

# 6

## Should Proper Names Still Seem So Problematic?

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### 6.1 Introduction

In a small undergraduate class at Harvard in 1967, Saul Kripke presented a revolutionary non-descriptivist view of the reference determination of proper names.<sup>1</sup> A later version of these lectures was delivered at Princeton in 1970, published as the paper “Naming and Necessity” (1972), and finally published as the book *Naming and Necessity* (1980). In these works, Kripke is careful to claim that he is not presenting a *theory* of reference but rather “a better picture—a picture which, if more details were to be filled in, might be refined so as to give more exact conditions for reference to take place” (1980: 94).

In a recent paper, Andrea Bianchi claims that “forty years later, we do not possess a fully blown theory built on this picture” (2011: 264). I found Bianchi’s claim both surprising and disappointing, for I thought that I had proposed just such a theory. Kripke’s Harvard lectures inspired my first attempt in a dissertation, “The Semantics of Proper Names: A Causal Theory” (1972). A more polished short version of the theory was published in “Singular Terms” (1974) and a more polished long version, in *Designation* (1981a). So, what is Bianchi’s problem? He thinks that what I proposed does not count as a *theory* of reference for proper names because it does not have the form of a *biconditional* along the lines of: “proper name *a* refers to *x* iff  $aRx$ .” And Bianchi insists that a theory must have that form.

Now I did offer a biconditional of the sort that Bianchi wants in my *initial* development of a causal theory in *Designation*, before considering the problems of confused reference and reference change. But Bianchi is right that I did not present the final theory, which takes account of those problems, as a biconditional. My focus

<sup>1</sup> About half those attending were graduate students. My notes of these lectures are available on the website of the Saul Kripke Center at <<http://kripkecenter.commons.gc.cuny.edu/resources/unpublished-works/fall-1967/>>.

throughout was on the *explanation* of the reference of names and I probably doubted that we should expect this to yield a Bianchi biconditional: as Kripke says, “One might never reach a set of necessary and sufficient conditions” (1980: 94). Indeed, is there any interesting explanatory theory in the social sciences that can be put in the form of a strictly true biconditional? I suspect that social reality is just too messy and complicated for that. So I am not confident that the biconditionals I shall offer in this chapter will stand up. Still, I aim to do my best to produce ones that will.

In fulfilling this aim I shall be presenting my causal theory of names in a somewhat different way from before. I will also be bringing it up to date, and highlighting its virtues. This is not the place to give all the details of the theory but I will address one potential reason, other than Bianchi’s, for thinking that we lack “a fully blown theory.” This is the concern that no causal theory is *complete*. Now my theory certainly leaves a lot unexplained, as I shall emphasize in presenting it later in the chapter. But this alone is not a cause for criticism: “explanation must stop somewhere,” as they say. What matters is: (a), that the theory is good so far as it goes; (b), that it goes as far as it is reasonable to expect a theory to go now. I think this is true of the theory I proposed.

Finally, I should acknowledge two features of my theory that may have alienated some potential subscribers. First, the theory aims for a naturalistic development of Kripke’s picture. Naturalism is not popular in semantics. Second, the theory takes a strong stance against the “Cartesian” view that semantic competence, whether with a proper name or anything else, is (partly) constituted by (“tacit”) propositional knowledge about meaning or reference. The Cartesian view is near ubiquitous in semantics. I remain unapologetic about these two features.<sup>2</sup>

Bianchi’s concern is with the theory of a proper name’s *reference*. But what about its *meaning*?<sup>3</sup> Kripke showed that its meaning could not be descriptive, but then what was it? Kripke has always been noncommittal on this issue, but many influenced by him have adopted “direct reference,” understood as the “Millian” theory that a name’s meaning (“semantic value,” “semantic content,” or . . .) simply is its bearer. I adopted a different view. From the start, I urged that a name’s meaning was its causal mode of referring (1972, 1974, 1981a). Frege was right in thinking that a name’s “sense” is its mode of “presenting” its bearer but wrong in thinking that that mode is descriptive. It was only much later that I came to realize that this idea, so natural to me as to seem almost uninteresting—it was presented in “Singular Terms” briefly in one paragraph with no fanfare (1974: 204)—struck many as what I later called “A Shocking Idea about Meaning” (2001). Shocking as it may be, it solves the meaning problem for names and seems to have nothing against it but a misguided attachment to traditional ways of thinking.

<sup>2</sup> For a defense of these features see my 1996, 2006a, 2011a, 2012a, 2012b, 2013c.

<sup>3</sup> Barbara Abbott has recently described this as “the main problem with proper names” (2012: 307).

In Part 2 I shall consider the reference of proper names, in Part 3, their meaning. The discussion of meaning will conclude by attending to the meaning of “descriptive” names like ‘Jack the Ripper.’ I aim to show that proper names should *not* still seem so problematic.

