case of mixed quotation, in which all the words are unquoted. Thus for a sentence like:

(244) Quine said that quotation has an anomalous character

We can give it the following analysis:

- $\llbracket \text{QUOT [that quotation has an anomalous character]} \rrbracket \lambda 1 \text{ Quine said } s_1$

Here  $\llbracket \text{QUOT [that quotation has an anomalous character]} \rrbracket$  will denote a generalised quantifier whose values are sentences, which is to say it will be of type  $\langle \langle l, t \rangle, t \rangle, t \rangle$ . The whole will be true provided there's some sentence which means what 'that quotation has an anomalous character' does, such that Quine said it, which seems like a good result.

<sup>8</sup>It of course sounds a bit strange to say that speakers utter functions: one might think that utterance is a phenomenon involving more concrete things, like sounds and marks on paper. But I think similar things can be said for belief: we surely think belief is a phenomenon involving mental states or brain states in some way, yet we don't blanch at saying the objects of beliefs are sets of worlds, or tuples, for example. We have no objection to the fact that our semantics for belief doesn't make mention of brain states; in the same vein, we shouldn't object that our semantics for saying makes no mention of sounds or inscriptions.

## 8.7 Assessment of My Proposal

We can now consider how my view deals with the data introduced in the second section. It should be clear that I get **verbatimness**. By assimilating mixed to direct quotation, which has a verbatimness requirement, I more or less get this for free. That this is so, I take it, is the primary advantage of my theory, since the verbatimness requirement with regard to the quoted material is one of the absolutely fundamental data points a theory of mixed quotation must account for. Other theories, such as that of Maier, to take the most recent, require (rather complicated) stories about presuppositions in order to account for verbatimness.

I do not get the behaviour of quotation with regard to **other languages**. My theory predicts that the following, repeated from earlier:

(245) Kant said concepts without intuitions 'are empty'

Requires that Kant said or rather, wrote 'are empty', but he didn't (he wrote, of course, 'sind leer'). I take solace, however, in the fact that other recent theories struggle here: indeed, it seems that this data point is fundamentally in tension with the verbatimness requirement, and a theorist must chose one to capture, and explain away the other. So I do, although I don't have an explanation for this at the moment.

Regarding the **cleaning up** data, typified by Bush's

(246) Bush said 'that position is preposterous'.

When what he said was full of pauses, this doesn't pose me any problems. I can just say that things like pauses and stumbles are extra semantic, and so in particular to utter 'that position uhh it's perpostrus' just is to utter 'that position is preposterous'. Such things are not to be captured by our semantic theory, in the same way that someone's saying something in a squeaky voice isn't.

My theory does well with **other dialects**, which have been important for recent theorists. For a sentence like

(247) Palin said she 'refudiated' the claim

I propose to follow Shan in holding that 'refudiated' is a bona fide word of Palin English, and so that the quoting expression denotes a function which maps Palin English to  $\lambda x.\lambda y. x$  refuted and repudiated  $y$ . and other languages to nothing. With that said, my theory can deal with such cases just as easily as the simple cases. Recall that theorists wedded to the Use Assumption struggled with cases in which a speaker didn't mean anything. I can deal with such cases provided I allow merely possible languages into the domain of the function quoting expressions denote. If I then assume that for any string, there's some possible language in which it means something, and also that for any pair of strings, there's a language in which they don't mean the same, which are both, in fact, reasonable assumptions given the arbitrary connection between a word and its meaning, I'll get the result that our 'barremious' will denote a function which maps at least one language to some meaning, and that there are no two nonsense words with the same intension, and so I'll have no problem dealing with such cases.

One might think this is somewhat precarious: we ignore 'uhhhs' as extra-semantic but allow 'refudiated'. But it strikes me there's a principled enough reason for doing so: in uttering 'refudiated' Palin certainly means something in a way which she doesn't when she says 'uhh'.

As concerns **indexicals**, it should again be clear I'll get the correct predictions out of the box for a sentence like:

(248) John said nobody 'likes me'

I can tell exactly the same story as above: there's no worry about monstrosity, since the indexical isn't used.

As concerns **syntactic agreement** I don't have much of interest to say. What I propose is that when we have an expression type trace which, relative to an assignment, denotes an expression of a different case or number to the quoting expression it combines with, there is some sort of syntactic procedure which repairs the mismatch by forcing the quoted material to be in consonance with the features of the expression denoted by the trace. **Dequotation** is not a problem for me. This shouldn't be surprising, since dequotation is a sort of quantifying into quotation, and my whole theory is based around this idea. Thus:

(249) Bush said 'I distrust [Obama]'.

