THE THEORY OF DESCRIPTIONS

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To set the scene, here are three contemporaneous quotations from Meinong, Russell, and Moore:

> Things may be said to be more or less real, according to the proportion of truth in the assertions that they do or do not exist . . . Being is an absolutely universal term; i.e. not only 'realities' and 'actualities', but propositions, whether true or false, and any terms that can be used in a proposition, have being or are entities.

> Existence is not a predicate.

> It is plain that ⍳ is a fundamental logical notion, and that it would be merely shirking to invent a dodge for getting on without it.

From Meinong, Russell, Moore; but not in that order. Half-real beings, which exist as much as they don't, were Moore's idea. It was Meinong who had to remind Russell that existence is not a predicate. And it was Russell himself who, only a year or so before writing *On Denoting*, condemned as shirking the very thing that he there declares to be imperative; for his Theory of Descriptions is the claim that it is necessary as well as possible to get on without descriptions and kindred expressions. I shall argue that it is impossible as well as unnecessary, and defend a theory in which they are treated as genuine singular terms.

I

The interesting thing about descriptions is that so many of them are functions in disguise. *Principia Mathematica* explains that its notation for descriptions is chiefly needed to lead up to what it calls descriptive functions—terms with free variables like 'the king of x' or 'the revolution of x round y'—and the examples of descriptions in *On Denoting* are all substitution instances of descriptive functions. Other descriptions may actually have to be reconstrued as descriptive functions, as when 'the woman every tribesman loves' (i.e. his wife) becomes 'the woman x loves' governed by a quantifier 'every tribesman x' elsewhere in the

sentence. The same goes for descriptions in temporal or modal contexts, where 'the so-and-so' may need to be read as elliptical for 'the so-and-so at time t' or 'in state of affairs w'.

As well as descriptive functions there are terms involving explicit function-symbols like . and $+$. Function terms of both kinds are central to logical theory because they are central to mathematical practice, and the real test of a theory of descriptions comes with its handling of functions. Neglecting them encourages the false belief that empty terms (terms that fail to stand for anything) represent mere waste cases which can be disposed of more or less arbitrarily. It also encourages a confusion between a logic of descriptions and an account of the uses of 'the', to the point where a proponent of a 'natural logic' of descriptions can put forward 'the same $\mathcal{N}$' (as in 'John and Bill live in the same house') as being a definite description.

Function terms are special partly because they can be nested, producing $fx, f(gx), f(g(hx))$, etc. This makes it possible to express concisely and manipulate easily, i.e. with little or no use of quantifier logic, information of great complexity. In Russell's theory, however, terms containing function-symbols have first of all to be replaced by descriptive functions, for example by postulating a relation $+(x,y,z)$ with the same meaning as (whisper it!) $x+y = z$, and reintroducing $x+y$ as short for 'the z such that $+(x,y,z)$'. Then these and any other descriptive functions have to be eliminated in the well-known way. Thus the move from $2+3 = 5$ to $2 = 5-3$, instead of being a simple instance of the simple move from $a+b = c$ to $a = c-b$, becomes the move from $(\exists x)((y)(+(2,3,y) \equiv y = x)$ & $x = 5)$ to $(\exists x)((y)(-(5,3,y) \equiv y = x)$ & $2 = x)$. This is only the beginning: an equation like $2x^4+3x^2 = 5$ requires fourteen more quantifiers to deal with the extra function-symbols. The numerals too must be eliminated, and even assuming the simplest zero-cum-successor notation this adds another twenty-six quantifiers to each of our examples. Russell's theory fails the function test by making the expression and manipulation of mathematical information humanly impossible.

Russell was aware that his theory would have 'horribly awkward' consequences; none the less he thought it could be proved to be correct. Let a *singular term* be whatever can be the logical subject of a sentence; then his semantical theory supplies these premises: singular terms stand for things and other expressions stand for concepts; sentences express propositions, which are non-linguistic wholes composed of things and concepts,

and a sentence is true if the constituents of the proposition are related in the way indicated by the sentence. There are then two proofs that descriptions cannot be singular terms, and that sentences containing them cannot have the simple logical form that their grammar might suggest. First, any true equation must be obviously true, for if $a = b$ is true a and b must stand for the same thing and so $a = b$ expresses the same proposition as $a = a$. But 'Scott is the author of *Waverley*' is true without being obvious. Second, any sentence containing an empty singular term will express something with a gap where there ought to be a constituent: not a whole proposition but a 'nonsense' like a jigsaw puzzle with a missing piece. But what is expressed by 'the king of France is bald' is not nonsense since it is plainly false. Parallel arguments apply to function terms.

