

## SPEAKER'S REFERENCE AND SEMANTIC REFERENCE<sup>1</sup>

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I am going to discuss some issues inspired by a well-known paper of Keith Donnellan, "Reference and Definite Descriptions,"<sup>2</sup> but the interest—to me—of the contrast mentioned in my title goes beyond Donnellan's paper: I think it is of considerable constructive as well as critical importance to the philosophy of language. These applications, however, and even everything I might want to say relative to Donnellan's paper, cannot be discussed in full here because of problems of length.

Moreover, although I have a considerable interest in the substantive issues raised by Donnellan's paper, and by related literature, my own conclusions will be methodological, not substantive. I can put the matter this way: Donnellan's paper claims to give decisive objections both to Russell's theory of definite descriptions (taken as a theory about English) and to Strawson's. My concern is *not* primarily with the question: is Donnellan right, or is Russell (or Strawson)? Rather, it is with the question: do the considerations in *Donnellan's paper* refute Russell's theory (or Strawson's)? For definiteness, I will concentrate on Donnellan versus Russell, leaving Strawson aside. And about this issue I will draw a definite conclusion, one which I think will illuminate a few methodological maxims about language. Namely, I will conclude that the considerations in Donnellan's paper, *by themselves*, do *not* refute Russell's theory.

Any conclusions about Russell's views *per se*, or Donnellan's, must be tentative. If I were to be asked for a tentative stab about Russell, I would say that although his theory does a far better job of handling ordinary discourse than many have thought, and although many popular arguments against it are inconclusive, probably it ultimately fails. The considerations I have in mind have to do with the existence of "improper" definite descriptions, such as "the table," where uniquely specifying conditions are not contained in the description itself. Contrary to the Russellian picture, I doubt that such descriptions can always be regarded as elliptical with some uniquely specifying conditions added. And it may even be the case that a true picture will resemble various

aspects of Donnellan's in important respects. But such questions will largely be left aside here.

I will state my preference for one substantive conclusion (although I do not feel completely confident of it either): that unitary theories, like Russell's, are preferable to theories that postulate an ambiguity. And much, though not all, of Donnellan's paper seems to postulate a (semantic) ambiguity between his "referential" and "attributive" uses. But—as we shall see—Donnellan is not entirely consistent on this point, and I therefore am not sure whether I am expressing disagreement with him even here.<sup>3</sup>

(1.) *Preliminary considerations.*

Donnellan claims that a certain linguistic phenomenon argues against Russell's theory. According to Russell, if someone says, "The  $x$  such that  $\phi(x) \psi$ 's," he means that there is an  $x$  which uniquely satisfies " $\phi(x)$ " and that any such  $x$  satisfies " $\psi(x)$ ." (I.e.,  $(\exists x) (\phi!(x) \wedge \psi(x))$ , where " $\phi!(x)$ " abbreviates " $\phi(x) \wedge (y)(\phi(y) \supset y = x)$ "). Donnellan argues that some phenomenon of the following kind tells against Russell: Suppose someone at a gathering, glancing in a certain direction, says to his companion,

(1) "The man over there drinking champagne is happy tonight."

Suppose both the speaker and hearer are under a false impression, and that the man to whom they refer is a teetotaler, drinking sparkling water. He may, nevertheless, be happy. Now, if there is no champagne drinker over there, Russell would regard (1) as false, and Frege and Strawson would give it a truth-value gap. Nevertheless, as Donnellan emphasizes, we have a substantial intuition that the speaker said something true of the man to whom he referred in spite of his misimpression.

Since no one is really drinking champagne, the case involves a definite description that is empty, or vacuous, according to both Russell and Frege. So as to avoid any unnecessary and irrelevant entanglements of the present question with the issues that arise when definite descriptions are vacuous, I shall modify this case (and all other cases where, in Donnellan's paper, the description was vacuous).<sup>4</sup> Suppose that "over there," exactly one man is drinking champagne, although his glass is not visible to the speaker (nor to his hearer). Suppose that he, unlike the teetotaler to whom the speaker refers, has been driven to drink precisely by his misery. Then *all* the classical theories (both Russellian and Fregean) would regard (1) as false (since exactly one man over there is drinking champagne, and he is *not* happy tonight). Now the speaker has spoken *truly* of the man to whom he refers (the teetotaler), yet this dimension is left out in all the classical analyses, which would assign falsehood to his assertion solely on the basis of the misery of *someone else* whom *no one* was talking about (the champagne drinker). Previously Linsky had given a similar example. He gave it as an empty case; once again I modify it to make the description non-vacuous. Someone sees a woman with a man. Taking the man to be her husband, and observing his attitude towards her, he says, "Her husband is kind to her," and someone else may nod, "Yes, he seems to be." Suppose the man in question is not her husband. Suppose he is her lover, to whom she has been driven precisely by her husband's cruelty. Once again both the Russellian analysis and the Fregean analysis would assess the statement as false, and both would do so on the basis of the cruelty of a man neither participant in the dialogues was talking about.

Again, an example suggested to me by a remark of L. Crocker: suppose a religious

narrative (similar, say, to the Gospels) consistently refers to its main protagonist as "The Messiah." Suppose a historian wishes to assess the work for *historical accuracy*—that is, he wishes to determine whether it gives an accurate account of the life of its hero (whose identity we assume to be established). Does it matter to this question whether the hero really was the Messiah, as long as the author took him to be so, and addressed his work to a religious community that shared this belief? Surely not. And note that it is no mere "principle of charity" that is operating here. On the contrary, if someone other than the person intended were really the Messiah, and if, by a bizarre and unintended coincidence, the narrative gave a fairly true account of *his* life, we would not for that reason call it "historically true." On the contrary, we would regard the work as historically *false* if the events mentioned were false of its intended protagonist. Whether the story happened to fit the true Messiah—who may have been totally unknown to the author and even have lived after the time the work was composed—would be irrelevant. Once again, this fact seems inconsistent with the positions both of Frege and of Russell.

On the basis of such examples, Donnellan distinguishes two uses of definite descriptions. In the "attributive" use, a speaker "states something about whoever or whatever is the so-and-so." In the "referential" use, a speaker "uses the description to enable his audience to pick out whom or what he is talking about and states something about that person or thing. In the first [attributive] case, the definite description might be said to occur essentially, for the speaker wishes to assert something about whatever or whoever fits that description; but in the referential use the definite description is merely one tool for . . . calling attention to a person or thing . . . and . . . any other device for doing the same job, another description or name, would do as well."<sup>6</sup> For example, suppose I come upon Smith foully murdered. The condition of Smith's body moves me to say, "Smith's murderer is (must be) insane." Then we have an *attributive* use: we speak of the murderer, whoever he may be. On the other hand, suppose that Jones is on trial for Smith's murder and that I am among the spectators in the courtroom. Observing the wild behavior of the defendant at the dock, I may say, "Smith's murderer is insane." (I forgot the defendant's name, but am firmly convinced of his guilt.) Then my use is referential: whether or not Jones was the real murderer, and even if someone else was, if Jones accused me of libel, his failure to fit my description would give me no defense. All of the previous cases, (the teetotaling "champagne" drinker, the lover taken for a husband, the false Messiah), are all referential in Donnellan's sense.

An intuitive mark of the attributive use is the legitimacy of the parenthetical comment, "whoever he is." In the first case, we may say "Smith's murderer, whoever he is, is insane," but not in the second. But we should not be misled: a definite description may be used attributively even if the speaker believes that a certain person, say, Jones, fits it, provided that he is talking about *whoever* fits, and his belief that Jones in fact fits is not relevant. In the case where I deduce the murderer's insanity from the condition of Smith's body, I use the description attributively even if I suspect, or even am firmly convinced, that Jones is the culprit.

I have no doubt that the distinction Donnellan brings out exists and is of fundamental importance, though I do not regard it as exclusive or exhaustive. But Donnellan also believes that Russell's theory applies, if at all, only to attributive uses (p. 293), and that referential uses of definite descriptions are close to proper names, even to Russell's "logically proper" names (see p. 282 and Section IX). And he appears to believe that the examples of the referential uses mentioned above are inexplicable on Russell's theory. It is these views that I wish to examine.

(2.) *Some alleged applications of the distinction*

Some alleged applications of Donnellan's distinction have entered the oral tradition, and even to an extent, the written tradition, that are not in Donnellan's paper. I will mention some that I find questionable. Unfortunately I will have to discuss these applications more briefly than the issues in question really deserve, since they are ancillary to the main theme.

(2a.) *De dicto-de re*

Many able people, in and out of print, have implied that Donnellan's distinction has something to do with, can be identified with, or can replace, the *de dicto-de re* distinction, or the small scope-large scope distinction in modal or intensional contexts.

