

## TERM LIMITS REVISITED

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### 1. INTRODUCTION

I am going to revisit the principal empirical theses of “Terms Limits” (henceforth TL), which I wrote for the 1993 issue of this annual.<sup>1</sup> A particular research programme drove the original article, and although certain features were evident from the way I motivated the *de jure* rigidity of anything to be classed as a genuine singular term, the general architecture was suppressed as I didn’t have everything worked out clearly and worried that even a preliminary sketch would have impaired readability. The theses in question were meant to flow from very general facts about cognition filtered through what I now like to call an *act-syntactic* framework for semantic explanation. The details of that programme are set out elsewhere, and for present purposes it will suffice to highlight the relations I believe must hold between acts of meaning, saying, referring, and predicating, and what I take to be the most empirically fruitful way of characterizing the relation between referring expressions and acts of referring. This is done in §§2–5. The principal empirical thesis of TL—that every natural language noun phrase is either a semantically atomic, rigid referring expression or else a semantically non-atomic restricted quantifier is broken down in §6 into sub-theses of complexity scepticism and flaccidity scepticism that can be redescribed in act-syntactic terms. The distinction between perspectival and non-perspectival acts of referring is addressed in §7 and the traditional relation between indexicality and perspective is stressed. The nature of reference itself and the status of Kripke’s notion of rigidity within an act-syntactic framework are the principal matters of concern in §§8–11. §§12–14 stress the distinction between conceptual theses about language and empirical hypotheses about natural language, and its importance to a certain type of binding argument against complexity scepticism, the action centred on demonstrative descriptions (expressions of the forms ‘that  $\phi$ ’ and ‘this  $\phi$ ’). Complexity scepticism and flaccidity scepticism themselves are revisited

in §15–17, where facts about the nature of referring and a property of certain classes of expressions are brought together to explain the relationship between semantic structure and *de jure* rigidity in a way that is both more flexible and more principled than the one found in TL. §18–19 address an argument from recursive structure that, if successful, would spell the end of both complexity scepticism and flaccidity scepticism. §§20–22 revisit the general problem for the complexity sceptic caused by definite and demonstrative descriptions.

## 2. MEANING, SAYING, AND IMPLYING

We *mean* things, we *say* things, we *imply* things. These are acts people perform when they speak or write (though not every time). A distinction between what speakers *say* (*state, claim, assert*) when they speak and what they (“merely”) *imply* (*suggest, hint, insinuate*) by saying what they say is an intuitive one.<sup>2</sup> It is illustrated by the following statements about a speaker, Smith, who utters the sentence ‘I am tired’ to a group of friends at his home late one night:

- (1) By uttering, ‘I’m tired,’ Smith said that he was tired
- (2) By saying that he was tired, Smith implied that we should leave.

I do not want to pin too much on this here, but I maintain, with other Griceans, that an adequate theory of what speakers *mean* when they speak will subsume sub-theories of what speakers say and what they imply by saying what they say. The relations saying and implying bear to meaning are illustrated thus:

- (3) By uttering, ‘I’m tired,’ Smith meant that he was tired
- (4) By saying that he was tired, Smith meant that we should leave.

Two related points are worth emphasizing here. First, when we describe speech act scenarios to informed native speakers, typically they have pretty robust intuitions (and are capable of rendering pretty robust judgments) about what the speaker meant, and about what he said and implied. Such intuitions constitute important data against which theories of meaning, saying and implying are tested. Second, in effect, saying and implying are *ways* of meaning, and theories of saying and implying earn their keep and derive their content in virtue of their rôles in a theory of meaning. Theories of saying and implying that make no contribution to a theory of meaning are devoid of empirical content.<sup>3</sup>

### 3. REFERRING AND PREDICATING

One of the central ways—perhaps *the* central way—in which people *say* (*state, assert, claim*) things is by performing complex speech acts that comprise two smaller acts: *referring* to something, and *saying something* about it (*predicating something* of it).<sup>4</sup> Thus we say (5) because we are prepared to say (6) and (7):

- (5) By uttering ‘I am tired,’ Smith said that he was tired
- (6) By uttering ‘I,’ Smith referred to himself
- (7) By uttering ‘am tired’ Smith said about himself that he was tired.

So a theory of referring, it would seem, must mesh with a theory of predicating (in accordance with the principles comprising a theory of syntactic structure) if the overall theory of saying is to hang together. When we describe speech act scenarios to informed, native speakers, typically they have pretty robust intuitions (and are capable of rendering pretty robust judgments) about what the speaker was referring to and what he was saying about that thing, hence intuitions about what the speaker was saying simpliciter.

### 4. REFERRING AND REFERRING EXPRESSIONS

With Strawson, Grice, Schiffer, and others I take *saying* and *referring* to be (in the first instance) things that speakers (or writers) do. They perform *acts* of saying and referring. Producing adequate theories of saying and referring will almost certainly involve producing a theory (or a collection of sub-theories) about various types of expressions in natural language that speakers use to refer to things. In accordance with fairly common practice, let us call such devices *referring expressions*. To a first approximation, the class of referring expressions in natural language consists in those expressions belonging to certain specifiable *categories*, very many of whose members speakers (or writers) use systematically to refer to things, and the rest of whose members many speakers (or writers) could so-use in the right circumstances. Given the place of a theory of referring within a theory of saying, it will be an empirical question which expressions in natural language are referring expressions under this preliminary definition. (At least if substance is given to the application of the ‘systematically’ here.)

Seemingly disparate questions about underdetermination, indeterminacy, relativism, indexicality, rigidity, direct reference, propositional attitude reports and hidden syntax converge in an important way when we ask the following two-part *why* question: why do we have *referring expressions* in natural language, and why do they come in different varieties? The question assumes we can articulate a notion of referring expression that does two things: (a) put us in a position to ask of any particular expression whether it is, as a matter of empirical

fact, a referring expression; and (b) allow us to subdivide the class of referring expressions in empirically significant ways.

Perhaps the notion of a referring expression can be given a purely semantic characterisation (in terms of a specific feature of the axioms governing such expressions according to one type of semantic theory, or in terms of a specific type of propositional contribution, according to another). But perhaps not, for it might need to draw upon a syntactic characterisation of the notion of a *noun phrase*. Then again, it might be argued that we should no more base our inquiries on the unthinking assumption that *only* noun phrases can be referring expressions (think of the words ‘here’ and ‘there’) than we should base it on the assumption that *all* noun phrases are referring expressions (think of the word ‘no-one’, or the word ‘it’ in the sentence ‘it’s dark already’). Certainly there are well-known techniques for unifying groups of noun phrases that appear semantically quite different; nonetheless it is common to assume that there is some theoretically significant property, the possession of which marks out certain of these phrases, and the underlying reason for this is surely the role that acts of referring play in basic acts of saying.

The varieties usually distinguished within the class of referring expressions may or may not correspond to theoretically significant notions—a blend of syntactic and semantic considerations are used in loosely specifying them—but certain appellations have certainly taken on theoretical colourings, and certain vintages loom large in discussion—1893, 1905, 1950, 1966, 1972, and 1977, for example. The simplest varieties are ordinary proper names (‘Bobby’, ‘Robert F. Kennedy’), demonstrative pronouns (‘this’, ‘that’), first- and second-person pronouns (‘I’, ‘you’), and third-person pronouns (‘he’, ‘she’, ‘him’, ‘her’) when used either deictically (perhaps with a demonstration) or anaphorically (as in ‘Bobby and his brother’). But expressions whose referential potentials appear tied to the meanings of their parts and the mode of assembly are often included too, particularly *descriptions*, occurring in four varieties: demonstrative descriptions (‘that tall man’), possessive descriptions (‘Bobby’s brother’), definite descriptions (‘the tallest man in Rome’), and indefinite descriptions (‘a man I thought we had agreed to keep out of the loop’).<sup>5</sup> Once we have an empirically significant notion of referring expression, it becomes an empirical matter whether the expressions of these varieties *are* referring expressions. (And, presumably, whether interrogative descriptions (‘which tall man?’) qualify.)

## 5. THE ACT-SYNTACTIC PROGRAMME

As the foregoing no doubt suggests, I do not want to assume the usual varieties at the outset. I prefer to watch those that are useful emerge from *act-syntactic* reflections, by which I mean reflections on the most fundamental features of speech acts as they are regulated by (or projected through the lens of) syntax. By proceeding in this way, I hope, the existence of distinct varieties can be

explained in terms of a conspiracy of *act-potential* and *syntactic distribution*. That may sound mysterious, but the basic idea is quite simple. We might put forward a preliminary act-syntactic, specification of the class of referring expressions that declares an expression *E* a referring expression if and only if (a) *E* is used regularly or systematically to perform such-and-such types of intentional acts—or at least *could* be so-used in a sense of ‘could’ that would require some explication—and (b) *E* has such-and-such syntactic distribution. And suppose the intentional acts in question could be specified without invoking the notion of a referring expression—for example, acts of *referring*, explained in terms of specific intentions involved in the use of language to *say* things. In principle, we could then go on to distinguish two *sorts* of referring expressions in terms of different types of acts of referring we need to perform, for example, *perspectival* referring and *non-perspectival* referring. And this might yield a division between what we might want to call “indexicals” and “names”, which may be closely related to varieties that bear homophonic labels.

Explaining the existence of types of referring expressions as conspiracies of act-potential and syntactic distribution might yield the following sorting, for example, where  $\pm R$  indicates whether or not an expression is used in acts of referring (an actic consideration),<sup>6</sup>  $\pm N$  indicates whether or not it is a noun phrase (a syntactic consideration),  $\pm P$  whether or not it is used to signal perspective (an actic consideration), and  $\pm A$  whether or not it is syntactically atomic (a syntactic consideration):

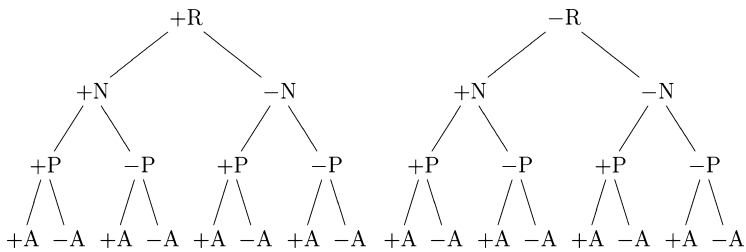


Fig. 1: Expressions.

This is a considerable simplification, but it serves to indicate the general idea and will suffice for present purposes. Our first cut ( $\pm R$ ) is made actically, in terms of potential for acts of referring; the second ( $\pm N$ ) is made syntactically; the third ( $\pm P$ ) is made actically, in terms of potential for perspectival acts; and the last ( $\pm A$ ) syntactically, in terms of having internal syntactic structure. In theory, we have already specified 16 sorts of expressions, including 8 types of referring expressions, four of which are types of noun phrases, two of which are perspectival, one of which is syntactically atomic.<sup>7</sup> The words ‘I’, ‘you’, and ‘he’, for example, belong to this type, viz. [+R, +N, +P, +A], whereas ‘Aristotle’ and ‘Athens’ belong to [+R, +N, -P, +A]. Matters get more interesting when we ask about ‘the king of France’. Is it [+R, +N, -P, -A], as one moved by Strawson

might suggest? If so, the distinction between a name and a definite description is just structural: names are +A, descriptions are -A. We can step beyond noun phrases and ask about 'here', 'there', and 'now'. Are they [+R, -N, +P, +A]?

## 6. TERM LIMITS

It is doubtful all of the 16 act-syntactic possibilities set out in Fig. 1 are found in English. So we would like to know which ones are not found in natural language at all, which ones are not found in this or that language, which pairs are not found together, which ones are found only in pairs, etc., and also why things have turned out as they have. We can make some progress here, I think, if we look at certain strong semantic hypotheses about natural language. I'm going to discuss three that I put forward in TL, where I attempted to home in on a distinction between natural language referring expressions and predicates by appealing to *de jure* rigidity of extension and *de jure* semantic atomicity. Classically, the extension of a referring expression *E* is an object, viz. the object *E* refers to; that of an *n*-place predicate *R* the set of *n*-tuples of objects of which *R* is true; that of a sentence its truth value; and that of an *n*-place sentence connective a function from *n*-tuples of truth-values to truth values.

It is *in the nature* or *constitutive* of being a natural language referring expression *in natural language*, I suggested, to be rigid in respect of extension, whereas it is not in the nature of a natural language predicate to be so (though some predicates are, *in fact*, rigid in respect of extension, for example the predicates of mathematics). My hypothesis was that an empirically satisfying theory of meaning for natural language would invoke a notion of referring expression satisfying this condition, and that facts about the contingent and dynamic nature of our surroundings, particularly in so far as they are populated with ordinary three-dimensional objects which we perceive, track, and think about, virtually dictate that our primary system for expressing thoughts have evolved upon (and are maintained by) a bedrock comprising one class of expressions naturally required to be rigid and singular in respect of extension (referring expressions) and another class of expressions not so required (predicates).<sup>8</sup>

Additionally, I suggested, it is in the nature or constitutive of being a natural language referring expression to be semantically atomic.

The fate of noun phrases with semantically relevant structure was sealed by a third hypothesis: the class of meaningful noun phrases in natural language comprises just referring expressions and quantifier phrases. In terms of the act-syntactic divisions in Fig. 1, this means the features [-R, +N] characterises the class of quantified noun phrases. (A potential problem here is the possibility of expression that function simultaneously as quantifier phrases and referring expressions.) Thus all semantically structured noun phrases are quantifier phrases.

These three empirical hypotheses were rolled up into the following:

(T1) Every meaningful noun phrase in natural language is either a semantically unstructured, rigid referring expression . . . or else a semantically structured, restricted quantifier (1993: 90).<sup>9</sup>

I made no attempt in TL to define *referring expression* or *quantifier phrase*. Rather, I assumed that the notions were intuitive, clear, basic, and exclusive, and that a distinction between the two was more or less underwritten by the robustness of (a) the distinction in standard, first-order logic between individual constants and individual variables, and (b) the binding of the aforementioned variables by the unrestricted quantifiers  $\forall$  and  $\exists$ .

I am uncomfortable with these assumptions today:

(i) The notion of a variable now seems to me a rather complex one that we are still striving to understand fully,<sup>10</sup> and the idea of treating individual constants as devices that vary their extensions across *models* (in the way predicates do) soon struck me as a dubious one to appeal to in explicating the semantics of proper names (or referring expressions more generally) in natural language. I shall say no more about this matter here.<sup>11</sup>

(ii) Presumably, there is some important semantic property that all referring expressions are supposed to have that makes philosophers think they constitute a theoretically significant group. But it is difficult to evaluate empirical hypotheses about the members of that class, or even to identify its purported members, without some sort of preliminary account of what that semantic property is. So the empirical hypotheses will be interesting only if the grouping principle is justifiable and potentially revisable, and does not stack the deck for or against the hypotheses in question.

(iii) As a good Gricean, and in the good company of Strawson, Searle, Schiffer, and Kripke, I regard the conceptually basic referential notion to be one exemplified in statements such as “I was referring to (talking about) the State of New Jersey, not Jersey in the Channel Islands”; “I was using ‘Jersey’ to refer to New Jersey”; “I was referring to New Jersey with ‘Jersey’”. Amplifying certain points I made in §§3–4, referring, on this use, is something speakers *do*, it is a species of intentional act. Speakers perform acts of *referring* which frequently function as subacts of acts of saying. It is in terms of this speaker-based notion that other referential notions must be explicable, for example the notion of an expression itself having a reference, the notion of a use, an utterance, or a tokening or an expression having one, and the notion of a referring expression itself. Furthermore, I hold that (when properly explicated) what a speaker is saying on a given occasion by uttering some sentence ‘ $\alpha$  is  $\phi$ ’ is determined by what the speaker is referring to with the referring expression  $\alpha$  and by what he is saying about (predicating of) that object with the predicate ‘is  $\phi$ ’, and not by any other referential notion involving  $\alpha$ , if there is one. If this is to mesh

cleanly with (T1), at some point I need to explain precisely how I propose to continue talking in a meaningful way about the (purported) rigidity of referring expressions.<sup>12</sup>

(iv) It was crucial to (T1) to reject the idea that definite descriptions (phrases of the form ‘the  $\phi$ ’) are referring expressions: i.e. it was crucial to reject the idea that the proposition a speaker expresses by uttering ‘the  $\phi$  is  $\psi$ ’ is object-dependent (singular).<sup>13</sup> That was fine with me as I had already defended an updated version of Russell’s Theory of Descriptions according to which the proposition a speaker expresses by uttering ‘the  $\phi$  is  $\psi$ ’ is object-independent (general, quantificational).<sup>14</sup> Russell’s Theory of Descriptions is “liberating”, I said in TL, for anyone sceptical about either semantically complex referring expressions (complexity sceptics) or non-rigid referring expressions (flaccidity sceptics). Definite descriptions certainly have semantic structure, and those appearing in ordinary conversation are typically non-rigid. (Mathematics provides uncontroversial examples that are rigid: ‘the sum of 3 and 4’, ‘the positive square root of 9’, etc.) But according to the Theory of Descriptions, they are quantifier phrases not referring expressions; furthermore, upon investigation many structured noun phrases in natural language turn out to be thinly disguised definite descriptions, hence quantifier phrases. So, on one side of the fence we find semantically unstructured, rigid referring expressions such as ‘Aristotle’, ‘9’, ‘whales’, ‘gold’, and ‘red’, and on the other semantically structured quantifier phrases such as ‘the greatest philosopher of antiquity’, ‘the number of planets in our solar system’, ‘the largest species of animal on Earth’, ‘the principle substance stored for safety at Fort Knox’, and ‘the colour that signifies *stop* at British traffic lights.’<sup>15</sup>

Thus liberated, the complexity sceptic rejects an assumption made by a distinguished line of logicians that includes Frege (1891, 1892), Hilbert and Bernays (1934), Church (1940), Carnap (1947), Tarski (1956), Scott (1967), Grandy (1972), Lambert (1991), and Smiley (1981, 2004), according to which expressions such as ‘the King of Norway’, ‘Herman’s father’, ‘the referent of “Norway”’, ‘the word “and”’, ““and””, ‘the successor of zero’, ‘ $S(S(S(0)))$ ’, ‘ $f(a)$ ’, ‘ $2 + 2$ ’, ‘ $3^2$ ’, ‘ $\sin x$ ’, and ‘ $x^2 - 1$ ’ are complex referring expressions, indeed singular terms, as well as the assumption (common in philosophy) that *that*-clauses such as ‘that Ralph is a spy’ refer to propositions or similar entities. The liberated flaccidity sceptic, when presented with a purported example of a non-rigid singular term, will now ask whether or not there is reason to think the expression is not, in fact, a disguised description.

I now think matters are more complex. I see no reason to abandon the general Russellian position on the semantics of descriptions; but I think getting clear about what we want from a theory of referring will lead us to see certain disputes and distinctions in new ways and to take seriously the idea, which might be problematic for the act-syntactic divisions given in Figure 1.



## 7. REFERRING: PERSPECTIVE AND INDEXICALITY

Suppose you want to *say* something about some object *X*. Simple acts of saying involve acts of *referring* (to something) and *predicating* (something about it). One way of drawing your hearer's attention to *X* is by displaying a "tag" that, by custom or convention, attaches to *X*. This is particularly useful if *X* is not in view or bears no obvious relation to you that is discernible in (or from) the physical environment. A proper name is effectively a tag, an expression purpose built for certain types of acts of referring.

Another way of drawing your hearer's attention to an object *X* is by indicating it in the shared physical environment by reference to your own position. This is particularly useful if you are not acquainted (for whatever reason) with a tag attached to *X*. A simple demonstrative pronoun such as 'that' is effectively a device that, by custom, can be used to indicate like this. If you think the hearer, given his position in the environment, can reasonably be expected to establish which object you have in mind, then unless you are a poor judge of such things or the hearer is just a poor interpreter, in all probability things will go well.

What we have now are two types of noun phrases that can be used by speakers to refer to things. One of them is perspectival, the other is not. They form a useful pair whose existence is readily explained. Now what do you do if you have no handy tag that attaches to *X* and no exploitable perspective on *X*? In a sense, you *improvise*: you *describe* *X*. That is, you specify *X* in terms of properties it has (or properties you take it to have, or properties you are willing to construe it as having) perhaps all the way up to uniqueness. That is, you produce a *non-atomic* expression drawing upon all sorts of [–R] expressions. If you thought hard enough about what you had done, you might well see yourself as caught in a no-man's land between Frege and Russell: you referred to *X*, but you sort of cheated. You didn't use an atomic expression purpose built for the job—a tag or an indicator—you used bits and pieces to construct a complex expression that would invoke properties that *X* has (or properties you take it to have etc.) perhaps up to uniqueness. Thus definite descriptions.