## 6.2 Reference

I call the mode of reference of a (paradigm) proper name like ‘Aristotle,’ “designation.” (In contrast, I call the mode of reference of a Russellian description, “denotation.”) I then express Kripke’s central idea for a causal theory of proper names as follows:

our present uses of a name, say ‘Aristotle’, designate the famous Greek philosopher Aristotle, *not* in virtue of the various things we (rightly) believe true of him, but in virtue of a causal network stretching back from our uses to the first uses of the name to designate Aristotle. (1981a: 25)

The challenge is to turn Kripke’s idea into a theory. The biconditional I offered at the end of my initial development of a causal theory in *Designation* was:

a name token designates an object if and only if underlying the name is a d[esignating]-chain grounded in the object. D[esignating]-chains consist of three different kinds of link: groundings which link the chain to an object, abilities to designate, and communication situations in which abilities are passed on or reinforced (reference borrowings). (1981a: 64)

This biconditional explicitly overlooked complications arising from confused designation, designation change, descriptive names, and, relatedly, the important distinction between speaker- and conventional designation. I will consider those complications in sections 6.2.5 to 6.2.7, leading to new biconditionals in section 6.2.8. But first I need to explain this preliminary statement of the theory. What mainly needs explaining are the constituents of a “designating-chain” and the idea of one “underlying” a name. We shall start with “underlying” and then go on to the constituents, “abilities to designate,” “groundings,” and “reference borrowings.”

### 6.2.1 “Underlying”

Proper names typically have more than one bearer. Thus, a contemporary token of ‘Aristotle’ might designate the famous philosopher or it might designate the late shipping magnate Onassis. In virtue of what does the token designate one person rather than the other?

One intuitively appealing way to start an answer is to say that the speaker *intended to designate one person rather than the other* by that token. But this answer has a problem. For a speaker literally to intend to designate *x*, given that intentions are propositional attitudes, seems to require that she entertain a thought involving the concept of *designation*. So she can’t designate without thinking about designation! There is no reason to believe this (one could designate without even *having* the concept of designation; 1981a: 97). The answer presents a far too intellectualized picture of designating. Aside from that, it is only

the start of a solution to our problem because it immediately raises the question: In virtue of what did the speaker intend to designate *x*? Without an answer to this question, the problem has simply been moved a short distance from the utterance to the intention.

Reflecting the influence of Peter Strawson (1959: 182) and Keith Donnellan (1966: 287), I have always preferred another intuitively appealing start: the speaker *had one person not the other in mind* by that token. But this also is only the beginning of an answer because it raises the question: In virtue of what did the speaker have that person in mind? We need an explanation—better, an explication—of this somewhat vague folk talk. What reality is it getting at? I argue that the reality is causal.<sup>4</sup> The token designated that person in virtue of being immediately caused by a thought that is grounded in that person by a designating-chain. In these circumstances, I say that the designating chain “underlies” the token.<sup>5,6</sup>

<sup>4</sup> In a highly UCLA-centric recent volume in honor of Donnellan, called “*Having in Mind*” (Almog and Leonardi 2012), Joseph Almog proposes a causal explanation of having in mind (2012: 177, 180–2) that has similarities to my old explanation, as Bianchi indicates (2012: 89 n. 7). Almog attributes the explanation to Donnellan. Other contributors to the volume make similar attributions. But, so far as I can see, nobody cites any evidence for these attributions. I certainly found no such explanation of having in mind, hence of referential uses of descriptions, in Donnellan’s early papers (1966, 1968, 1972), as I noted (1981a: xi, 283–4 n. 12), and that was what led me to provide one (1972, 1974, 1981a, 1981b). As David Kaplan remarked about Donnellan’s original paper, “the notion of having someone in mind is not analyzed but used” (1978: 222). Donnellan’s talk of “having in mind” is, as Tyler Burge points out, a “metaphor” that needs development (2012: 107–8). One might well think that a causal explanation is the natural development. It certainly seemed so to me and that was why I made it. Having made it, however, we should see this folk talk of “having in mind” as but “a stepping stone” to a causal theory of designation (1974: 202; see also 1981a: 33–4).

<sup>5</sup> In her chapter in the present volume, Genoveva Martí takes a strong stand against the view of “neo-cognition advocates” that “the things we have in mind determine what we refer to.” Her main targets are contributions to the just-mentioned volume, Almog and Leonardi (2012), particularly Kaplan (2012) and Almog (2012). Despite noticing that “a lot of the arguments in Kaplan (2012) and Almog (2012) are remindful of Devitt’s presentation,” she excuses me from her criticism. But is she right to do so? I think so. First, I do not rest my theory on *having in mind* but rather on the causal story, as indicated by the “stepping stone” remark, noted by Martí. And I endorse her externalist view that “no matter how hard the Martian anthropologist looks into the minds of speakers, he will not find there who they are referring to when they use proper names.” If there were to be any aspect of my view open to her criticism, it would have to be the roles I give to mental states in determining reference. I have just claimed that one sort of mental state plays an essential causal role in the process of *underlying* and hence, as we shall see (6.2.5, 6.2.8), in *speaker-reference*. But Martí’s concern is not really with speaker-reference but with conventional-reference. We shall see that I give an essential role to another sort of mental state in determining that reference, the state of *participating in a convention* (6.2.5, 6.2.8). But I assume that Martí is giving the same role to the same mental state in claiming that reference is explained by the fact that speakers “rely on and conform to a practice.” So it does look as if she is right to excuse me from her criticisms. Finally, in light of the role of mental states in reference, I think that we should understand Hilary Putnam’s famous externalist slogan, inelegantly, as “meanings just ain’t just in the head.”