"The number of planets is necessarily odd" can mean two things, depending on whether it is interpreted *de dicto* or *de re*. If it is interpreted *de dicto*, it asserts that the proposition that the number of planets is odd is a necessary truth—something I take to be false (there might have been eight planets). If it is interpreted *de re*, it asserts that the actual number of planets (nine) has the property of necessary oddness (essentialists like me take this to be true). Similarly, if we say, "Jones believes that the richest debutante in Dubuque will marry him," we may mean that Jones's belief has a certain content, viz., that the richest debutante in Dubuque will marry him; or we may mean that he believes, *of a girl who is (in fact) the richest in Dubuque*, that she will marry him. The view in question suggests that the *de dicto* case is to be identified with Donnellan's *attributive* use, the *de re* with the *referential*.

Any such assimilation, in my opinion, is confused. (I don't think Donnellan makes it.) There are many objections; I will mention a few. First, the *de dicto* use of the definite description cannot be identified with either the *referential* or the *attributive* use. Here the basic point was already noticed by Frege. If a description is embedded in a (*de dicto*) intensional context, we cannot be said to be talking *about* the thing described, either *qua* its satisfaction of the description or *qua* anything else. Taken *de dicto*, "Jones believes that the richest debutante in Dubuque will marry him," can be asserted by someone who thinks (let us suppose, wrongly) that there are *no* debutantes in Dubuque; certainly then, he is in no way talking about the richest debutante, even "attributively." Similarly, "It is possible that (France should have a monarchy in 1976, and that) the king of France in 1976 should have been bald" is true, if read *de dicto*; yet we are not using "the king of France in 1976" attributively to speak of the king of France in 1976, for there is none. Frege concluded that "the king of France in 1976" refers, in these contexts, to its ordinary sense; at any rate, if we wish to speak of "reference" here, it cannot be to the non-existent king. Even if there were such a king, the quoted assertion would say nothing about *him*, if read *de dicto*: to say that *he* might have been bald, would be *de re* (indeed, this *is* the distinction in question).

Second, and even more relevantly, Donnellan's referential use cannot be identified with the *de re* use. (I think Donnellan would agree.) Suppose I have no idea how many planets there are, but (for some reason) astronomical theory dictates that that number must be odd. If I say, "The number of planets (whatever it may be) is odd," my description is used attributively. If I am an essentialist, I will also say, "The number of planets (whatever it may be) is necessarily odd," on the grounds that all odd numbers are necessarily odd; and my usage is just as attributive as in the first case. In "Smith's murderer, whoever he may be, is known to the police, but they're not saying," or, more explicitly, "The police know concerning Smith's murderer, whoever he is, that he

committed the murder; but they're not saying who he is," "Smith's murderer" is used attributively, but is *de re*.

Finally: Russell wished to handle the *de dicto-de re* distinction by his notion of the *scope* of a description. Some have suggested that Donnellan's referential-attributive distinction can replace Russell's distinction of scope. But *no* twofold distinction can do this job. Consider:

(2) The number of planets might have been necessarily even.

In a natural use, (2) can be interpreted as true: for example, there might have been exactly eight planets, in which case the number of planets would have been even, and hence necessarily even. (2), interpreted as true, is neither *de re* nor *de dicto*; that is, the definite description neither has the largest nor the smallest possible scope. Consider:

(2a)  $\diamond\Box(\exists x)$  (There are exactly  $x$  planets and  $x$  is even)

(2b)  $(\exists x)$  (There are exactly  $x$  planets and  $\diamond\Box(x$  is even)).

(2c)  $\diamond(\exists x)$  (There are exactly  $x$  planets and  $\Box(x$  is even)).

(2a)–(2c) give three alternative Russellian analyses of (2). (2a) gives the description the smallest possible scope (*de dicto*); it says, presumably falsely, that it might have been necessary that there was an even number of planets. (2b) gives the description the largest possible scope (*de re*); it says, still falsely, of the actual number of planets (*viz.*, nine) that it might have been necessarily even. (2c) is the interpretation which makes (2) true. When intensional operators are iterated, intermediate scopes are possible. Three analogous interpretations are possible, say, for "Jones doubts that Holmes believes that Smith's murderer is insane"; or (using an indefinite description) for "Hoover charged that the Berrigans plotted to kidnap a high American official." (I actually read something like this last in a newspaper and wondered what was meant.)<sup>6</sup> This may mean: (a) there is a particular high official such that Hoover charged that the Berrigans plotted to kidnap him (largest scope, *de re*, this was the interpretation intended); or (b) Hoover charged that the Berrigans plotted as follows: let's kidnap a high official (smallest scope, *de dicto*); or (c) Hoover charged that there was a high official (whose identity may have been unknown to Hoover) whom the Berrigans planned to kidnap (intermediate scope).

As intensional (or other) constructions are iterated, there are more and more possible scopes for a definite description. No *twofold* distinction can replace Russell's notion of scope.<sup>7</sup> In particular, neither the *de dicto-de re* distinction nor the referential-attributive distinction can do so.

(2b.) *Rigid definite descriptions.*

If definite descriptions,  $\iota x\phi(x)$ , are taken as primitive and assigned reference, then the conventional non-rigid assignment assigns to such a description, with respect to each possible world, the unique object, if any, which would have  $\phi$ 'd in that world. (Forget the vacuous case, which requires a further convention.) For example, "the number of planets" denotes eight, speaking of a counterfactual situation where there would have been eight planets (and "the number of planets is even" is true of such a situation). Another type of definite description,  $\iota x\phi x$ , a "rigid" definite description, could be introduced semantically by the following stipulation: let  $\iota x\phi x$  denote, with respect to all possible worlds, the unique object that (actually)  $\phi$ 's (then "the number of planets is odd," as interpreted, expresses a necessary truth). Both kinds of definite

descriptions can obviously be introduced, theoretically, into a single formal language, perhaps by the notations just given. Some have suggested that definite descriptions, in English, are *ambiguous* between the two readings. It has further been suggested that the two types of definite descriptions, the nonrigid and the rigid, are the source of the *de dicto-de re* distinction and should replace Russell's notion of scope for the purpose. Further, it has been suggested that they amount to the same thing as Donnellan's attributive-referential distinction.<sup>8</sup>

My comments will be brief, so as to avoid too much excursus. Although I have an open mind on the subject, I am not yet convinced that there is any clear evidence for such an ambiguity. Being a twofold distinction, the ambiguity alleged cannot replace Russell's notion of scope, for the reasons given above. Once Russell's notion is available, it can be used to handle the *de dicto-de re* distinction; a further ambiguity seems unnecessary. More relevantly to the present context, the "rigid" sense of a definite description, if it exists, cannot be identified with Donnellan's "referential" use. I take it that the identification of the referential use with the rigid definite description was inspired by some line of reasoning like this: Donnellan holds that referential descriptions are those close to proper names, even to Russell's "logically proper names." But surely proper names, or at least, Russellian "logically proper names," are rigid. Hence Donnellan's referential descriptions are just the rigid definite descriptions.

If we assume that Donnellan thinks of names as rigid, as I think of them, his referential definite descriptions *would* most plausibly be taken to refer rigidly to their referents. But it is not clear that he does agree with me on the rigidity of such reference.<sup>9</sup> More important, a rigid definite description, as defined above, still determines its referent via its unique satisfaction of the associated property—and this fact separates the notion of such a description from that of a referential description, as Donnellan defines it. David Kaplan has suggested that a demonstrative "that" can be used, in English, to make any definite description rigid. "That bastard—the man who killed Smith, whoever he may be—is surely insane!" The subject term rigidly designates Smith's murderer, but it is still attributive in Donnellan's sense.<sup>10</sup>

(2c.) In "Naming and Necessity,"<sup>11</sup> one argument I presented against the description (or cluster-of-descriptions) theory of proper names concerned cases where the referent of a name, the person named by the name, did not satisfy the descriptions usually associated with it, and someone else did. For example, the name "Gödel" might be taken to mean "the man who proved the incompleteness of arithmetic"; but even if Gödel had been a fraud, who had proved nothing at all and had misappropriated his work from an unknown named "Schmidt," our term "Gödel" would refer to the fraud, not to the man who really satisfied the definite description. Against this it has been said that although the argument does succeed in its main purpose of refuting the description theory as a theory of reference (that is, it shows that the descriptive properties cited do not determine the referent), it does nothing to show that names are not abbreviated definite descriptions, because we could take the descriptions in question to be referential in Donnellan's sense. Referential descriptions can easily refer to things that fail to satisfy the descriptions; nothing in my argument shows that names are not synonymous with such descriptions.<sup>12</sup>

My reaction to such an argument may become clearer later. For the moment, (too) briefly: In the case of "Her husband is kind to her," and similar cases, "her husband" can refer to her lover, as long as we are under the misapprehension that the man to whom we refer (the lover) is her husband. Once we are apprised of the true facts, we will no longer so refer to him (see, for example, pp. 300–301 of Donnellan's paper).