By proceeding in the act-syntactic way I am recommending, we rightly rob ourselves of opportunities to indulge in uninspired appeals to "indexicality" of the sorts philosophers and linguists sometimes make when simple accounts of the relation between the propositions we express (the things we say) and the compositionally determined meanings of the sentences we use to express them appear to break down in ways that seem traceable to "contextual factors". Claims to the effect that some seemingly troublesome expression is "indexical" are all the rage in lawless quarters, "indexicality" regarded as some sort of semantic panacea. There are those in epistemology, for instance, who claim the verb 'know' is indexical, the strength of the justification required for felicitous usage altering with context.<sup>16</sup> And there are those in the philosophy of language,

who claim there are unheard “indexical” expressions in the syntax of all sorts of natural language sentences, expressions that can refer to more or less anything the theorist chooses, without being in the least bit perspectival (or the least bit descriptive or tag-like for that matter).<sup>17</sup>

It was a virtue both of early discussions of deixis, egocentricity, indexicality or what-have-you (and of more recent discussions that are sensitive to the early discussions) that an underlying phenomenon of some philosophical and psychological interest was the target: *referring perspectivally*.<sup>18</sup> Words used to refer in this way include ‘I,’ ‘you,’ ‘he,’ ‘she,’ ‘this,’ ‘that,’ ‘here,’ ‘there,’ ‘now,’ and ‘then’. Traditionally, perspective has been a distinguishing feature of indexical (deictic, egocentric, indicator) words, and since the need to refer perspectivally has a ready explanation, we have an explanation of *why* we have indexical referring expressions, the existence of any particular expression attributable to the particular perspective it signals. (The perspective may be higher-order.)<sup>19</sup> Many contemporary discussions start with more or less the same list of indexical expressions and add to it without reflection on the underlying phenomenon, quite oblivious to the rôle of perspective. The justification for adding some expression *E* to the list often amounts to no more than the realisation that *E* is an irritant for compositional semantics as it stands and the belief that the relevant irritation can be treated by construing *E* as an “indexical” whose interpretation is “determined by context” (whatever exactly that means). As such, expansion of the list is unconstrained and grounded in nothing more than the urge to obliterate some problem or paradox of interpretation.<sup>20</sup>

## 8. REFERRING: RIGIDITY

I am frequently told that if we make what a speaker says directly dependent upon what the speaker is referring to—rather than on what the referring expressions he is using refer to—we are effectively (a) prevented from talking sensibly about both the (purported) *rigidity* and the (purportedly) *directly referential* nature of (e.g.) proper names and demonstratives, (b) prevented from drawing upon the fertile idea that reference is determined causally, and (c) facing immediate and certain disaster because referring (or *speaker’s reference* as it is often called) is a thoroughly intentional notion that is undermined by an array of cases involving perceptual, epistemic, or linguistic error, or just plain ignorance, on the speaker’s part. All three claims are incorrect, I believe. It is (a) I want to address here.<sup>21</sup>

Thanks to Kripke and Kaplan it is now established doctrine that proper names and demonstrative pronouns are *de jure* rigid referring expressions. (Some go further: drawing upon the work of Kaplan, they say that proper names and demonstratives are directly referential: their references exhaust their specific contributions to the propositions speakers express when using sentences containing them.) But strange as it may sound initially, *rigidity has nothing directly to do with either referring expressions or even reference*.

Based on what Kripke says in the body of *Naming and Necessity*, it is usual to say that an expression *E* rigidly designates or refers rigidly to some object *x* if and only if *E* refers to *x* in every possible world in which *x* exists and to nothing distinct from *x* in any world in which *x* does not exist.<sup>22</sup> Some philosophers, Kaplan (1989b) for example, have urged a cleaner, simpler characterisation—sometimes because they think that Kripke himself suggests it in certain passages, but more importantly because they think it is preferable on logical, philosophical, or methodological grounds: an expression *E* refers rigidly to some object *x* if and only if *E* refers to *x* in every possible world.

Three facts about these characterisations of rigidity are noteworthy. (i) They invoke a concept of *reference* (which itself needs to be characterised). (ii) They construe rigidity as a property of *expressions*. (iii) They assume that no particularly serious problems are gendered by making linguistic distinctions such as the following: (a) expressions and (their) occurrences; (b) expressions and (their) uses, (c) expressions and particular utterances or inscriptions (thereof); (d) word types and word tokens, (e) names as items individuated partly in terms of their bearers and names individuated in such a way that Stephen Neale the film theorist, Stephen Neale the character in Graham Greene's *Ministry of Fear*, and Stephen Neale the philosopher have the same name.

But, in fact, it turns out we can characterise rigidity cleanly (i) without invoking a concept of reference, (ii) without construing it as a property of referring expressions, and (iii) without worrying about linguistic distinctions (a)–(e).

In a famous passage from the 1980 preface to *Naming and Necessity*, Kripke himself says what the rigidity thesis is without mentioning reference, focussing directly on the *truth conditions* of whole sentences containing expressions that are candidate rigid referring expressions. The passage in question is readily and harmlessly transposed to incorporate talk of *what the speaker says* in connection with a particular dated utterance of sentence (8):

(8) Aristotle was fond of dogs.

A proper understanding of the speaker's statement, i.e. a proper grasp of *what the speaker said* by uttering (8) on this occasion, involves an understanding both of the (extensionally correct) conditions under which it (what the speaker said) is in fact true, *and* of the conditions under which a counterfactual course of history, resembling the actual course of history in some respects but not in others, would be correctly (partially) characterised by what the speaker said. There is a certain man *x*—the ancient Greek philosopher we call 'Aristotle'—such that, as a matter of fact, what the speaker said on this occasion by uttering (8) is true if, and only if, *x* was fond of dogs. The rigidity thesis *I* hold is simply—subtle points aside [*Footnote*: In particular, we ignore the question of what to say about counterfactual situations in which Aristotle would not have existed]—that the

same paradigm applies to the truth conditions of what the speaker said as it characterises *counterfactual* situations. That is, what the speaker says correctly (but only partially) characterises a counterfactual situation if, and only if, the same aforementioned man would have been fond of dogs, had that situation obtained.

Thus, the rigidity thesis stated—in connection with Kripke's sentence (8)—and, *mutatis mutandis*, with any variant of (8) containing a different proper name in subject position, and any variant containing a different predicate—in a way that is fully compatible with the present framework. To talk about rigidity in connection with an expression *E* is to talk about *truth conditions* that are singular with respect to *E*, just as talk about direct reference in connection with *E* is to talk about *propositions* that are singular with respect to *E*.

The generalisation others capture by saying that proper names refer rigidly can be easily restated: an expression *E* rigidly refers to *x* if and only if the truth of what a speaker says by uttering a sentence containing *E* depends upon how things are with *x*. That is, what the speaker says always has *x*-dependent truth conditions.

The seemingly important rigidity difference between the proper name 'Aristotle' and the definite description 'the greatest philosopher of antiquity' is a difference in respect of *truth conditions*: object-*dependent* truth conditions in connection with sentences containing the name but object-*independent* truth conditions in connection with sentences containing the description. That is, in connection with sentence (9), we simply deny the rigidity thesis:

(9) The greatest philosopher of antiquity was fond of dogs.

That is, we deny that what the speaker said by uttering (9) correctly (but only partially) characterises a counterfactual situation if, and only if, the person who was *actually* the greatest philosopher of antiquity would have been fond of dogs, had that situation obtained. Since (8) and (9) contain exactly the same predicate, 'was fond of dogs', let us summarize the contrast between (8) and (9) with respect to rigidity with the following shorthand: 'Aristotle' is rigid whereas 'the greatest philosopher of antiquity' is not.

Notice that we have still said nothing overtly about reference; everything has been handled in terms of the truth conditions of what the speaker said. And since, on my account, what the speaker said is determined by what the speaker referred to and what he predicated of it, it is simply false that I am prevented from talking sensibly about the (purported) rigidity of (e.g.) proper names and demonstratives.

And this prompts a seemingly bizarre question: is the concept of rigidity really parasitic upon the concept of a referring expression? It is not, as we shall see.

## 9. REFERRING WITHOUT REFERRING EXPRESSIONS

We often refer without using a referring expression. Various sorts of construction lend themselves to this very naturally. Here are three examples.

1. *Kripke's Colloquy*. We are to imagine two people, *A* and *B*, gazing down the street at a man busy with some implement. The following exchange takes place:

- (10) *A*: What is Jones doing?  
       *B*: Raking the leaves.

According to Kripke (1977), both participants in the colloquy referred to Smith. Now the *first* speaker referred to Smith *by* or *with* the name "Jones" But the *second* speaker used no designator! What we seem to need here is the idea of the second speaker referring to Smith with the *implicit* or *ellipted* subject of "Raking the leaves." This is unproblematic, I think. Some syntacticians might be attracted to the idea of an *aphonic* noun phrase—a noun phrase that is *phonetically unrealised*—occurring as the subject of "Raking the leaves". And they might opt for one of several possibilities about the aponic's interpretation: (i) the aponic functions as if it were another occurrence of "Jones"; (ii) the aponic functions as if it were a deictic use of "he"; (iii) the aponic functions as if were anaphoric on the *other* speaker's utterance of "Jones".

Any of these positions could form a reasonable component of a theory according to which the second speaker referred to Smith. But there are more interesting cases, at least one of which certainly cannot be explained in terms of syntactic ellipsis.

2. *Perry's Condition Statements*. As Perry (1986) observes, I may utter one of these sentences to *say* that it's raining/midnight/dark in Paris, for example:

- (11) It's raining  
 (12) It's midnight  
 (13) It's dark

(As Perry notes, I needn't *be* in Paris, or even be *close* to Paris to do this.) In the terminology of Stephen Schiffer, I may refer to Paris (or London, or . . .), *in* using sentence (11), (12) or (13) despite the fact that there is no overt expression *with which* I refer to Paris (or London, or . . .). Of course, one might add an indexical ('here'), a name ('Paris') or a description 'the capital of France' in order to be more explicit on a given occasion, but this is certainly not required by either facts about syntax or facts about communication.<sup>23</sup>

3. *Incomplete Descriptions.* The third type of example is the oldest and also the most interesting as the case for an aponic in syntax is pretty hopeless, as even those hooked on aponics point out:<sup>24</sup>

- (14) The mayor can ban demonstrations.  
 (15) Most people earn more than €1,000 a month.

I may utter (14) to say that the Mayor of Paris can ban demonstrations in Paris, that the mayor of Nantes can ban demonstrations in Nantes, and even, in a time of extraordinary politics, that the mayor of Paris can ban demonstrations in Nantes, indeed even that the mayor of Paris can ban demonstrations anywhere in France, or anywhere in the world. Similarly, I can use (15) to say that most people in Paris earn more than €1,000 a month, that most people in France earn more than €1,000 a month, that most people in the EU earn more than €1,000 a month. In each of these cases I would refer to a place that is the semantic value of no particular part of the sentence used.

In summary, the use of a referring expression is neither necessary nor sufficient for an act of referring. That it is not necessary is shown by the fact that I can use sentence (14) to say that the mayor *of Paris* can ban demonstrations *in Paris*. In such cases, I refer to Paris although there are no referring expressions *with which* I refer to Paris. That the use of a referring expression is not sufficient is shown by the fact that if I say, 'John, hurry,' I am not referring to John, I am *addressing* him.

## 10. RIGIDITY WITHOUT REFERRING EXPRESIONS

Given the way we have now characterised rigidity, a question arises that seems at once natural and bizarre: Is the concept of rigidity really rooted in talk of referring expressions? It is not. Kripke's example sentence, (8),

- (8) Aristotle was fond of dogs

contains a candidate referring expression, the proper name 'Aristotle'. But just as his statement of the rigidity thesis in connection with (8) does not mention *reference*, so it does not mention *referring expressions*.<sup>25</sup> And this raises the question whether we can state the rigidity thesis in connection with *a sentence that does not even contain a referring expression*.

We can. We have already seen case in which a speaker refers to something even though there is no expression *with* which he succeeds in doing this. Take (16):

- (16) The mayor is fond of dogs

Suppose I use (16) to say that the mayor of Paris is fond of dogs. Lo and behold, as with Kripke's example (8), we can readily state the rigidity thesis in connection with (16) by a simple and harmless transposition of the previously mentioned passage from the 1980 preface to *Naming and Necessity*:

A proper understanding of the speaker's statement, i.e. a proper grasp of *what the speaker said* by uttering (16) on this occasion, involves an understanding both of the (extensionally correct) conditions under which what the speaker said is in fact true, *and* of the conditions under which a counterfactual course of history, resembling the actual course of history in some respects but not in others, would be correctly (partially) characterised by what the speaker said. There is a certain place  $x$ —viz. the French city we call 'Paris'—such that, as a matter of fact, what the speaker said is true if, and only if, the mayor of  $x$  (at the time of utterance) was fond of dogs. The rigidity thesis (*I* hold) is simply—subtle points aside [*Footnote*: In particular, we ignore the question of what to say about counterfactual situations in which Paris would not have existed]—that the same paradigm applies to the truth conditions of what the speaker said as it characterises *counterfactual* situations. That is, what the speaker says correctly (but only partially) characterises a counterfactual situation if, and only if, the mayor of the same aforementioned place would have been fond of dogs, had that situation obtained.

Rigidity without a rigid designator! Let's be clear about what we have here: a statement of the rigidity thesis that (a) mentions neither reference nor referring expressions, and (b) uses as its sole example one that does not contain a referring expression! (I am here assuming, of course, the Russellian view that 'the mayor' is not a referring expression.) Thus liberated from referring expressions *per se* in our talk of rigidity, we can locate the underlying property where it belongs: with the speaker's act of referring. The speaker not only referred to Paris in uttering (16), but that he did so *rigidly*, despite not using a referring expression to do so.

## 11. REFERRING NON-RIGIDLY?

This invites another obvious question: do speakers ever refer *non-rigidly*? Or is it part of what referring *is* that when we refer we do so rigidly? If rigidity is, in the first instance, a property of acts of referring, it is far from self-evident that a theory of referring will make use of the idea of referring non-rigidly, and (so) not self-evident that it will need the concept of a non-rigid referring expression (despite the seeming evidence of their existence in the form of definite descriptions).

We now have something approximating an *argument* for the rigidity of natural language referring expressions: (i) when we refer we refer rigidly (whether or not we use referring expressions); (ii) the class of referring expressions in

natural language consists in those expressions belonging to certain specifiable categories, very many of whose members speakers (or writers) use systematically to refer to things, and the rest of whose members many speaker (or writers) could be so-use in the right circumstances; so (iii) referring expressions are rigid. Of course (i) might be resisted on the grounds that we often use definite descriptions to refer non-rigidly, and (ii), might be thought to leave plenty of wiggle room. The idea would be something like this: (a) definite descriptions are referring expressions (because they satisfy the definition in (ii)); (b) when we use descriptions in roughly the way that is called *referential* in the literature spawned by Donnellan's (1966) distinction between *referential* and *attributive* uses of descriptions (e.g., in lieu of, say, demonstratives or proper names, as in the examples Donnellan and others use) we refer rigidly with them; but (c) sometimes we use them non-rigidly, for example when we use them in roughly the way that is called *attributive*. This seems to me a perfectly legitimate response at this stage. But when more is on the table—in particular when we reintroduce certain things I mentioned in TL—I think we will find reasons to reject it.

Let me be clear: I am *not* saying that we might find the concept of a non-rigid referring expression semantically *incoherent* if we push on with our work. I am simply saying that on the basis of two examples of referring we have looked at—one involving a proper name, the other involving nothing at all—we cannot rule out the possibility that (i) as a matter of empirical fact, natural language does not avail itself of non-rigid referring expressions, but (ii) there may be deep reasons for this that are connected to the ways in which we track the objects we refer to when considering the properties they have at different times and in counterfactual situations, i.e. reasons of the sort I gave in TL. The rigidity of natural language referring expressions, I suggest, is a reflex of the fact that we refer rigidly (with or without referring expressions); and this is itself a reflex of a property of *the nature of thought*, which itself assumes a basic ontology of individuals in a contingent and dynamic world.

## 12. CONCEPTUAL CLAIMS VS. EMPIRICAL HYPOTHESES

There is only trouble in store if we do not distinguish empirical hypotheses (conjectures) about the syntax or semantics of actual natural languages (or, for that matter, empirical hypotheses about a particular natural language or group of such languages) from *conceptual claims* about syntax and semantics quite generally. When I hypothesized in TL that every meaningful noun phrase in natural language is either a semantically unstructured, rigid referring expression or else a semantically structured, restricted quantifier, I was putting forward a complex *empirical* hypothesis, one that might be fuelled or falsified by serious empirical investigation, one that had no bearing whatsoever on either (a) what sorts of referring expressions may exist in artificial languages (the syntax and semantics of which are stipulated rather than discovered by empirical methods)



or (b) what sorts of referring expressions we could have had in natural language under different conditions. I gave no argument meant to demonstrate the truth of the hypothesis, and for good reason. There is something deeply confused about attempting to construct such an argument. The hypothesis was about the empirical facts, not about how things have to be, and it was motivated not by direct argument—how could it be?—but by reflection on two things: (i) antecedently attractive accounts of the semantics of proper names, indexical and demonstrative pronouns, and definite descriptions; and (ii) certain properties natural languages might be expected to have if they are to serve the most basic purposes to which we put them.

Of course, the inspiration, intuition, or rationale behind a particular empirical hypothesis might have its origins in reflections of a logical or conceptual nature, and in the case of natural language these might revolve around features built into one's favourite formal (artificial) languages.<sup>26</sup> But nothing in the etiology of an empirical hypothesis alters the fact that it is an *empirical* hypothesis.

Kripke's (1972) claim that proper names in natural language refer rigidly is an empirical hypothesis not a conceptual claim—even if some of the inspiration for the claim may have come from reflections on the semantics of quantified modal logics, particularly those containing individual constants.<sup>27</sup> In TL I pushed a stronger empirical hypothesis: *all* referring expressions in natural language are rigid. I did not claim that the notion of a non-rigid referring expression is *semantically incoherent*. (It is easy to construct a formal language containing non-rigid referring expressions. Some people hold that (many) definite descriptions in natural language are non-rigid referring expressions. There is nothing semantically incoherent in that claim, even if it turns out to be false.) My hypothesis, then, was that, *as a matter of empirical fact*, natural language referring expressions are rigid. I motivated this not by appealing directly to the *concept* of a referring expression, but by doing some philosophical psychology, i.e. by taking into account the nature of perception and cognition in creatures with our needs, and looking at what referring expressions must do for such creatures in linguistic systems used to communicate or otherwise represent thought. The idea was that non-rigid referring expressions would serve so little purpose for such creatures that in the ordinary course of things they would not emerge naturally, and that even if they were introduced by means of stipulation they would likely die out through lack of use (except in specialized theoretical discussions perhaps). At least that was the idea. So pointing out that that there is nothing conceptually, formally, or semantically incoherent about the idea of non-rigid referring expressions hardly constitutes a sensible objection to (T1).

Similarly, pointing out that that there is nothing is conceptually, formally, or semantically incoherent about the idea of referring expressions having *semantic structure* hardly constitutes a sensible objection to (T1). My hypothesis in TL was the empirical one that natural language referring expressions are rigid and

lack semantic structure. So, amongst the things I did *not* claim in TL are the following:

- (A) The idea of meaningful noun phrases that are neither referring expressions nor quantifier phrases is semantically incoherent;
- (B) The idea of non-rigid referring expressions is semantically incoherent;
- (C) The idea of semantically structured referring expressions is semantically incoherent.

(B) and (C) are pretty obviously false as it's child's play to construct a formal language containing non-rigid, semantically structured referring expressions. I don't know what to say about (A). It's not obvious that we would be much inclined to call something a meaningful noun phrase if it weren't a referring expression or a quantifier. Anyway, (A) is unimportant and we can forget it.

Another claim I did *not* make in TL is (D), which is a trivial consequence of (C):

- (D) The idea of referring expressions that contain, as proper parts, variables bound by exterior quantifiers is semantically incoherent.

Like (B) and (C), (D) strikes me as obviously false—at least, assuming pretty standard philosophical usage of “variable” and “binding” and a plausible amplification of the classical theory of extensions that assigns extensions to individual variables and open sentences. On these assumptions—which turn out to be less trivial than one might think initially—it is possible to construct a formal language that contains referring expressions that contain as proper parts variables bound by exterior quantifiers, hence, again, a language containing semantically structured referring expressions.<sup>28</sup>

But here are four *empirical* claims that seem to me eminently worthy of investigation if we are genuinely interested understanding how natural works, claims I was urging philosophers and linguists to take seriously in TL:

- (A\*) Natural language does not exploit the possibility meaningful noun phrases that are neither referring expressions nor quantifier phrases;
- (B\*) Natural language does not exploit the possibility of non-rigid referring expressions;<sup>29</sup>
- (C\*) Natural language does not exploit the possibility of semantically structured referring expressions;
- (D\*) Natural language does not exploit the possibility of referring expressions that contain, as proper parts, variables bound by exterior quantifiers.