<sup>6</sup> Stavroula Glezakos overlooks the causal relation of *underlying*. She imagines that she gained ‘Aristotle’ as the name of the philosopher in one conversation and as the name of someone else in another later one. What, then, makes her use of the name in a particular utterance about the philosopher? She answers: “Maintaining that there is a ‘causal’ or ‘historical’ connection between my utterance and the first of those past conversations will not suffice to account for my having retrieved one name rather than another. After all, neither one of the earlier conversations ‘caused’ me to produce my utterance today” (2009: 320). She overlooks that the use of the name in her utterance was *caused by* a certain thought and *the nature of* that thought is partly explained by its causal connection to an object via the first conversation, not the later one. And the object thus linked to her thought was the philosopher.

### 6.2.2 “Ability to Designate”

What are we to say about abilities to designate with a proper name? In virtue of what is one an ability to designate one object *in particular* with one name *in particular*? In virtue of what is something an ability to designate Aristotle rather than Plato with ‘Aristotle’? In virtue of what is it an ability to designate Aristotle with ‘Aristotle’ not ‘Plato’? The causal theory has neat answers. But these are best presented against a background consideration of what abilities to designate with names are *in general*.

Such abilities are part of our general linguistic competence. What are we to say about that competence? Accepting, as we surely should, that “language expresses thought,”<sup>7</sup> I recently gave this as an initial answer:

the competence is the ability to use a sound of the language to express a thought with the meaning that the sound has in the language in the context of utterance; and the ability (together with some pragmatic abilities) to assign to a sound a thought with the meaning that the sound has in the language in the context of utterance (similarly for inscriptions, etc.). (2006a: 148)

We can move to a more theory-laden view of competence if we adopt the popular, and in my view correct, representational theory of the mind according to which any thought involves standing in a certain functional relation to a *mental representation*. Competence is then “the ability to *translate* back and forth between mental representations and the sounds of the language.” And if we go further to the controversial language-of-thought hypothesis according to which the mental representation is language-like, the translation is “between mental *sentences* and the sounds of the language” (Devitt 2006a: 148). Finally, linguistic competence is complex, consisting of syntactic competence and lexical competence. Thus, going along with the language-of-thought hypothesis, syntactic competence is the ability to translate back and forth between the syntactic structures of the sounds of the language and the structures of mental sentences. And lexical competence is the ability to translate back and forth between the words of the language and mental words.

To say more about linguistic competence we must, of course, look to psycholinguistics. Despite much progress, the consensus there is that we still have a great deal to learn about language processing. A while back, Jerry Fodor remarked: “Very little is known about how [a device for sentence comprehension] might operate, though I guess that, if we started now and worked very hard, we might be able to build one in five hundred years or so” (1975: 167). This is still in the spirit of the consensus. And the situation is much the same with sentence production.<sup>8</sup>

In light of this, we cannot now expect to be able to explain abilities to designate with a name, for that is one species of lexical competence. I did not attempt an

<sup>7</sup> “There is much to be said for the old-fashioned view that speech expresses thought, and very little to be said against it” (Fodor et al. 1974: 375).

<sup>8</sup> I give my take on the state of play in language use in (2006a: ch. 11).

explanation and claimed that the failure to do so at that time should not be of much concern to a theory of names (1974: 186; 1981a: 28–9). That still seems right to me.

But the causal theory does have something to say about what makes something an ability to designate *one object in particular with one name in particular*, about what ties the ability to that object and that name. In light of the above, the ability is one to translate back and forth between mental representations of the particular object and the sounds (inscriptions, etc.) of the particular name. That straightforwardly ties the ability to the name. And, according to the theory, the mental representation is of that particular object because, briefly, a designating-chain grounded in that object brought it about; or, less briefly, because it was directly caused by groundings in the object or indirectly caused by them via reference borrowings. That ties the ability to the object.<sup>9</sup>