Similarly, someone can use "the man who proved the incompleteness of arithmetic," as a referential definite description, to refer to Gödel; it might be so used, for example, by someone who had forgotten his name. If the hypothetical fraud were discovered, however, the description is no longer usable as a device to refer to Gödel; henceforth it can be used only to refer to Schmidt. We would withdraw any previous assertions using the description to refer to Gödel (unless they also were true of Schmidt). We would *not* similarly withdraw the *name* "Gödel," even after the fraud was discovered; "Gödel" would still be used to name Gödel, not Schmidt. The name and the description, therefore, are not synonymous. (See also footnote 27 below.)

(3.) *The main problem.*

(3a.) *A disagreement with Russell?*

Do Donnellan's observations provide an argument against Russell's theory? Do his *views* contradict Russell's? One might think that if Donnellan is right, Russell must be wrong, since Donnellan's truth conditions for statements containing referential definite descriptions differ from Russell's. Unfortunately, this is not so clear. Consider the case of "Her husband is kind to her," mistakenly said of the lover. If Donnellan had roundly asserted that the quoted statement is true if and only if the *lover* is kind to her, regardless of the kindness of the husband, the issue between him and Russell would be clearly joined. But Donnellan doesn't say this: rather he says that the speaker has referred to a certain person, the lover, and said *of him* that he is kind to her. But if we ask, "Yes, but was the statement he made true?", Donnellan would hedge. For if *we* are not under the misimpression that the man the speaker referred to was her husband, *we* would not express the same assertion by "Her husband is kind to her." "If it ['her husband'] is being used referentially, it is not clear what is meant by 'the statement.' . . . To say that the statement he made was that her husband is kind to her lands us in difficulties. For we [in so reporting what the speaker said must use the definite description] either attributively or referentially. If the former, then we misrepresent the linguistic performance of the speaker; if the latter, then we ourselves are referring to someone," and ordinarily we can refer to someone as "her husband" only if we take him to be her husband.<sup>13</sup>

Since Donnellan does not clearly assert that the statement "her husband is kind to her" ever has non-Russelian truth conditions, he has *not*, so far, clearly contradicted Russell's theory. His argument, as he presents it, that there is a problem in reporting "the statement" is questionable, in two ways.

First, it uses the premise that if we say, "Jones said that her husband is kind to her," we ourselves must use the description attributively or referentially; but, as we saw, a definite description in indirect discourse is *neither* referential nor attributive.<sup>14</sup>

Second, there is an important problem about the nature of the referential-attributive distinction. Donnellan says that his distinction is neither syntactic nor semantic:

The grammatical structure of the sentence seems to me to be the same whether the description is used referentially or attributively: that is, it is not syntactically ambiguous. Nor does it seem at all attractive to suppose an ambiguity in the meaning of the words; it does not appear to be semantically ambiguous. (Perhaps we could say that the sentence is pragmatically ambiguous: the distinction between roles that the description plays is a function of the speaker's intentions.) These, of course, are intuitions; I do not have an argument for these conclusions. Nevertheless, the burden of proof is surely on the other side.<sup>15</sup>

Suppose for the moment that this is so. Then if the referential-attributive distinction is pragmatic, rather than syntactic or semantic, it is presumably a distinction about speech acts. There is no reason to suppose that in making an indirect discourse report on what someone else has said I myself must have similar intentions, or be engaged in the same kind of speech act; in fact, it is clear that I am not. If I say "Jones said the police were around the corner," Jones may have said it as a warning, but I need not say it as a warning. If the referential-attributive distinction is neither syntactic nor semantic, there is no reason, without further argument, to suppose that my usage, in indirect discourse, should match the man on whom I report, as referential or attributive. The case is quite different for a genuine semantic ambiguity. If Jones says, "I have never been at a bank," and I report this, saying, "Jones denied that he was ever at a bank," the sense I give to "bank" must match Jones's if my report is to be accurate.

Indeed, the passage seems inconsistent with the whole trend of Donnellan's paper. Donnellan suggests that there is no syntactic or semantic ambiguity in the statement, "Her husband is kind to her." He also suggests that Russell may well give a correct analysis of the attributive use but not of the referential use. Surely this is not coherent. It is not "uses," in some pragmatic sense, but *senses* of a sentence which can be analyzed. If the sentence is *not* (syntactically or) semantically ambiguous, it has only *one* analysis; to say that it has two distinct analyses is to attribute a syntactic or semantic ambiguity to it.

Donnellan's arguments for his refusal to give a truth value to the speaker's assertion, "Her husband is kind to her," seem to be fallacious. My own suggested account of the matter below—in terms of a theory of speech acts—creates no problem about "the statement"; it is simply the statement that her husband is kind to her. But Donnellan's cautious refusal to say, under the circumstances mentioned, that "Her husband is kind to her" is true, seems nevertheless to be intuitively correct. The man to whom the speaker refers is—let us suppose—kind to her. But it seems hard for us to say that when he uttered, "Her husband is kind to her," it expressed a truth, if *we* believe that her husband is unkind to her.

Now Donnellan thinks that he has refuted Russell. But all he has clearly claimed, let alone established, is that a speaker can refer to the lover and say, of him, that he is kind to her by saying "Her husband is kind to her." So, first, we can ask: *If* this claim is correct, does it conflict with Russell's views?

Second, since Donnellan's denial that he advocates a semantic ambiguity in definite descriptions seems inconsistent with much of his paper, we can try ignoring the denial, and take his paper to be arguing for such an ambiguity. Then we may ask: has Donnellan established a (semantic) ambiguity inconsistent with Russell's theory?

(3b.) *General remarks: apparatus.*

We need a general apparatus to discuss these questions. Some of the apparatus is well known, but I review it for its intrinsic importance and interest. First, let us distinguish, following Grice,<sup>16</sup> between what *the speaker's words meant*, on a given occasion, and what *he meant*, in saying these words, on that occasion. For example, one burglar says to another, "The cops are around the corner." What *the words meant* is clear: the police were around the corner. But *the speaker may well have meant*, "We can't wait around collecting any more loot: Let's split!" That is not *the meaning of the words*, even on that occasion, though that is *what he meant in saying those words, on that occasion*. Suppose he had said, "The cops are inside the bank." Then on that occasion, "bank" meant a commercial bank, not a river bank, and this is relevant to what the



*words* meant, on that occasion. (On other occasions, the same words might mean that the police were at a river bank.) But, if the speaker *meant* "Let's split," this is no part of the *meaning of his words*, even on that occasion.

Again (inspired by an example of Grice)<sup>17</sup>: A magician makes a handkerchief change color. Someone says, recalling the trick, "Then he put the red handkerchief on the side of the table"; and someone else interjects, cautiously, "It *looked* red." The words meant, on that occasion, that the object referred to (the handkerchief) looked red. What we speak of when we speak of the meaning of his words, on that occasion, includes a disambiguation of the utterance. (Perhaps, on some occasions, where "it" refers to a book, a phonetically identical utterance might mean, "it looked read," well-thumbed and well-perused). But the speaker meant, on this occasion, to suggest that perhaps the handkerchief wasn't really red, that perhaps the trick relied on some kind of illusion. (Note that, on this occasion, not only do the *words* "it looked red" mean what they mean, but also the *speaker* means that it looked red, as well as that it may not have been red. On the other hand, the speaker has no intention of producing a belief in the hearer that the handkerchief looked red, or a belief in the hearer that he (the speaker) believed it looked red. Both facts are common knowledge. The same *could* hold for "The cops are around the corner."<sup>18</sup> Do these examples contradict Grice's analysis of "meaning"? Grice's theory has become very complex and I am not quite sure.)

The notion of what words can mean, in the language, is semantical: it is given by the conventions of our language. What they mean, on a given occasion, is determined, on a given occasion, by these conventions, together with the intentions of the speaker and various contextual features. Finally what the speaker meant, on a given occasion, in saying certain words, derives from various further special intentions of the speaker, together with various general principles, applicable to all human languages regardless of their special conventions. (Cf. Grice's "conversational maxims.") For example, "It looks red" replaced a categorical affirmation of redness. A plausible general principle of human discourse would have it that if a second speaker insists that a stronger assertion should be replaced by a weaker one, he thereby wishes to cast doubt on the stronger assertion; whence, knowing the semantics of English, and the meaning of the speaker's words on this occasion, we can deduce what was meant (the Gricean "conversational implicature").<sup>19</sup>

Let us now speak of speaker's reference and semantic reference: these notions are special cases of the Gricean notions discussed above. If a speaker has a designator in his idiolect, certain conventions of his idiolect<sup>20</sup> (given various facts about the world) determine the referent in the idiolect: that I call the *semantic referent* of the designator. (If the designator is ambiguous, or contains indexicals, demonstratives, or the like, we must speak of the semantic referent on a given occasion. The referent will be determined by the conventions of the language plus the speaker's intentions and various contextual features.)