Whether (A\*), (B\*), (C\*) and (D\*) are true or false is something upon which empirical investigation must shed light—if (C\*) is true, so is (D\*). Such investigation

is worthwhile for standard Chomskyan reasons. In principle, languages can have all sorts of interesting phonological, syntactic and semantic properties. But not all possibilities are exploited, and it is at least arguable—indeed it has been argued with great power by Chomsky himself—that to the extent that we understand which logical possibilities are and are not *in fact* exploited, we may well come to understand which possibilities are and are not within the range of those that our cognitive architecture *could* actually exploit in connection with the ordinary acquisition and use of language. And to that extent we might throw some light on the problem of language acquisition itself by drastically simplifying the “task” the neonate faces.<sup>30</sup>

The matter of which possibilities are actually exploited (or exploitable) in natural language syntax has an obvious analogue in semantics. Beginning with the work of Barwise and Cooper (1981), for example, there have been attempts to articulate constraints on which of the mathematical possibilities for generalised quantifiers are actually exploited by natural language quantifiers. Again, to the extent that we understand which logical possibilities are and are not *in fact* exploited, we may well come to understand which possibilities are and are not within the range of those that our cognitive architecture *could* actually exploit in connection with the ordinary acquisition and use of language. And to that extent it might be possible to shed some light on the problem of language acquisition itself, perhaps even on the nature of the relation between the mind of the pre-linguistic infant and the mind of the fully fluent speaker, at least in certain respects. (A\*), (B\*), (C)\* and (D\*) belong very much to this tradition. As I said in TL, it is more to my taste to examine stronger, simpler, more general hypotheses, even when there are seemingly obvious counterexamples, to see where they really fail (if they do) and see what ideas emerge from reflecting on the nature of the failure. This, it seems to me, is the way to make interesting progress in any discipline in which the relevant examinations cannot hurt people.

### 13. BINDING INTO TERMS

It has been recognised for many decades that definite descriptions may contain variables bound by exterior quantifiers. The following well-known examples were discussed in Neale (2005):<sup>31</sup>

- (17) Every man danced with the woman he loved
- (18) The million pound fortune of every Englishman is in jeopardy
- (19) The man who bought each donkey vaccinated it.

Equally well-known (and clear from *Principia Mathematica* onwards) is the fact that Russell’s Theory of Descriptions handles these descriptions as a matter of course— $(\forall x\phi(iy)(Ryx))$  is a perfectly well-formed description for Russell that

unpacks neatly into a formula in primitive notation. To the extent, then, that one is happy with a quantificational account of descriptions such as Russell's, sentences such as (17)–(19) present no threat to (D\*), hence no threat to (C\*). (Demonstrative descriptions, phrases of the form 'that  $\phi$ ', are a different matter, as we shall see in a moment.)

The distinction between empirical hypotheses about the syntax or semantics of natural languages and conceptual claims about syntax or semantics generally is not always appreciated fully in the philosophy literature. Nathan Salmon (1994, 2002, 2006a, 2006b), for example, fails to respect the difference between (D) and (D\*) in several discussions of binding into singular terms. Salmon begins his 2002 discussion with a misrepresentation of my own views:

A frequently heard objection to the hypothesis that compound expressions of a given category (for example, definite descriptions) are singular terms is that expressions of the given category can be coherently quantified into (that is, they can contain a variable bound by an external quantifier) while genuine singular terms cannot. The objection evidently originated with Benson Mates... but has been endorsed or echoed by others (for example, Stephen Neale, in *Descriptions* (Cambridge: MIT Press, 1990), at 56 n. 28. (Salmon, 2002, p. 534, n. 47.)

The claim made in the final sentence is plain false. Certainly, some logicians and philosophers have maintained in print that singular terms cannot be coherently quantified into (see below); but I am not one of them. I discuss standard examples of sentences containing definite descriptions themselves containing variables bound by exterior quantifiers, viz. (17)–(19) above. But I neither “endorse” nor “echo” the claim that genuine singular terms cannot be coherently quantified into, either in the footnote in *Descriptions* Salmon mentions or in the text to which it is appended, or indeed anywhere else in the book. What I actually say is that the quantification into the descriptions in (17)–(19) poses problems (a) for the presuppositional theory of descriptions proposed (in various forms) by Strawson (1950, 1952, 1954, 1964), and (b) for the Type II quantifier theory of descriptions proposed by Hornstein (1984). The problems in question do not concern (and presuppose no particular position on) the matter of whether genuine singular terms can be coherently quantified into. They concern presupposition and scope. More precisely, they concern the consequences of the *perfectly acceptable* quantification exemplified in (17)–(19) for Strawson's presuppositional theory of descriptions and Hornstein's large scope theory of descriptions.

The problem for Hornstein is simple (and not obviously soluble). On his account definite descriptions are so-called Type II quantifiers, and one of the characteristic properties of such expressions distinguishing them from Type I quantifiers (e.g. 'every  $\phi$ ') is that they insist on large scope. But there are perfectly natural interpretations of (17)–(19) upon which they have small scope and contain variables bound by quantifiers with larger scope. So the problem for

Hornstein—which seem to me insurmountable—is how to modify his theory in a systematic way to account for these natural readings of (17)–(19).<sup>32</sup>

The problem that example (17)–(19) poses for Strawson is how to provide a systematic, compositional account of the presuppositions carried by (17)–(19), and at the same time provide a systematic account of the impact on *whether or not* a statement is even made (on some versions of his theory) or on the *truth-value* of the statement that is made (on others), when some of the presuppositions are true and others false.<sup>33</sup> (In short, contrary to Salmon’s assertion, I neither “endorse” nor “echo” the claim that genuine singular terms cannot be coherently quantified into. I just (i) point out that Russell’s theory elegantly and automatically handles descriptions that are quantified into, and (ii) add that such descriptions present problems for one particular singular term theory of descriptions (Strawson’s) and one particular quantificational theory of descriptions (Hornstein’s). I say nothing more general and I make no claim whatsoever about the “coherence” of quantifying into singular terms.)

Is it plausible to hold that what Salmon *meant* to say was not that I endorse or echo the conceptual claim (D), but that I endorse or echo the empirical claim (D\*)? Not really. For a start, I mention neither (D) nor (D\*) in the passage of *Descriptions* he cites, or indeed anywhere else of the book. Since (D\*) is a consequence of (D\*), of course it is implicit in TL; so is it plausible to hold that Salmon *meant* to say was not that I endorse or echo the *conceptual* claim (D) in *Descriptions*, but that I endorse or echo the *empirical* claim (D\*) in TL? Not really. Salmon’s own words make it very clear he really does mean to pin on me (whether in *Descriptions* or in TL) the *conceptual* claim (D): concerning the “frequently heard objection” to complex singular terms (“endorsed or echoed” by “for example, Stephen Neale in *Descriptions*”) he says,

The objection relies on a  $\lambda$ -abstraction theorem. . . if it were sound, the assumed abstraction theorem would establish more generally that that the very notion of an *open designator* (a designating expression containing a free variable) is semantically incoherent (Salmon, 2003, p. 534, n. 47.)

Salmon’s talk of reliance on a *theorem* reveals that he has failed to see that I commit myself only to a serious exploration of the *empirical* hypothesis (D\*)—and do so in TL, not in *Descriptions*—and that the commitment is a trivial consequence of my commitment to a serious exploration of empirical hypothesis (C\*). I agree with Salmon that the conceptual thesis (D) is false, that the abstraction theorem is unsound, that there is nothing semantically incoherent about quantifying into singular terms. But it is an empirical issue whether or not natural language actually avails itself of such quantification, not an issue to be decided by looking at formal languages and finding fault with alleged theorems.

As far as *Descriptions* is concerned, Salmon has misconstrued me as making a fully general claim about theories of descriptions rather than one specific to the theories of Strawson and Hornstein. And as far as TL is concerned, he has

misinterpreted my *empirical* hypothesis (that natural language actually fails to exploit the possibility of non-atomic singular terms) for an in-principle, technical claim, susceptible of formal proof.

Salmon is right, however, that some logicians and philosophers have maintained that quantifying into singular terms is incoherent, or maintained that doing so is incoherent if the singular term in question is directly referential, or maintained that doing so is incoherent if the singular term in question is directly referential and Russellian, structured propositions are semantic contents. And I agree with him that even the weakest of these claims is false. As he points out, Lepore and Ludwig (2000) and King (2001) in their respective discussions of complex demonstratives maintain that the idea of directly referential singular terms containing variables bound by exterior quantifiers is unworkable, indeed semantically incoherent; and Stanley (2002b) maintains that within a general semantic framework of structured propositions “it is fairly straightforward to show that if [King’s] data is taken at face-value, the direct reference account of complex demonstratives is false” (2002b, p. 607). I shall postpone discussion of data involving complex demonstratives themselves until the next section. For the moment it is enough to note that there is a very good reason that Stanley does not actually go on to demonstrate just how “straightforward” it is to show the direct reference account is false (when King’s data is taken at face-value and a structured proposition framework is assumed): it cannot be demonstrated. Salmon (2006a, 2006b) has shown that it is, in fact, a straightforward technical exercise to provide precisely the direct reference account that King and Stanley claim is unworkable. The idea of referring expressions—directly referential or otherwise—containing variables bound by exterior quantifiers is no more problematic, syntactically or semantically, than the idea of bound variables themselves being referring expressions; or the idea of sentences containing variables bound by exterior quantifiers. (Direct reference adds no special wrinkle since the variable is itself the paradigm of the directly referential expression.) The relevant facts ought to be clear from the fact that the occurrences of the open sentences  $Fx$  and  $Gx$  in  $\exists x(Fx \wedge Gx)$  do not undermine the technical ideas behind the compressed claims that (i) individuals are the extensions of singular terms, (ii) truth-values are the extensions of sentences, or (iii) functions from  $n$ -tuples of truth values to truth values are the extensions of  $n$ -place sentence connectives.<sup>34</sup> So Salmon is right on this particular matter, and Lepore and Ludwig, King, and Stanley are wrong.

#### 14. BINDING INTO DEMONSTRATIVE DESCRIPTIONS

As noted already, definite descriptions may contain variables bound by exterior quantifiers. Can demonstrative descriptions contain such variables? An example of a non-deictic use of a demonstrative description I mentioned in TL was one that Jamie Tappenden gave me:

- (20) Every man eagerly awaits that day when he retires.

What's interesting about this example is that on the interpretation Tappenden intended the pronoun 'he' must be bound by the exterior quantifier phrase 'every man', within whose scope it (and the demonstrative description as a whole) lie. This seems like a straightforward example of a well known use of a demonstrative description in lieu of, or interpreted as, a definite description.<sup>35</sup> On a non-deictic use of this sort, 'that  $\phi$ ' is a quantifier phrase by virtue of being understood as equivalent to a Russellian definite description; and this captures the fact that such cases do not appear to involve reference—Tappenden fastened onto the future-directed sentence (20) precisely to stress the absence of genuine reference.

Similar examples are discussed by King (2001) and Dever (2002). The latter suggests that binding into 'this  $\phi$ ' is more problematic than binding into 'that  $\phi$ ', contrasting examples similar to the following:

- (21) Every man dreads that moment when his eldest child leaves home.  
 (22) ? Every man dreads this moment when his eldest child leaves home.

Interestingly, it appears easier to bind into 'this  $\phi$ ' when the binding is only "implicit". The day arrives when Dever's eldest child leaves home. I happen to be staying with him in Austin that day. He says to me over breakfast, 'I've been dreading this moment.' I reply with (iii):

- (23) It's only natural—every man dreads this moment.<sup>36</sup>

Notoriously, quantifying into demonstrative descriptions used *deictically* is an ugly business. In TL I contrasted the following:

- (24) [every driver]<sub>1</sub> knows the mechanic working for him<sub>1</sub>  
 (25) ? [every driver]<sub>1</sub> knows that mechanic working for him<sub>1</sub>

Certainly (25) is strained when 'that mechanic working for him' is used deictically. But there seems to be nothing *semantically incoherent* going on here, for there is nothing incoherent about a scene in which I utter (25) while using four fingers, two from each hand let us suppose, to demonstrate each of four mechanics, each one of whom works for exactly one of the four drivers in question. It would be easy enough to *work out* what I as trying to *communicate*; yet (25) does not naturally lend itself to this use. Two explanations suggest themselves once we take care to separate: understanding 'that  $\phi$ ' used deictically as a referring expression and understanding it as a quantifier phrase that insists on large scope. Obviously, if it is a quantifier phrase that (for some reason) insists on larger scope than 'every driver', then we would have an explanation of the strained nature of (25). But we would have an equally good explanation if (a) 'that  $\phi$ ' is a referring expression, and (b) there is a prohibition (for some empirical reason) on referring expressions properly containing variables bound by exterior quantifiers—for example, because all natural language referring expressions are,

as a matter of empirical fact, semantically atomic. This is not a matter to be decided by fiat. The idea of referring expressions, even directly referential ones, having semantically relevant structure and containing variables bound by external quantifiers is perfectly coherent, syntactically and semantically. The interesting question is the empirical one I confronted in TL: is the possibility of semantically structured referring expressions actually exploited in natural language? And if not, *why* not?<sup>37</sup>

In Neale (1999) I gave examples which suggested it was not binding *per se* that was the problem:

- (26) Keith<sub>1</sub> likes that guitar he<sub>1</sub> is playing
- (27) [That/this guitarist]<sub>1</sub> likes that guitar he<sub>1</sub> is playing.
- (28) [The guitarist]<sub>1</sub> likes that guitar he<sub>1</sub> is playing.<sup>38</sup>

These seem fine even when ‘that guitar he is playing’ is being used deictically. What differentiates these examples from (25) is that *semantically singular* noun phrases are binding ‘he’, which means there is no *relativisation* of guitar to guitar player. The real issue, I suggested, is relativity rather than binding, an idea I have taken up elsewhere.<sup>39</sup>

## 15. ATOMISM: COMPLEXITY SCEPTICISM REVISITED

On the matter of referring expressions lacking semantic structure, in TL I took myself to be treading—up to a point—a path beaten by Russell. I said very little about what having or lacking semantic structure ultimately amounted to, mentioning only that my complexity scepticism squared with my reading of Russell and Wittgenstein. In “On Denoting” (1905), Russell pointed to the syntactic similarity descriptions bear to other denoting phrases; and a few years later in “Knowledge by Acquaintance and Knowledge by Description” (1911), he said that,

... *Scott* is merely a noise or shape conventionally used to designate a certain person. . . . But *the author of Waverley* is not merely conventionally a name for Scott; the element of mere convention belongs here to the separate words, *the* and *author* and *of* and *Waverley* . . . A man’s name is what he is called, but however much Scott had been called *the author of Waverley*, that would not have made him be the author; it was necessary for him actually to write *Waverley*, which was a fact having nothing to do with names. (1911: 218).

Reference appears to be a primitive, arbitrary, and often stipulated relation here, holding between a symbol and an individual (or, if general terms have references, a primitive etc. relation holding between a symbol and a universal, property, attribute, kind or whatever). Put in the terms of contemporary compositional semantics, it would appear to be Russell’s view that if



semantic composition must be invoked to determine the semantical import of some particular expression *E*—i.e. if composition must be invoked to determine *E*'s specific contribution to the proposition expressed by someone using a sentence containing *E*—then something quite different from this reference relation is being invoked (whether the contribution in question is an individual (universal, property, attribute, kind or whatever), or a condition uniquely satisfied by an individual (universal, property, attribute, kind or whatever)).

In the vocabulary relevant to TL—in particular, the hypothesis that referring expressions are rigid in extension—this becomes the idea that if semantic composition plays a part in determining the extension of a natural language expression *E*, then *E* is not a referring expression. No more is involved than this, though some philosophers might find it natural to go a step further by endorsing a generalisation of the “Millian” idea that if a natural language expression *E* is a referring expression, then *E*'s extension exhausts the special contribution that *E* makes to the proposition expressed when a sentence containing *E* is being used to express a proposition.<sup>40</sup> Russellianism about the reference relation itself and Millianism about the propositional contributions of referring expressions are distinct positions, but it is easy to see why they are likely to be joined at the hips for some philosophers. Thus we reach the position (which I neither needed nor endorsed in TL) that semantic composition plays no part in determining the propositional contributions of referring expressions. All I committed myself to was the position that the extension of a referring expression in natural language is never determined by extensional composition.

There are two obvious ways of making trouble for the complexity sceptic where singular terms are concerned.

(i) Demonstrative descriptions: ‘this poem’, ‘that tall woman shaking hands with Herman’ etc. seem to function as *bona fide* rigid, referring expressions but they also seem to have semantic structure that is implicated in determining their references. So, *prima facie*, demonstrative descriptions constitute straightforward counterexamples. (And if it can be demonstrated that one can quantify into demonstrative descriptions, the case for assigning them semantic structure looks watertight.)

(ii) The other way of making trouble for the complexity sceptic is more drastic: shake the sceptic's world by arguing that his Russellian bedrock is illusory: definite descriptions cannot be quantifier phrases and must be treated as singular terms. (This will also rock the world of the flaccidity sceptic as many definite descriptions are non-rigid.) This is the tack taken by Smiley (2004), who uses what I call an Argument from Nesting against Russellian analyses of descriptions. If sound, Smiley's argument would spell doom for the complexity sceptic and the flaccidity sceptic. But as we shall see, it turns on oversights concerning semantic composition and erroneous assumptions about natural language that

are readily exposed by working through a well known problem in compositional semantics.

## 16. TETHERING: FLACCIDITY SCEPTICISM REVISITED

If it is constitutive of expressions belonging to some class to be rigid in extension, let us say the class as a whole is a *tethering* class. The class of predicates is non-tethering because many predicates are non-rigid in extension. If the class of natural language referring expressions is tethering, and if members of the same class receive the same semantic treatment, then we can have little complaint with the idea that proper names and simple indexical pronouns are referring expressions. But definite descriptions will not be referring expressions because they comprise a provably non-tethering class. Let  $\phi$  be a predicate that is non-rigid in respect of extension. Suppose, for purposes of *reductio*, that ‘the  $\phi$ ’ is a referring expression. By hypothesis the extension of ‘the  $\phi$ ’ is an object, viz. the object it refers to. Which object is that? Any answer doing justice to the idea that ‘the  $\phi$ ’ is a referring expression must hold that if  $\phi$  is true of exactly one thing  $X$ , then  $X$  is the referent, hence the extension, of ‘the  $\phi$ ’. (A full answer must also say what happens if  $\phi$  is *not* true of exactly one thing, of course.) But by hypothesis  $\phi$  is not rigid in extension. So ‘the  $\phi$ ’ is not rigid in respect of extension. So the class of descriptions is non-tethering. So ‘the  $\phi$ ’ is not a referring expression. (Nor is any other description ‘the  $\psi$ ’, even if  $\psi$  is rigid in extension. ‘The square of 3’ is no more a referring expression than ‘the number of planets’ is.)

Under this hypothesis, sentences of natural language are not referring expressions either: only those that are necessarily true or necessarily false are rigid in respect of extension (truth value). Contingent sentences have different extensions in different possible states of the world (henceforth *world-states*). Why does this happen? Because the *predicates* these sentences contain may have different extensions in different world-states.<sup>41</sup> The general point here is that an expression  $E(f)$  belongs to a non-tethering class if it has as a constituent an expression  $f$  belonging to a non-tethering class. Call this the Principle of Permanent Pollution. Sentences and definite descriptions are not referring expressions because they do not comprise tethering classes. And they do not comprise tethering classes because they are permanently polluted by virtue of having as parts expressions belonging to non-tethering classes, viz. predicates.

## 17. TETHERING: COMPLEXITY SCEPTICISM REFINED

Two related questions now arise: (i) Is every tethering class a class of referring expressions? (ii) Can referring expressions contain proper parts—for example, a conjunctive noun phrase such as (29):

(29) Russell and Whitehead.