The first proof is the more far-reaching of the two, for it implies that only logically simple expressions can be singular terms. As originally formulated, however, Russell's semantics contained a complication calculated to frustrate the proof. This was the idea that certain 'denoting phrases' stand for more than one propositional constituent; in particular, descriptions will express a concept and denote a thing. If a and b denote the same thing but express different concepts $a = b$ can after all be true without expressing the same proposition as $a = a$. It was therefore to be one of the chief tasks of *On Denoting* to refute and undo the complication and produce the simplified version of the semantics for which the proof is valid. The refutation takes the form of a dilemma over how to specify the concept expressed by a description. *Using* the description is no good, because that only serves to specify the thing the description denotes. But if the concept can only be specified by *mentioning* the description, on the lines of 'the concept expressed by "a"', then the relation between concept and denotation, which ought to be a logical one, is made out to be 'merely linguistic through the phrase'.

To me this argument is noteworthy for the way it brings out an extraordinary feature which Russell's semantics inherited from his mentor Moore. This is the idea that the study of propositions and their constituents, which here is called logic, can be divorced from any study of language. The latter may perhaps provide a 'useful check on the correctness' of the former, but 'meaning, in the sense in which words have meaning, is irrelevant to logic'. Only someone imbued with this idea could so briskly dismiss linguistic relations as 'mere' or be so confident that a relation mediated through a phrase cannot be 'logical'. Russell himself presented

the dilemma as if it were a criticism of Frege's theory of sense and denotation, which, though different from Russell's in almost every other respect, has the same effect of frustrating his first proof. But until it is shown that Frege's theory shares the feature we have been discussing, the 'mention' horn of the dilemma presents no threat to it. As for the 'use' horn, the second most prominent contention of *Ueber Sinn und Bedeutung* is a flat denial of it, through the doctrine of indirect denotation. There may or may not be serious criticisms of this doctrine as an answer to the problems of indirect speech, but an argument that overlooks its very existence can hardly be one of them.

Turning now to the second proof and its minor premiss that it is false that the king of France is bald: this has been challenged, notably by Strawson's championing the 'nonsense' alternative (more on this later). Even if it had not been challenged the premiss would in any case be inconclusive until it has been shown why the label 'plainly false' shouldn't be covering Russellian nonsenses, much as 'nonsense' is a common label for very plain falsehoods. What we need are examples of empty terms occurring in sentences that are plainly *true*. Clearly they won't be found occurring as subjects, but for any other sort of occurrence the theory of partial recursive functions is an unimpeachable source of examples. Outside mathematics there are plenty of examples of the form 'there is no such thing as *a*', but for other forms we need to look at cases where there is dispute rather than agreement over the existence or non-existence of something. Thus if one consults the literature of astronomy for the 1860s or geology for the 1920s one does indeed find scientists, agnostic or sceptical about the existence of the now-debunked planet Vulcan or the debunked and then rebunked continent Pangaea, using these names to propound true conditionals—and to ask questions, which is another difficulty for the 'nonsense' alternative.

Another assumption of the second proof is that if 'the king of France' is a singular term it does not stand for anything. This needed to be defended against Frege, who proposed to secure by fiat that all descriptions stand for something or other, and against Meinong, who held that there is a king of France in some sense weaker than 'exists'. Russell understandably found Frege's proposal artificial and Meinong's contention incredible, but thought that Meinong could also be convicted of actual logical error:

the chief objection is that such objects, admittedly, are apt to infringe the law of contradiction. It is contended, for example, that the existent present King of France exists, and does not exist; that the round square

is round, and also not round, etc. But this is intolerable; and if any theory can be found to avoid this result, it is surely to be preferred.

We have seen Russell misrepresenting Frege in the course of his other argument; here it is Meinong's turn. As a German speaker he naturally used the definite article where English usually has the indefinite article or a plural noun, namely to express generic propositions. So where he says 'Das runde Viereck ist rund' we should translate 'a round square is round' or 'round squares are round'. No wonder he maintained that (as we would put it) 'round squares are round and square', far from being contradictory, is analytic. No wonder that (anticipating Moore's celebrated contrast between 'tame tigers growl' and 'tame tigers exist') he distinguished the true 'existent golden mountains are existents' from the false 'existent golden mountains exist', pointing out that 'exists' is not a predicate and that predication here has no existential import: 'so gewiß das Dasein kein Sosein und auch das Sosein kein "So"'. In short, what Russell calls the 'result' of Meinong's theory of objects is actually a travesty of the data which that theory attempted to explain.