Speaker's reference is a more difficult notion. Consider, for example, the following case, which I have mentioned elsewhere.<sup>21</sup> Two people see Smith in the distance and mistake him for Jones. They have a brief colloquy: "What is Jones doing?" "Raking the leaves." "Jones," in the common language of both, is a name of Jones; it *never* names Smith. Yet, in some sense, on this occasion, clearly both participants in the dialogue have referred to Smith, and the second participant has said something true about the man he referred to if and only if Smith was raking the leaves (whether or not Jones was). How can we account for this? Suppose a speaker takes it that a certain object *a* fulfills the conditions for being the semantic referent of a designator, "*d*." Then, wishing to say

something about  $a$ , he uses " $d$ " to speak about  $a$ ; say, he says " $\phi(d)$ ." Then, he said, of  $a$ , on that occasion, that it  $\phi$ 'd; in the appropriate Gricean sense (explicated above), he *meant* that  $a$   $\phi$ 'd. This is true even if  $a$  is not really the semantic referent of " $d$ ." If it is not, then *that*  $a$   $\phi$ 's is included in what he meant (on that occasion), but not in the meaning of his words (on that occasion).

So, we may tentatively define the speaker's referent of a designator to be that object which the speaker wishes to talk about, on a given occasion, and believes fulfills the conditions for being the semantic referent of the designator. He uses the designator with the intention of making an assertion about the object in question (which may not really be the semantic referent, if the speaker's belief that it fulfills the appropriate semantic conditions is in error). The speaker's referent is the thing the speaker referred to by the designator, though it may not be the referent of the designator, in his idiolect. In the example above, Jones, the man named by the name, is the semantic referent. Smith is the speaker's referent, the correct answer to the question, "To whom were you referring?"<sup>22</sup>

Below, the notion of speaker's reference will be extended to include more cases where existential quantification rather than designation is involved.

In a given idiolect, the semantic referent of a designator (without indexicals) is given by a *general* intention of the speaker to refer to a certain object whenever the designator is used. The speaker's referent is given by a *specific* intention, on a given occasion, to refer to a certain object. If the speaker believes that the object he wants to talk about, on a given occasion, fulfills the conditions for being the semantic referent, then he believes that there is no clash between his general intentions and his specific intentions. My hypothesis is that Donnellan's referential-attributive distinction should be generalized in this light. For the speaker, on a given occasion, may believe that his specific intention coincides with his general intention for one of two reasons. In one case (the "simple" case), his specific intention is simply to refer to the semantic referent: that is, his specific intention is simply his general semantic intention. (For example, he uses "Jones" as a name of Jones—elaborate this according to your favorite theory of proper names—and, on this occasion, simply wishes to use "Jones" to refer to Jones.) Alternatively—the "complex" case—he has a specific intention, which is distinct from his general intention, but which he believes, as a matter of fact, to determine the same object as the one determined by his general intention. (For example, he wishes to refer to the man "over there" but believes that he *is* Jones.) In the "simple" case, the speaker's referent is, *by definition*, the semantic referent. In the "complex" case, they may coincide, if the speaker's belief is correct, but they need not. (The man "over there" may be Smith and not Jones.) To anticipate, my hypothesis will be that Donnellan's "attributive" use is nothing but the "simple" case, specialized to definite descriptions, and that the "referential" use is, similarly, the "complex" case. If such a conjecture is correct, it would be wrong to take Donnellan's "referential" use, as he does, to be a use of a description as if it were a proper name. For the distinction of simple and complex cases will apply to proper names just as much as to definite descriptions.

(3c). *Donnellan's argument against Russell: methodological and substantive considerations.*

In the light of the notions just developed, consider the argument Donnellan adduces against Russell. Donnellan points to a phenomenon which he alleges to be inexplicable on a Russellian account of English definite descriptions. He accounts for it by positing

an ambiguity. Alternatively, we wish to account for the phenomenon on pragmatic grounds, encapsulated in the distinction between speaker's reference and semantic reference. How can we see whether Donnellan's phenomenon conflicts with a Russellian account?

I propose the following test for any alleged counterexample to a linguistic proposal: If someone alleges that a certain linguistic phenomenon in English is a counterexample to a given analysis, consider a hypothetical language which (as much as possible) is like English except that the analysis is *stipulated* to be correct. Imagine such a hypothetical language introduced into a community and spoken by it. *If the phenomenon in question would still arise in a community that spoke such a hypothetical language (which may not be English), then the fact that it arises in English cannot disprove the hypothesis that the analysis is correct for English.* An example removed from the present discussion: Some have alleged that identity cannot be the relation that holds between, and only between, each thing and itself, for if so, the nontriviality of identity statements would be inexplicable. If it is conceded, however, that such a relation makes sense, and if it can be shown that a hypothetical language involving such a relation would generate the same problems, it will follow that the existence of these problems does not refute the hypothesis that "identical to" stands for this same relation in English.<sup>23</sup>

By "the weak Russell language," I will mean a language similar to English except that the truth conditions of sentences with definite descriptions are *stipulated* to coincide with Russell's: for example, "The present King of France is bald" is to be true iff exactly one person is king of France, and that person is bald. On the weak Russell language, this effect can be achieved by assigning semantic reference to definite descriptions: the semantic referent of a definite description is the unique object that satisfies the description, if any; otherwise there is no semantic referent. A sentence of the simple subject-predicate form will be true if the predicate is true of the (semantic) referent of its subject; false, if either the subject has no semantic referent or the predicate is not true of the semantic referent of the subject.

Since the weak Russell language takes definite descriptions to be primitive designators, it is not fully Russellian. By "the intermediate Russell language," I mean a language in which sentences containing definite descriptions are taken to be abbreviations or paraphrases of their Russellian analyses: for example, "The present king of France is bald" *means* (or has a "deep structure" like) "Exactly one person is at present king of France, and he is bald," or the like. Descriptions are not terms, and are not assigned reference or meaning in isolation. The "strong Russell language" goes further: definite descriptions are actually *banned* from the language and Russellian paraphrases are used in their place. Instead of saying "Her husband is kind to her," a speaker of this language must say "Exactly one man is married to her, and he is kind to her," or even (better), "There is a unique man who is married to her, and every man who is married to her is kind to her," or the like. If Russell is right, long-windedness is the only defect of these versions.

Would the phenomenon Donnellan adduces arise in communities that spoke these languages? Surely speakers of these languages are no more infallible than we. They too will find themselves at a party and mistakenly think someone is drinking champagne even though he is actually drinking sparkling water. If they are speakers of the weak or intermediate Russell languages, they will say, "The man in the corner drinking champagne is happy tonight." They will say this precisely because *they think, though erroneously, that the Russellian truth conditions are satisfied.* Wouldn't we say of these

speakers that they are referring to the teetotaler, under the misimpression that he is drinking champagne? And, if he is happy, are they not saying of him, *truly*, that he is happy? Both answers seem obviously affirmative.

In the case of the weak Russell language, the general apparatus previously developed seems fully adequate to account for the phenomenon. The semantic referent of a definite description is given by the conditions laid down above: it is a matter of the specific conventions of the (weak) Russell language, in this case that the referent is the unique object satisfying the descriptive conditions. The speaker's referent, on the other hand, is determined by a general theory of speech acts, applicable to all languages: it is the object to which the speaker wishes to refer, and which he believes fulfills the Russellian conditions for being the semantic referent. Again, in asserting the sentence he does, the speaker means that the speaker's referent (the teetotaler) satisfied the predicate (is happy). Thus the rough theoretical apparatus above accounts fully for our intuitions about this case.

What about the other Russellian languages? Even in the strong Russell language, where explicit descriptions are outlawed, the same phenomena can occur. In fact, they occur in English in "arch" uses of existential quantification: "Exactly *one person* (or: *some person* or other) is drinking champagne in that corner, and I hear he is romantically linked with Jane Smith." The circumlocution, in English, expresses the delicacy of the topic, but the speaker's reference (in quite an ordinary sense) may well be clear, even if he in fact is drinking sparkling water. In English such circumlocutions are common only when the speaker wishes to achieve a rather arch and prissy effect, but in the strong Russell language (which of course isn't English), they would be made more common because the definite article is prohibited.

This example leads to an extension of the notion of speaker's reference. When a speaker asserts an existential quantification,  $(\exists x)(\phi x \wedge \psi x)$ , it may be clear which thing he has in mind as satisfying " $\phi x$ ," and he may wish to convey to his hearers that that thing satisfies " $\psi x$ ." In this case, the thing in question (which may or may not actually satisfy " $\phi x$ ") is called the "speaker's referent" when he makes the existential assertion. In English, as I have mentioned, such cases ("arch" uses) are rather rare; but they can be carried off even if the existential quantification is expressed in a highly roundabout and apparently nonreferring fashion. "Not *everyone* in this room is abstaining from champagne, and any such nonabstainer. . . ." <sup>24</sup>

If the notion of speaker's reference applies to the strong Russell language, it can apply to the intermediate Russell language as well, since the speaker's referent of " $\psi(\iota x \phi(x))$ " is then the thing he has in mind as uniquely instantiating " $\phi(x)$ " and about which he wishes to convey that it  $\psi$ 's.