### 6.2.3 “Grounding”

Kripke’s central idea was that we succeed in designating Aristotle, say, with his name not in virtue of associating true descriptions of Aristotle with his name but in virtue of a chain of reference borrowings that takes us back to the original users who fixed the name’s reference in Aristotle. So we designate Aristotle because others in our linguistic community did so before: the determination of our reference is a social matter. But how did the original users fix reference? Kripke did not say much about this in his 1967 lectures, but he seemed to be favorably disposed to a “description theory”: a person fixes the reference in an object by a description of the object in a dubbing.<sup>10</sup> Later, in *Naming and Necessity*, he talks of “fixing a reference by description, or ostension” (1980: 97). My dissertation was built on the idea that something like ostension was the right way to go for *paradigm* names like ‘Aristotle.’ The reference of such a name is fixed in the object, directly or indirectly, by the causal link between a person and that object when it is the focus of that person’s perception.<sup>11</sup> This is what I call a “grounding.”<sup>12</sup> So on this view of reference fixing, the original users have their ability to designate Aristotle by ‘Aristotle’ in virtue of their causal link to him and then we inherited this ability to designate him by reference borrowing. A description theory is only true for the reference fixing of *non-paradigm* “descriptive” names like ‘Jack the Ripper,’ which I will discuss in section 6.2.7.

<sup>9</sup> For more on abilities, see my (1981a: 129–33).

<sup>10</sup> And that is Putnam’s view: “even in externalist theories of reference descriptions play a key role: the original dubber or dubbers identify or have the capacity to identify what they are talking about by definite descriptions” (2001: 496–7).

<sup>11</sup> So, the following view that Almog attributes to Kripke is quite false on my theory: “Perception works one way, semantic reference quite a different way” (2012: 177). Almog cites no evidence for his bold attribution and I am dubious of it. As noted, Kripke did seem to favor a descriptive theory of reference fixing—Howard Wettstein thinks this was his “paradigm” (2012: 115)—but Kripke is rather noncommittal on the matter.

<sup>12</sup> Note that whereas reference borrowing is essentially social, reference fixing is not.

The sort of direct grounding that leaps to mind is a formal dubbing. I acquire a kitten. Holding it and looking at it, I say, "Let's call her 'Nana.'" But dubbings can be informal: I might simply say, "Nana," whilst looking intently at the kitten. Or the dubbing may be only implicit: without any ceremony I might say, "Nana is a striking-looking kitten," and thus start the practice of calling the kitten "Nana."<sup>13</sup> Nicknames are often introduced in this way. Sometimes there is no need for a dubbing because names are bestowed automatically; thus, babies in many cultures have their father's surname waiting for them at birth. What is it about all these situations that ground the name in a certain object? It is the causal-perceptual link between the first users of the name and the object named. What made it the case that this particular object got named in such a situation was its unique place in the causal nexus in the grounding situation.<sup>14</sup>

It is important to note that this sort of situation will typically arise many times in the history of an object after it has been initially named: names are typically *multiply grounded* in their bearers. These other situations are ones where the name is used as a result of a direct perceptual confrontation with its bearer. The social ceremony of introduction provides the most obvious examples: someone says, "This is Nana," demonstrating the kitten in question. Remarks prompted by observation of an object provide many others: thus, observing Nana's behavior, someone says, "Nana is skittish tonight." Such remarks are likely to happen countless times during Nana's life. All these uses of a name ground it in its bearer just as effectively as does a dubbing because they involve just the same reference-fixing causal-perceptual links between name and bearer. There is no basis for distinguishing the role of that utterance of "Nana is skittish tonight" as a reference fixer from that of the earlier implicit dubbing, "Nana is a striking-looking kitten": both are uses of the name prompted by the kitten as the object or focus of perception. Dubbings and other first uses of a name do not bear all the burden of linking a name to the world.

Multiple grounding is vital in explaining confused designation and designation change, as we shall see (6.2.5–6.2.6). But it is not an *ad hoc* device introduced for those purposes: it is a consequence of the causal-perceptual account of groundings.

<sup>13</sup> As Wettstein aptly remarks about the naming of a human baby: "Parents may muse for quite a while . . . about names, and then the baby is born and they just start using the name; no baptism; no act of giving the name" (Wettstein 2012: 116).

<sup>14</sup> I accompanied this theory of the reference fixing of names with related theories of the reference fixing of other referential devices that might identify the object in a grounding: according to these theories, the reference of such a device, whether a personal pronoun, demonstrative, or definite description, is fixed by a causal-perceptual link to the object (1974: 191–7; 1981a: 36–46). The theory for definite descriptions includes the Donnellan-inspired idea that these descriptions have not just referential *uses* but referential *meanings* (1981b; 2004; 2007). However, that idea is not strictly necessary for the theory of names: it is sufficient that the *speaker-reference* of a referentially used description is fixed by the causal-perceptual link.

I presented these theories as an across-the-board causal approach to referential devices. Brian Loar (1976) attributed something close to this unified approach to Kripke and Donnellan as part of what Loar called "the radical two-use theory." Yet, as I pointed out (1980), this theory is not to be found in the works he cites. Almog has recently proposed such a unified approach, also attributing it to Donnellan without evidence (2012: 181–3). However, some evidence of this approach can be found in Donnellan 1974.

So far we have been considering direct groundings. But names can be indirectly grounded in an object via other terms that are grounded in it. Thus, we can ground the nickname ‘Slim’ in our friend Frank by saying, “Let’s call Frank ‘Slim.’” We can ground a name by using a definite description referentially. And we can ground a name in a person via a photograph of the person.