Will just receive more or less the same treatment. We'll have a logical form like:

- [QUOT Obama]  $\lambda 1$  Bush said 'I distrust [ $w_1$ ]'

One should note that this logical form is slightly different to the one given above for our leading example, which was:

- [QUOT quotation]  $\lambda 1$ . Quine said  $w_1$  'has an anomalous feature'

I cannot account, out of the box, for the mixed quotational phenomena which **go beyond said**, as Maier and Shan are able to do. Consider again Pott's example:

(250) A: The Godfather 2 was a total snooze  
 B: Well Pauline Kael said this 'total snooze' was one of the defining moments in American cinema

My proposed analysis for such cases is the following. We propose a rule to the effect that whenever one finds a mixed quote without *said*, one finds a contextually salient language, plugs it in, and then goes on with composition. So, in particular, in this case one will plug in A English, yielding  $\lambda x$ .  $x$  is boring, which will then compose with the determiner 'this' as normal. This strikes me as a simple and principled analysis, and so although I don't get these cases for free, they cause me no problem.

Overall, then, my theory does pretty well at satisfying the empirical requirements of a theory of mixed quotation. Moreover, it provides a satisfyingly unified theory of direct, indirect, and mixed quotation. And it doesn't face the problems other theories do. Let me now turn to six ultimately unsuccessful objections to my view. Objection one is the following. Consider as simple a sentence as:

(251) "cat" has three letters

My theory cannot account for it: functions don't have letters. This might seem like a problem: any theory of quotation, one might think, must be able to deal with the simple cases like the above. But this isn't so. Although my theory cannot account for pure quotation, it can account for each of direct, mixed and indirect together. Not only that, but we saw above that any theorist is committed to two theories to cover all the quotational phenomena. Either, like me, a theory for pure and a separate one for indirect, direct, and mixed, or, like my opponents, a theory for pure and direct, a



separate one for indirect, and a mixed one for mixed<sup>9</sup>. The upshot is I can't capture the above sentence, but am at liberty to bring to bear a completely different theory to do so. Having done so, both theories will be on a par, in terms of theoretical commitment, while I will be ahead, because I have a successful theory of mixed quotation.

However, the above sentence leads to another worry, objection two<sup>10</sup>. Consider the following:

(252) "Roses are red" is a sentence of English and John said it

On the way I've divided things up here, we have a pure quotation, and an anaphoric pronoun which picks up on it. But this anaphoric pronoun is functioning as the syntactic object of *said*, and so, according to my view, it is of the type of direct quotation. So it seems as if my attempt to remain agnostic about pure quotation is doomed to failure. But I think there's a simple response to this problem. The first thing to note is that problems like this are ubiquitous in the theory of quotation. For example, it's well known that one can anaphorically refer back to an expression in quotation marks, as in:

(253) 'Hillary will win' is a sentence which we'll be hearing a lot more, especially if she gets a good running mate.

The puzzle here is that it's unclear how the 'she' can pick up Hillary as referent, since the occurrence of 'Hillary' which appears to be functioning as 'she's antecedent is quoted. But really, it needn't be much of a puzzle: we can say that just by having her name mentioned, Hillary becomes sufficiently salient for a subsequent pronoun to refer to her *deictically*. Similarly, I can say that in our problem case, it denotes a function from languages to contents are usual, and that this function is made salient by the previous pure quotation, even if that pure quotation doesn't denote a function from languages to content.

A third worry concern the differing requirements of indirect and direct speech reports<sup>11</sup>. An indirect speech report places many fewer constraints on what the speaker actually said than a direct speech report. Consider the following mundane example from Cappelen and Lepore, 1997. Person A says:

(254) I bought an expensive pair of Bruno Magli shoes

I can completely felicitously report his speech as so, leaving out the word 'expensive':

(255) A said he bought a pair of Bruno Magli shoes

For me, the truth conditions of the sentence are such that it's true provided there's some sentence which means what 'A bought a pair of Bruno Magli shoes' means such that A said it. But, one might think, there is no such sentence: A only said a sentence which means he bought an *expensive* pair of Bruno Magli shoes. My response to these cases is to retreat from the strict demand of identity of meaning. It is not the case that

<sup>9</sup>Incidentally, this enables us to see where Quine's quoted claim, in the introduction to this essay, goes wrong. There he says that a mixed quotation must be used, because otherwise it would function as a singular term, and so the sentence as a whole would be a typemismatch. It's plausible that pure quotations function as singular terms, but it's open to me to point out that this just doesn't hold for mixed quotations. Quine's argument only works if one assumes one theory to capture everything quotational, but we've already seen it's implausible to assume that.