To vindicate Meinong's data is not, of course, to vindicate his theory, either applied to those data or as it might be applied to descriptions. For a genuine refutation both of Meinongian and Fregean theories of descriptions we may apply the function test, looking at their handling of the theory of partial recursive functions. The simplest Fregean method is to convert each partial function f into a total function $f*$ by setting $f*(n) = 0$ whenever fn is undefined. The trouble is that the class of functions obtained in this way from the partial recursive functions is not recursively invariant; that is to say, it is not stable with respect to recursive numerical transformations (the proof is sketched below, among the references). But recursive invariance is the condition which, in the words of Rogers's *Theory of Recursive Functions and Effective Computability*, 'characterizes our theory and serves as a touchstone for determining possible usefulness of new concepts'. The objection in short is that what is offered as a surrogate for a branch of the theory of computability fails to constitute an intelligible theory of anything. The argument can easily be adapted to apply to Meinongian or sophisticated Fregean versions which go outside the domain of numbers for their total functions. For lying behind it is the fact that there are three possibilities for any computation, whether numerical or not: it may produce an appropriate output or halt without doing so or soldier on for ever; but a theory restricted to total functions can only represent the first two cases.

I began this section by using the function test to argue that Russell's theory of descriptions is untenable, but there is an *ad hominem* argument to the same effect. This concerns the notion of the variable, which Russell took as fundamental, thinking not of a symbol like *x* but of the 'essentially and wholly undetermined' propositional constituent that such a symbol would have to stand for. Moore seized on this as soon as *On Denoting* appeared. Referring to Russell's claim that the constituents of any proposition we understand must be entities with which we are immediately acquainted, he asked 'Have we, then, immediate acquaintance with the variable? and what sort of an entity is it?' Russell replied

I admit that the question you raise about the variable is puzzling, as are all questions about it. The view I usually incline to is that we have immediate acquaintance with the variable, but it is not an entity. Then at other times I think it is an entity, but an indeterminate one. In the former view there is still a problem of meaning and denotation as regards the variable itself. I only profess to reduce the problem of denoting to the problem of the variable. The latter is horribly difficult, and there seem equally strong objections to all the views I have been able to think of.

Could there be a more candid admission that he had as much reason to reject the Theory of Descriptions as for rejecting the alternatives to it?

Either way, whether his theory is rejected on external or internal grounds, the direction of Russell's proofs becomes reversed, and they turn into a *reductio ad absurdum* of his semantic premisses. The first proof incidentally becomes an argument in favour of Frege's theory of sense and denotation (naturally, since it was to explain the existence of unobviously true equations that the theory was propounded). The second proof becomes a warning that there must be some way other than Frege's of accommodating empty terms within his theory.

II

Why then did Frege reject empty terms? He says they breed fallacy and error, and it has to be admitted that even professionals are not immune: *Principia*'s treatment of descriptive functions is muddled by the idea that 'the wife of *x*' is ambiguous, rather than empty, when *x* has more than one wife. But fallacy and error could never be a reason for rejecting empty terms out of hand. They are no more than an invitation to the logician to earn his living by devising a systematic remedy, and it remains to be seen how drastic the remedy has got to be.