In TL, I took being *de jure* rigid in respect of extension to be connected in some way I did not articulate with being a semantic *atom*, so it was a short step to the hypothesis that natural language referring expressions have no semantically relevant structure. This amounted to the hypothesis that, in natural language, if semantic composition plays any rôle in determining the extension of an expression  $E$ , then  $E$  is not a referring expression; and if the extension of a referring expression exhausts its specific contribution to propositional content we reach the hypothesis that if semantic composition plays any rôle in determining the propositional contribution of an expression  $E$ , then  $E$  is not a referring expression. On that account, (29) is not a referring expression, which is why I inclined towards a quantificational treatment of conjunctive noun phrases. But with the concept of tethering in play, we can do better: if ‘and’ belongs to a tethering class, then (29) contains no parts belonging to non-tethering classes. In which case there is no impetus to say that (29) belongs to a non-tethering class and so no reason to deny that it is a referring expression, unlike, say, (30):

(30) Russell and the tallest mathematician at Cambridge in 1911.

Surely ‘and’ does belong to a tethering class. Let us begin with its use as an extensional sentence connective. The extension of an  $n$ -place extensional sentence connective is a function from  $n$ -tuples of truth-values to truth values, so every such expression has a rigid extension. (The extension of ‘and’, for example, is a function that maps the pair  $\langle \text{Truth}, \text{Truth} \rangle$  onto Truth, and the other three pairs onto Falsity. And no matter what world-state a sentence ‘ $\phi$  and  $\psi$ ’ is evaluated in, ‘and’ itself has the same extension.) So the class of extensional sentence connectives is tethering. But is that enough? What about the broader class of sentence connectives simpliciter, one that also includes *non*-extensional sentence connectives such as ‘before’, ‘after’, and ‘because’ (if such they are)? The truth of both  $\phi$  and  $\psi$  in two distinct world-states guarantees that ‘ $\phi$  and  $\psi$ ’ has the same truth-value (Truth) in both world-states; but it does *not* guarantee that ‘ $\phi$  before  $\psi$ ’ has the same truth-value in both states. Does this show that ‘before’ is not rigid in respect of extension? No. All it shows is that whatever the extension of ‘before’ is, it is not a function from pairs of sentence extensions to sentence extensions, i.e. all it shows is that ‘before’ is not an extensional connective, which we knew already. If we are to make sense of the idea of ‘before’ having as its extension a function from something-or-other to sentence extensions (truth-values), that something-or-other will have to be more fine-grained than pairs of truth-values. One natural idea would be to take it to be a function from pairs of *pairs of truth-values and times* to truth-values.<sup>42</sup> If that works, then ‘before’ (similarly ‘after’) will be rigid in extension. Something more complex would be required for ‘because’, of course; but, in principle, judicious selections of extensions for individual non-extensional connectives could provide the means of characterizing

tethering classes of sentence connectives that properly include the tethering class of extensional sentence connectives. The interesting question is which of these classes are of theoretical significance. Ideally, one would like expressions that function in the same way as one another *syntactically* to function in the same way as one another *semantically*, but of course “the same way” admits of finer and coarser characterizations in both the syntactic and semantic realms. This is a huge topic that cannot be explored beneficially here.<sup>43</sup> But, on the face of it, there is no obvious reason not to hold that ‘Whitehead and Russell’ is a referring expression while endorsing the Principle of Permanent Pollution, yielding a position that is less stringent than, and at least as principled as, the one adopted in TL.

## 18. A PROBLEM OF COMPOSITION

Explaining the interpretation of sentence (31) in Frege-style function-argument fashion is straightforward:

(31) [<sub>S</sub> George [<sub>VP</sub> respects Scott]].

Names and sentences are the Fregean building blocks in terms of which other expressions are defined compositionally. (i) The names ‘George’ and ‘Scott’ are expressions of type *e* (in Montague’s sense) and each stands for an individual. (ii) The sentence as a whole is an expression of type *t* (again in Montague’s sense) and stands for a truth-value. Now we start the functional building. (iii) The whole VP ‘respects Scott’ is an expression of type  $\langle e, t \rangle$  and stands for a (first-level) function from individuals to truth-values. (iv) The transitive verb ‘respects’ is an expression of type  $\langle e, \langle e, t \rangle \rangle$  and stands for a (first-level) function from individuals to functions from individuals to truth-values.

Now replace the subject expression in (31) by a quantifier phrase such as ‘every prince’ or (assuming descriptions are quantifier phrases) ‘the king of England’:

(32) [<sub>S</sub> every poet [<sub>VP</sub> respects Scott]]

(33) [<sub>S</sub> the king of England [<sub>VP</sub> respects Scott]].

Let us focus on (33). By hypothesis, ‘the king of England’ is not of type *e* and does not stand for an individual, so our compositional machinery grinds to a halt with (33) until we specify how quantifiers work. The Fregean answer—which comes quickly into view if one thinks of ‘every  $\phi$  is  $\psi$ ’ as predicating of  $\psi$  that it is true of every  $\phi$ —is that quantifiers are *second-level* predicates: they stand for second-level functions. Whereas in (31) the (first-level) function that the VP ‘respects Scott’ stands for operates on the individual that ‘George’ stands for, in (33) that (first-level) function VP stands for is itself the operand of the

(second-level) function that ‘the king of England’ stands for.<sup>44</sup> That is, ‘the  $\phi$ ’ (similarly ‘every  $\phi$ ’, ‘some  $\phi$ ’ etc.) is an expression of type  $\langle\langle e, t \rangle, t\rangle$  and stands for a (second-level) function from (first-level) functions from individuals to truth values, to truth-values.<sup>45</sup> Since the determiner ‘the’ (similarly ‘every’, ‘some’ etc.) combines with an expression of type  $\langle e, t \rangle$  to form a quantifier phrase, it is of type  $\langle\langle e, t \rangle, \langle\langle e, t \rangle, t\rangle\rangle$ .

So far, so good. But when we turn our attention to (34), where the quantifier is not in subject position, our compositional machinery grinds to a halt:

(34) [<sub>S</sub> Scott [<sub>VP</sub> respects the king of England]].

The transitive verb ‘respects’ is of type  $\langle e, \langle e, t \rangle \rangle$  and stands for a (first-level) function from individuals to functions from individuals to truth-values. Where the direct object is an expression of type  $e$ , as it is in (31)–(33), all goes smoothly. But we are stuck when the direct object is a quantifier as in (34): the quantifier is of type  $\langle\langle e, t \rangle, t\rangle$ , not of type  $e$ , and we have no principle for composing the function an expression of type  $\langle e, \langle e, t \rangle \rangle$  stands for with the function an expression of type  $\langle\langle e, t \rangle, t\rangle$  stands for. Of course, insisting that descriptions are really of type  $e$  would solve this particular instance of the problem, but the general problem would remain unsolved because it would leave us with no account of, say, (35a), (35b), (35c) etc.:

- (35) a. George respects every poet  
 b. George respects no poet  
 c. George respects only poets with limps.

What are we to do, then, when quantifier phrases are direct objects, indeed when they are quite generally not in subject position? We can tease out what is at issue here by looking at three common ways of getting the ball rolling again.

The first two methods build upon a semantic unification of the class of noun phrases achieved by the direct analysis of names (and other purported referring expressions) as quantifier phrases (perhaps as Russellian descriptions).<sup>46</sup> The semantics of noun phrases formed by conjoining names and quantifiers will now be simplified: ‘Russell and some of his students’ is now a co-ordination of two expressions of the same semantic type, and this will harmonise with the condition from syntactic theory that only expressions of the same category may be co-ordinated. Every expression in our unified category of noun phrase will now stand for a second-level concept that takes as its operand a first-level concept. But our original problem is not solved because the relevant first-level concepts are not unified. The type of first-level concept that serves as operand depends upon whether the operating noun phrase is in subject position or in direct object position. If it is in subject position it combines with a VP, an expression of type  $\langle e, t \rangle$ . If it is in object position it combines with a transitive

verb, an expression of type  $\langle e, \langle e, t \rangle \rangle$ . So although we have a unified category of noun phrases (encompassing name and quantifiers, intuitively speaking) every noun phrase will now have to be an expression of type  $\langle \langle e, t \rangle, t \rangle$  when in subject position, and an expression of type  $\langle \langle e, \langle e, t \rangle \rangle, \langle e, t \rangle \rangle$  when the direct object of a verb (and an expression of some other type altogether when appearing in a different grammatical position). This duplicity (ultimately multiplicity) of semantic function is unsatisfactory if left in such a crude form, but the general nature of the problem we are facing is now clearer: how are we to provide a systematic, compositional treatment of sentences containing more than one noun phrase standing for a second-level concept? In short, how are we to handle multiple quantification?<sup>47</sup>

(a) The first method involves adjusting the types to which expressions that combine with noun phrases belong, in particular transitive verbs. If we can restrict ourselves to noun phrases appearing in just subject and direct object positions, we continue to treat all occurrences of noun phrases as uniformly of type  $\langle \langle e, t \rangle, t \rangle$  and obtain the desired results by a complementary adjustment to transitive verbs: they are no longer of type  $\langle e, \langle e, t \rangle \rangle$  but themselves functors of type  $\langle \langle \langle e, t \rangle, t \rangle, \langle e, t \rangle \rangle$ .<sup>48</sup>

(b) Alternatively, we could permit systematic *type-shifting*: the semantic type to which quantifier phrases belong shifts systematically with structural position (subject, object, etc.).<sup>49</sup> (In theory, opening the door to type-shifting makes it possible to abandon the idea of unifying names and quantifier phrases, the semantic types to which transitive verbs and prepositions belong shifting systematically according as their objects are referring expressions or quantifier phrases, but this leads into complexities that are irrelevant to present concerns.)

(c) A third method of moving things along has an explicitly syntactic dimension: the grammatical structure of a sentence is factorized into two tightly connected representations: PF (“Phonetic Form”), the level relevant to its pronunciation and perception; and LF (“Logical Form”), the level relevant to its interpretation. At LF, quantifier phrases do not appear in argument positions, which are occupied by variables the quantifiers bind. For example, (34) and (34’) approximate the PF and LF of a single sentence.<sup>50</sup>

(34) [<sub>S</sub>Scott [<sub>VP</sub> respects the king of England]]

(34’) [the king of England]<sub>x</sub> [<sub>S</sub> Scott respects *x*].

(34) and (34’) are related by a syntactic operation of *quantifier movement* (lowering or raising, depending upon one’s point of departure, which will depend upon the general shape of the proposed grammar). The main point is that the restricted quantifier in (34’) binds a variable serving as the direct object of the verb (cf. ‘the king of England is such that Scott respects him’) and variables are of type *e*. In short the direct object of the verb is a variable and hence an expression of type *e* just as it is in examples (31)–(33).<sup>51</sup>

There is no need to choose between these approaches to the problem here. It is enough to see the generality of the problem to which these approaches are meant to be the beginnings of solutions.

## 19. THE ARGUMENT FROM NESTING

Some philosophers have argued that a unitary Russellian account of descriptions cannot succeed because descriptions are ambiguous between quantificational and referential readings.<sup>52</sup> Others have argued that descriptions are ambiguous between quantificational and predicational readings.<sup>53</sup> Still others have proposed sets of quantificational, referential, and predicational readings.<sup>54</sup>

Unitary predicational readings have also been proposed.<sup>55</sup> And, of course, so have unitary referential readings.<sup>56</sup> I want here to examine a formal argument for a unitary referential reading recently presented by Smiley (2004). According to Smiley, neo-Russellians have been “over-influenced by the similarities between the ‘the *N Fs*’ and the quantified sentences ‘every *N Fs*’, ‘some *N Fs*’, ‘no *N Fs*’ etc.’ (2004: 153).<sup>57</sup> Smiley’s complaint is that sentences involving certain nested descriptions produce insurmountable problems for Russell’s theory and the neo-Russellian theories it has engendered. In short, Smiley argues that such theories of descriptions are unworkable. Singular descriptions are singular terms. And plural descriptions are plural terms.<sup>58</sup>

If Smiley’s argument is sound, complexity scepticism is refuted. But upon examination, Smiley’s argument crumbles, and seeing how it crumbles is of considerable pedagogical value (whether or not structure scepticism is eventually refuted).

Russell’s favourite natural language examples of definite descriptions involve ordinary descriptive functors: ‘the king of ()’, ‘the author of ()’, and ‘the father of ()’.<sup>59</sup> But it would be a mistake to see in this any sort of lingering attachment on Russell’s part to the idea that descriptions are singular terms. The favouritism is merely a reflex of Russell’s philosophy of mathematics—in particular, the attempted logicist reduction—within which the definite descriptions of interest are those that logicians often construe as standing for functions, seemingly composed of descriptive functors such as ‘the successor of ()’ and ‘the sum of () and ()’—though in \*30 of *Principia Mathematica* even these are further contextually defined. Hence Russell’s genuine concern with uniqueness and his adaptation of Peano’s *iota* notation.

Logicians typically construe descriptive functors as syntactic and semantic units. Specifically, (i) they typically construe them as *syntactic constituents* of English sentences which operate syntactically on other constituents to form larger constituents (‘the square of two’, ‘the father of Charles II’, etc.); and (ii) they typically construe the semantic values of the larger constituent as determined in function-argument fashion from the semantic values of the purported functor (‘the square of’, ‘the father of’) and its argument (‘two’, ‘Charles II’), i.e. they

construe functors as expressions of type  $\langle e, e \rangle$ . These ideas may well have been inherited from Frege, who in ‘Function and Concept’ (1891) talks of splitting ‘the capital of the German Empire’ into the parts ‘the capital of’ and ‘the German Empire’. The former is the expression of a function: “If we take the German Empire as the argument, we get Berlin as the value of the function” (1891: 31–32).<sup>60</sup> Interestingly, in ‘On Sense and Reference’ (1892), Frege suggests a different parsing, essentially the one arrived at much later by generative linguistics on the basis of empirical considerations (one of which I am about to discuss). Of the expression ‘the negative square root of 4’, Frege says it is “a compound proper name constructed from the expression for a concept with the help of the singular definite article” (1892: 71). This later parsing—which Jason Stanley informs me is also to be found in *Grundgesetze* (1893)—is compatible with treating the definite article as of type  $\langle \langle e, t \rangle, \langle \langle e, t \rangle, t \rangle \rangle$  (in a ‘Russellian’ vein) or type  $\langle \langle e, t \rangle, e \rangle$  (which seems to be Frege’s idea in the remark just quoted from ‘On Sense and Reference’).

If contemporary logicians work with Frege’s earlier parsing this is presumably because of their interest in functions; but there is strong evidence from linguistic theory that this parsing is incorrect for descriptions in natural language. (Similarly the analogous parsings of ‘no multiple of two’, ‘some relative of Charles II’ etc., according to which the first three words would form a constituent.) One standard test for constituent structure is co-ordination with conjunction. In the predicate calculus conjunction is used only to conjoin two whole sentences (open or closed) to form a larger sentence. In English, however, the word ‘and’ is used to conjoin two phrases of the same type more generally (to form a constituent of that same phrasal type). For example, two names (or perhaps two NPs) are conjoined in ‘Whitehead and Russell wrote *Principia Mathematica*’, two nouns (simple or complex) in ‘Peter is my colleague and friend’ and in ‘Peter is a good colleague and an even better friend’, two VPs in ‘Oswald took aim and shot Kennedy’, and two transitive verbs in ‘Oswald shot and killed Kennedy’. The generalization can be stated using a schematic phrase structure rule:

$$X \rightarrow [_{XP} X \text{ and } X].$$

The co-ordination test correctly predicts that (36a)–(36d) below are all grammatical strings, whereas (36e) is not (indicated with ‘\*’):

- (36) a.  $[_{NP}[_{NP} \text{ the king of France}] \text{ and } [_{NP} \text{ the queen of France}]]$  are here  
 b. the  $[_N[_N \text{ king}] \text{ and } [_N \text{ queen}]]$  of France are here  
 c.  $[_{NP}[_{NP} \text{ the king}] \text{ and } [_{NP} \text{ the queen}]]$  of France are here  
 d. the  $[_N[_N \text{ king of France}] \text{ and } [_N \text{ queen of Spain}]]$  are here  
 e. \*  $[_{?P} [_{?P} \text{ the king of}] \text{ and } [_{?P} \text{ the queen of}]]$  France are here.<sup>61</sup>



The grammaticality of (36c) shows that ‘king of France’ is a constituent of ‘the king of France’, and the ungrammaticality of (36d) below shows that ‘the king of’ is not. It seems, then, that we must abandon the idea that the apparent functors ‘the king of’ and ‘the successor of’ are constituents of English sentences. And unless we give up the assumption that it is syntactic units that compose semantically in function-argument fashion, it would seem the semantic value of ‘the king of France’ (whatever it is) is not obtained by function-argument application involving the semantic values of ‘the king of’ and ‘France’ because ‘the king of France’ is actually the syntactic result of combining ‘the’ with ‘king of France’.<sup>62</sup>

As we shall see, this fact alone undermines both the Argument from Nesting for a unitary referential semantics for descriptions put forward by Smiley and his own theory which assumes ‘the father of’ is a syntactic and semantic unit.

Let us begin with Smiley’s attack on the ‘neo-Russellian’ treatment of descriptions, which he sees exemplified in the work of Evans (1977, 1982). On Evans’s account, says Smiley, ‘the  $\phi$ ’ (similarly ‘some  $\phi$ ’ and ‘every  $\phi$ ’) is a quantifier phrase, construed as a second-level predicate. (2004: 136.)<sup>63</sup> According to Smiley, Evans’s theory crumbles under the weight of nested descriptions such as (37):

(37) the father of the father of Charles II.

In the categorial grammar Evans works with, the name ‘Charles II’ belongs to the category N of genuine singular terms, corresponding to Montague’s type  $e$ , and VPs belong to the derived category S/N, corresponding to type  $\langle e, t \rangle$ , where S is the category of sentences, corresponding to type  $t$ . Descriptions are second-level predicates belonging to the category S/(S/N), corresponding to  $\langle \langle e, t \rangle, t \rangle$ . Smiley objects that this makes it “impossible for ‘the father of’ to be fitted consistently into any category” (2004: 136). Why? Because the second occurrence of ‘the father of’ in (37) combines with a name (‘Charles II’) to form a second-level predicate, which requires it to belong to the category (S/(S/N))/N, corresponding to type  $\langle e, \langle \langle e, t \rangle, t \rangle \rangle$ ; whereas the first occurrence combines with a second-level predicate (‘the father of Charles II’) to form a second-level predicate, which requires it to belong to the category (S/(S/N))/(S/(S/N)), corresponding to type  $\langle \langle \langle e, t \rangle, t \rangle, \langle \langle e, t \rangle, t \rangle \rangle$ . The expression ‘the father of’ is “torn”, says Smiley, between belonging to (S/(S/N))/N and belonging to (S/(S/N))/(S/(S/N)) (2004: 136, n. 8).

The way out, according to Smiley, is to: (a) accept that descriptions are singular terms after all, and (b) treat ‘the father of’ as a functor that uniformly combines with a singular term to form another singular term, i.e. as uniformly of the category N/N, corresponding to type  $\langle e, e \rangle$ .<sup>64</sup>

Smiley assumes without comment Frege’s (1891) parsing of descriptions. So a crucial premise in his Argument from Functors is that ‘the father of’ constitutes a genuine constituent belonging to some particular category.

But this we have already rejected on good empirical grounds: it fails the co-ordination test for constituent structure. Given Evans's (1985) pioneering work on grammatical constraints on anaphora, doubtless he was familiar with the co-ordination test (and other standard tests for constituent structure) and would have rejected Frege's 1891 parsing, assumed by Smiley, so Evans's theory would be untouched by the Argument from Nesting as presented.

But even if Evans *had* assumed the rejected syntactic structure nothing of special interest to neo-Russellian accounts of *descriptions* would flow from Smiley's argument. In its best light the argument would merely illustrate a well-known fact I discussed earlier: there is a fully general compositional problem to be solved involving quantifier phrases in non-subject positions. Reflection on examples (38)–(41), and countless variations, makes it clear that whatever Smiley's argument would demonstrate about descriptions if successful, it would demonstrate about *all* quantifier phrases:

- (38) every enemy of every enemy of Nixon
- (39) some denigrator of some denigrator of Nixon
- (40) the mayor of every former capital of Germany
- (41) every former lover of some former lover of Casanova.

By Smiley's lights it should be impossible for 'every enemy of' to be fitted consistently into any category. Why? Because the second occurrence of 'every enemy of' in (8) combines with a name ('Nixon') to form a second-level predicate, which requires it to belong to the category  $(S/(S/N))/N$ , corresponding to type  $\langle e, \langle \langle e, t \rangle, t \rangle \rangle$ ; whereas the first occurrence of 'every enemy of' combines with a second-level predicate ('every enemy of Nixon') to form a second-level predicate, which requires it to belong to the category  $(S/(S/N))/(S/(S/N))$ , corresponding to type  $\langle \langle \langle e, t \rangle, t \rangle, \langle \langle e, t \rangle, t \rangle \rangle$ . The expression 'every enemy of' is "torn", Smiley would have to say, between belonging to  $(S/(S/N))/N$  and belonging to  $(S/(S/N))/(S/(S/N))$ .