I had a good deal more to say filling out the details of groundings but, just as with abilities to designate, much was left unexplained (1974: 185–6, 198–200; 1981a: 26–9, 56–64, 133–6). (a) People in grounding situations are engaged in a species of lexical acquisition and/or production and/or understanding. I made only a little attempt to describe these psychological processes. We must look to psycholinguistics to cast real light on such processes but we should not expect too much now. (b) I talked of a causal–perceptual link between an object and a person when it is the focus of her perception, but I did not say much about the nature of this link. We must look to psychology to say more.<sup>15</sup> But again we should not expect too much now. I think that it is acceptable to leave a theory of groundings in this state at this time.

A name may be “empty” because it fails to be grounded in the way described. It may be a “fictitious” name, arising out of a work of fiction, or a “failed” name, introduced for an entity that is wrongly assumed to exist (1981a: 167–88).

#### 6.2.4 “Reference Borrowing”

Kripke emphasized that our contemporaries have gained their abilities to designate Aristotle with his name by reference borrowing; thus the reference determination for us is a social matter. Kripke made two influential remarks about borrowing. First he said that a person in borrowing a name “must ... intend when he learns it to use it with the same reference” (1980: 96). Second, he pointed out that this “hardly eliminates the notion of reference; on the contrary, it takes the notion of intending to use the same reference as a given” (97). I shall outline my account of reference borrowing in the context of discussing these two remarks.

The first remark has frequently been misunderstood. Note that Kripke says that a borrower must *at the time of borrowing* intend to use the name with the same reference as the lender. Yet this is standardly misunderstood as requiring that the borrower, *whenever using the name thereafter*, must intend to use it with the same reference as the lender. Thus, John Searle (1983: 234) quotes Kripke’s first remark but then interprets it as follows:

each speaker must intend to refer to the same object as the person from whom he learned the name. So this does give us some Intentional content associated with each use of a name “*N*” in the causal chain, viz., “*N* is the object referred to by the person from whom I got the name”. (235–6)

<sup>15</sup> One thing we look for is a solution to what became known as “the *qua* problem”: In virtue of what is a certain object the focus of perception rather than a spatial or temporal part of the object? I have struggled mightily with this problem (1981a: 61–4; Devitt and Sterelny 1999: 79–80), but I now wonder whether this was a mistake: perhaps the problem is more for psychology than philosophy.



So Searle takes Kripke to require a speaker, *at the time of using* a name, to have an intention directed at the reference lender.<sup>16</sup> But Kripke does not require such a “backward-looking” intention at the time of use. Indeed, such a requirement is quite alien to his discussion and the contrast he draws between his and Strawson’s description theory of reference borrowing. Strawson’s theory fails precisely *because* it requires competent users of names to have knowledge of reference lenders (Kripke 1980: 92–3). And Donnellan, in discussing the way our uses of proper names are “parasitic on uses of the names by other people,” emphasizes that “the history behind the use of a name may not be known to the individual using it” (1972: 373).<sup>17</sup>

What matters for the successful use of a name that has been acquired by reference borrowing is that the use be caused by an ability with that name that is, *as a matter of fact*, grounded in the bearer via that borrowing: *the efficacious ability must have the right sort of causal history*. That is all that is required. Contrary to what Thomas Blackburn (1988: 184) claims, we need not require a borrower to recognize or acknowledge this history. And contrary to what Adèle Mercier claims (1999), we need not require that a borrower’s successful use of a name be accompanied by an intention to “defer.”<sup>18</sup> The speaker exploits an ability that is, *as a matter of fact*, borrowed, but the speaker need not defer to the lender. Indeed, the speaker need not know who the lender was or even that she *has* borrowed the name. There is no need for her to have any semantic thoughts about the name at all. Use of language does not require any thoughts about language.

Where Kripke follows Strawson in talking of “reference borrowing” and Donnellan talks of “parasitic” uses, many have since talked of “deference.”<sup>19</sup> That change may be part of the problem. (1) If *x* borrows the reference of a term from *y*, then that is an act at the time of receiving *y*’s communication. In contrast, if *x* defers to *y*’s use of a term, then that suggests an act at the time of *x*’s using the term herself to communicate.

<sup>16</sup> The first example of this misunderstanding that I know of is by Michael Dummett (1973: 147–51).

<sup>17</sup> We have noted that neither Kripke nor Donnellan requires a backward-looking intention. Did any causal theorist require it? (1) Perhaps Putnam did in his discussion of the “division of linguistic labor.” His introduction of this idea does not suggest the requirement (1973: 705), but his later discussions might be seen to: “my denotation may be, by general consent, the denotation assigned by persons distant from me in space and even in time, but linked to me by relations of cooperation”; he thinks he is thus “giving up my right to be the authority on the denotation of my own words” (1975: 274); “speakers defer to experts for the fixing of reference in a huge number of cases” (1978: 114). (2) It rather looks as if Kaplan did. He talks of the (non-creative) use of a proper name being associated with “the intention to use the word with the meaning given it by the person from whom you learned the word.” His subsequent talk of “one who uses a proper name with [this] intention” (1989a: 559) implies that the intention accompanies each use rather than simply being present at the time of learning.