Since the phenomenon Donnellan cites *would* arise in all the Russell languages, if they *were* spoken, the fact that they *do* arise in English, as *actually* spoken, can be no argument that English is not a Russell language.

We may contrast the Russell languages with what may be called the D-languages. In the D-languages the apparent ambiguity between referential and attributive definite descriptions is explicitly built into the semantics of the language and affects truth conditions. (The D-languages are meant to suggest "Donnellan," but are not called the "Donnellan languages," since Donnellan, as we have seen, is "ambiguous" as to whether he posits a semantic ambiguity.) The *unambiguous D-language* contains two distinct words, "the" and "ze" (rhymes with "the"). A statement of the form ". . . the *F* . . ." is true iff the predicate represented by the dots is true of the unique object fulfilling *F* (we need not specify what happens if there is no such thing; if we wish to

follow Russell, take it to be false). A statement of the form “. . . ze F . . .” is to be true iff the predicate represented by the dots is true of the unique thing the speaker thinks F is true of. (Once again, we leave free what happens if there is no such thing.) *The ambiguous D-language* is like the unambiguous D-language except that “the,” ambiguously, can be interpreted according to the semantics either of “the” or of “ze.” The general impression conveyed by Donnellan’s paper, in spite of his statement at one point to the contrary, is that English is the ambiguous D-language; only on such a hypothesis could we say that the “referential use” (really, referential *sense*) diverges from Russell’s theory. The truth-conditions of statements containing “ze,” and therefore of one sense of “the” in the ambiguous D-language, are incompatible with Russell’s theory.<sup>25</sup>

We have two hypotheses: one says that English is a Russell language, while the other says that English is the ambiguous D-language. Which hypothesis is preferable? Since, as we have argued, the phenomena Donnellan adduces would arise in a hypothetical society that spoke any of the Russell languages, the existence in English of such phenomena provides no argument against the hypothesis that English is a Russell language. If Donnellan had possessed a clear intuition that “Her husband is kind to her,” uttered in reference to the kind lover of a woman married to a cruel husband, expressed the literal truth, then he *would* have adduced a phenomenon that conforms to the ambiguous D-language but is incompatible with any Russell language. But Donnellan makes no such assertion: he cautiously, and correctly, confines himself to the weaker claim that the speaker spoke truly of the man to whom he referred. This weaker claim, we have seen, *would* hold for a speaker of a Russell language.

So Donnellan’s examples provide, in themselves, no evidence that English is the ambiguous D-language rather than a Russell language. Granting that this is so, we can ask whether there is any reason to favor the Russell language hypothesis over the D-language hypothesis. I think there are several general methodological considerations that are relevant.

The Russell language theory, or any other unitary account (that is, any account that postulates no semantic ambiguity), accounts for Donnellan’s referential-attributive phenomenon by a general pragmatic theory of speech acts, applicable to a very wide range of languages; the D-language hypothesis accounts for these same phenomena by positing a semantic ambiguity. The unitary account appeals to a general apparatus that applies to cases, such as the “Smith-Jones” case, where it is completely implausible that a semantic ambiguity exists. According to the unitary account, far from the referential use constituting a special namelike use of definite descriptions, the referential-attributive distinction is simply a special case of a general distinction, applicable to proper names as well as to definite descriptions, and illustrated in practice by the (leaf-raking) Smith-Jones case. And anyone who compares the Smith-Jones case, where presumably no one is tempted to posit a special semantic ambiguity, with Donnellan’s cases of definite descriptions, must surely be impressed by the similarity of the phenomena.<sup>26</sup>

Under these circumstances, surely general methodological principles favor the existing account. The apparatus of speaker’s reference and semantic reference, and of simple and complex uses of designators, is needed *anyway*, to explain the Smith-Jones case; it is applicable to all languages.<sup>27</sup> Why posit a semantic ambiguity when it is both insufficient in general and superfluous for the special case it seeks to explain?<sup>28</sup> And why are the phenomena regarding proper names so similar to those for definite descriptions, if the one case involves no semantic ambiguity while the other does?

It is very much the lazy man's approach in philosophy to posit ambiguities when in trouble. If we face a putative counterexample to our favorite philosophical thesis, it is always open to us to protest that some key term is being used in a special sense, different from its use in the thesis. We may be right, but the ease of the move should counsel a policy of caution: Do not posit an ambiguity unless you are really forced to, unless there are really compelling theoretical or intuitive grounds to suppose that an ambiguity really is present.

Let me say a bit more in defense of this. Many philosophers, for example, have advocated a "strong" account of knowledge according to which it is very hard to know anything; stiff requirements must be satisfied. When such philosophers have been confronted with intuitive counterexamples to such strong requirements for knowledge they either have condemned them as popular and loose usages or they have asserted that "know" is being used in a different "weak" sense. The latter move—distinguishing two or more "strong" and "weak" senses of "know"—strikes me as implausible. There *are* different senses of "know," distinguished in German as "kennen" and "wissen," and in French as "connaître" and "savoir"; a person is usually known in the one sense, a fact in the other. It is no surprise that other languages use distinct words for these various senses of "know"; there is no reason for the ambiguity to be preserved in languages unrelated to our own. But what about the uses of "know" that characteristically are followed by that-clauses, knowing that *p*? Are these ambiguous? I would be very surprised to be told that the Eskimos have two separate words, one for (say) Hintikka's "strong" sense of "know," another for his "weak" sense. Perhaps this indicates that we think of knowledge as a unitary concept, unlikely to be "disambiguated" by two separate words in any language.

We thus have two methodological considerations that can be used to test any alleged ambiguity. "Bank" is ambiguous; we would expect the ambiguity to be disambiguated by separate and unrelated words in some other languages. Why should the two separate senses be reproduced in languages unrelated to English? First, then, we can consult our linguistic intuitions, independently of any empirical investigation. Would we be surprised to find languages that used two separate words for the two alleged senses of a given word? If so, then, to that extent our linguistic intuitions are really intuitions of a unitary concept, rather than of a word that expresses two distinct and unrelated senses. Second, we can ask empirically whether languages are in fact found that contain distinct words expressing the allegedly distinct senses. If no such language is found, once again this is evidence that a unitary account of the word or phrase in question should be sought.

As far as our main question is concerned, the first of these two tests, that of our intuitive expectation, seems to me overwhelmingly to favor a unitary account of descriptions, as opposed to the ambiguity postulated in the ambiguous D-language. If English really is the ambiguous D-language, we should expect to find other languages where the referential and attributive uses are expressed by two separate words, as in the *unambiguous* D-language. I at least would find it quite surprising to learn that say, the Eskimo, used two separate words "the" and "ze," for the attributive and referential uses. To the extent that I have this intuition, to that extent I think of "the" as a unitary concept. I should have liked to be able to report that I have reinforced this guess by an actual empirical examination of other languages—the second test—but as of now I haven't done so.<sup>29</sup>

Several general methodological considerations favor the Russell language (or some other unitary account) against the ambiguous D-language as a model for English. First,

the unitary account conforms to considerations of economy in that it does not “multiply senses beyond necessity.” Second, the metalinguistic apparatus invoked by the unitary account to explain the referential-attributive distinction is an apparatus that is needed in *any case* for other cases, such as proper names. The separate referential sense of descriptions postulated by the D-language hypothesis, is an idle wheel that does no work: if it were absent, we would be able to express everything we wished to express, in the same way. Further, the resemblance between the case of descriptions and that of proper names (where presumably no one would be tempted to postulate an ambiguity) is so close that any attempt to explain the cases differently is automatically suspect. Finally, we would not expect the alleged ambiguity to be disambiguated in other languages, and this means we probably regard ourselves as possessing a unitary concept.

Aside from methodological considerations, is there any direct evidence that would favor one of our two rival accounts? As I remarked above, if we had a direct intuition that “Her husband is kind to her” could be true even when her actual husband is cruel, then we would have decisive evidence for the D-language model; but Donnellan rightly disclaims any such intuition. On the other hand, I myself feel that such a sentence expresses a falsehood, even when “her husband” is used referentially to refer to a kind man; but the popularity of Donnellan’s view has made me uncertain that this intuition should be pressed very far. In the absence of such direct intuitions that would settle the matter conclusively, it would seem that the actual practice of English speakers is compatible with either model, and that only general methodological considerations favor one hypothesis rather than another. Such a situation leaves me uneasy. If there really is no direct evidence to distinguish the two hypotheses, how are they different hypotheses? If two communities, one of whom spoke the ambiguous D-language and the other of whom spoke the (weak) Russell language, would be able to intermingle freely without detecting any linguistic difference, do they really speak two different languages? If so, wherein is the difference?