His polemic about partial definition needs to be mentioned next. The definition of division may be called partial, since 'sun/moon' is senseless. Frege heavily criticizes those who treat such definitions as unfinished and so extendible, because they do not see that a definition fixes the senseless as much as the sensible side of its boundary of application. He goes on to condemn partial definition itself and to argue that a sense must be given to celestial arithmetic and to expressions like $(2 = 2) = (2 + 3 = 5)$. This paradoxical conclusion is not, however, supported by his arguments. The proper conclusion is not that domains of definition must be universal but that they must be decidable. This would explain why they are appropriately mirrored by grammatical rules of well-formedness, and it would incidentally dispose of Hilbert and Bernays's theory of descriptions. It also has the corollary that at a certain level it must be decidable what sort of thing a singular term stands for. This will be achieved for descriptions through the relevant common noun or through one used in defining it, and for function terms through the definition of the relevant function-symbol; while for proper names it invites the systematic use of symbols to do the job done in, for example, 'Fujiyama' or 'Mount Fuji' (status symbols?) But even if Frege were right about partial definition, this wouldn't tell against empty terms. It only appears to do so because of an ambiguity in ideas like 'domain of definition' or 'undefined'. We say that division by the moon is undefined, meaning that '1/moon' has no sense; but we say too that division by zero is undefined, meaning merely that '1/0' has no denotation. (It is because it has a perfectly good sense, namely 'the number which gives 1 when multiplied by 0', that we can tell it has no denotation.) We ought to distinguish between partial definitions, which make fa sometimes meaningless, from definitions of partial functions, which make fa meaningful but empty for some non-empty a; and only the latter are relevant here.

We come, finally, to Frege's argument that the presence of an empty singular term prevents a sentence from having a truth-value. The outcome of this is similar to the outcome of Russell's argument that an empty term prevents a sentence from expressing a proposition: similar enough to present the same difficulties. The difference lies in the mechanism. In Russell's semantics the things terms stand for are literally parts of what sentences express, but in Frege's semantics things are only mapped on to truth-values in the notional way that a function maps things of one sort (its arguments) on to things of another sort (its values). He must therefore

be assuming that a function can't map nothing on to something, i.e. that if a is empty fa must be empty too. But this is false. An obvious counter-example is the set-forming function {. . .}, for the term {the king of France} is not empty but stands for a set—the empty set. Other counter-examples are the constant functions; these have been noted by Scott, who calls them 'non-strict' functions. Anyone who is used to partial functions, which map some things on to nothing, will find nothing disconcerting about non-strict ones, for the two are merely opposite sides of the same coin. And if the functions mapping things on to truth-values need not be strict, this reason for supposing that empty terms create truth-value gaps collapses.

III

The proposal, then, is to enrich the classical predicate calculus by adding a description operator and provision for function-symbols, using them in the obvious way to create a class of logically complex singular terms. Some of these may be empty, and we stipulate that every atomic sentence containing an empty term is false. Sketchy as this is, it suffices: the logician only has to push his canoe so far into the stream for the semantics of the connectives and quantifiers to bear him on inexorably.

Our stipulation about the truth-value of atomic sentences agrees with Russell's verdict on 'the king of France is bald', but it turns out that at most one person in three shares his feeling that this is 'plainly' false; indeed a comparable minority feel that it is plainly neither true nor false. If we reject a truth-value-gap semantics we need an alternative explanation for these truth-value-gap responses. And one plausible explanation is that those who respond in this way do so because, for them, calling 'the king of France is bald' false involves more than simply denying its truth; it also involves being willing to subscribe to the truth of its contrary, 'the king of France is not bald'. The interest of this for the logician lies in the challenge to enrich the formal system so as to allow for the expression of such contrary pairs of predications, to see if the idea can be extended to cover sentences in general, and to explore its consequences. But this calls for a lecture to itself, and I pass right over it to deal with two other grounds of objection to our stipulation.

Identity. Most of the literature treats $a = a$ as true even for empty a, and some of it appeals to the evidence of intuition. On putting a simple numerical example to a group of innocents I found that

three-quarters did indeed feel that $a = a$ was plainly true for empty a—but the proportion halved as soon as the wording was changed to 'a is the very same number as a', while as many people's intuitions told them that $a = a$ was true but $a \leqslant a$ false as that both were true! One couldn't hang a dog, let alone a point of logic, on such evidence.

If $a = a$ is an atomic sentence the Russellian line makes it false for empty a, but controversy over this is liable to be spurious. For anyone who takes partial functions seriously soon finds that he needs two readings of identity, call them $=$ and $\equiv$. They are interdefinable and agree in every case except that $a = b$ is false and $a \equiv b$ true when a and b are both empty. Algebra needs $=$ which, unlike $\equiv$, excludes empty roots of equations and allows terms to be freely moved across equations; but $\equiv$ is right for expressing, for example, the basic law of functions $a \equiv b \supset fa \equiv fb$, or identity conditions like $(x)(fx \equiv gx)$.