Obviously we would not be moved by an Argument from Nesting that concluded, from the well-formedness of (38), that 'every enemy of Nixon' cannot be a second-level predicate, that it is in fact a singular term; but these are precisely the conclusions Smiley's overall argument would lead us to draw! So the Argument from Nesting goes precisely nowhere.<sup>65</sup> And once we get the parsing right, we can see the real issue has nothing especially to do with descriptions. A preposition such as 'of' may combine with either a singular term or a quantifier phrase to form a PP (prepositional phrase). And a nominal such as 'father' or 'enemy' may combine with a prepositional phrase ('of Nixon' or 'of every man') to form a nominal phrase—let us agree to put aside differences between argument and adjuncts for present purposes. We are now in familiar territory, covered earlier. The name 'George' (category N) may combine with the VP 'respects Scott' (category S/N) to form sentence (31)

(31) [S George [VP respects Scott]].

The VP is the functor here, and ‘George’ its argument. When we replace the singular term in *subject* position by a quantifier phrase, say, ‘the king of England’ to produce

(33) [S the king of England [VP respects Scott]]

we are replacing an expression of category N with one of category S/(S/N), and *this* is the functor with the VP as its argument. A problem arises when we replace the *direct object* in (31) by a quantifier phrase, say, ‘the author of *Waverley*’ to produce

(34) George respects the author of *Waverley*.

To what category does the verb ‘respects’ belong? Is it “torn” (as Smiley would be committed to saying) between belonging to (S/N)/N and (S/N)/(S/(S/N))? No. We have already seen three common solutions to this familiar problem, which is all that Smiley’s objection to neo-Russellian accounts of descriptions reduces to (but with quantifier phrases as the objects of *prepositions* rather than of verbs).<sup>66</sup> In summary, Smiley has raised not even the hint of a problem for Russell or neo-Russellians that is not already part of a well-worn, well-understood, fully general problem in the theory of natural language quantification to which there are several well-known approaches. Equally well-known are the reasons for favouring this or that approach, which concern such controversial issues as the existence of a level of syntactic representation distinct from surface form and preferred statements of a principle of semantic composition, as well as syntactic and semantic issues involving scope, binding, VP-deletion, cross-over, and adverbial modification.<sup>67</sup>

## 20. INCOMPLETENESS AND REFERENTIAL USAGE

Two separate phenomena involving the use of definite descriptions seem to further the case that the complexity sceptic cannot provide an adequate semantics for such expressions: (i) using descriptions that are *incomplete*, and (ii) using descriptions *referentially*.

(i) Let us say that a description ‘the  $\phi$ ’ is *proper* if and only if its nominal  $\phi$  is true of exactly one thing, and *improper* otherwise. And let us say that an improper description ‘the  $\phi$ ’ is *empty* if and only if  $\phi$  is true of nothing, and *incomplete* if and only if  $\phi$  is true of more than one thing.<sup>68</sup> It beggars belief to think Russell was unaware that incomplete descriptions such as ‘the table’, ‘the book’ and so on are used regularly and felicitously in ordinary talk. He displayed virtually no interest in them when setting out the Theory of Descriptions, however, because

he was concerned with descriptions of more philosophical significance—the successor of zero’, ‘the square of the successor of zero’, ‘*sin x*’ (‘the sin of *x*’) and so on—and he appears to have had little interest in incompleteness per se, evidently regarding it as a feature of our ordinary use of language that should be banished from technical work and the use of a formal language. Indeed, as Smiley (2004) observes, Russell’s favourite natural language examples involve “descriptive functors”: ‘the king of (*x*)’, ‘the author of (*x*)’, and ‘the father of (*x*)’. But the following question looms large for anyone with more of an interest in the use of natural language (and less of an interest in its reform): How are we to explain the fact that a speaker can use ‘the  $\phi$  is  $\psi$ ’ to say something true even though  $\phi$  is (apparently) true of more than one thing—and, often enough, known by the speaker and the hearer to be so—a fact that is (apparently) inconsistent with the truth conditions provided the Theory of Descriptions?<sup>69</sup>

Quine (1940) was perhaps the first to say anything useful about incomplete descriptions in natural language: a felicitous utterance of an incomplete description ‘the  $\phi$ ’ is an “elliptical” utterance, Quine says, one whose content, in context, can be captured by an utterance of a fuller description ‘the  $\phi$  that  $\xi$ s’.<sup>70</sup> Sellars (1954) took more or less the same tack when responding to Strawson’s (1950) discussion of incomplete descriptions.<sup>71</sup> On this account, then, the felicitous use of an incomplete description ‘the  $\phi$ ’ is perfectly consistent with Russell’s quantificational, general, object-independent treatment of ‘the’.<sup>72</sup> In Neale (1990), I called this the *explicit approach* to the problem of incompleteness, and it was the position towards which I gravitated.<sup>73</sup>

(ii) A very different approach to many felicitous utterances of incomplete descriptions can be found in work influenced by Strawson (1950) and Donnellan (1966): On at least some occasions of use, definite descriptions function as referring expressions rather than as devices of quantification; I may use a sentence ‘the  $\phi$  is  $\psi$ ’ (e.g. ‘the table is dirty’) on a particular occasion to say of some particular object in the immediate perceptual environment (presumably one that is recognisably  $\phi$ , though this might be debated), that it is  $\psi$ ; in such a case I express a singular (or object-dependent) proposition; so an adequate semantics of English must posit a referential reading of descriptions that is semantically distinct from the Russellian reading; when ‘the  $\phi$ ’ is used in the Russellian way, the proposition I express using ‘the  $\phi$  is  $\psi$ ’ is general (or object-independent), but when I use it in the referential way, the proposition is singular (or object-dependent); in short, definite descriptions are systematically ambiguous between Russellian and referential readings; and (assuming some sort of compositionality requirement) the definite article itself is systematically ambiguous.

In *Descriptions*, I defended the following claims in connection with (i) and (ii):

(1) We must separate cleanly using a definite description that is incomplete, and using a definite description referentially (a) because incomplete descriptions can be used felicitously when not being used referentially (1990: 94, 114 n 43), and

(b) because a description does not have to be incomplete to be used referentially (1990: 94).<sup>74</sup>

(2) Following Evans (1982), we can usefully separate referential<sub>D</sub> and referential<sub>N</sub> uses of descriptions, the former akin to uses of *demonstratives* in the manner of Kaplan (1978) and Wettstein (1982) (hence 'D'), the latter akin to uses of *names* (hence 'N') (1990: 85–86).

(3) Natural accounts of both referential<sub>D</sub> and referential<sub>N</sub> uses of descriptions that are consistent with a unitary Russellian, quantificational semantics emerge from reflections on Grice's distinction between what a speaker *says* and what he *means*—rather than, say, Kripke's distinction between semantic reference and speaker reference, which seemed to me highly problematic—and by contrasting the object-independent proposition forming the content of what a speaker says by uttering 'the  $\phi$  is  $\psi$ ' with the object-dependent proposition forming the content of something else that he may mean (1990: 83–91).<sup>75</sup>

(4) The usual arguments for semantically distinct referential readings—including the Argument from Incompleteness—fail to demonstrate the desired conclusion (1990: 91–102).

(5) Methodological considerations of the sort advanced by Grice (1969) and Kripke (1977) strongly favour a unitary Russellian analysis (1990: 90–91).

Today I take issue with (3): I still see the neo-Russellian analysis of descriptions as basically correct; but I no longer see the invocation of an antecedently required distinction between *saying* and *meaning* as getting to the heart of the matter. I made a quick stab at explaining why in Neale (2004). I want now to set out the proposal in more detail and examine its consequences for complexity scepticism.

## 21. DEFINITE DESCRIPTIONS AS HYBRIDS?

I began with the intuition that speakers often *refer* to things using certain types of expressions and with the Gricean idea that a speaker's referring to something on a given occasion is the logically basic notion of reference in a theory of meaning. I then suggested that speaker's reference is the *only* referential notion of theoretical significance, the notion that figures in (a) an account of what speakers are *saying*, and (b) an account of what referring expressions *are*. Prima facie, definite descriptions qualify as referring expressions under the preliminary characterisation I gave—even the staunchest of Russellians is apt to concede that speakers often use definite descriptions to refer to things. Thus a clash with (T1), the central thesis of TL: definite descriptions are not semantically atomic, so according to (T1) they are not referring expressions. There is no wriggling out of this by taking the present article to concern *speaker's* reference and the article it revisits as concerning *semantic* reference, and taking a distinct notion of referring expression to correspond to each notion: for I was implicitly construing the superficially semantic notion of reference I worked with in TL as a

place-holder for a full-fledged speaker-based notion, just as I was in *Descriptions* (where I explicitly rejected Kripke's speaker's reference vs. semantic reference analysis of referential uses of descriptions). It would seem, then, that I am in trouble: I can maintain (T1) only by modifying the provisional characterisation of referring expression or by denying that we regularly use descriptions to refer to things!

But matters are not as bleak as they may seem initially. We need to invoke two bits of theory to appreciate this: (i) Gödelian completions, and (ii) Schiffer's distinction between referring *with* and referring *in*. I want to work up to this slowly to avoid misunderstanding.

(i) There is nothing to prevent a quantifier phrase from containing a referring expression as a proper part. Consider 'France' in the following: 'every politician in France', 'some countries bordering France', 'no country other than France', and 'the king of France' (assuming definite descriptions are quantifiers). Moreover there is nothing to prevent speakers from performing acts of referring in uttering sentences containing quantifiers understood *as if* it they contained referring expressions as proper parts ('the mayor can ban demonstrations', 'no other mayor can ban demonstrations', 'every hour of rain increases the risk of disease'.)

(ii) The mere presence of a referring expression  $\alpha$  inside a quantifier [*every*  $x$ :  $\phi x\alpha$ ], [*no*  $x$ :  $\phi x\alpha$ ], [*the*  $x$ :  $\phi x\alpha$ ] etc. does not mean that the quantifier itself is a referring expression.<sup>76</sup> But it does force us to be careful when we are using the theoretical expressions 'singular proposition' and 'object-dependent proposition'. Typically we will say that the proposition someone expresses by uttering (42) is general, object-independent:

- (42) Some countries bordering France are complaining about territorial integrity.

But we must accept that when we say this we are focussing on the subject phrase as a whole, not on the word 'France', which is a proper part of that phrase. Propositions themselves are language-independent entities; but when we talk about the propositions we express using particular sentences, often there is a "linguistic wink": the proposition someone expresses by uttering (42) is object-independent with respect to the subject phrase as a whole, so to speak, because of its semantic type, but object-dependent with respect to one of its parts, viz, 'France'.<sup>77</sup>

(iii) When we articulate plausible completions for a particular use of an incomplete description 'the  $\phi$ ' (or the use of some other quantified noun phrase, 'every  $\phi$ ', 'no  $\phi$ ', etc.), nothing mandates that we use *just* predicates or *just* referring expressions. We might try to explain in words a particular use of 'the king' with the help of 'the king of France', or for that matter, 'the French king'. When the completer—rather than the completed phrase—contains a referring expression, let us talk of an *object-dependent completer*. (It is

plausible to hold that some completers which appear to involve only predicates are implicitly object-dependent, ‘French’, for example, on at least one of its uses.)

(iv) There is nothing to prevent a quantifier phrase whose full nominal is satisfied by exactly one object from containing as a constituent a referring expression that is being used to refer to the very same object. If Fred is the tallest person in his family, then ‘the tallest person in Fred’s family’ is a straightforward example.

(v) There is nothing to prevent the identity predicate from occurring inside a quantifier’s nominal. The definite descriptions ‘the film theorist Stephen Neale’, ‘the philosopher Stephen Neale’, and ‘the Graham Greene character Stephen Neale’ are plausible examples. In our formal language, such descriptions have the Gödelian form

$$(43) [the\ x: \phi x \bullet x = \alpha].^{78}$$

For the sake of precision, let us say that a quantifier  $[Qx: \phi]$  is Gödelian if and only if  $Qx$  binds at least one variable in an open identity (or non-identity) sentence occurring in  $\phi$ , where  $\alpha$  is a singular term (open or closed).<sup>79</sup> ‘Some country other than France’ can be rendered schematically as

$$(44) [some\ x: \phi x \bullet x \neq \alpha].$$

(vi) Taken to its limit, the idea of Gödelian descriptions presents us with

$$(45) [the\ x: x=\alpha]$$

which, based on a remark I made in *Descriptions* (see below), I shall call a *hollow* Gödelian description by way of contrast with a *solid* Gödelian description (43), which contains a *predicate other than identity*. If there are hollow Gödelian descriptions in natural language they are arch—‘the thing identical to Aristotle’ for example—and for good reason. Assuming a quantificational analysis of descriptions, (45) is *technically* a quantifier; but there is a very strong temptation to say that (45) is always equivalent to the singular term  $\alpha$  in respect of its impact on truth conditions and therefore semantically distinct from  $\alpha$  only in some uninteresting *formal* sense. That is the position I took in Neale (1990) when I said,

A phrase of the form  $[the\ x: x=\alpha]$  is technically a Russellian definite description; but the claim that referential uses of descriptions do not require distinctive non-Russellian interpretations would indeed be *hollow* [emphasis added] if the Russellian position could be maintained only by employing the identity relation to concoct descriptions of this form (e.g.,  $[the\ x: x=that]$ .)” (Neale 1990: 115 n. 53).<sup>80</sup>

It is also the position I took in Neale (2001):

...there is some inclination, even amongst the staunchest Russellians, to view [ $\iota x(x=a)$ ] as a verbose form of  $a$ , which *technically* it is not (since it is an incomplete symbol). ... Attaching a one-place predicate  $\phi$  to  $a$  yields  $\phi a$ , while attaching it to  $\iota x(x=a)$  yields [(1)], which, on Russell's account, is shorthand for [(2)]:

- [(1)]  $\phi \iota x(x=a)$ ,  
 [(2)]  $\exists x(\forall y(x=a \equiv y=x) \bullet \phi x)$ .

And while *technically* [(2)] expresses a general proposition, one feels that it differs only in some ephemeral way from the singular proposition expressed by  $\phi a$ . In short, one feels that if there is a real difference between  $\phi \iota x(x=a)$  and  $\phi a$ , it can manifest itself only in the context of an explicit metaphysics allied to a precise account of the relation between language and reality. Much the same might be said about the difference between  $\phi a$  and [(3)]

- [(3)]  $\exists x(\phi x \bullet a=x)$

which... Wittgenstein [*Tractatus* (5.441, 5.47)] and Quine [(1970: 25)] see as equivalent. (Neale 2001: 125–26).<sup>81</sup>

Because of the explicitly conjunctive nature of solid Gödelian descriptions, I used them in *Descriptions* only when analysing items that seemed to me semantically conjunctive. The appositive constructions (46) and (47), for example, I analysed as (48):<sup>82</sup>

- (46) John Smith, the man who threw ice cream at the pope, is  $\psi$   
 (47) The man who threw ice cream at the pope, John Smith, is  $\psi$   
 (48) [*the  $x$ : man  $x \bullet x$  threw ice cream at the pope  $\bullet x = John Smith$ ]  $\psi x$ .*

It is not the English description itself that is given a Gödelian analysis in these sentences, but the *combination* of the name and the description when one of them occurs in apposition to the other.

Similarly, I analysed the noun phrase ‘another man’ as a solid Gödelian *indefinite* description

- (49) [*an  $x$ :  $x \neq \alpha \bullet man x$* ]

where  $\alpha$  is a referring expression, perhaps a variable bound by another quantifier phrase, as in (51), the analysis offered of (50):<sup>83</sup>



- (50) every man who lives with another man saves money  
 (51) [every  $y$ :  $man\ y \bullet [an\ x: x \neq y \bullet man\ y]$   $y$  lives with  $x$ ]  $y$  saves money.

In Neale (2004), I continued to sideline hollow Gödelian descriptions. But I was more liberal with the *solid* ones—those containing at least one predicate other than identity—pushing them into the realm of constructions that seemed only *implicitly* conjunctive.<sup>84</sup> There were several related reasons this: (i) The need to respond to an argument by Wilson (1991) for a semantically distinct bound (hence referential) reading of descriptions; (ii) The need to respond to a new version of the Argument from Incompleteness for a semantically distinct referential reading due to Schiffer (1995); (iii) The need to respond to a fully general, methodological argument by Devitt (1997) and Reimer (1997), who claimed that referential uses of descriptions, particularly incomplete descriptions, were too common, regular or systematic to be explained as the non-semantic products of Gricean reasoning of the sort I offered in Neale (1990); and (iv) a desire to maintain a Russellian position consistent with the idea that the speaker-based notion of referring is the one figuring in what a speaker *says*.

As noted earlier, some care is needed in setting out the relation between the use of incomplete descriptions and referential uses of descriptions, but it seemed to me in Neale (2004) that I could deal simultaneously with (i)-(iv), by positing a regular, Gödelian way of completing definite descriptions, particularly—though neither exclusively nor exhaustively—when they are used referentially, and I suggested that in such cases the truth conditions of ‘the  $\phi$  is  $\psi$ ’ are captured by (52):

- (52) [the  $x$ :  $\phi x \bullet x = \alpha$ ]  $\psi x$

where  $\alpha$  is either a (closed) referring expression or a variable bound by an exterior quantifier. “The underlying idea is simple and natural: completion of an incomplete description used referentially is effected *non-descriptively* (as non-descriptively as possible, at any rate)” (2004: 171).

On this account, the theoretically important notion is *the nature of completion* itself. Gödelian completions, I said, provide “a fool-proof way of *regularly* interpreting utterances of incomplete descriptions used referentially” (2004: 171), a way that is also *available* in interpreting incomplete descriptions used *non-referentially* and, indeed, actually exploited (2004: 176–180).<sup>85</sup>

We might bring this account into line with TL by invoking Schiffer’s distinction again, comparing five cases. (1) A speaker who uses ‘it’s raining in Paris’ (or ‘it’s raining here’) to say that it’s raining in Paris, refers to Paris *in* using that sentence but not *with* that sentence. There is, however, some *part* of the sentence *with* which he refers to Paris, viz. ‘Paris’ (or ‘here’). (2) A speaker who uses the simpler sentence ‘it’s raining’ to say that it’s raining in Paris, also refers to Paris *in* using this sentence but not *with* that sentence or, indeed, *with* any part of that sentence. (iii) A speaker who uses ‘the mayor of Paris can ban

demonstrations in Paris' to say that the mayor of Paris can ban demonstrations in Paris, refers to Paris *in* using this sentence (twice) but not *with* that sentence. Similarly, he refers to Paris *in* using the subject 'the mayor of Paris' and *in* using the predicate 'can ban demonstrations in Paris', but not *with* that subject or *with* that predicate. However, both the subject and the predicate contain an expression *with* which the speaker refers to Paris, viz. (occurrences of) 'Paris'. (iv) A speaker who uses 'the mayor can ban demonstrations' to say that the mayor of Paris can ban demonstrations in Paris, refers to Paris *in* using this sentence (perhaps twice) but not *with* that sentence or, indeed, with any *parts* of that sentence. (v) Finally, the case of interest, involving a referential use of a description. A speaker who uses 'the mayor is a fool' to say that Pierre is a fool and uniquely a mayor of Paris refers to Paris *in* using this sentence but not *with* that sentence. Similarly, the speaker refers to Paris *in* using the subject 'the mayor', but not *with* that subject. Furthermore, there is no *part* of the subject, or indeed, of the whole sentence, *with* which he refers to Paris. So, using Schiffer's distinction and our provisional characterization of referring expression, we reach the conclusion that 'the mayor' is not functioning as a referring expression when it is used referentially. Which means it is not functioning as a semantically structured referring expression, so there is no clash with (T1).

An alternative idea would be to say that when 'the  $\phi$ ' is used referentially and interpreted as [*the*  $x$ :  $\phi x \bullet x=\alpha$ ], it is functioning simultaneously as a quantifier and as a referring expression. But it is questionable whether the hybrid proposal is really consistent with the spirit of TL, even if consistent with the letter. The principal thesis, recall, was that every natural language noun phrase is either a semantically unstructured, referring expression or a semantically structured, restricted quantifier. I suppose this is technically consistent with a given noun phrase being *both*—assuming the intelligibility of a noun phrase being semantically unstructured in respect of its referential properties and semantically structured in respect of its quantificational properties—but I confess the possibility of noun phrases that were simultaneously referential and quantificational was not something I had intended to exploit in TL as I was tacitly assuming no expression had more than one semantic structure. Again, there is nothing semantically incoherent about hybrids understood with dual semantic structures, but it is an empirical question whether natural language exploits their possibility in its ordinary employment. Certainly natural language contains descriptions containing referential components, and certainly there is nothing to prevent that referential component from being used to refer to the unique object satisfying the matrix of the description as a whole (recall 'the tallest member of Fred's family is Fred'). The interesting question is whether such identities are exploited in some way that is linguistically systematic (or *de jure*) rather than accidental (or *de facto*).