<sup>10</sup>Thanks to Herman Cappelen

<sup>11</sup>Thanks to Torfinn Huvenes.

the values of the quantifier over sentences must contain only sentences which literally express what the complement does. Rather, there's some looseness: it's sufficient for them to express roughly the same thing, and this can be heavily guided by contextual factors and pragmatics. As Cappelen and Lepore put it, where the samesay relation is that born between the complement of an indirect speech report and the values of the existential quantifier over sentences which, according to my theory, verify the report :

[Examples such as the shoe one] establishes that the perfectly viable practice of indirect speech requires the samesay relation to be broader than [identity]... it's no role for semantic theory to place a priori constraints on what can samesay what. Whether two utterances samesay each other often depends on nonsemantic considerations. Competent speakers of English, those who competently use the "says that" locution, are able to render such judgments. Having such competence consists, in part, in being able to judge whether a given report is correct or incorrect, accurate or inaccurate, misleading or exactly what the speaker said. Cappelen and Lepore (1997, p291).

This is of course a small foray into a large topic, but I hope what little I have said shows that such examples need not be fatal to my analysis.

The fourth worry is the following. One might have the concern that I've, as it were, *de-semanticised* indirect quotation by making it like direct quotation. A standard propositions based account, for example, can easily make sense of such obvious arguments as the following:

(256) John said that snow is white; snow is white; so John said something true

It can do so because the that-clause, being propositional, has informational content: it places conditions on how the world must be (snow must be white). It can thus interact with other informational content bearing pieces of discourse such as assertive utterances to explain the goodness of the above argument. It's less clear on my theory how this goes. After all, the first premise merely asserts that John uttered a sentence of a certain sort (one, as it happens, that means that snow is white), and sentences aren't intrinsically contentful or information bearing: on my view, they are not contents, but functions from languages to contents. But it's this last fact that reveals the worry about de-semanticising is no problem. For even if sentences aren't per se contentful, content is nevertheless easily recoverable from them by supplying them with a language. To say something true, on this story, will not be to express a proposition, but to utter a sentence whose content, when the language the speaker is speaking is plugged in, is a true one. I would prefer to say that rather than de-semanticising indirect quotation, I have semanticised direct quotation.

And this leads nicely to our fifth objection. As it stands, my theory doesn't capture sentences in which the quoted complement to *said* is subsentential, as in:

(257) John said 'The winner of the race is...' before falling silent.

(258) Waking from his coma, John said 'Ughpl', clearing his throat.

According to the composition rules I've given, the quotation internal composition should fail. Roughly, the semantic value of the quoted 'The winner of the race is' will look for a determiner phrase which isn't there, causing a crash. But this is of course a purely fine utterance. Now I have a clear response in the offing: that this is a case of pure, rather than direct quotation, its appearance following 'said' notwithstanding. But this

might seem uncomfortable for me: one of the selling points of my theory was meant to be the unified treatment it gives 'said', but now it seems like it'll have to, in the end, be ambiguous as between taking sentence type meanings, and pure quotation meanings.

I agree it would be better to do without the ambiguity. But I think that the ambiguity I posit is better than that posited by rival theorists. And here the point about semanticising direct quotation becomes salient. Previous theories would have no trouble dealing with a sentence like the above, because their treatment of pure and direct quotation would be the same. But such a treatment, on my way of looking at things, obliterates the important difference between the above sentence and, say:

(259) John said 'The winner of the race is Jones'

This sentence reports, to coin a phrase, John's making a move in a language game: even if, on my story, what John says isn't per se contentful, nevertheless the overriding point of such utterances *is* to report John's putting forward something as semantically evaluable. And that's why it's bad, I think, to lump together such sentences with pure quotation cases like the ones above in which one is not reporting on the putting forward something as semantically evaluable. And in turn that's why I think the ambiguity I'm forced to posit for 'said' is not a bad one, because it tracks the important distinction between the semantic (mixed, direct, and indirect quotation) and the extrasemantic (pure quotation).

Let's now turn to our sixth and final objection. It is that I have worries about over-generation<sup>12</sup>. The following sentence, a simple variant on one of my key data points, is bad:

(260) \*Your mum said don't even attempt 'any monkey business'

It's bad, of course, just because one can't indirect quote imperatives. But my theory doesn't predict this: rather, it predicts that it's straightforwardly true provided there's some expression which means what the occurrence above of 'don't even attempt' does, and it, composed with the quoted material, yields a sentence which the mother in question said.