The coexistence of $=$ and $\equiv$ is an instance of a phenomenon that is not peculiar either to identity or to the logic of singular terms. A look at English sentences of the form $F(\text{an } \mathcal{N})$ seems to show up two large classes of predicates. One class calls for an existential reading of the sentence, i.e. equating 'an $\mathcal{N}$' with 'some $\mathcal{N}$'. The other leaves room for a generic reading in which 'an $\mathcal{N}$' is roughly equated with 'any $\mathcal{N}$' or 'whatever is an $\mathcal{N}$'. The continuous present tense, for example, seems to belong to the first class while the simple present may or may not belong to the second: compare 'a tame tiger is growling', 'a tame tiger growls', and 'a tame tiger exists'. Sentences containing descriptions seem to exhibit a similar division, as befits the equivalence between 'the $\mathcal{N}$' and 'an $\mathcal{N}$ but for which there are no $\mathcal{N}$s'. It therefore in no way impugns the Russellian line over truth-conditions to concede that many simple sentences with empty subjects can be read as true. For this will be because they can be read as $F(\text{whatever is } a)$ and so are to be formalized by $(x)(x = a \supset Fx)$ rather than by the atomic Fa. This seems to be the route that leads to $\equiv$, with its intuitively gratifying corollary that $a \equiv a$ is always true. There is also the counter-intuitive corollary that $a \equiv b$ is true whenever both terms are empty, but this is like the counter-intuitive truth of $(x)(Fx \equiv Gx)$ whenever both predicates apply to nothing. It is part of the price of using truth-functional connectives to help formalize 'whatever . . .', and it would be inconsistent to put up with it in the predicate calculus and balk at it over singular terms.

Free logic. Free logic is conceived as a version of the predicate calculus that accommodates empty singular terms without taking

any stand over the truth-value of atomic sentences containing them. Qualified by an adjective, the name has also been applied to logics that do take a uniform stand, and my own proposal would thus be called negative free logic. If I reject this description it is because it implies an acceptance of the methodology of free logic which I do not share. Consider, for example, 'John prevented the accident at the corner of such-and-such streets', 'Ponce de Léon sought the fountain of youth', or 'Heimdal broods' (Heimdal coming from Norse myth). These are typical of test sentences routinely cited by the leading free logicians, and it can hardly be a coincidence that they appear to vindicate the refusal to take a uniform line over the truth-values of atomic sentences with empty terms. A negative free logician is supposed to say that the non-existence of the accident or fountain or Heimdal makes them false. I should certainly not want to say this, but in trying to account for their truth or potential truth I should start by denying that any version of the predicate calculus was an appropriate vehicle, or that they were of the aRb or Fa form. Surely the first involves a counterfactual (there was no accident but there would have been if he had not acted); the second requires expansion in a non-extensional logic; and the third calls for a distinction between language used with tacit reference to a story (legend has it that . . .) and its use within a story or as a record of everyday fact. As I see it, the business of a theory of descriptions is to do for singular terms what the predicate calculus does for 'and' and 'every' and so on; and this does not include a logic of prevention or seeking or myth. These are separate—and quixotic—undertakings which are given no unity by accidents of grammatical form, nor by the fact that they all involve singular terms: descriptions occur everywhere, but a logic of descriptions is not a logic of everything.

IV

Given our stipulation about atomic sentences, $F(\imath xGx)$ is equivalent to $(\exists x)((y)(Gy \equiv y = x) \ \& \ Fx)$, and this provides the start for a proof that any sentence is equivalent to a descriptionless one. This in turn makes up half of a demonstration that descriptions are eliminable, in the sense that the 'outer' system containing them is equivalent to the 'inner' system which would have obtained if they had not been introduced. The other and more difficult half lies in showing that the introduction of descriptions is conservative, i.e. that it does not affect logical relationships within the inner system. I mention eliminability to caution against

exaggerating its importance. For the equivalence between sentences with and without descriptions, on which the equivalence between the outer and inner systems depends, only means that they have the same truth-conditions, not the same sense or the same behaviour under logically significant operations. Talk of 'equivalent' systems and 'eliminability' is therefore liable to beg the question. In particular, going back to my original argument against Russell's theory and considering functions along with descriptions, the equivalence between the outer and inner systems does not alter the fact that the outer one is a possible vehicle for mathematics as practised by human beings while the inner one is not.