<sup>18</sup> Robin Jeshion (2010) has raised doubts about the transfer of “singular thoughts” by means of reference borrowing. In my view (2004: 290; 2014), the only theoretically interesting notion of a singular thought ties it to nondescriptive causal modes of reference (so I agree with Jeshion’s rejection of what she calls “semantic instrumentalism”). In borrowing the reference of a name, a person gains the ability to think thoughts that refer in virtue of such a nondescriptive mode. Hence reference borrowing can transfer singular thoughts.

<sup>19</sup> Where did this talk of “deference” come from? The first such talk in this context seems to have been by Gareth Evans (1973: 205–6) in the process of *criticizing* the causal theory and presenting his own.

(2) Furthermore, talk of deference invites a confusion between *epistemic* deference to experts *when seeking knowledge*, which we should all be in favor of, and *semantic* deference to experts *when referring*, which causal theorists oppose.

With this clarified, it is time to say more about the process of reference borrowing. A speaker who already has an ability to designate a particular object with a particular name as a result of groundings or reference borrowings can pass on that ability by using the name in conversation. Someone who perceives this use and processes it correctly—on which more later—thereby gains the ability herself. This acquisition, like one in a grounding, is a causal process. The borrower's ability, hence her future uses of the name, are causally grounded in the object via the ability of the lender.

We are talking here of the *acquisition* of a name by reference borrowing. In fact, most uses of a name are to hearers that already have it. Consider, for example, a contemporary use of 'Aristotle' to designate the great philosopher. Any of us hearing this and correctly understanding it—a matter of assigning it to our ability that concerns the great philosopher—thereby borrow the reference. This borrowing *reinforces* the ability we already have to designate the great philosopher with 'Aristotle.' The borrowing causally links our uses of the name to the object just as effectively as did the initial borrowing that created our ability. Thus, underlying a person's use of a name may be many designating-chains involving multiple reference borrowings and, ultimately, multiple groundings in the object: there may be a *causal network* of designating-chains underlying her use.

I had a good deal more to say filling out the details of reference borrowings but, just as with abilities to designate and groundings, much was left unexplained (1974: 186–8; 1981a: 29–32; 137–8). And this brings us to Kripke's second remark: that he has not eliminated the notion of reference because he requires that the borrower intends to use the name *with the same reference* as does the lender. This intention seems necessary because not just any old response to the use of a name will count as a reference borrowing; thus, Kripke notes, a person who hears the name 'Napoleon' and decides that it would be a nice name for his pet has not thereby gained the ability to use the name to refer to the famous general (1980: 96). But I think that it is a mistake to try to capture what is required to borrow reference by talking of intentions to use a name with the same reference, for the same reason that I earlier urged that it is a mistake to talk of intentions to refer (section 6.2.1).

We need to characterize reference borrowing in a much less intellectualized way. I have recently (2006b) attempted this in responding to criticisms by Dunja Jutronic (2006). What Kripke's talk of intentions is getting at is that reference borrowing is in some sense an *intentional act*. But we do not have to explain this act in terms of an intention deploying the concepts of reference borrowing or deference. Rather we require that the borrower process the input supplied by the situation in whatever way is appropriate for gaining, or reinforcing, an ability to use the name to designate its referent. The borrower must intentionally set in motion this particular sort of mental processing even though largely unaware of its nature and perhaps not conscious of

doing so. Similarly, a person walking or talking must intentionally set in motion the sort of mental processing appropriate to that activity. So, reference borrowing is not just any old causal process in the communication situation: it is a special one involving that particular mental process.<sup>20</sup>

This explains reference borrowing in mental terms just as we have earlier explained groundings. It leaves much unexplained, of course. Reference borrowing is a species of lexical acquisition or understanding and so we must look to psycholinguistics to throw more light on it. Still, given the current state of our knowledge of language processing, we should not expect too much now. It seems to me respectable for a contemporary theory of reference to leave it at that.<sup>21</sup>

We need to take account of various complications before revisiting the theory's biconditional. In particular, our discussion of multiple groundings and reference borrowings raises the possibility of confusions in the causal network for a name: an object other than the original referent may get involved in the network through misidentification in a grounding or misunderstanding in a reference borrowing. I have considered these at some length elsewhere (1974: 200–3; 1981a: 138–52). In the next section I will discuss confusions through misidentification (but not through misunderstanding). Then we must consider designation changes of the sort illustrated by Gareth Evans' excellent example of 'Madagascar' (1973). I will do that in section 6.2.6. And, finally, we must consider descriptive names, which I will do in section 6.2.7.