Thankfully, however, I think I can at least begin to sketch a reply to this objection. To see it, we need to ask what it is that explains the differential acceptability of quoted and unquoted imperatives, evinced by:

(261) Your mum said 'don't even attempt any funny business'

(262) \* Your mum said don't even attempt any funny business

I suggest that it's a syntactic matter. Note that the following is also bad:

(263) \*He said do I have a pen?

Although the following is fine:

(264) \*He said 'do you have a pen?'

One can't embed interrogatives under *said*. And there's a good explanation for this. An interrogative sentence introduces its own complementizer: the (simplified) underlying logical form would be something like (see, e.g. Carnie (2013, pp206ff))

<sup>12</sup>Thanks to Josh Dever

- [Q [TP [DP you] [VP have a pen]]]

It's generally assumed that *said* introduces its own optional complementizer, 'that'. We could then hold that a sentence like 263 is bad because a given phrase can't have two complementizers, but that 264 is fine because if a complementizer occurs in quotation marks, it is ignored. And we could then suggest that the same thing is going on here: imperatives have their own complementizer, which explains why they can't be indirect quoted, but can be direct quoted because then it gets ignored. While the exact details are matters for the syntactician rather than the semanticist, this seems plausible, and so objection six is defanged.

## 8.8 Back To Belief

The central result of this chapter so far, as it concerns the big picture of this part of the thesis, is that an environment thought previously to be at least sometimes intensional—the 'said' of indirect quotation—has been shown amenable to a hyperintensional analysis. As we've seen, it's a straightforward consequence of the move we made, contra previous theorising on the matter, when we treated unquoted words in mixed quotations as not supplying their regular content, but rather getting shifted to hyperintensional quantifiers over words.

I'll now argue that almost precisely the same move can be made here: the only difference is to the domain of the quantifier. Whereas previously we quantified over words, now we'll quantify over senses, as previously note, which we'll conceive of as functions from epistemically possible worlds to extensions. In particular, recall that a sentence like:

(265) Quine believes that the president is a democrat

Can have a *de re* or a *de dicto* reading. On the former, it seems to report a belief relating Quine and Obama, while the latter could be purely general, based on a hunch of Quine's about the result of a recent election. The analysis I propose is as follows. I make the Fregean assumption that words occurring under 'believes' denote their senses, and conceive of senses as functions from epistemically possible worlds to extensions. We capture *de re* readings by scoping the *de re* term out, and turning it into an existential quantifier over senses. So we'll have:

- [SENSE the president]  $\lambda 1$  Quine believes  $s1$  is a democrat

We can assume the same sort of sense composition rules that we did for words: thus, where  $s$  is a variable for epistemically possible worlds, we might have, for 'is a democrat' having the sense  $[\lambda I \langle s, e \rangle. \lambda s. I(s) \text{ is a democrat in } s]$ . Composition will yield a sentence true provided Quine believe something with the same sense as 'the leader of the free world is a democrat', or 'the commander in chief is a democrat'. It's not true, on this story, provided he believed something with the same sense as 'the president belongs to the party formed in 1828'. So we see that we can account for *de re* belief by treating it as a species of *de dicto* belief. And that means that a sort of construction which has been thought to mandate directly referential e-type expressions actually does not do so.

Not only that, but this view can capture the intuition that there's something object-involving about *de re* constructions. Although I argued that ultimately this idea, conceived of as a distinction between singular and general beliefs, is not one on which we

should rest too much semantic weight, nevertheless there *is* an intuition in the vicinity, and it would be nice to account for. I do so by means of domain restriction. Since on this view, our LF constituent '[SENSE the president]' is a quantifier, it can be restricted like one. So we could say that the object-involving feeling we get is a result of the quantifier being restricted to only a certain class of senses: those that, in the case above, pick out Obama in a particularly direct way.

We can, then, capture both the Fregean data concerning truth values, as well as as much singular thought as we should allow into the semantics, on my proposal. Singular thought, on this story, is nothing more than a particular type of domain restriction, and certainly doesn't motivate a metaphysical distinction between singular and general types of belief. I think this is a good result.