Two hypothetical communities, one of which was explicitly taught the ambiguous D-language and the other of which was taught the (weak) Russell language (say, in school), would have direct and differing intuitions about the truth-value of “Her husband was kind to her”; but it is uncertain whether English speakers have any such intuitions. If they have none, is this a respect in which English differs from both the Russell languages and the D-languages, and thus differentiates it from both? Or, on the contrary, is there a pragmatic consideration, deriving no doubt from the fact that the relevant rules of language are not explicitly taught, that will explain why we lack such intuitions (if we do) without showing that neither the D-language nor the Russell language is English?

Some commentators on the dispute between Russell and Frege and Strawson over sentences containing vacuous definite descriptions have held that no direct linguistic phenomena conclusively decide between the two views: we should therefore choose the most economical and theoretically satisfying model. But if this is so, are there really two views, and if there are, shouldn’t we perhaps say that neither is correct? A hypothetical community that was explicitly taught Russellian or Frege-Strawsonian truth-conditions for sentences containing vacuous definite descriptions would have no difficulty producing direct intuitions that decide the Russell-Strawson dispute. If the commentators in question are correct, speakers of English have no such intuitions. Surely this fact, too, would be a significant fact about English, for which linguistic theory should give an account. Perhaps pragmatic considerations suffice for such an

account; or, perhaps, the alleged lack of any such intuition must be accounted for by a feature built into the semantics of English itself. In the latter case, neither the Russellian nor the Frege-Strawsonian truth-conditions would be appropriate for English. Similar considerations would apply to the issue between Donnellan and Russell.<sup>30</sup>

I am uncertain about these questions. Certainly it would be best if there were directly observable phenomena that differentiated between the two hypotheses. Actually I can think of one rather special and localized phenomenon that may indeed favor the Russellian hypothesis, or some other unitary hypothesis. Consider the following two dialogues:

Dialogue I: A. "Her husband is kind to her."  
B. "No, he isn't. The man you're referring to isn't her husband."

Dialogue II: A. "Her husband is kind to her."  
B. "He is kind to her, but he isn't her husband."

In the first dialogue the respondent (B) uses "he" to refer to the semantic referent of "her husband" as used by the first speaker (A); in the second dialogue the respondent uses "he" to refer to the speaker's referent. My tendency is to think that both dialogues are proper. The unitary account can explain this fact, by saying that pronominalization can pick up *either* a previous semantic reference or a previous speaker's reference.<sup>31,32</sup> In the case of the two contrasting dialogues, these diverge.

If English were the ambiguous D-language, the second dialogue would be easy to explain. "He" refers to the object that is both the semantic referent and the speaker's referent of "her husband." (Recall that the notions of speaker's reference and semantic reference are general notions applicable to all languages, even to the D-languages.<sup>33</sup>) The first dialogue, however, would be much more difficult, perhaps impossible, to explain. When A said "her husband," according to the D-language hypothesis he was using "her husband" in the referential sense. Both the speaker's referent and the semantic referent would be the kind lover; only if B had misunderstood A's use as attributive could he have used "he" to refer to the husband, but such a misunderstanding is excluded by the second part of B's utterance. If the first dialogue is proper, it seems hard to fit it into the D-language model.<sup>34</sup>

#### (4.) Conclusion

I said at the beginning that the main concern of this paper was methodological rather than substantive. I do think that the considerations in this paper make it overwhelmingly probable that an ultimate account of the phenomena behind Donnellan's distinction will make use of the pragmatic ambiguity between "simple" and "complex" uses, as I defined them above, rather than postulating an ambiguity of the D-language type. But any ultimate substantive conclusion on the issue requires a more extensive and thorough treatment than has been given here. First, I have not here examined theories that attempt to explain Donnellan's distinction as a *syntactic* ambiguity, either of scope or of restrictive and non-restrictive clauses in deep structure.<sup>35</sup> Both these views, like the line suggested in the present paper, are compatible with a unitary hypothesis such as the hypothesis that English is a Russell language. Although I am not inclined to accept either of these views, some others have found them plausible and unless they are rebutted, they too indicate that Donnellan's observations cannot be taken as providing a conclusive argument against Russell without further discussion.



Second, and most important, no treatment of definite descriptions can be complete unless it examines the complete range of uses of the definite article and related linguistic phenomena. Such a treatment should attempt, as I have argued above, to make it clear why the same construction with a definite article is used for a wide range of cases. It would be wrong for me not to mention the phenomena most favorable to Donnellan's intuitions. In a demonstrative use such as "that table," it seems plausible, as I have mentioned above,<sup>36</sup> that the term rigidly designates its referent. It also seems plausible that the reference of such a demonstrative construction can be an object to which the descriptive adjectives in the construction do not apply (for example, "that scoundrel" may be used to refer to someone who is not, in fact, a scoundrel) and it is not clear that the distinction between speaker's reference and semantic reference should be invoked to account for this. As I also said above, it seems to me to be likely that "indefinite" definite descriptions<sup>37</sup> such as "the table" present difficulties for a Russellian analysis. It is somewhat tempting to assimilate such descriptions to the corresponding demonstratives (for example, "that table") and to the extent that such a temptation turns out to be plausible, there may be new arguments in such cases for the intuitions of those who have advocated a rigid vs. non-rigid ambiguity in definite descriptions, or for Donnellan's intuitions concerning the referential case, or for both.<sup>38</sup>

Because I have not yet worked out a complete account that satisfies me, and because I think it would be wrong to make any definitive claim on the basis of the restricted class of phenomena considered here, I regard the primary lessons of this paper as methodological. They illustrate some general methodological considerations and apparatus that I think should be applied to the problems discussed here and to other linguistic problems. They show in the present case that the argument Donnellan actually presents in his original paper shows nothing against a Russellian or other unitary account, and they make it highly probable to me that the problems Donnellan handles by semantic ambiguity should instead be treated by a general theory of speech acts. But at this time nothing more definitive can be said. I think that the distinction between semantic reference and speaker's reference will be of importance not only (as in the present paper) as a critical tool to block postulation of unwarranted ambiguities, but also will be of considerable constructive importance for a theory of language. In particular, I find it plausible that a diachronic account of the evolution of language is likely to suggest that what was originally a mere speaker's reference may, if it becomes habitual in a community, evolve into a semantic reference. And this consideration may be *one* of the factors needed to clear up some puzzles in the theory of reference.<sup>39,40,41</sup>

#### FOOTNOTES

<sup>1</sup> Versions of this paper—not read from the present manuscript—were given from 1971 onward to colloquia at New York University, M.I.T., the University of California (Los Angeles), and elsewhere. The present version was written on the basis of a transcript of the M.I.T. version prepared by the editors of this volume. Donnellan himself heard the talk at U.C.L.A., and he has a forthcoming paper, "Speaker Reference, Descriptions and Anaphora," that to a large extent appears to be a comment on considerations of the type mentioned here. (He does not, however, specifically refer to the present paper.) I decided *not* to alter the paper I gave in talks to take Donnellan's later views into account: largely I think the earlier version stands on its own, and the issues Donnellan raises in the later paper can be discussed elsewhere. Something should be said here, however, about the pronominalization phenomena mentioned on p. 270 below. In his forthcoming paper, Donnellan seems to think that these phenomena are incompatible with the suggestion that speaker's reference is a pragmatic notion. On the contrary, at the end of the present paper (and of

the talk Donnellan heard), I emphasize these very phenomena and argue that they support this suggestion. See also footnote 31 below.

<sup>2</sup> *The Philosophical Review* 75 (1966): 281–304. See also Keith S. Donnellan, "Putting Humpty Dumpty Together Again," *The Philosophical Review* 77 (1968): 203–215.

<sup>3</sup> In his later paper mentioned above in footnote 1, Donnellan seems more clearly to advocate a semantic ambiguity; but he hedges a bit even in the later paper.

<sup>4</sup> I will also avoid cases of "improper" descriptions, where the uniqueness condition fails. Such descriptions may or may not be important for an ultimate evaluation of Donnellan's position, but none of the arguments in his paper rest on them.

<sup>5</sup> "Reference and Definite Descriptions," p. 285. My discussion in this paragraph and the next is based on Donnellan's paper, pp. 285, 289–90.

<sup>6</sup> At the time, it had not yet been revealed that Kissinger was the official in question.

<sup>7</sup> In fact, no  $n$ -fold distinction can do so, for any fixed  $n$ . Independently of the present writer, L. Karttunen has argued similarly that no dual or  $n$ -fold distinction can replace scope distinctions. I discussed the matter briefly in "Identity and Necessity," *Identity and Individuation*, ed. M. Munitz (New York, 1972), p. 149, n. 10.

<sup>8</sup> See the papers of Stalnaker and Partee in *The Semantics of Natural Language*, eds. D. Davidson and G. Harman (Dordrecht, 1971) for such suggestions and also for some of the views mentioned in the previous section. I should emphasize that most of the stimulating discussion in these papers can be made independent of any of the identifications of Donnellan's distinction with others which are rejected here.