### 6.2.5 *Designation Confusion*

I shall start my discussion of confusions by considering a famous example presented by Kripke in arguing against Donnellan's view of definite descriptions:

Two people see Smith in the distance and mistake him for Jones. They have a brief colloquy: "What is Jones doing?" "Raking the leaves". (1979a: 14)<sup>22</sup>

For convenience, let us suppose that the final remark was

(\*) Jones is raking the leaves.

And let us suppose that the speaker was Ralph. What are we to say about the reference of Ralph's 'Jones' in this case of misidentification? Kripke introduced an important Gricean distinction to deal with it. Expressing Kripke's distinction in my terminology, he thinks that this token of 'Jones' *conventionally* designates Jones but *speaker-designates* Smith (with the result that what Ralph *literally said* is false but what he *meant* is

<sup>20</sup> Jutronic (2008) and Devitt (2008d) continue the exchange on the issue. A similar theory of reference borrowing is also appropriate for referential descriptions (Devitt 1974: 191–2; 1981a: 38–9). Kaplan has recently also urged such a view (2012: 142, 147).

<sup>21</sup> The view is not open to the following criticism: "These causal–historical chain arguments for Millian externalism rely heavily on vague, impressionistic metaphors, such as 'inheriting' referents or 'carrying' meanings" (Sullivan 2010: 255).

<sup>22</sup> Kripke thinks that this example, involving a name, is similar to another, "Her husband is kind to her," involving a definite description. I argue that it is crucially different (1981b: 512–16).

true). I think he is right about the former but not quite right about the latter. To see why, we need to say something about the distinction.

The biconditional from *Designation* that began this part of the chapter might tempt one to say that a person speaker-designates an object by a name if and only if underlying the name is a designating-chain grounded in the object. Considerations in this section will lead to a significant revision of this. Still, we can now say that all the users of ‘Nana’ in the earlier stories of the name’s grounding (section 6.2.3) speaker-designated Nana.

What about conventionally designating Nana by ‘Nana’? Conventions can be established by explicit agreement but they often arise from regular practices, as David Lewis demonstrated in his classic discussion (1969). What constitutes a linguistic convention with an expression in a community and what is it to participate in one? It is difficult to say exactly, and this is not the place to try to say.<sup>23</sup> Lewis’ own view, with its requirement of “mutual knowledge,” seems too intellectualized.<sup>24</sup> Nonetheless, it points us toward what is surely the right core idea. That idea is that for there to be a convention in a community of using an expression with a certain speaker meaning is for members of the community to be disposed to use that expression with that meaning *because other members are disposed to do so*: there is a certain sort of causal dependency of the disposition of each member of the community on the dispositions of others. And for a person to participate in this convention is for her to use that expression *because* she has that dependent disposition. Consider ‘Nana.’ There came to be a convention of using that name to speaker-designate Nana when members of a certain community so used it because others did. At that point, ‘Nana’ entered the language of that community; its members acquired what I am calling “an ability to designate Nana by ‘Nana,’” a new part of their linguistic competence. When did that happen? Probably somewhere very near the beginning of the causal network for ‘Nana.’ The community’s early groundings and reference borrowings established the convention. Reference lending is the standard way of creating the mutual dependencies that spread the convention. For, a person who borrows the reference of ‘Nana’ from someone who is participating in the convention, from someone exercising her ability with ‘Nana,’ comes thereby to participate in that convention herself.

It is important to recognize that there is unlikely to be a determinate fact of the matter about precisely when a convention like that for ‘Nana’ is established. For, in general, there is a *sortes* problem about when a regular practice becomes a convention. Consider the convention among graduate students of meeting in O’Reilly’s for a drink around 6 p.m. on Fridays. At semester beginning there was no such convention. By semester end, there was. There was never any explicit agreement. Rather, two or three went there the first week. A few more went the next week. The word of this

<sup>23</sup> I have made one attempt to say more (2006a: 178–84) and am about to make another (forthcoming).

<sup>24</sup> Stephen Laurence (1996, 1998) rejects “convention-based semantics” largely on the basis of criticisms of Lewis’ account of conventions in general and of linguistic conventions in particular. These criticisms are partly of the highly intellectualized nature of Lewis’ account. I sympathize with those criticisms. I reject the rest (2006a: 180).

got around. More and more people started to go regularly. People started to expect others would go. A convention was established. But there was no determinate point in the semester at which it was established.