On the proposal just outlined, everything that needs to be said can be said, I think, without appealing to the moth-eaten labels 'referential' and 'attributive', for the idea is not so much a proposal for treating *referential* uses of definite

descriptions as a proposal for short-circuiting the pragmatic completion of many utterances of *incomplete* descriptions that are pragmatically completed—sufficiently many to warrant viewing them as amounting to a *regularity* of sorts when the Gödelian completion is non-relativized as in classic referential uses.<sup>86</sup> What is at issue is the *style of completion*, and obviously it extends to cases that have been called ‘attributive’, or at least ‘non-referential’. (It would be quite wrong to claim to read into Neale (2004) the claim that Gödelian completions are in one-one correspondence with referential uses.) The non-referential case I focussed on in Neale (2004) was one that provided a neat response to Wilson’s (1991) attempt to show that some descriptions are understood as bound variables, and hence as referring expressions:

- (53) [every scientist who was fired from the observatory at Sofia]<sup>1</sup> was consoled by [someone who knew [*the fired scientist*]<sub>1</sub> as a youth]

I found fault with Wilson’s own proposal, and, amplifying a remark by Kripke, noted that

A natural enrichment manifests itself in ordinary talk where we might find the overtly relativized description ‘the fired scientist in question.’ In a representation of the truth conditions of an utterance of [(53)], a Gödelian description containing variables on *both* sides of the identity sign gives us exactly what we want (2004: 178–179):

- (54) [every  $x$ : scientist  $x$ .  $x$  was fired from the observatory at Sofia]  
 [the  $z$ : fired scientist  $z$ .  $z=x$ ] [some  $y$ :  $y$  knew  $z$  as a youth]  
 ( $x$  was consoled by  $y$ ).

The matrix of [*the  $z$ : fired scientist  $z$ .  $z=x$* ] in (54) is just another example of a description that is *bound-into* and is understood as uniquely satisfied relative to values of  $x$ .<sup>87</sup> The Russellian says that the incomplete description in (53) is not, *pace* Wilson, a bound variable, but just another incomplete description, one that is relativized and Gödelian, and whose completion contains an expression understood as a variable bound by the subject expression.

Three things should be clear about this proposal, given points (i)-(vi) made at the beginning of this section.

(a) Just as it would be absurd to claim that an analysis of a particular use of ‘another general’ as a Gödelian *indefinite* description

- (55) [*an  $x$ :  $x \neq \alpha$  • general  $x$* ]

where  $a$  is being used to refer to Napoleon, is not quantificational because (55) contains  $x \neq \alpha$  as a constituent, so it would be absurd to claim that a Gödelian analysis of a particular use of ‘the general’ as

(56) [*the*  $x$ : *general*  $x \bullet x=\alpha$ ]

is not quantificational (or not Russellian) because (56) contains  $x=a$  as a constituent. (Similarly the analysis of demonstrative descriptions as Gödelian indefinites in the next section.) *Of course* it is quantificational, and it is this feature (together with *solidity*) that ensures the property of being a general gets into the proposition expressed, or into truth conditions. The proposal is no less quantificational (or Russellian) than one that construes a particular utterance of ‘the  $\phi$  is  $\psi$ ’ as having the truth conditions captured by

(57) [*the*  $x$ :  $\phi x \bullet \xi x$ ] $\psi x$

where  $\xi x$  is *not* of the form  $x=\alpha$ .

(b) In many (but not all) cases, a Gödelian completion will have a distinctly *referential* character because it will be a (non-relativised) object-dependent completion, and given what identity *is* this means  $\alpha$  is used to refer to the object that uniquely satisfies the condition specified by the whole matrix ( $\phi x \bullet x = \alpha$ ), just as in the case of ‘the tallest person in Fred’s family’, when Fred is, in fact, the tallest member of his own family.

(c) The proposal utilises only solid Gödelian descriptions [*the*  $x$ :  $\phi x \bullet x = \alpha$ ], moreover only those whose solidity is determined by the semantics of the description’s nominal, a feature that makes it quite different from a direct reference account, and also makes any charge of *hollowness* (of the sort I levelled against using [*the*  $x$ : $x=\alpha$ ] in Neale (1990)) completely groundless.

Nonetheless, there are some serious issues to be addressed by anyone inclined to the Gödelian-Russellian analysis.

(i) By trading the two distinct propositions upon which the Gricean-Russellian proposal of referential usage is based for a single conjunctive proposition, the Gödelian-Russellian is effectively deprived—as the ambiguity theorist is deprived—of an elegant explanation of what is happening in cases of misdescription such as those used by Donnellan.<sup>88</sup>

(ii) This can lead to trouble in cases involving collective readings. Imagine a party in 1930. Among the guests are exactly one of the two United States senators from New York and exactly one of the two authors of *Principia Mathematica* (Bertrand Russell). Now consider utterances of the following sentences with the definite descriptions used referentially<sub>D</sub> in connection with the New York senator who is present and Russell, respectively:

(58) The United States senator from New York favours a system of proportional representation

(59) The author of *Principia Mathematica* favours a system of proportional representation

The utterance of (58) is fine, but the utterance of (59) is, at best, strained because *Principia Mathematica* was co-authored by Whitehead and Russell. Without a precise semantics for collectives, and a precise account of the relation between the noun ‘author’ and the corresponding verb, it is unclear precisely what this heralds. For while it seems clear that Russell satisfies ‘ $x$  is an author of *Principia Mathematica*’ and ‘ $x$  is one of the authors of *Principia Mathematica*’, it is not so clear that he satisfies ‘ $x$  authored *Principia Mathematica*’ because the book was co-authored. Of course, this is not the first time that perplexities involving collective readings of predicates have been brought up in connection with descriptions.<sup>89</sup>

(iii) It is questionable whether the hybrid proposal is really consistent with the spirit of TL, even if consistent with the letter. The principal thesis, recall, was that every natural language noun phrase is either a semantically unstructured, referring expression or a semantically structured, restricted quantifier. I suppose this is technically consistent with a given noun phrase being *both*—assuming the intelligibility of a noun phrase being semantically unstructured in respect of its referential properties and semantically structured in respect of its quantificational properties—but I confess the possibility of noun phrases that were simultaneously referential and quantificational was not something I had intended to exploit in TL as I was tacitly assuming no expression had more than one semantic structure. Again, there is nothing semantically incoherent about hybrids understood with dual semantic structures, but it is an empirical question whether natural language exploits their possibility in its ordinary employment. Certainly natural language contains descriptions containing referential components, and certainly there is nothing to prevent that referential component from being used to refer to the unique object satisfying the matrix of the description as a whole (recall ‘the tallest member of Fred’s family is Fred’). The interesting question is whether such identities are exploited in some way that is linguistically systematic (or *de jure*) rather than accidental (or *de facto*).

## 22. DEMONSTRATIVE DESCRIPTIONS AS HYBRIDS?

It was a short step, in Neale (2004), from a context-specific Gödelian account of certain incomplete definite descriptions to a more general Gödelian account of demonstrative descriptions that seemed to me superior to the account floated in Neale (1990) and to the two accounts floated in TL. In Neale (1990), I treated demonstrative descriptions as genuine referring expressions but attempted to say as little as I could get away with about the matter of whether they had genuine semantical structure. (It was not pertinent to my theme.) I restricted myself to (a) the claim that when I use ‘that  $\phi$  is  $\psi$ ’ (a) I say *of* some particular  $\phi$  that it is  $\psi$  (thus making the object referred to a constituent of the proposition I express, and (b) the claim that the proposition in question is “true at some worlds in which I fail to point in my lifetime...[and] at some worlds in which (e.g.) I

never utter a word or think about anyone” (thus ruling out the idea that my demonstration or my utterance make it into the proposition). In “Term Limits,” by contrast, I sketched two proposals under which demonstrative descriptions were supposed to respect (T1), one quantificational, the other referential. The referential account, call it TL1, involved (a) treating a demonstrative use of ‘that  $\phi$ ’ as the use of a rigid referring expression, and (b) falling back on Kaplan’s (1978) idea that demonstrative descriptions lack genuine semantic structure, at least in so far as we are talking about contributions to propositional content and truth conditions that are the products of non-trivial composition. Such an idea might be fleshed out in various ways depending upon whether, for example, (i) ‘that  $\phi$ ’ is treated as not only rigid but also directly referential, (in which case its reference exhausts its specific contribution to propositional content and truth conditions), and (ii) whether being the reference of ‘that  $\phi$ ’ requires being  $\phi$ . In short, the attractiveness of any implementation of TL1 will be, in part, a function of its answers to questions about the precise relationship between syntax and semantics (broadly construed).

The TL quantificational account of demonstrative descriptions, call it TL2, seemed more natural because it was guided by syntactic structure. But before TL even rolled off the press, Scott Soames pointed out to me that TL2 was fatally flawed. The original idea was that a demonstrative description ‘that  $\phi$ ’ is exactly what it appears to be: a quantifier phrase of the form determiner + nominal, just like ‘the  $\phi$ ’, ‘every  $\phi$ ’, etc. TL2 treated a demonstrative use of ‘that  $\phi$ ’ as equivalent to the use of an “actualized” definite description: specifically, ‘that  $\phi$  is  $\psi$ ’ has a quantificational semantics, its truth conditions given by (1),

$$(61) [the\ x: @(\phi x \bullet \tau x)]\psi x$$

where @ is an actuality operator (read “actually”) and  $\tau x$  (read “ $x$  is targeted”) is a complex predicate understood as involving some favoured method of targeting a particular object—with a physical demonstration or a directing intention, for example.<sup>90</sup>

There was an important rider, however. If demonstrative descriptions are tantamount to actualised descriptions, there is an initial expectation of scope ambiguities mirroring those found with overt descriptions. But the existence of such ambiguities is debatable. If (62) is equivalent to (63),

(62) John thinks that man is a fool

(63) John thinks the actual man I am targeting is a fool

then why is it not ambiguous between (63’) and (63’), the way (63) is?

(63’) [the  $x: @(I\ am\ targeting\ x \bullet man\ x)] (John\ thinks\ (x\ is\ a\ fool))$

(63’’)  $John\ thinks\ ([the\ x: @(I\ am\ targeting\ x \bullet man\ x)] (x\ is\ a\ fool))$ .

I suggested a quick patch: somehow build into the specific semantic properties of the (purportedly) quantificational determiner ‘that’ the requirement that the demonstrative description it introduces is to be understood with larger scope than any sentence connectives or any non-extensional material in the rest of the sentence.<sup>91</sup>

Lepore and Ludwig (2000) say there is something ad hoc about this proposal. Perhaps. But the real problem runs much deeper. To think that a large scope requirement solves the underlying problem is to make more or less the mistake Dummett (1973) made when he answered Kripke’s powerful objections to descriptive theories of proper names with the claim that they evaporate if names are equivalent to descriptions that have large scope. We can eliminate irrelevant scope considerations by looking at simpler sentences, say (64) and (65):

(64) that man is a fool

(65) the actual man I am targeting is a fool.

The fatal problem for TL2 is that someone in a counterfactual situation can believe the proposition I express by uttering (64), yet not believe the proposition I express by uttering (65). The latter is a proposition that involves *me* and the *actual* situation, and surely it cannot be a requirement that someone in a counterfactual situation cannot grasp that proposition without grasping a proposition that is, in part, about me, about the actual situation, and about some targeting mechanism.

So TL2 is dead. But that does mean that all is lost for those who hold that demonstrative descriptions are quantifier phrases. In Neale (2004), I put forward an account that was meant to avoid the fatal error, call it TTO. It involved treating a demonstrative use of ‘that  $\phi$ ’ as a use of a Gödelian” *indefinite* description: ‘that  $\phi$  is  $\psi$ ’ has a quantificational semantics, its truth conditions given by (66),

(66) [ $\text{an } x: \phi x \bullet x=a$ ] $\psi x$

where  $a$  is directly referential (i.e. where the contribution that  $a$  makes to truth conditions is identical to the contribution the simple English demonstrative ‘that’ makes on a direct reference analysis). This account of ‘that  $\phi$  is  $\psi$ ’ is equivalent to a direct reference account of a different sentence, ‘that is  $\phi$  and  $\psi$ ’ (or ‘that is a  $\phi$  that is  $\psi$ ’).<sup>92</sup>

The good thing about TTO is that it keeps me, the property of targeting, and the actual situation out of the proposition expressed. But my good friend Stephen Schiffer, who has banged his head on the table many times about this, says, “You should have kept out the property of being  $\phi$  out too!” For according to Schiffer, even before banging his head, when I refer to some man  $X$  in uttering (64), the proposition I express is true in any counterfactual situation in which  $X$  has had a sex-change operation and is a fool. That is, according to Schiffer, someone in a counterfactual situation can believe precisely the proposition I

express by uttering (64), even if, in his situation,  $X$  is a woman, indeed, even if  $X$  is known by him to be a woman.

I'm not convinced. Intuitions are simply not as robust here as they are in Soames's example. But one thing is clear: it will not do to respond to Schiffer by modifying the theory so the 'that  $\phi$  is  $\psi$ ' truth conditions given by (67)

$$(67) [an\ x: @(\phi x \bullet x = b)]\psi x$$

because that just reintroduces the problem TTO was meant to circumvent, i.e. it succumbs to Soames's objection. In the absence of stronger evidence against TTO, I am inclined to push on with it. Unlike definite descriptions used referentially, demonstrative descriptions so-used are *de jure* Gödelian.<sup>93</sup>

In one respect, a Gödelian analysis is more natural for 'that  $\phi$ ' used referentially than it is for 'the  $\phi$ '. The former, on Gödelian accounts, is basically a demonstrative version of the indefinite description 'a  $\phi$ ' (and the plural, 'those  $\phi$ s', a demonstrative version of ' $\phi$ s'). The demonstrative aspect should engender uniqueness, its purpose being to point to a specific object (or group of objects), and it is this that the identity conjunct in (67) is meant to capture. But there would seem to be uniqueness *overkill* whenever a *definite* description receives a Gödelian interpretation (except in bound-into cases), for we get it once from the identity conjunct and once again from the logic of the determiner 'the'. And, as we have seen already, that poses at least the semblance of a problem. The fate of the complexity sceptic's position may seem now to turn on terminological points, but I don't think that can be right. We certainly use demonstrative descriptions in acts of referring, but whether we refer with the whole rather than with some part is a question that is not obviously one of pure terminology within an act-syntactic framework. When it comes to definite descriptions *de facto* understood as [*the*  $x: \phi x \bullet x = \alpha$ ] matters are more interesting. Certainly they can be used in acts of referring. But we know already that one can perform an act of referring without using a referring expression—recall the discussions of 'the mayor can ban demonstrations', 'no other mayor can ban demonstrations' etc. To the linguistic pragmatist already up his or her neck in propositional constituents that are not paired with items in syntax, this is hardly shocking, and complexity scepticism can be readily embraced.

## Notes

1. Early versions of this paper were presented in 1995 at the School of Advanced Study, University of London; the University of California, Berkeley; the University of Maryland, College Park; and Rutgers University. Comments by Charles Chihara, Josh Dever, Michael Devitt, Jerry Fodor, Leon Henkin, Peter Klein, Ernie Lepore, and John Searle disheartened me for some years. But the paper has gone through many versions since then and profited from the feedback



of audiences exposed to various sections of the final paper in talks at CREA, Ecole Polytechnique, Paris; Simon Fraser University; McGill University; the University of Tromsø; the University of Washington; the University of Haifa; University College, London; Tulane University; the Graduate Center of the City University of New York; the University of Rio de Janeiro; and the pilot meeting of Semantics and Philosophy in Europe, held at the Ecole Normale Supérieure, Paris. I am particularly grateful for comments and advice from Josh Armstrong, Jonathan Berg, Emma Borg, Michael Devitt, Kevan Edwards, Maite Ezcurdia, Graeme Forbes, Marcus Giaquinto, John Hawthorne, Saul Kripke, Guy Longworth, Angel Pinillos, Frank Pupa, Marco Ruffino, Stephen Schiffer, Barry Smith, Robert Stainton, Zoltan Szabó, and especially Herman Cappelen, who encouraged me to resuscitate and rework the paper for publication, and Kyle Ferguson for preparing the bibliography. There are points of contact with Neale (2004, 2005, 2007a,b), in that the general framework for utterance interpretation I call *linguistic pragmatism* is ever-present. No familiarity with those articles is assumed, but it will be clear to readers who know them that the (act-syntactic) framework for semantic explanation and the (linguistic pragmatic) framework for utterance interpretation are ultimately two sides of the same coin. The threads are all pulled together in an unfinished manuscript called “Linguistic Pragmatism”.

2. See Grice (1989). In his 1961 paper on this topic Grice uses ‘state’; in his 1975 paper he uses ‘say’. Although it is common to use ‘say’ and ‘state’ interchangeably in both ordinary and philosophical talk, a good case can certainly be made—indeed, a good case *has* been made by Bach (1994, 2001), in particular—for separating them in certain types of philosophical discourse about meaning. Bach himself draws, and drawn heavily upon, a theoretical distinction between *what is said* and *what is stated*. I am sympathetic to Bach’s concerns and not wholly unsympathetic to the proposals he makes for addressing them. I suspect many of our differences will ultimately turn out to be terminological, and to the extent that that a mapping between our respective proposals is possible at present, I think my use of ‘say’ and ‘state’ here is close to his use of ‘state’.
3. The examples used suggest that implying is parasitic upon saying, but there are two interesting wrinkles to iron out because of cases in which someone implies something by *feigning* to say something and cases in which someone implies something by *failing* to say anything.
4. Strawson (1950, 1952).
5. Following Russell, it is very common (though not universal) to see possessive descriptions as definite descriptions, stylistic variants of the common form ‘the  $\phi$ ’. Resisting this idea generates an awkward problem: where the head of the possessive is a pronoun (inflected in the way the particular language requires) we find a simple match between English and, say, French: ‘his brother’-‘*son frère*’. But where the head of the possessive is some other noun phrase, French must revert to the definite article form: ‘Bobby’s brother’-‘*le frère à Bobby*’, etc. The theorist does not want to end up committed to the view—at least not on the basis of *this* data—that we can say things in English that cannot be said in French. If possessive descriptions are definite descriptions, that issue, at least, does not arise.

6. I apologise for the mildly barbaric neologism “actic”, but it has the virtue of brevity and is of a piece with talk of act-syntactic considerations, Ditto the impending adverb “actically”.
7. Of course, we could have obtained the same combinations by making the first cut syntactically ( $\pm N$ ), the second actically ( $\pm R$ ), the third syntactically ( $\pm A$ ), and the fourth actically ( $\pm P$ ). While this alternative way of effecting a ping-pong sorting would have comported with the general programme of explaining semantic categories in act-syntactic terms, it would have been inconsistent with another feature of the programme mandated by Gricean constraints engendered by the accompanying theory of interpretation (linguistic pragmatism). Semantic differences between individual words within a given sort are also to be explained act-syntactically, but that is not something that needs to be taken up here.
8. The hypothesis had no bearing whatsoever on what one can and cannot do in *artificial* languages—the syntax and semantics of which are stipulated rather than discovered by empirical methods—and we must beware of insidious technical objections made by philosophers or logicians running together (i) conceptual claims about what is or isn’t possible in syntactic and semantic theories quite generally and (ii) empirical hypotheses about the syntax and semantics of natural language.
9. (a) Notice that (T1) is consonant with the present article in being silent on the matter of whether all referring expressions in natural language are noun phrases. It is consistent with ‘here’ and ‘there’ being referring expressions, for example.
  - (b) The ellipted material is “(singular term)”. At the time I was writing TL I was attracted to the idea of treating all plural noun phrases as quantifier phrases, so I used “referring expression” and “singular term” interchangeably. I am not sure these days, so I shall not insist on that identification here. Without it, the truth of (T1) still requires that every plural noun phrase is, by virtue of being a noun phrase, either a semantically unstructured, rigid, referring expression or else a semantically structured, restricted quantifier.
  - (c) I defended (T1) in two other 1993 papers, “Logical Form and LF” and “Logical Form, Grammatical Form, and Incomplete Symbols”. The dominant idea in those papers was that a noun phrase with semantic structure triggers a particular type of syntactic operation (“Quantifier Raising”) relevant to matters of interpretation.
  - (d) Two additional theses were prominent in TL: (T2) The linguistic contexts governed by modal and causal sentential operators (or connectives) are non-extensional but referentially transparent; (T3) The scope of an expression in natural language is formally (tree-geometrically) identical to the scope of an expression in a simple formal language (e.g. the language of first-order logic). I shall say nothing about (T2) here, but I shall touch on (T3) in connection with referential theories of descriptions.
10. In this connection, see Fine (2003), Salmon (2006a, b), and King (2007).
11. See Neale (forthcoming a) where I explore the consequences of abandoning the standard distinction in *extensional* logic between logical and non-logical vocabulary in favour of a proposal according to which only predicates may vary their extensions across models—it was always overkill allowing both predicates and individual constants to do so—and that it is part of what it is to be predicate (but not part of what it is to be any other sort of expression) to admit of different

extensions in different models (even if, as in mathematics, one does not actually need or want to vary their extensions across models). In effect, then, individual constants are treated as if they are items of logical vocabulary. If “ $a = b$ ” is true in any model it is true in all models. Models (rather than possible worlds) can then be used to characterize a basic notion of necessity: a sentence is necessarily true iff it is true in all models. For applications, see Friedman (2005), who also presents problems for my favoured fixed-domain system. (Similar problems have been raised in conversation by Nick Asher, Kit Fine, and Kripke.)