## 8.9 Conclusion

I have shown that we've been thinking about mixed quotation, and thereby quotation, and thereby opacity and transparency incorrectly. Properly understood, mixed quotation is not a mix at all: deep problems attend the attempt to make sense of how quoted expressions are meant to be used. I have suggested that mixed quotation is an instance of quantifying into quotation, contrary to the received wisdom which bans this, and presented a rigorous compositional semantics which bears this out. To key idea is that what was previously thought of as a case where an expression occurring in an oblique context (after 'said') provides its extension, is in fact not so: in the famous example, the thought went, 'quotation' is used and supplies the thing, quotation, to the composition procedure. This thought is wrong: 'quotation' actually supplies a quantifier over words. What we thought of as a construction which required expressions which refer to objects is in fact not so. I then showed how this could be turned into a neoFregean account of attitude reports, and in particular which presented a way to understand *de re* reports as simply being a species of *de dicto* report, and thus as not requiring, contrary to what is generally thought, directly referring e-type expressions.

## 8.10 Appendix

The aim of this appendix is to present a derivation of our main example, based on the simplified logical form assumed in the text, and repeated below:

- [QUOT quotation]<sub>1</sub> [ $\lambda 1$ . Quine said  $w_1$  'has an anomalous feature']

$w_1$  here functions as a trace the type of which is  $\langle l, e \rangle$ , and in general for any expression of type  $\langle \alpha, \beta \rangle$ , it will leave behind a trace of type  $\langle l, \langle \alpha, \beta \rangle \rangle$ . We'll assume the standard treatment of traces according to which they function like variables: they have a semantic value only relative to an assignment function. For at least clarity's sake, I'll assume that this variable assignment is different from the one which we use for the semantics of quantificational noun phrases and their e-type traces. I'll call it  $h$ .  $h$  is a function whose domain is the natural numbers and whose codomain is the set of functions of type  $\langle l, \alpha \rangle$  where  $\alpha$  is any type formed recursively from base types  $e$  and  $t$  in the standard fashion. In symbols, then, we'll have:

- $\llbracket w_1 \rrbracket^h = h(1)$

That said, let's look at composition. The semantic value of the complement is derived by straightforward function application of its parts:

- $\llbracket w_1 \text{ 'has an anomalous feature'} \rrbracket^h = \llbracket \text{'has an anomalous feature'} \rrbracket^h (\llbracket w_1 \rrbracket^h)$

If we then compose with 'said' and then 'Quine', we'll get:

- $\llbracket \text{Quine said } w_1 \text{ has an anomalous feature} \rrbracket = 1 \text{ iff Quine said } \llbracket w_1 \text{ 'has an anomalous feature'} \rrbracket^h$

The semantic value, next, of the branching node containing just the lambda binder and the above expression will be as so, where, in the familiar way, the lambda binder binds the free variable and forms a property from a sentence:

- $\llbracket \lambda l \text{ Quine said } w_1 \text{ has an anomalous feature} \rrbracket^h = \lambda v_{\langle l, e \rangle}. \text{Quine said } v \text{ 'has an anomalous feature'}$ .

Composition with 'QUOT quotation' will then yield

- $\llbracket \text{QUOT quotation } \lambda l \text{ Quine said } w_1 \text{ has an anomalous feature} \rrbracket^h = \text{there's some word } w_{\langle l, e \rangle} \text{ and language } l \text{ such that } w(l) = \llbracket \text{quotation} \rrbracket \text{ such that } [\lambda v_{\langle l, e \rangle}. \text{Quine said } v \text{ 'has an anomalous feature'}](w) = 1$

Beta Reducing:

- there's some word  $w_{\langle l, e \rangle}$  and language  $l$  such that  $w(l) = \llbracket \text{quotation} \rrbracket$  such that Quine said  $w$  'has an anomalous feature'

I.e.:

- there's some word  $w_{\langle l, e \rangle}$  and language  $l$  such that  $w(l) = \llbracket \text{quotation} \rrbracket$  such that Quine said 'has an anomalous feature'(w)

Since 'has an anomalous feature' is denotes  $[\lambda s_{\langle l, e \rangle}. \lambda l. \text{ what } s \text{ means in } l \text{ is what 'has an anomalous feature' means in } l]$ , when it's applied to  $w$  we'll get:

- there's some word  $w_{\langle l, e \rangle}$  and language  $l$  such that  $w(l) = \llbracket \text{quotation} \rrbracket$  such that Quine said  $\lambda l. \text{ what } w \text{ means in } l \text{ is what 'has an anomalous character' means in } l$ .

And this is the correct result, being true if Quine said a sentence which contains a word meaning quotation applied to the expression 'has an anomalous character'.