Whatever one thinks of the eliminability of descriptions, one needs to stress the difference between it and Russell's 'elimination' of them, for the whole point of Russell's theory is the denial that there can be an 'outer' system. The contextual definitions offered by *Principia Mathematica* are a substitute for a logic of descriptions, not a corollary of one. Could one then meet Russell halfway by developing a logic of descriptions in which they do not figure as singular terms? Yes, by treating them as quantifier expressions. The idea goes back to a remark of Geach's thirty years ago, and several logicians have propounded systems in which description is represented by a binary quantifier, producing sentences on the lines of $(\imath x)(Fx, Gx)$. I should only like to stress something that the binary quantifier approach implicitly or explicitly rejects, namely a place in logic for general terms, i.e. nouns and noun phrases like 'number', 'prime number', 'prime number that divides ten'.

Logic has never been at ease with such terms. Sometimes they are replaced by singular terms under the guise of class-names; other times they are replaced by predicates (Russell was an influential advocate of this); and other times again they are simply ignored, leaving the choice of a domain of individuals for the predicate calculus as the sole evidence of a systematically suppressed general term calling for interpretation. Actually, general terms differ from singular terms and predicates as much as either differs from the other. In particular, predication presupposes that (in Dummett's crisp phrase) the world has already been sliced up into objects, whereas general terms determine the principles by which the slicing is effected. Hence predicates but not general terms can be negated, and there are universal predicates but no universal general term, for 'same non-number' or 'same thing' fail to supply the requisite principles. The exclusion of general terms is also a waste of good workaday logic. It

leaves no place to explore the nesting of restrictions in complex terms, or the interplay between more and less complex terms exemplified on the one side by 'some N that Fs, Gs' and 'there are no Ns that F' and on the other by 'some N Fs and Gs' and 'no Ns F'. Nor does it do justice to the syntactic variety of quantifiers, which besides the familiar ones that go with a single term and a single predicate, and the 'binary' ones that go with a single term and a pair of predicates (more Ns F than G), include, for example, those that require more than one term (more Ms than Ns F) and the especially interesting class that require terms but no predicates (there are Ns, there are more Ms than Ns). The addition of a descriptive quantifier to a logic of general terms leads at once to an equivalence between the quantifier expressions 'the M' and 'some (M but for which there are no Ms)'; and if we take the special case where M is of the form 'N that Fs', and use the interplay cited above to eliminate the complexity of the bracketed general term, we obtain a 'Russellian' equivalence between 'the N that Fs, Gs' and a sentence with quantifier expressions involving only the bare N, namely $(\exists Nx)(Fx \ \& \ (Ny)(Fy \supset y = x) \ \& \ Gx)$.

It is all very well to say that description can be treated as quantification, but why should it be? One answer is interesting but irrelevant. This is that every singular term is paralleled by what, following Faris, we should call a singular quantifier. For as well as seeing Fa as predicating F of a we can see it as predicating something of F, namely that it applies to a. In other words, for each singular term a it is possible to introduce a quantifier $(a \ \ )$, which might be read 'a is such that it . . .' and which goes to make up sentences on the lines of $(ax)Fx$. Given descriptions as singular terms, the possibility—one might say the inevitability—of singular quantification is interesting as explaining their apparent ability to double as quantifier expressions. But the pressing question is whether it was right to admit them as singular terms in the first place or whether they must be treated as quantifier expressions exclusively.

If one asks why the standard quantifier expressions are not singular terms, a decisive answer is the presence of ambiguity in sentences like 'everyone R someone', which would be inexplicable if they were logically of the aRb form. Frege's proposal to reconstrue quantifier expressions as second-level predicates allows these ambiguities to be explained as straightforward cases of ambiguity of scope, just as $\log x^2$ involves an ambiguity of scope between a pair of first-level functions. It has been argued, notably by Prior, that sentences like 'it is not true that the king of France is

bald' create the same difficulty and call for the same solution. The argument rests, however, on overstretching the analogy between the formation of composite predicates and the standard formation of composite functions. The latter takes a pair of functions f and g and produces a new function (fg), defined so that $(fg)a \equiv f(ga)$ for every choice of a. The two sides here have a quite different structure—f and g occur on the right in their normal role as functions while on the left they occur rather as arguments, namely of a second-level composition function—but the equivalence allows us to write fga indifferently without coming to harm. In an analogous way a connective like $\sim$ and a predicate F can be used to form a new predicate $(\sim F)$. This is naturally defined so that wherever possible $(\sim F)a \equiv \sim(Fa)$, but equally naturally the two sides diverge for empty a. In that case $(\sim F)a$ is false, following the general rule for a subject-predicate sentence with empty subject; while $\sim(Fa)$, which is the negation of such a sentence, is true. Here, therefore, it is not safe to omit the brackets, any more than in $a(2^3)$ and $(a^2)^3$, and that is all there is to it. To suppose that $(\sim F)a$ cannot possibly differ from $\sim(Fa)$ is to suppose that there cannot be more than one useful notion of composition. As with non-strict functions, the moral is that the approach to logic in terms of function and argument, profound and liberating though it may be, is a potent source of error if handled uncritically.