12. The need to provide something precise here was urged upon me by John Hawthorne in a seminar we co-taught at Rutgers. Hawthorne did not seem to me sceptical that it could be done, he just pushed for details.
13. Under the influence of Evans’s and McDowell’s neo-Fregeanism, I used to be happier talking about object-dependent senses rather than objects themselves being constituents of proposition expressed. Here I shall talk freely of objects, properties etc. being constituents of propositions, as is the vogue. Thus there is nothing to chose between my use of ‘singular proposition’ and ‘object-dependent proposition’, modulo a syntactic nicety I will get to.
14. In Neale (1990). Following custom, I shall frequently (but not always) use the quasi-English expressions ‘the  $\phi$  is  $\psi$ ’, ‘that  $\phi$  is  $\psi$ ’, etc. in lieu of the more inclusive formalisms ‘ $\psi$ (the  $\phi$ )’, ‘ $\psi$ (that  $\phi$ )’, where  $\psi$  is what remains of a sentence, however complex, after removing a single occurrence of ‘the  $\phi$ ’, ‘that  $\phi$ ’, etc. *Prima facie*, some care has to be taken with such usage because of the putative existence of alternative scope possibilities involving ‘the  $\phi$ ’, ‘that  $\phi$ ’ and parts of  $\psi$ , a matter that will be taken up soon enough.
15. (a) I could have used so-called rigid descriptions. Assuming various essentialist theses, the following would qualify: ‘the human born of such-and-such gametes’, ‘the species with such-and-such DNA’, ‘the positive square root of 81’, ‘the substance with atomic number 79’, ‘the colour that reflects such-and-such wavelength’. As I stressed in TL, for the Russellian, a rigid description is no more a referring expression than a non-rigid one is, for a rigid description is just a quantifier ‘the  $\phi$ ’ whose predicate  $\phi$  is rigid in respect of *extension*. That is, the quantifier ‘the positive square root of 81’ is no more a referring expression than ‘both square roots of 81’, ‘every square root of 81’, or ‘no square root of 81’. (b) Soames (2002) has raised interesting worries about the relationship between rigidity as a property of singular terms and rigidity as a property of general terms. (For discussion, see Marti (2004) and Salmon (2005).) The worries are interesting, but not germane to my main points here as I shall be focussing on singular terms. (c) One might want to argue that ‘9’ is actually a disguised definite description (‘the successor of the successor of... the successor of 0’, for example.) My own favoured view, is that in our base-10 system ‘0’ to ‘9’ are referring expressions whereas ‘10’, ‘11’, ‘12’ etc. are all definite descriptions. This is a position Saul Kripke drew me to in seminars at the University of Oslo in 1991, and I am much influenced by his arguments that the base makes all the difference—on my favoured view, in a base-2 system ‘0’ and ‘1’ are referring expressions and ‘10’, ‘11’, ‘100’ etc. are all definite descriptions—especially when it comes to which numbers one can entertain genuinely singular thoughts about. Unfortunately, Kripke’s work on this topic is not yet published.

16. See Lewis (1979, 1996), Cohen (1999), DeRose (1999).
17. See Stanley (2000, 2002a, 2002b), Stanley and Szabó (2000).
18. See Reichenbach (1947), Russell (1948), and Bar-Hillel (1954), for example. More recently, see Barwise and Perry (1983), Kaplan (1989a,b), Montague (1974), Perry (1977, 1978, 1986, 1998, 2001).
19. Perry (1986) provides an illuminating way of thinking about his when he says that whereas a non-indexical expression is one whose meaning specifies a contribution to propositional content that is constant across different uses, an indexical expression is one whose meaning specifies only *a relation to the user* that is constant across different uses (different entities capable of serving as the specified relatum and hence as contributions to propositional content, on different occasions of use).
20. See Lewis (1979, 1996), Cohen (1999), DeRose (1999), and Stanley (2000, 2002a, 2002b). The crowning absurdity of this idea is surely the *silent* indexical, a type of expression that is not only non-perspective, non-tagging, and non-descriptive, but in addition inaudible.
21. My reasons for rejecting claims (b) and (c) are set out in detail in “Saying and Referring”. Roughly, I argue that claims (b) and (c) turn on mistaken conceptions of what is involved in acts of saying and referring that stem from overlooking (i) the fact that the contents of speakers’ mental states—including their referential intentions—are partly a function of externalist considerations, (ii) the fact that genuine intentions are subject to an epistemic constraint—roughly that one cannot intend what one believes to be impossible—(iii) the fact that a speaker’s tacit grasp of both the meanings of the words he uses and their syntactic arrangement usually place constraints on what he can intend to convey with them—which is not to deny the existence of metaphor, irony, or conversational implicature, or the importance of Searle’s captured soldier examples or Kripke’s toy-duck examples—and (iv) the fact that the epistemic situations of speakers and audiences are reciprocal, a speaker’s assessment of his audience’s ability to work out what he means placing constraints on what he can intend to convey with his words.
22. To say that *E* refers rigidly is to make no claim about languages spoken in other possible worlds; it is to make a claim about *E* as used in *our* actual language. It is, in any case, a question over which there is considerable disagreement whether, on the most theoretically significant use of the word ‘expression’, *E* itself even exists in any other language, and if so whether it exists in languages in which it refers to something other than the thing it refers to in our language. There is no need to take up this matter here.
23. One might argue that the sentences contain an aponic “indexical” that a given speaker is using—whether he thinks so or not—to refer to, say, Paris. Someone holding this view denies that the example is a good one for demonstrating that a speaker can refer to something without using a referring expression to do so. But given the superficial facts about the syntax of (11)-(13), the burden of proof surely lies with such a theorist, and the burden cannot be relieved without supplying the relevant syntactic details (for example a statement of what syntactic category the aponic belongs to and of where precisely in the phrase markers for (11)-(13) it occurs. Let us use *loc* as the name of this indexical and (since

syntactic details are non-existent in the relevant literature) represent the logical forms of (11)-(13) abstractly as (i):

(i) It's  $\phi$  *loc*.

Notice that the aphonic “indexical” *loc*, if it exists, does not have the Kaplanian character of any known *phonic* expression, indexical or otherwise. (a) It does not have the Kaplanian character of either ‘here’ or ‘there’ because a speaker in Paris or outside Paris would be able to use (i) to say that it’s raining/dark/midnight in Paris. So *loc* signals no perspective on a location; indeed it is wholly non-perspectival, making a mockery of traditional talk of indexicality—see the work of Reichenbach, Russell, Kaplan, Perry, and Evans—which is fuelled by perspective. (b) Nor does *loc* have the character of any proper name-involving prepositional phrase such as ‘in Paris’, for then it would be impossible (pathology aside) to use (i) to say that it’s raining/midnight/dark in, say, London. (c) Nor does *loc* have the Kaplanian character of any description-involving P such as ‘in the capital of France’, for then (again) it would be impossible (pathology aside) to use (i) to say that it’s raining/midnight/dark in, say, London, or to say that it’s raining/midnight/dark in the capital of England. So, speaking semantically, *loc*, if it exists, is like no known phonic expression. Since, by hypothesis, it has no phonology, without some evidence of its existence motivated by grammatical considerations, i.e. considerations of syntactic distribution, the postulation of such an expression would seem to be little more than a wooden attempt to cling to the dogma that every constituent of a proposition expressed is the value of some element in syntax. One must be careful not to claim that one has “syntactic” arguments for the existence of aphonic indexical expressions that amount to no more than the trivial point that whenever Perry or anyone else posits an unarticulated constituent, it is possible to concoct a semi-formal, semi-English formula that contains a variable whose value we could take to be the purported unarticulated constituent. On this matter, see Neale (2007a).

24. See (e.g.) Stanley and Szabó (2000).
25. The proper name, ‘Aristotle’ is mentioned once for expository convenience in the interjected noun phrase “the philosopher we call ‘Aristotle’”, but nothing hinges on this—Kripke could just as well have used “the philosopher Aristotle”.
26. The basic properties of formal languages are stipulated; to the extent that more complex properties are “discovered”, this is by mathematical rather than empirical methods (set-theoretic techniques or mathematical induction, for example).
27. When Kripke was investigating quantified modal systems in the late 1950s and early 1960s, the idea that proper names were disguised Russellian descriptions was dominant, so on the assumption that individual constants were the formal analogues of proper names there seemed to be no gain in adding individual constants to quantified modal systems. By the early 1960s it was clear to Kripke—as it was perhaps clear to others working in modal logic at the time—that treating individual constants in this way would lead to formal difficulties. The realization that with Kripke’s model theory variables were rigid designators played a non-trivial rôle in Kripke’s coming to form the hypothesis that pronouns and proper names in natural language were rigid.

28. On this matter, see Salmon (2006a, b).
29. Another way of putting this—one that reveals a connection with recent talk of relativism about truth—is to say that natural language does not exploit the possibility of referring expressions whose references are world-relative.
30. According to Chomsky, a central task of generative grammar is to render explicit and systematic what the native speaker-hearer implicitly knows about the structure of his or her language simply in virtue of *being* a native speaker-hearer, and thereby to shed light on the nature of the human language faculty. We acquire language because we have a *language faculty*, a component of our shared biological endowment that unfolds in accordance with a preset programme under the triggering and shaping effect of linguistic experience. Whilst the final state may differ across individuals—particularly in so far as their exposure is to different speaker-hearers in different communities—the shaping effect of experience is quite limited. And it is this limited variation alone that leads us to talk informally about people being speakers of ‘different languages’. Although the study of conditions (or constraints) on rules of grammar had begun in the 1960s, it was not until the early 1980s that the emphasis in generative linguistics shifted dramatically from rules for generating (and interpreting) particular linguistic structures to constraints on possible structures and their interpretations. (See, e.g., Chomsky (1977, 1981, 1986).) Diverse linguistic phenomena, seemingly governed by intricate rules that differed from language to language, were now viewed as consequences of the interaction of general principles of the human language faculty, principles that were meant to be invariant across typologically distinct languages, superficial differences between particular languages reflecting only the setting of different values to each of a batch of structural parameters as part of the process of language acquisition, and the peripheral effects of relatively unimportant, learned idiosyncrasies.
31. I say the examples are well-known because they were discussed earlier and in varying degrees of detail by Geach (1962), Mates (1973), Evans (1977, 1985), and May (1985).
32. The problems for Hornstein’s theory run much deeper than dealing with (i)-(iii). For detailed discussion see Neale (1990) Ch 4, n. 4, and Soames (1985).
33. Perhaps Strawson’s problems have been solved. Paul Elbourne and Daniel Rothschild, who have delved deeply into the matter of the presuppositions attending uses of descriptions, have given me reason to think they are soluble at a reasonable price.
34. There are several well-known methods of decompressing so as to bring open sentences into the fold. For discussion, see Evans (1977).
35. As I said, “Certainly an analysis that makes ‘that day when he retires’ equivalent to ‘the day (when) he retires’ looks plausible here” (1993c: 120, n 39). For a more detailed discussion of the many different uses of demonstrative descriptions, see Neale (2007b), much of which deals with the perceived shortcomings of the discussion in King (2001).
36. Perhaps this is something like a semanticist’s pun, the implicit nature of the binding allowing me to play against one another readings that are similar to “strict” and “sloppy” readings in the linguists’ sense. See Neale (2005) for discussion.

37. To reiterate, the issue is not a conceptual one as Lepore and Ludwig (2000) and King (2001) claim. Like Salmon (2002, 2006a, 2006b), Stanley realises that King (2001) is making a conceptual claim, as is clear from his observation that “King takes the existence of QI [‘Quantifying In’] cases to provide a decisive refutation of the thesis that complex demonstratives are referring expressions” (2002b, p. 607). And while Stanley is careful not to endorse King’s position, recall that Stanley does claim that if a general semantic framework of structured propositions is assumed, it is “fairly straightforward” to show that the idea of directly referential singular terms containing variables bound by exterior quantifiers must fail. As he puts it, “the data provide a “damning class of cases for the direct reference account.” (2002b, p. 607). Oddly, Stanley says TL is “The first paper I know of that clearly recognizes the threat QI cases pose for the direct reference account of complex demonstratives” (2002, p. 607). But *no-one* can “clearly recognize” the threat Stanley mentions, since there is no such threat, as Salmon (2006a, 2006b) has shown. Moreover, I did not even *mention* in TL—nor had I mentioned anywhere else for that matter—any threat to direct reference posed by the possibility of binding into complex demonstratives. The examples I brought up of attempted quantification into demonstrative descriptions—viz. (20) and (25) above, both of which Stanley alludes to—were *not* brought up to raise conceptual or technical trouble for (or pose a conceptual or technical “threat” to) direct reference accounts; they were brought up in the course of defending *the empirical hypothesis that natural language does not avail itself of the perfectly coherent possibility of referring expressions that are semantically non-atomic*. Like Salmon, Stanley has misinterpreted an empirical hypothesis for an in-principle, technical claim.

Remarks in King’s (2008) reply to Salmon (2006a, 2006b) suggest that his position, *today* at least, is *not* that binding into directly referential singular terms is incoherent after all, even within a general semantic framework of structured propositions! So it would seem King, *now* at least, sides with Salmon against Lepore and Ludwig and against Stanley on the relevant conceptual issue. King (2008) focuses on what he claims are *empirical* shortcomings of Salmon’s direct reference semantics. In effect, King (2008) shifts the grounds, or at least the emphasis, of the debate from a conceptual issue to an empirical one about coverage. But given that Salmon is well aware of different uses of demonstrative descriptions and never claimed to be giving a fully general, direct reference semantics for all uses, the particular empirical issue is one with little bite. (For discussion, see Neale (2007b).) Nonetheless, the King-Salmon debate reinforces just how important it is in this area to separate clearly conceptual and empirical claims.

38. In (26)—and depending upon how things turn out, also in (27)—I am assuming something that I and many other semanticists find uncontroversial: referring expressions may bind pronouns in exactly the same way that quantifier phrases may. See Neale (2006) for discussion.
39. In Neale (2004, 2005, 2007a).
40. Of course, Millians who think there are referring expressions that are not rigid in extension will not say this.

41. I don't want to discuss temporal or spatial rigidity in connection with sentences because I agree with Evans (1980) in thinking there are problems involved in thinking about locating the same proposition at different times (or places) and comparing its truth-values at those times (or places).
42. Another natural suggestion, which may accord better with syntactic considerations (see next footnote), is that 'before' has as its extension a function from a pair of a truth value and a time to a function from a pair of a truth value and a time to a truth-value.
43. The words 'and' and 'or' can be used to join pairs of nouns, NPs, verbs, VPs, prepositions, PPs, etc.); but the conjoining possibilities for 'if' 'when', 'unless', 'until', 'before', 'after', and 'because' are more restricted along this dimension. This may be reason enough to view 'and' and 'or' as belonging to different semantic classes. (Though if one permits predicate abstraction and elimination, one might posit a level of semantic analysis at which sentential conjunction is always at work, any one-place predicate combining with, say, 'Russell and Whitehead' emerging from rules of composition (which would have to be supplied). For discussion of a generalized notion of conjunction, see Partee and Rooth (1983). Systematization of the relevant data involving 'if/when/before/after/because' is rendered tricky because of all sorts of intersecting syntactic and semantic issues involving ellipsis, sequence of tense, anaphora, and scope. Cf. 'Tigers are dangerous if/when/\*after cornered' and 'Tigers are dangerous if/when/after they are cornered.') So might the fact that we can say not only (i) but also (ii), yet not (iii):

- (i)  $\phi$  and/or/if/when/before/after/because  $\psi$
- (ii) if/when/unless/until/before/after/because  $\psi, \phi$ .
- (iii) \*and/or/but  $\psi, \phi$ .

This and other considerations might suggest that 'if/when/unless/until/before/after/because  $\psi$ ' is a syntactic constituent of both (i) and (ii), viz. a one-place sentence connective. On such a proposal, 'if/when/before/after/because' is a device that combines with a single sentence to produce a device that combines with a single sentence to produce a sentence (just as a transitive verb is a device that combines with a single noun phrase to form a device that combines with a single noun phrase to form a sentence).

44. Of course, Frege himself doesn't treat *descriptions* as quantifier phrases, but that local fact is not pertinent to the general exposition of a Fregean account of quantification. I used (33) instead of (32) to make the point only because our main worry is going to be 'the' rather than 'every'.
45. As Smiley (2004) points out, viewing Russell's Theory of Descriptions through the lens of Frege's account of quantifiers as second-level predicates goes back at least to Geach who notes that 'the  $\phi$ ' will stand for a second-order concept "*within* which a concept falls if and only if there falls *under* it" a  $\phi$  and apart from which nothing else is a  $\phi$  (1952: 51). The question might arise whether, in such a system, descriptions are still incomplete symbols (though contrary to what Geach suggests, there is no connection between Russell's talk of incomplete symbols and Frege's talk of incomplete (i.e. unsaturated) expressions).
46. See Montague (1974), for example, where NPs are uniformly of type  $\langle\langle e, t \rangle, t \rangle$ .



47. I am grateful to Josh Dever here for pushing me to present enough detail here to make the generality of the problem transparent.
48. When pushed to its terminus, this opens up the way to a possible treatment of intensional transitive verbs such as ‘worship’ and ‘seek’.
49. See, e.g. Montague (1974) and Partee (1986). Partee distinguishes three ways in which a noun phrase may be interpreted: (i) *referentially*, (ii) *predicatively*, and (iii) *quantificationally*. On her account, all noun phrases have  $\langle\langle e, t \rangle, t\rangle$  interpretations, but only some have  $e$  and  $\langle e, t \rangle$  interpretations. Definite descriptions have all three, with the following semantics: *Referential*:  $(tx)(\phi x)$ , corresponding to type  $e$  and construed as an expression that refers to  $\alpha$  if  $\alpha$  and nothing else satisfies  $\phi$ , and to nothing otherwise; *Predicational*: (ii)  $\lambda x(\phi x \wedge \forall y(\phi y \supset y = x))$ , corresponding to type  $\langle e, t \rangle$ ; *Quantificational*:  $\lambda P(\exists x(\forall y(\phi y \supset y = x) \wedge Px))$ , corresponding to type  $\langle\langle e, t \rangle, t\rangle$ .
50. Chomsky (1986, 2000). This is the approach to a full solution I have always assumed in my own work, but not for any reason I find overwhelmingly compelling. I skate over the fact that for Chomsky himself syntactic labels have fallen away in a full-fledged PF. Because I have retained labels in what I am calling PFs here, they might be closer to what some linguists call *surface structures*, or to what others call *S-Structures*. None of this matters here.
51. The introduction of quantifier-variable structures in this way does not actually get us that far if we are attempting to solve the underlying compositionality problem. Using just the categories we can define in terms of  $e$  and  $t$ , we do not obtain the truth conditions of the quantified sentence (i) compositionally if we proceed straight from simple quantifier-variable structures such as those given by (ii) and (iii) as the classical semantics for such structures is not compositional:

- (i) every poet is wise
- (ii)  $\forall x(Px \supset Wx)$
- (iii)  $[every\ x: Px]Wx$ .