Let me end by adding a necessary complexification, namely a treatment of indexicality and variable binding. I have said that a word like 'quotation' quantifies over those words which mean quotation. I thereby took advantage of the fact that 'quotation' has a fixed meaning. But many words don't, and in particular indexicals and bound pronouns do not. Accordingly, it's at present a mystery how the theory here advanced can deal with cases such as:

(266) Joan said I 'have no social graces'

(267) Every soldier swore he 'will never betray his country'

The problem is that 'I' means something different relative to different contexts, and 'he' means something different relative to different variable assignments.

To modify for indexicality doesn't provide any real challenge, as far as I can tell. The following semantic value seems to work, where we now relativise semantic values to contexts, which I'll understand in the standard Kaplanian way:

- $\llbracket I \rrbracket^{h,c} = \lambda P. \text{there's a word } w \text{ and } l \text{ such that } w(l) = \llbracket I \rrbracket^{h,c} \text{ and } P(w)$

Bound variables are trickier. We need somehow to capture the fact that unquoted pronouns both induce semantic dependence on the word-type trace they leave, but also themselves semantically depend on the quantifier which, intuitively speaking, binds them. My provisional solution is to add an argument place to mixed quoted pronouns. I assume a derivation for our sentence as follows. Firstly, and as standard, the quantifier every soldier raise out:

- $[\text{Every soldier}] \lambda t2 \text{ swore he 'will never betray his country'}$

The presence of the second pronoun he in the quotational environment will trigger movement, and the leaving behind of a word type trace, and the transforming of 'he' into 'QUOT he':

- $[\text{Every soldier}] \lambda 2. \llbracket \text{QUOT he}1 \rrbracket. \lambda 1. t2 \text{ swore } w1 \text{ 'will never betray his country'}$

Finally, and here's the novelty, I'll assume that a trace bound by the quantifier ad-joins 'QUOT he':

- $[\text{Every soldier}] \lambda 2. \llbracket \text{QUOT he}1 \rrbracket t2]. \lambda 1. t2 \text{ swore } w1 \text{ 'will never betray his country'}$

If we then assume the following semantic value:

- $\llbracket \text{he}1 \rrbracket^{h,c} = \lambda x. \lambda P. \text{there's a word } w \text{ and } l \text{ such that } w(l) = x \text{ and } P(w)$

With this in place, composition should go smoothly: once we compose  $\llbracket \text{he}1 \rrbracket$  with  $\llbracket t2 \rrbracket$ , we'll get:

- $\lambda P. \text{there's a word } w \text{ and a language } l \text{ such that } w(l) = t2 \text{ and } P(w).$

And thereafter things will be straightforward<sup>13</sup>

Let me end with the treatment of quantification. Consider a sentence like:

(268) John said two people 'shouted'

I take it this has two readings: John said a sentence like 'two people shouted', or John said two sentences one of which might be 'Joan shouted' and the other 'Bill shouted'. The narrow scope reading is no problem for me: we simply raise out 'two people' leaving behind a word trace standing for a quantifier whose values are words which mean 'two people'. And the wide scope reading can be captured too. We could hold that what happens is that we wide-scope the quantifier 'two people' and then we repair the typemismatch. So first we have:

<sup>13</sup>There remains another outstanding problem with indexicality, naming none of the sentences in the domain of the quantifier of say 'John' will be indexicals. I would need to quantify over contexts too in order to get this: *there's a word w, language l, and context c such that....* As far as I can tell, this shouldn't cause too many problems, so I'll omit the details.

(269) [Two people]  $\lambda_2$  John said he<sub>2</sub> 'shouted'

Then, because that's a mismatch, we raise again, and make of the semantics for unquoted pronouns given immediately above:

(270) Two people  $\lambda_2$  [QUOT he<sub>1</sub>][t<sub>2</sub>] John said w<sub>1</sub> shouted

And this, a little inspection will reveal, will get the right truth conditions. While what I've given here is not quite a complete and rigorous formal semantics, I hope that it suffices as a proof of concept of the view of mixed quotation I've given in this paper.





## Chapter 9

# Conclusion

### 9.1 Introduction

The aim of this final, concluding chapter, is three-fold. Firstly, I want to provide a resume of the main positive results of this thesis, as well as the big picture lessons which I think we can learn from them. Secondly, I want to consider the prospects for developing *Stage Semantics*, and in particular for adding a treatment of other referring expressions beyond names. Third, I want to consider a couple of directions for further research the work indicates.

### 9.2 What We've Seen

In this thesis, with the support of the view I've termed metaphysical externalism, I have argued that our semantics should be sensitive to our metaphysics, just as natural kind externalism tells us it should be sensitive to natural kinds. And—assuming without argument the existence of temporal parts, and the non-existence, or at least non-importance, of enduring objects— I have presented a semantic theory that is so sensitive. In particular, I have given a semantics sensitive to the view that there are temporal parts, and that in a sense objects just are their temporal parts.