<sup>9</sup> See his paper "The Contingent *A Priori* and Rigid Designators," this volume, pp. 12–27. In that paper, Donnellan asks whether I think proper names (in natural language) are *always* rigid: obviously, he thinks, proper names *could* be introduced to abbreviate nonrigid definite descriptions. My view is that proper names (except perhaps, for some quirky and derivative uses, that are not uses as *names*) are always rigid. In particular this applies to "Neptune." It would be logically possible to have single words that abbreviated nonrigid definite descriptions, but these would not be *names*. The point is not merely terminological: I mean that such abbreviated nonrigid definite descriptions would differ in an important semantical feature from (what we call) typical proper names in our actual speech. I merely state my position and do not argue it; nor can I digress to comment on the other points raised in Donnellan's paper in this volume.

<sup>10</sup> See Kaplan's (unpublished) paper "Dthat." In that paper, however, he also has some tendency to confuse rigidity with Donnellan's referentiality.

<sup>11</sup> In the Davidson-Harman volume mentioned in footnote 8.

<sup>12</sup> For this view, see Jerrold J. Katz, "Logic and Language: An Examination of Recent Criticisms of Intensionalism," in *Minnesota Studies in the Philosophy of Science*, vol. VII (Minneapolis, 1975), pp. 36–130. See especially sections 5.1 and 5.2. As far as proper names are concerned, Katz thinks that *other* arguments tell against the description theory even as a theory of meaning.

<sup>13</sup> See Donnellan, "Reference and Definite Descriptions," p. 302.

<sup>14</sup> So I argued in the talks, and rightly, if Donnellan is taken literally. See footnote 25 below, however, for a more charitable reading, which probably corresponds to Donnellan's intent. We must, however, take descriptions to be *semantically* ambiguous if we are to maintain the reading in question: see the point raised immediately after this one.

<sup>15</sup> "Reference and Definite Descriptions," p. 297.

<sup>16</sup> For Grice, see the following papers, which I follow loosely in a good deal of the discussion at the beginning of this section: "The Causal Theory of Perception," *Proceedings of the Aristotelian Society*, supplementary vol. 35 (1961); "Logic and Conversation" (unpublished lectures); "Meaning," *Philosophical Review* 66 (1957): 377–88; "Utterer's Meaning, Sentence-Meaning and Word-Meaning," *Foundations of Language* 4 (1968): 225–42; "Utterer's Meaning and Intentions," *Philosophical Review* 78 (1969): 147–77.

<sup>17</sup> In "The Causal Theory of Perception."

<sup>18</sup> Suppose the second burglar is well aware of the proximity of the police, but procrastinates in his greed for more loot. Then the first burglar inparts no *information* by saying what he does, but simply urges the second burglar to “split.”

<sup>19</sup> Although conversational principles are applicable to *all languages*, they may apply differently to *different societies*. In a society where blunt statement was considered rude, where “it looks red” replaced “it is red” just because of such a custom, “it looks red” might carry different conversational implicatures from our own. This might be the case even though the members of the society spoke *English*, just as we do. Conversational principles are matters for the psychology, sociology, and anthropology of linguistic communities; they are applicable to these communities no matter what language they may speak, though the applicable principles may vary somewhat with the communities (and may even, to some extent, be conditioned by the fact that they speak languages with certain structures.) Often, of course, we can state widely applicable, “cross-cultural,” general conversational principles. Semantic and syntactic principles, on the other hand, are matters of the conventions of a language, in whatever cultural matrix it may be spoken. *Perhaps* sometimes it is difficult to draw the line, but it exists in general nonetheless.

<sup>20</sup> If the views about proper names I have advocated in “Naming and Necessity” are correct (Donnellan, in fact, holds similar views), the conventions regarding names in an idiolect usually involve the fact that the idiolect is no mere idiolect, but part of a common language, in which reference may be passed from link to link.

As the present paper attests, my views on proper names in “Naming and Necessity” have no special connection with the referential-attributive distinction.

<sup>21</sup> “Naming and Necessity,” p. 343, n. 3.

<sup>22</sup> Donnellan shows in his paper that there are “referential” uses, of a somewhat exceptional kind, where the speaker, or even both the speaker and the hearer, are aware that the description used does not apply to the thing they are talking about. For example, they use “the king,” knowing him to be a usurper, but fearing the secret police. Analogous cases can be given for proper names: if Smith is a lunatic who thinks he is Napoleon, they may humor him. Largely for the sake of simplicity of exposition, I have excluded such both from the notion of speaker’s reference and from Donnellan’s “referential” use (and the “D-languages” below). I do not think that the situation would be materially altered if both notions were revised so as to admit these cases, in a more refined analysis. In particular, it would probably *weaken* the case for a semantic ambiguity if these cases were allowed: for they shade into ironical and “inverted commas” cases. “He is a ‘fine friend,’” may be ironical (whether or not inverted commas are used in the transcription). “‘The king’ is still in power”; “‘Napoleon’ has gone to bed” are similar, whether or not explicit inverted commas are used. It is fairly clear that “fine friend,” “brilliant scholar,” etc., do not have ironical and inverted commas *senses*: irony is a certain form of speech act, to be accounted for by pragmatic considerations. The case for a semantic ambiguity in definite descriptions is similarly *weakened* if we include such cases as referential uses.

In ordinary discourse, we say that the speaker was referring to someone under a wide variety of circumstances, including linguistic errors, verbal slips, and deliberate misuses of language. (If Mrs. Malaprop says, “The geography teacher said that equilateral triangles are equiangular,” she *refers* to the geometry teacher.) The more such phenomena one includes in the notion of speaker’s reference, the further one gets from any connection of the notion with semantical matters.

<sup>23</sup> See the discussion of “schmidentity” in “Naming and Necessity,” p. 310.

<sup>24</sup> Or, using variables explicitly, “There is a person  $x$  such that . . .” Notice that in an utterance of “ $(\exists x) (\phi x \wedge \psi x)$ ,” as long as it is clear *which* thing allegedly satisfying “ $\phi x$ ” the speaker has in mind, there can be a speaker’s referent, even if both the speaker and the hearer are aware that many things satisfy “ $\phi x$ .”

<sup>25</sup> This description of the D-languages specifies nothing about semantical features more “intensional” than truth conditions. It is plausible to assume that “ze F” is a *rigid* designator of the thing believed to be uniquely F, but this is not explicitly included in the extensional truth conditions. Nor has anything been said about the behavior of “ze F” in belief and indirect discourse contexts.

If we stipulate that “ze F,” even in such contexts, designates the thing the speaker believes uniquely F’s, then indeed “Jones said that ze man she married is kind to her,” will not be a proper way of reporting Jones’s utterance “Ze man she married is kind to her” (even if Jones and the speaker happen to have the same belief as to who her husband is; the difficulty is more obvious if they do not.) No doubt it is this fact that lies behind Donnellan’s view that, in the referential case, it is hard to speak of “the statement,” even though his exposition of the matter seems to be defective. Such implications, which are not present in the Russell language, lend only further implausibility to the supposition that English is the ambiguous D-language.

To repeat footnote 22, actually there are many other ways, other than taking something uniquely to satisfy “F,” that might be included under referential uses of “the F.” The best short way to specify the semantics of “ze F” would seem to be this: “ze F” refers, in the unambiguous D-language, to what would have been the speaker’s referent of “the F” in the weak Russell language (under the same circumstances)! But this formulation makes it very implausible that the ambiguous D-language is anything but a chimerical model for English.

<sup>26</sup> There is one significant difference between the case of proper names and that of definite descriptions. If someone uses “Jones” to refer to Smith, he has *misidentified* Smith as Jones, taken Smith for someone else. To some extent I *did* think that *Jones* was raking the leaves. (I assume that “Jones” is already in his idiolect as a name of Jones. If I am introduced to an impostor and am told, “This man is none other than Albert Einstein,” if I am fooled I will have *taken* him, falsely, to be Einstein. Someone else, who has never heard of Einstein before, may merely be mistaken as to the impostor’s name.) On the other hand, if I think that someone is “her husband” and so refer to him, I need not at all have confused two people. I merely think that one person possesses a property—that of being married to her—that in fact he lacks. The real husband is irrelevant.

<sup>27</sup> In terms of this apparatus, I can sharpen the reply to Katz, p. 261 above. If Schmidt had discovered the incompleteness of arithmetic but I had thought it was Gödel who did so, a complex (“referential”) use of the description has a semantic reference to Schmidt but a speaker’s reference to Gödel. Once I am apprised of the true facts, speaker’s reference and semantic reference will coincide thereafter and I will no longer use the description to refer to Gödel. The name “Gödel,” on the other hand, has Gödel as its *semantic* referent: the name will always be applied to Gödel in the presence of correct information. Whether a term would be withdrawn in the presence of correct information (without changing the language) is a good intuitive test for divergence of semantic reference and speaker’s reference (disregarding the cases in footnote 22).