Descriptions are also alleged to create scope ambiguities in temporal and modal contexts. Description is only incidental to this phenomenon, which pervades the logic of general terms: the ambiguity of 'the king was bald' or 'the king could be bald' is all of a piece with that of 'several kings were bald' or 'every king could be bald'. And to talk of scope here is to prejudge the issue. Perhaps these ambiguities can be explained as ambiguities of scope as between a quantifier and a tense or modal operator. But it seems to me that they can be explained as well or better in terms of ellipsis, i.e. 'king' being read as elliptical for 'king at t' or 'king in w'. In the temporal case this merely follows up the natural distinction between 'the present king' and 'the then king'. In the modal case it might be objected that it depends on a possible-worlds semantics. This is true, but the scope solution calls for quantification through modal operators, and how is this to be explained if not through a possible-worlds semantics? Anyone who wants to use modal ambiguities to show that descriptions are not genuine singular terms has both to produce some other and better way of explaining quantification into modal contexts and show that it rules out the invocation of ellipsis.

V

It may have sounded odd to cite Strawson as championing Russell's 'nonsense' alternative, for surely one of his principal criticisms was that the disjunction between nonsense and falsity is a bogus one, reflecting Russell's failure to distinguish between sentences and the statements they may be used to make in different contexts of utterance. As to Russell, I think there is abundant evidence, e.g. his use of 'about' as a technical term belonging to his theory of propositions and his regular use of quotation marks to refer to propositions, to suggest that the disjunction between nonsense and falsity in *On Denoting* is a perfectly coherent one, concerned not with sentences but with what they express. And Strawson's own language is so strikingly consistent with this way of construing Russell—Russell's idea of a proposition reduced to nonsense by the absence of a constituent reappears as the idea of a statement 'suffering from a deficiency so radical as to deprive it of the chance of being true or false'—as to suggest a disagreement within the Russellian semantics rather than about it.

Tempting though it is to depict Strawson as a rival player in the same game as Russell, he is really trying to take over the pitch for a different game altogether. I'm not thinking here of his discussion of Russell but of his claim that the significant truths about descriptions, including whatever can be salvaged from the Russellian equivalences, are 'necessarily omitted from consideration' by *any* formal logic. For, he argues, formal logic by its very nature ignores questions of context, and hence the formal logician's ideal is the sentence whose truth is unaffected by context. But the vast majority of contingent sentences are highly sensitive to context, and perhaps the only ones that meet the ideal are the quantified sentences of the predicate calculus. And this, he says, explains the 'acharnement' (a French word meaning 'desperate eagerness') with which logicians try to reduce subject-predicate sentences to quantified ones in the way typified by Russell's theory. For naturally 'the formal logician is reluctant to admit, or even envisage the possibility, that his analytic equipment is inadequate for the dissection of most ordinary types of empirical statement'.

If this is true I have been wasting my time, but is it true? Granted that formal logic takes no account of context, does it follow that it cannot handle context-sensitive sentences? Some logicians have said so, including at times Russell—the same Russell whose short list of singular terms comprised 'I' and 'this'.

But they are wrong. All we need do is assume the same context of utterance for the sentences on each side of an implication, say. Provided they are affected by context in matching ways they can then be handled for all the world as if they were context-independent. This assumption can be made explicit by mentioning the sentences, on the lines of

the statement made by uttering 'I am taller than you' in any context implies the statement made by uttering 'you are shorter than me' in the same context

a formula which naturally gets shortened in practice to

'I am taller than you' implies 'you are shorter than me'.

Alternatively the assumption can be left tacit by using both sentences, as components of one on the lines of

the statement that I am taller than you implies the statement that you are shorter than me.