There are several things to note about this. Firstly, this means that my claim about the semantics I have presented is conditional: if objects are congeries of temporal parts, then you should like my semantics. I haven't directly argued for the truth of the antecedent. Secondly, I have presented a new way of thinking, from within the temporal parts framework, of what objects are. For the worm theorist, they are temporal parts stuck together thanks to unrestricted composition, and for the stage theorist they are stages the question of whose persistence rests on being suitably related to other stages (those which stand in the appropriate temporal counterpart relation). What the worm theorist accounts for by fusions, and the stage theorist accounts for with relations, I account for with predicates

Third, I have argued for a new form of predicativism. Unlike normal predicativisms, I don't think there's but one predicate 'John' which has as its extension those called John. Rather, for each person named 'John', there's a different predicate, which has as its extension John's stages. This idea, I have shown, can be embedded in a Pietroski-esque conjunctive semantics resulting, in my view, in the cleanest form of predicativism yet.

Fourth, a consequence of this is that names, generally thought to be the paradigm singular term, a view only partially challenged, in my view, by normal predicativisms, are shown to be through and through predicates. And this, I suggested, has important consequences for issues concerning singular thought and acquaintance: we can effect something of a rapprochement between the acquaintance and the anti-acquaintance

person by noting that perception is certainly a means by which one comes to understand predicates standing for spatio-temporal terms, but is certainly not the *only* way. Most importantly, however, what this shows, contrary to what is often thought, is that the semantics of names gives no reason to appeal to e-type expressions.

A similar lesson is to be learned from the second extended argument which comprises this thesis. Here I argue against there being a semantically relevant category of de re attitude ascriptions. I did so in a somewhat roundabout way, by concentrating on mixed quotation, as in:

(271) Quine said quotation 'has an anomalous character'

I argued, following recent work, we should view this as a paradigm of transparency in opacity, and that we should leverage an understanding of these constructions to develop an account of de re belief reports. And the overall positive thought here was that an expression which occurs transparently in an opaque context should be understood as a quantifier over certain intensional or hyperintensional entities. In particular, the occurrence of 'quotation' above should be thought of as a quantifier over words which mean quotation. I gave compositional rules that made it come out that the sentence was true iff Quine said—roughly—one of the words in the domain of the quantifier followed by 'has an anomalous character'. In the same vein, in

(272) Quine didn't know the queen *was* the queen

The first occurrence of 'the queen' functions as a quantifier over senses which have the same reference as 'the queen'. Making the innocent Fregean assumption that names occurring in attitude complements contribute sense rather than reference to the composition procedure, we get the result that it's true provided there's a sense in the domain of the quantifier such that when composed—in a way I showed—with the sense of the predicate yields something Quine didn't know. We can then, if we so wish, capture the fact that it feels like such a belief involves an object more strongly than a de dicto belief by holding that the domain of the quantifier is restricted so that it ranges only over certain more epistemically intimate or informative senses. And so we get the result that a de re belief report is just a more general instance of a de dicto report. It doesn't express some particularly epistemically or semantically direct object-involving belief. Given this, there's no need to appeal to semantically direct object-involving terms: that is to say, again, there's no reason to appeal to e-type expressions.

### 9.3 Extending The View

So there's reason to think, then, that e-type expressions are less important than is traditionally thought. A natural thought to ask is: how much less important? In the first part of the thesis I concentrated on names, to the exclusion of other purportedly referring expressions. Can what I said here be extended to indexicals, demonstratives, and descriptions?

I think the answer to this is possibly. Certainly adding in context sensitivity would not, in and of itself, prove hugely difficult (indeed, I provided a sketch of how to do so). What would be the challenge would be making the case that indexicals, demonstratives, and descriptions are to be assimilated to predicates. While the data concerning names being predicates is extensive and to my mind convincing, this is not so apparent for other referring expressions. We've already noted several times the unproductivity of indexicals and demonstratives post-determiner, as witness:

(273) # Every that is salient

(274) # No theys are allowed to say in the single room

On the other hand, of course, there is the Schoubye idea that there could be some sort of phi-feature transfer which would explain uses like:

(275) My cat is a she.