(Cf. vexing questions about the extensions of  $Px$  and  $P_y$ , and their relations to the extensions of  $\forall xPx$  and  $\forall xP_y$ .) The usual solution for getting (i) to come out as (ii) uses  $\lambda$ -abstraction: we capture the semantics of ‘every poet’ using (iv), where  $\phi$  is a variable of type  $\langle e, t \rangle$ :

$$(iv) \lambda\phi\forall x(Px \supset \phi x).$$

(iv) itself is of type  $\langle\langle e, t \rangle, t\rangle$  and can be read as “ $\phi$  is a property every poet has.” Applying this to the property of being wise we get (v):

$$(v) \lambda\phi\forall x(Px \supset \phi x)W$$

which can be read as “the property of being wise is one of the properties every poet has.” By  $\lambda$ -conversion this is equivalent to (ii), so we have now obtained the truth conditions captured by (ii) in a compositional fashion. Well almost. For thoroughness we must explain the semantic composition of ‘every’ and ‘poet’. We can capture the semantics of ‘every’ using (vi), where  $\psi$  is a variable of type  $\langle e, t \rangle$ :

$$(vi) \lambda\psi\lambda\phi\forall x(\psi x \supset \phi x).$$

(vi) itself is of type  $\langle\langle e, t \rangle, \langle\langle e, t \rangle, t \rangle\rangle$ . Applying this to the property of being a poet, we get (vii):

$$(vii) \lambda\psi\lambda\phi\forall x(\psi x \supset \phi x)P$$

And by  $\lambda$ -conversion this is equivalent to (iv), which gives the semantics of ‘every poet’. Done.

52. Barwise and Perry (1983), Bezuidenhout (1997), Chastain (1975), Devitt (1981, 2004), Donnellan (1966, 1978), Kaplan (1978), Larson and Segal (1995), Reimer (1998), Récanati (1981, 1986, 1989), Rundle (1965) Schiffer (2005, 2006), Wettstein (1981), Wilson (1978, 1991). Four of the arguments—the Argument from Misdescription (Donnellan), the Argument from Incompleteness (Wettstein), the Argument from Opacity (Rundle), and the Argument from Anaphora (Chastain)—are examined and rejected in Neale (1990). Two more—the Argument from Convention (Devitt/Reimer) and the Argument from Binding (Larson and Segal)—are examined in Neale (2004), the former rejected along with a refined version of the Argument from Anaphora (Wilson). Two more—the Argument from Parity (Schiffer) and the Argument from Priority (Schiffer)—are examined and rejected in an expanded edition of *Descriptions* (forthcoming).
53. Geach (1962), Wiggins (1965). Of course, Russell’s theory is a predicational theory of sorts. When implemented using a Fregean account of quantification, descriptions are *second*-level predicates. And more generally, in saying that the  $\phi$  is  $\psi$  one is surely predicating at the first-level with  $\phi$  just as one is predicating at that level with  $\psi$ .
54. Linsky (1963) and Partee (1986), for example.
55. Graff (2001).
56. Frege (1892, 1893), Hilbert and Bernays (1934), Carnap (1947), Scott (1967), Grandy (1972), Sainsbury (2005, 2006), Smiley (1981, 2004).
57. Presumably Smiley’s criticism applies equally to Russell himself, given the list of denoting phrases Russell gives in the first paragraph of ‘On Denoting’ and his emphasis (in the final sentence of that paragraph) on a phrase being a denoting phrase “solely in virtue of its *form*”.
58. See Oliver and Smiley (2005), who would like a theory that makes sense of ‘ $\sqrt[4]{4}$ ’. Unlike Frege and Russell, they embrace many-valued functions and see ‘ $\sqrt[4]{4}$ ’ as a plural term denoting both 2 and  $-2$ , better rendered in ordinary English as ‘the square roots of 4’ than as ‘the square root of 4’.
59. This point is stressed by Smiley (2004) and by Oliver and Smiley (2005).
60. This parsing and the associated semantics are only partial of course as they do not, as stated, explain how ‘the’, ‘father’ and ‘of’ combine.
61. These results are readily replicated with ‘the square of two and the cube of two’, ‘every relative of Charles and every friend of Charles’, ‘no brother of Charles or sister of Charles’, etc.

62. Of course, there is nothing to prevent a logician inventing an *artificial* language containing functors *stipulated* to be constituents of its formulae (for example, a language containing  $s(0)$ ,  $s(s(0))$ ,  $s(s(s(0)))$  etc.), but that is not the issue at hand.
63. Scholars of Evans will correctly point out that, strictly speaking, this is *not* Evans's theory but a theory Evans explicitly rejects a single page after mentioning it on the basis of a technical mistake he makes in connection with examples involving donkey anaphora. (See Neale (1993c) for discussion.) It may well have been the position Evans *wanted* to hold, but his donkey error led him to believe and claim that 'the' could not be a 'unary quantifier former' (a device *the x* that combines with a predicate  $\phi$  to form a quantifier phrase [*the x*:  $\phi x$ ] (which itself combines with a predicate  $\psi$  to form a sentence [*the x*:  $\phi x$ ] $\psi x$ )), but must instead be a binary quantifier (a device [*the x*] that combines directly with two predicates to form a sentence [*the x*]( $\phi x$ ;  $\psi x$ )). Since Evans had no good reason to give up the theory Smiley attributes to him, let us harmlessly call it "Evans's theory".
64. Smiley rejects an alternative way out: (a') accept that names are disguised Russellian descriptions after all (or, in the fashion of Montague, some other type of quantifier phrase), and (b') treat the 'the father of' as a functor that uniformly combines with a second-level predicate to form another second-level predicate, i.e. as uniformly of the category  $(S/(S/N))/(S/(S/N))$ , corresponding to type  $\langle\langle e, t \rangle, t \rangle$ ,  $\langle\langle e, t \rangle, t \rangle$ .
65. Matters may be worse than I have just made out. By Smiley's lights it should be impossible for 'and' to be fitted consistently into any category, even when it conjoins only NPs. Is it "torn" between  $N/(N,N)$  and  $(S/(S/N))/(S/(S/N))$  in (i) and (ii)?
- (i) Jack and Jill went up the hill.
  - (ii) Every girl and every boy went up the hill.
- Is it further torn by its behaviour in (iii) and (iv)?
- (iii) Jack and every girl went up the hill.
  - (iv) Every girl and Jack went up the hill.
- Is it fully shredded when we bring variable-binding into the picture as in (v)-(vii)?
- (v) Jack and every other student in his class went up the hill.
  - (vi) Every girl and every boy she loved went up the hill.
  - (vii) Every girl and the student she loved most went up the hill.
- One thing is certain: we would not be inclined to conclude from these elementary distributional facts about 'and' when it conjoins NPs that 'every girl', 'every other student in his class', 'every boy she loved', and 'the student she loved most' cannot be second-level predicates, that they are, in fact, singular terms; but these appear to be the conclusions an argument parallel to Smiley's Argument from Nesting would lead us to draw!
66. Smiley also objects to Russell's account of singular descriptions on the grounds that 'its elimination of functional terms makes even the simplest mathematical manipulations (e.g. solving a quadratic equation) humanly impossible'. I am aware of no empirical results supporting this claim, so I pass over it.
67. Notice that any adequate theory of descriptions, whether quantificational or referential, must comport with the possibility of both strict and sloppy readings of sentences (i) and (ii):

- (i) Smith loves his wife, and Jones does too
- (ii) Smith loves the woman he married, and Jones does too.

It is widely held that the pairs of readings for (i) and (ii) arise because (a) VP ellipsis involves parallelism of both form and interpretation and (b) in both (i) and (ii) the simple sentence serving as the first conjunct has two formally distinct, albeit truth-conditionally equivalent, “readings” that impinge upon the semantics of the whole conjunction differently precisely because of the formal difference. The two readings of ‘Smith loves his wife’, for example, are

- (iii)  $\lambda x(x \text{ loves } \textit{Smith's wife})\textit{Smith}$
- (iv)  $\lambda x(x \text{ loves } x\text{'s wife})\textit{Smith}$

which might be loosely paraphrased as ‘Smith is a Smith’s wife lover’ and ‘Smith is a self’s wife lover’. In effect, then, ‘Smith’ binds ‘his’ on the sloppy reading of (i), though this is cashed out as ‘Smith’ merging with a VP whose lambda operator binds ‘his’. This gives us the necessary general notion of binding that makes it possible to say that what is said to be true of every man in (v) is said to be true of Smith in (vi):

- (v) every man loves his wife
- (vi) Smith loves his wife

This in turn explains why the VP ellipsis is impeccable in (vii):

- (vii) If every man loves his wife, then it follows that Smith does since Smith is a man.

The Russellian captures the two readings of (i) as follows:

- (viii)  $\lambda x([\textit{the } y: y \textit{ wife of Smith}](x \text{ loves } y))\textit{Smith}$   
 $\lambda x([\textit{the } y: y \textit{ wife of Smith}](x \text{ loves } y))\textit{Jones}$
- (ix)  $\lambda x([\textit{the } y: y \textit{ wife of } x](x \text{ loves } y)) \textit{Smith}$   
 $\lambda x([\textit{the } y: y \textit{ wife of } x](x \text{ loves } y))\textit{Jones}.$

We see here once again that any adequate singular term treatment of descriptions will have to countenance singular terms containing variables bound by exterior quantifiers.

- 68. I shall sometimes slip into loose but intelligible talk of the *matrix* (rather than the *nominal*) of an English description being incomplete, which will allow me to move back and forth between talk of the formal matrix *table*  $x$ , for example, and (loose) talk of the natural language matrix ‘table’. No confusion should arise.
- 69. The question generalizes: How are we to explain the fact that for certain determiners,  $D$ , a speaker can use ‘ $D \phi$  is  $\psi$ ’ to say something true, even though  $\phi$  is (apparently) true of some things that are irrelevant to the truth or falsity of what the speaker said—and is known by the speaker and hearer to be so—a fact that is (apparently) inconsistent with the truth conditions provided by

standard quantificational theories of *D*? This is only a rough statement of the fully general question. Issues involving negation, monotonicity, persistence, and binding dictate a more careful statement, but this is not the place to explore this matter.

70. I suspect a thorough search of the literature on The Theory of Descriptions would reveal earlier discussions. The matter had already been broached earlier by Husserl in 1913 in connection with descriptive functors. But he suggests that ‘When a contemporary German speaks of ‘the Emperor’, he means the present German Emperor’ (1913: 85). As I pointed out in Neale (1990: 116, n. 54) a literal reading of this remark suggests that one’s *nationality* is a contextual coordinate!
71. Strawson and Sellars both appear strangely ignorant of Quine’s (1940) discussion.
72. More generally, on the ellipsis proposal, the felicitous use of an incomplete quantifier phrase ‘*D*  $\phi$ ’ (see footnote 69) is meant to be quite consistent with the standard quantificational treatment of *D*. For reasons that are obscure, the mere fact that there is often nothing to choose between alternative completions of ‘the  $\phi$ ’ has been taken by some of these philosophers to constitute a barrier to a unitary quantificational account of ‘the  $\phi$ ’ (and thereby indirect evidence for a semantically distinct referential reading, in the sense about to be discussed). That there must be something deeply misguided in the reasoning behind this claim is clear from the fact that the existence of alternative completions of  $\phi$  between which there is nothing to choose is not specific to cases in which the determiner combining with  $\phi$  is ‘the’; the phenomenon extends to cases involving uses of ‘every  $\phi$ ’, ‘no  $\phi$ ’, ‘some  $\phi$ ’, ‘most  $\phi$ ’s’ etc., but this hardly constitutes evidence that these expressions are not quantificational!
73. I contrasted the explicit approach with what I called the *implicit* approach, which is essentially the result of assuming restrictions on quantifier domains of the sort customary in much mathematics. Since I now believe the implicit method is unworkable as a component of a theory that can explain the actual use of natural language—a point which has no bearing whatsoever on its utility in mathematics or any other theoretical concern that does not aim to capture in any way what ordinary speakers are up to when they talk—I will say no more about it. In discussing the explicit approach, I talked of elliptical *utterances* (with Quine) or elliptical *uses* (with Sellars). It never occurred to me in 1990 that anyone working in the philosophy of language would anachronistically identify the sort of ellipsis Quine and Sellars were talking about in 1940 and 1954 with the rule-governed *syntactic* operation that emerged in later work on generative grammar and became a hot topic in connection with VP-deletion, VP ellipsis, and VP anaphora in the early 1970s. Had I issued an explicit injunction against making such a confused identification—Bach (1981) issued precisely such an injunction in an important paper on the same topic—perhaps Stanley and Szabó (2000) and Stanley (2002a) would not have made the error in question in their discussions of the explicit approach to incompleteness. For a detailed historical discussion, see Neale (2004).
74. Again, the point generalizes to other quantified noun phrases.
75. On my unhappiness with appeals to Kripke’s distinction, see Neale (1997).
76. Contrary to what Wettstein (1982) appears to claim in connection with [*the*  $x$ :  $\phi x\alpha$ ], at least. For discussion see Neale (1990) pp. 99–100.
77. For more on linguistic winks, see Neale (2007a).

78. I say they are Gödelian because a definite description of this form is used by Gödel (1944) in his slingshot argument: [*the*  $x$ :  $\phi x \bullet x=b$ ] is the restricted quantifier counterpart of  $(\iota x)(\phi x \bullet x=b)$ , which is what Gödel actually uses. For discussion, see Neale (2001).
79. There is nothing to prevent  $\alpha$  from being a variable bound by some exterior quantifier as in [*every*  $y$ :  $\xi y$ ][*the*  $x$ :  $\phi x \bullet x=y$ ] $\psi xy$ . This fact will soon assume some importance.
80. I have replaced the (closed) singular term  $b$  with  $a$  in this quotation to preserve harmony of symbols with the quotation from Neale (2001) that follows. In the section of Neale (1990) from which the passage is taken I was considering referential<sub>D</sub> uses of descriptions such as ‘the man in the corner drinking a martini’ or ‘the murderer’.
81. In the chapter of Neale (2001) from which the passage is taken, I was discussing Russellian facts. Russell certainly allows distinct true sentences to stand for the same fact. Officially, he must say that  $\phi a$  and  $\phi \iota x(x=a)$ , if true, stand for distinct facts, because the former stands for a singular fact and the latter a general fact (assuming no intersection of singular and general facts). But at the same time he would surely say (i) that  $\phi a$  stands for a fact just in case  $\phi \iota x(x=a)$  does, and (ii) that there is no conceivable way the world could be in which exactly one of them stands for a fact. The putative logical equivalence of  $\phi a$  and  $\phi \iota x(x=a)$  does not seem to get to the heart of the matter, for  $\phi \iota x(x=a)$  seems more tightly bound to  $\phi a$  than is, say,  $(\phi a \bullet (\psi b \vee \sim \psi b))$ , which contains a *predicate* and a *singular term* not present  $\phi a$ , i.e. expressions that contribute what I call *new material content*. Similarly,  $(\lambda x \phi x)a$ —and, for that matter,  $\phi a$ ,  $\sim \sim \phi a$ , and  $(\phi a \bullet \phi a)$ —seem more tightly bound to  $\phi a$  than is  $(\phi a \bullet (\psi b \vee \sim \psi b))$ . There is a strong temptation to provide purely formal characterizations of whatever it is that takes the tighter relationships beyond logical equivalence.
- The feeling that  $\phi a$  and  $a = \iota x(\phi x \bullet x=a)$  are equivalent is respect of *material content* seems to be what Gödel (1944) is responding to when he argues for the dependence of Russell’s Theory of Facts upon the Theory of Descriptions. For it seems to be Gödel’s thought that one would struggle in vain to explain how  $\phi a$  and  $a = \iota x(\phi x \bullet x=a)$  could stand for distinct facts (assuming the identity sign to be part of the logical vocabulary). Nonetheless,  $\iota x(\phi x \bullet x=a)$  and  $a$  differ in material content—or, if we want to reserve the expression ‘material content’ for whole sentences,  $\psi \iota x(\phi x \bullet x=a)$  and  $\psi a$  differ in material content—because the latter contains a predicate,  $\phi$ , that  $\psi a$  does not. And  $\psi \iota x(\phi x \bullet x=a)$  and  $\psi \iota x(x=a)$  differ in material content for the same reason. So on a Russellian treatment, Gödel’s description  $\iota x(\phi x \bullet x=a)$ , unlike  $\iota x(x=a)$ , is not even a *contender* for being cast aside as a verbose rendering of  $a$ .
82. Neale (1990) p. 116, n. 55.
83. Neale (1990) p. 247. I have simplified the example, principally by removing the donkey anaphora, which was the actual topic of discussion (‘every man who lives with another man shares the bills with him’).
84. To the best of my knowledge, there is only one area of natural language semantics in which any sort of case might be made for theoretically interesting, hollow Gödelian descriptions. That is in connection with a quirky (and far from common) way of treating the syntax and semantics of gendered pronouns of

the sort I touched on briefly at the end of Neale (2004) and explored in Neale (2005). If—and it's a *very big* if—(i) the third-person pronouns 'he' ('him', 'his') and 'she' ('her') are the superficial forms of underlying definite descriptions  $[[he][e]]$ ,  $[[she][e]]$ , etc., composed of a gendered version of the definite article and an aponic nominal complement, (ii)  $[[he][e]]$  is interpreted as  $[he\ x: x=\alpha]$ , where  $\alpha$  is interpreted as a directly referential referring expression or as variable, (iii) the binding of seemingly bound pronouns is to be accomplished by the binding of  $y$  in  $[he\ x: x=y]$  by an exterior operator, (iv)  $[[he][e]]$ ,  $[[she][e]]$ , etc. contribute gender to the propositions speakers express using sentences that contain them, (v) the articles  $[he]$ ,  $[she]$ , etc. are syntactically and semantically atomic, and (vi) contributions of gender come from the articles alone (rather than from the descriptions as a whole), then it might be arguable that hollow Gödelian descriptions do have *one* theoretical use, at least when both term positions are occupied by variables: they formally facilitate binding. But there other ways of implementing binding, of course. Furthermore, it might be countered that the only reason the hollow description *seems* to have a rôle in this theory is because it is just a quirky and less than transparent rendering of the theory according to which the bound  $[[he][\phi]]$  is being interpreted as a *solid* Gödelian description  $[he\ x: male\ x \bullet x=y]$ . In both Neale (2004) and Neale (2005), I display a preference for the solid  $[he\ x: male\ x \bullet x=y]$ .

85. Schiffer (2005, 2006) has an objection to the Gödelian analysis based on considerations of psychological implausibility. Devitt (2004, 2007), and Buchanan and Ostertag (2005) have related objections. For my response, see Neale (forthcoming b).
86. Notoriously, there are all sorts of problems with Donnellan's labels. For one thing, 'referential' and 'attributive' uses are neither exclusive nor exhaustive. For another, Donnellan oscillates between characterising referential usage in terms of the speaker having a communicative intention involving a specific individual, in terms of the speaker merely having a specific individual in mind, and in terms of there being a specific individual, the properties of which furnish the speaker's grounds for his utterance. For discussion, See Kripke (1977), Searle (1979), Davies (1981), Neale (1990), and Ludlow and Neale (1991).
87. Talk of being *bound*-into is ultimately of more value than talk of being *quantified*-into because, as noted earlier, singular terms may bind variables too. On the readings in question, we use (i) to predicate of Hugo Wexler precisely what we predicate of every scientist who was fired from the observatory at Sofia in (54)

(i)  $[Hugo\ Wexler]^1$  was consoled by  $[someone\ who\ knew\ [the\ fired\ scientist]^1$  as a youth].

88. This point has been stressed in a recent article by Pupa (2007).
89. Pupa (2007) brings up a similar example involving a twist on Donnellan's example of a referential use of 'the murderer' (or 'the man who murdered Smith'). The twist is that two men murdered Smith in Pupa's scenario. It seems to me his example has more or less the same overall murkiness as my example involving (59) because it is not clear that Jones actually satisfies 'x murdered Smith' since Smith was, as it were, co-murdered.

90. The idea of treating demonstrative descriptions as quantifiers goes back at least to Taylor (1980) and Barwise and Cooper (1981). (In the spirit of Montague, the latter suggest all noun phrases are generalised quantifiers.) More recently, King (1999, 2001) has joined the quantificational ranks. Lepore and Ludwig (2000) suggest that Davidson (1984) “may be construed as anticipating a quantificational treatment”. For discussion, see Neale (forthcoming c).
91. I suggested that the rider might also explain certain facts about binding. See below.
92. In respect of truth conditions, TTO is, I believe, equivalent to the one given by Lepore and Ludwig (2000), at least if a common account of simple demonstratives can be assumed.
93. I stress, the proposal is for referential uses of ‘that  $\phi$ ’ and is inapplicable to cases (see §14 and, for more detail, Neale (2007b)) in which ‘that  $\phi$ ’ is used as a stylistic variant of ‘the  $\phi$ ’ (perhaps anaphorically).
- Schiffer does have another objection: the analogue of his psychological implausibility argument against the hybrid account of definite descriptions. See Neale (forthcoming b).

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