<sup>28</sup> There is another problem for any theory of semantic ambiguity. Donnellan says that if I say “Smith’s murderer is insane,” solely on the basis of the grizzly condition of Smith’s body, my use of “Smith’s murderer” is attributive (even if I in fact have a belief as to who the murderer is), but if I say it on the basis of the supposed murderer’s behavior at the dock, my use is referential. Surely, however, my reasons can be mixed: perhaps neither consideration would have sufficed by itself, but they suffice jointly. What is my use then? A user of the unambiguous D-language would have to choose between “the” and “ze.” It seems very implausible to suppose that the speaker is confused and uncertain about what sense he gives to his description; but what else can we say if we suppose that English is the ambiguous D-language? (This problem arises even if the man at the dock is guilty, so that in fact there is no conflict. It is more obvious if he is innocent.)

A pragmatic theory of the referential-attributive distinction can handle such cases much more easily. Clearly there can be borderline cases between the simple and the complex use—where, to some extent the speaker wishes to speak of the semantic referent and to some extent he wishes to speak of something he believes to be the semantic referent. He need not sort out his motives carefully, since he thinks these things are one and the same!

Given such mixed motives, the speaker’s reference may be partially to one thing and partially to another, even when the semantic reference is unambiguous. This is especially likely in the case of proper names, since divergences between speaker’s referent and semantic referent are characteristically *misidentifications* (see footnote 26). Even if the speaker’s referent of “Jones” in “Jones is raking the leaves” is Smith, to some extent I have said of *Jones* that he is raking the leaves. There are gradations, depending on the speaker’s interests and intentions, as to what extent the speaker’s

reference was to Jones and to what extent it was to Smith. The problem is less common in the case of descriptions, where misidentification need not have occurred.

<sup>29</sup> Of course these tests must be used with some caution. The mere fact that some language subdivides the extension of an English word into several subclasses, with their own separate words, and has no word for the whole extension, does not show that the English word was ambiguous (think of the story that the Eskimos have different words for different kinds of snow). If many unrelated languages preserve a single word, this in itself is evidence for a unitary concept. On the other hand, a word may have different senses that are obviously related. One sense may be metaphorical for another (though in that case, it may not really be a separate sense, but simply a common metaphor.) "Statistics" can mean both statistical data and the science of evaluating such data. And the like. The more we can explain relations among senses, and the more "natural" and "inevitable" the relationship, the more we will expect the different senses to be preserved in a wide variety of other languages.

The test, therefore, needs further exploration and refinement. It is certainly wrong to postulate an ambiguity without any explanation of some connection between the "senses" that explains why they occur in a wide variety of languages. In the referential-attributive case, I feel that any attempt to explain the connection between the referential and the attributive uses will be so close to the kind of pragmatic account offered here as to render any assumptions of distinct senses implausible and superfluous.

<sup>30</sup> That is, the *concept* of truth conditions is somehow inappropriate for the semantics of English.

The vague uneasiness expressed in these paragraphs expresses my own rather confused occasional doubts and is ancillary to the main theme. Moore's "paradox of analysis" may be a related problem.

Quine's philosophy of language characteristically is based on a naturalistic doubt about building any "rules" or "conventions" into a language that are not recoverable from actual linguistic practices, even if such rules may be necessary to stipulate the language. In this sense, the uneasiness expressed is Quinean in spirit. I find Quine's emphasis on a naturalistic approach to some extent salutary. But I also feel that our intuitions of semantic rules as speakers should not be ignored cavalierly.

<sup>31</sup> Geach, in his book "Reference and Generality," Emended edition (Ithaca, 1970), and elsewhere, has argued vigorously against speaking of pronominalization as picking up a previous reference. I do not wish to argue the extent to which he is right here. I use the terminology given in the text for convenience, but to the extent Geach's views are correct I think the example could presumably be reformulated to fit his scheme. I think the views expressed in this paper are very much in the spirit of Geach's remarks on definite descriptions and speaker's reference in the book just cited. See Geach's discussion, e.g., on p. 8.

<sup>32</sup> Donnellan, in his forthcoming paper "Speaker Reference, Descriptions and Anaphora," thinks that the fact that pronouns can pick up a previous semantic reference somehow casts doubt on a view that makes speaker's reference a nonsemantical notion. I don't see why: "he," "she," "that," etc., can, under various circumstances, refer to anything salient in an appropriate way. Being physically distinguished against its background is a property that may make an object salient; having been referred to by a previous speaker is another. In "Naming and Necessity," footnote 3, I suggested tentatively that Donnellan's "remarks about reference have little to do with semantics or truth conditions." The point would be put more exactly if I had said that Donnellan's distinction is not itself a semantical one, though it is relevant to semantics through pronominalization, as many other non-semantical properties are.

Pronominalization phenomena are relevant to another point. Often one hears it argued against Russell's existential analysis of *indefinite* descriptions that an indefinite description may be anaphorically referred to by a pronoun that seems to preserve the reference of the indefinite description. I am not sure that these phenomena do conflict with the existential analysis. (I am not completely sure there are some that don't, either.) In any event, many cases can be accounted for (given a Russellian theory) by the facts that: (i) existential statements can carry a speaker's reference; (ii) pronouns can refer to the speaker's referent.

<sup>33</sup> The use of "ze" in the unambiguous D-language is such that the semantic reference automatically coincided with the speaker's reference, but nevertheless, the notions are applicable. So are the notions of simple and complex uses of designators. However, speakers of the unambiguous D-language might be less likely ever to use "the" in a complex case: for, one might be inclined to argue, if such are their intentions, why not use "ze"?

<sup>34</sup> Various moves might be tried, but none that I can think of seem to me to be plausible. It has been suggested to me that sometimes the respondent in a dialogue deliberately feigns to misunderstand an ambiguous phrase used by the first speaker, and that, given the supposed ambiguity of "her husband" in the ambiguous D-language, the first dialogue can be interpreted as such a case. For example, the following dialogue: "Jones put the money in a bank." "He put the money in one all right, but it wasn't a commercial bank; he was so much afraid it would be discovered that he hid it near the river." It seems implausible to me that the first dialogue in the text fits into such a very jocular model. But notice further that the joke consists in a mock *confirmation* of the first speaker's assertion. It would be rather bizarre to respond, "He didn't put the money in the bank, and it wasn't a commercial bank." The first dialogue would have to conform to such a bizarre pattern on the hypothesis in question.

Alternatively, it might be suggested that B uses "he" as a pronoun of laziness for A's "her husband," taken in the supposed referential sense. This move seems to be excluded, since B may well be in no position to use "her husband" referentially. He may merely have heard that she is married to a cruel man.

<sup>35</sup> I believe that Karttunen has advocated the view that the referential-attributive distinction arises from a scope ambiguity; I do not know whether this has been published. Since the referential-attributive "ambiguity" arises even in simple sentences such as "Smith's murderer is insane," where there appears to be no room for any scope ambiguity, such a view seems forced to rely on acceptance of Ross's suggestion that all English assertive utterances begin with an initial "I say that," which is suppressed in "surface structure" but present in "deep structure."

For the view that derives the referential-attributive "ambiguity" from a distinction of restrictive and non-restrictive clauses in "deep structure," see J. M. Bell, "What is Referential Opacity?", *The Journal of Philosophical Logic* 2 (1973): 155–180. See also the work of Emmon Bach on which Bell's paper is based, "Nouns and Noun Phrases," in *Universals in Linguistic Theory*, ed. E. Bach and R. T. Harms (New York, 1968), pp. 91–122. For reasons of space I have not treated these views here. But some of my arguments that Donnellan's distinction is pragmatic apply against them also.

<sup>36</sup> See p. 260 above; also see footnote 10 above.

<sup>37</sup> The term is Donnellan's. See "Putting Humpty Dumpty Together Again," p. 204, footnote 5.

<sup>38</sup> I believe that when Donnellan heard the present paper, he too mentioned considerations of this kind. The cases are mentioned briefly in Donnellan's paper, "Putting Humpty Dumpty Together Again," *ibid.* Donnellan's forthcoming paper mentioned in footnote 1 above also makes use of the existence of such incomplete descriptions but I do not find his arguments conclusive.

<sup>39</sup> See the Santa Claus and Madagascar cases in "Naming and Necessity." See "Naming and Necessity," pp. 300–302 for the Santa Claus case and pp. 768–9 of the "Addenda" to that paper for the Madagascar case.

<sup>40</sup> It seems likely that the considerations in this paper will also be relevant to the concept of a supposed "± Specific" distinction for indefinite descriptions, as advocated by many linguists.

<sup>41</sup> I should like to thank Margaret Gilbert and Howard Wettstein for their assistance in the preparation of this paper.