The only essential is to treat both sides the same way. Mention on one side and use on the other leads either to

the statement made by uttering 'I am taller than you' in any context implies that you are shorter than me.

or else to

the statement made by uttering 'I am taller than you' in any context implies that the person or persons addressed are shorter than the speaker.

The first is absurd and the second introduces Strawson's 'very special and odd sense of "implies"', but neither exposes any inadequacy on the part of formal logic. They are simply the products of a gratuitously lopsided form of expression.

The Russellian truth-conditions for descriptions are a prime example of all this. Imagine the assertion 'the table is covered with books' eliciting the query 'which table?', and this getting the answer 'there is only one table'; and you see how the two assertions have that matching sensitivity to context which makes possible a strictly formal treatment of the implication between them. How could Strawson of all people have overlooked this? How explain the *acharnement* with which he adopts the lopsided form of expression I have just been discussing? Can it be that the informal logician is 'reluctant to admit or even envisage the possibility' that his contribution is limited to such theorems as

the word 'I' is correctly used by a speaker to refer to himself; the word 'you' is correctly used to refer to the person or persons whom the speaker is addressing . . .?

Fortunately there is a more interesting explanation. If we spell out the truth-conditions of 'the table is covered with books' not as ($\exists$ table x) ... but as ($\exists x$)(x is tabular & ...), with the general term replaced by a predicate and a variable ranging over everything whatever, then we are asking for Strawson's criticisms. For, as he has observed, general terms are typically highly sensitive to context while predicates are not. And though I do not believe that the desire to get away from context-dependence played the slightest part in Russell's policy of replacing general terms by predicates, Strawson's observation provides further evidence that the policy is profoundly mistaken. It is a mistake that can easily be avoided in a theory of descriptions, so the threat of a contextual takeover evaporates; but Russell shares the blame for the misunderstanding that gave rise to it.

VI

I have been advocating a theory of descriptions that admits them as genuine singular terms, allows for empty terms, and accepts the Russellian truth-conditions for the relevant sentences. The resulting logic adds little of direct philosophical interest to the classical predicate calculus on which it builds, for it calls neither for any radical change in the expression of our thoughts nor for any departure from bivalence. It throws up no discoveries to compare with Church's theorem or the Löwenheim–Skolem theorem, and for better or worse it is unlikely to emulate the work of Gentzen or Tarski by inspiring a philosophical programme that would have been inconceivable without it. What makes it remarkable is that the one thing on which the philosophers who have written most influentially upon the subject are agreed is that it is wrong in principle: recall Frege's rejection of empty terms, Strawson's takeover bid on behalf of informal logic, and above all Russell's claim that a description is really a wff in sheep's clothing. If I am right, then, the philosophical significance of the logic of descriptions is like the curious incident of the dog in the night-time:

'Is there any point to which you would wish to draw my attention?'
'To the curious incident of the dog in the night-time.'
'The dog did nothing in the night-time.'
'That was the curious incident', remarked Sherlock Holmes.

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Russell: (on shirking and on its awkward consequences) 'On Meaning and Denoting', unpublished manuscript in the McMaster archives; (on the logical versus the linguistic) *The Principles of Mathematics*, pp. 42 and 47; (on the variable) 'On Denoting' in, e.g., *Logic and Knowledge*, p. 42, and letter of 25 October 1905 in the McMaster archives.

Meinong versus Russell: Meinong, *Über die Stellung der Gegenstandstheorie im System der Wissenschaften* (1907), p. 17; Russell, 'On Denoting', p. 45, in *Logic and Knowledge*, and reviews in *Mind*, 14 (1905), 531-3 and in *Mind*, 16 (1907), 439. It should be added that a decade later Meinong was to dig for himself something akin to Russell's trap, in his *Über Möglichkeit und Wahrscheinlichkeit* (1915), but that is a chapter in another story.

'The same N' as a description: Lakoff, 'Linguistics and Natural Logic', in *Semantics of Natural Language*, eds. D. Davidson and G. Harman.

Recursive invariance: the arguments for which a partial recursive function takes a given value have to form a recursively enumerable set, but those for which it is undefined need not do so. It thus turns out that for some $f*$ in our class the set of n for which $f*(n) = 0$ is not recursively enumerable. Consequently the recursive transformation that consists of swapping 0 and 1, say, transforms $f*$ into a function that cannot belong to the class, since for every partial recursive function g the set of n for which $g*(n) = 1$ has to be recursively enumerable.

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