However, I suggested we be worried about how good this data was. And I still think that. With regards to definites and indefinites, one might think, the situation is even worse. After all, the following are beyond horrible:

(276) # Every a man likes potatos

(277) # Some the man likes potatos

However, in fact, the situation is far from so bleak. Delia Graff Fara (Graff Fara, 2001) puts forward what is to my mind quite a convincing case for the treatment of descriptions as predicates. I won't make her whole case here, but let's consider some interesting suggestive data. Firstly, she notes that a predicative, <e,t> analysis of the definite in the below makes analysis go by far smoothest, as we then have simply a case of predicate modification:

(278) He is tall, handsome, and the love of my life

To consider a case involving indefinites, we can note that the following does not give rise to scope ambiguities as quantifiers do:

(279) Aristotle was not a philosopher

This does not have two readings, and in particular it does not have a reading on which it reports that there's a philosopher such that Aristotle was not him. The existence of such a reading, made true as it is by, say, Kant, would make the sentence ambiguous as between true and false, which it clearly isn't.

So I think—although the case would have to be made in much more detail—that there's at least the possibility of extending my view to referential descriptions. Moreover, an important thing to note about this line of argument is that predicativism about a particular sort of expression doesn't stand or fall with its ability to occur in post determiner position. This opens up the space to find clever new ways for arguing that indexicals and so on actually contribute predicates. And that doesn't seem to me to be an impossible task.

## 9.4 Directions for Further Research

There are at least three ways in which I would like to use the results of this dissertation to reexamine certain philosophical problems.

The first concerns certain issues pertaining to what one might call 'descriptive metaphysics'. In (Strawson, 1959) Strawson puts forward a very interesting argument to the effect that having a conception of an external spatio-temporal world depends on our having the conception of particulars which can be reidentified as the same on different occasions despite being differently located on those occasions. Particular thoughts about objects play a special role in our thought about objectivity. Now this argument is

controversial, to say the least (some responses include (Campbell, 1993), (Burge, 2010), chapter 6). But even if one doesn't buy it, it seems hard to deny that particular thoughts about objects plays *some* important role in our conceptual scheme. However, it may seem that my view does without it: what one might have thought of as such thoughts turn out to be in fact existential quantifications asserting the coinstantiation of certain properties. Is this enough to do justice to the way we think about the world? I would like to explore this question.

The second concerns the philosophy of religion, and in particular Buddhist philosophy of language and metaphysics. A central Theravadin buddhist tenet is that an object is a mere congeries of parts. From a metaphysical perspective, there is no abiding thing over and above the changeable qualities or parts of an object. On the other hand, from a semantic perspective, we have terms which seem to pick out abiding things. The way that these two views, which appear to be in tension, are made consistent is by holding that names stand for all the parts or qualities, even though these qualities do not together compose an entity. Here is the case made, famously, for chariots:

"How did you come here, by foot or in a chariot?"

"In a chariot, venerable sir."

"Then, explain sir, what that is. Is it the axle? Or the wheels, or the chassis, or reins, or yoke that is the chariot? Is it all of these combined, or is it something apart from them?"

"It is none of these things, venerable sir."

"Then, sir, this chariot is an empty sound. You spoke falsely when you said that you came here in a chariot."

"Venerable sir, I have spoken the truth. It is because it has all these parts that it comes under the term chariot." (*The Debate of King Milinda*, p33)

There are two key moves made here: first, it is argued that nothing unifies the various spatial parts of the chariot into one whole. But then second, it's argued that this doesn't make any difference because to come under the term 'chariot' is just to be an ununified congeries of spatial parts.

Now let's add to that the central Buddhist claim that everything is impermanent and without an abiding ego. We can view this, I think, as the claim that there is nothing which unifies what we call an object's *temporal* parts into one. But again we can suggest that this is not a problem, because to be an object is just to be a congeries of temporal parts.

Oddly enough, it strikes me that the position I have put forward matches this view quite well. For me, there is no unified entity as secured by the worm theorist and his universal fusion. It seems quite reasonable to say that everything *is* impermanent: all we have are instantaneous temporal parts which fall together under the same term.

Analytic christian philosophy has seen something of a renaissance with the rebirth of metaphysics, and the new tools that can be used to approach it, such as modal logic. It strikes me that the same thing can be done for Buddhist philosophy, and I think this dissertation shows a way of doing so, and one which I would like to explore further.

The third concerns recent work in the metaphysics of content. Here it's been argued that the solution to the unity of the proposition is to be found in certain facts about representational entities such as sentences or mental acts (a representative recent publication is King, Soames, and Speaks, 2014). Arguably I make a somewhat similar move by arguing that certain semantic data previously captured by positing higher order semantic types is instead to be captured by appeal to different higher level relations

of reference. I'd like to explore just how similar this move in fact is, and whether it can be used to shed light on these debates.



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