A person can, of course, speaker-designate an object by a name without there being any convention of so doing. All that is required is that a token of the name have underlying it a designating-chain grounded in the object. So I could now speaker-designate Aristotle with any old name simply on the strength of the link to Aristotle that is constitutive of my ability to designate him by 'Aristotle.' But that ability with 'Aristotle' was brought about by a causal network that would not exist but for the earlier establishment of the convention of designating Aristotle by 'Aristotle.' It is that convention that made the network grow via reference borrowing, spreading abilities with 'Aristotle.' So, because of the convention, I now have access to a designating chain grounded in Aristotle which can underlie my use of any old name to speaker-designate him. I can thus speaker-designate Aristotle only because I, and others, can conventionally designate him by 'Aristotle.'<sup>25</sup>

Return to Kripke's example. We can assume that the two people in the colloquy are both competent with the name 'Jones' and are participating in the convention of using it to designate a certain person, Jones. There are, of course, many other conventions for the name 'Jones,' but these two people are not here participating in any of them. So, Ralph's use of 'Jones' in (\*) conventionally designates that person Jones, just as Kripke thinks. Kripke also thinks that Ralph speaker-designates Smith. My theory gives some support to this idea: underlying Ralph's 'Jones' is a causal-perceptual link to Smith of just the sort that constitutes a grounding in Smith. However, we should not conclude that Ralph simply speaker-designates Smith because underlying that 'Jones' is also a designating-chain grounded in Jones. This reflects the fact that Ralph is participating in the convention of designating Jones by 'Jones.' Indeed, imagine that in conversation prior to sighting Smith, Ralph remarks, "Jones has been acting strangely lately." Ralph would speaker-designate Jones (as well as conventionally designate him) in virtue of underlying designating-chains grounded in Jones. *Just the same chains underlie* (\*). So, there is no determinate fact of the matter whether Ralph, in uttering (\*), speaker-designates Smith or Jones. However, borrowing an idea from Hartry Field (1973), we can say that Ralph *partially* speaker-designated both.<sup>26</sup>

In light of this we must revise what we might have been initially tempted to say about speaker-designation. For a person to speaker-designate an object with a name, it is not enough that there be a designating-chain grounded in the object underlying that use of the name: *all* the designating-chains that underlie the use must be grounded in the object.

<sup>25</sup> My concept <Aristotle> is about Aristotle in virtue of a designating-chain back to him involving people participating in the convention of designating him by 'Aristotle.' But that convention is not constitutive of the aboutness. Rather it causes the chain that constitutes the aboutness. This differs from an earlier discussion (2006a: 139–40).

<sup>26</sup> My discussion of Kripke's example draws on my (1981b: 512–16).

We need to consider an important objection to my judgment on Kripke's example. I have emphasized that *underlying* is a causal relation (6.2.1). Might it not then be the case that, contrary to what I claimed, the designating-chains that underlie "Jones has been acting strangely lately" do *not* underlie (\*)? Those chains might be *causally inert* in the utterance of (\*). So the only causally efficacious chain, hence the only one *underlying* (\*), is the one grounded in Smith by Ralph's act of perceiving him. This is possible, and if it were actual Ralph would indeed have speaker-designated Smith. However, I doubt that it is actual. Observation judgments like (\*) are "theory-laden." It is likely that the judgment would not have been made but for background beliefs grounded in Jones, not Smith (1981a: 145; 1981b: 515).

So much for what is going on in this example. But what are the consequences of this for the convention for 'Jones'? (\*) grounds the causal network for 'Jones' in Smith. At least it does unless Ralph comes to realize that he has misidentified Smith, thus nullifying the grounding (1981a: 150).<sup>27</sup> If there is no nullification, the network will be grounded in both Jones and Smith. We shall tackle this sort of problem in a moment.

We turn now to Evans' example of 'Madagascar' (1973). The major interest of this is as an example of designation change. But set that aside until the next section and consider the early misidentification that led to the change. According to the account repeated by Evans, Marco Polo, on the basis of a hearsay report of Malay sailors, took a corrupted form of the name of a portion of the African mainland as the name of the great African island. So, on the one hand, Marco Polo borrows the reference of 'Madagascar' which is grounded in that portion of the mainland. On the other hand, we presume, he had causal-perceptual links to the island in virtue of which he grounded the name in that. The situation is analogous to Ralph's use of 'Jones': initially Marco Polo conventionally designates the portion of the mainland and partially speaker-designates both the mainland and the island.

However, there is an interesting difference between the two examples. Whereas Ralph's mistake, whether nullified or not, would be unlikely to lead to future groundings of 'Jones' in Smith and hence to designation change, Marco Polo's mistake (presumably) did lead to future groundings of 'Madagascar' in the island and hence did lead to designation change.

### 6.2.6 Designation Change

Forty years ago, Evans claimed that examples of reference change like his nice one of 'Madagascar' were "decisive against the Causal Theory of Names" (1973: 195). The claim is quite false and yet Evans' view persists to this day. Arthur Sullivan recently provided an exuberant example of the persistence.<sup>28</sup> He cites 'Madagascar' as one of the "flat-out counterexamples" to the causal theory because in this example "the criteria specified

<sup>27</sup> Although the grounding of the established 'Jones'–Jones network in Smith is thus nullified, Ralph could, of course, attempt to make that grounding the beginning of a new 'Jones'–Smith network.

<sup>28</sup> Searle provides an earlier example (1983: 237–8), Imogen Dickie, a later one (2011: 48).

by Bach (1987)—or Devitt (1981[a]), or Kaplan (1989[b]), and so on—are satisfied, and yet successful transmission of reference does not occur” (2010: 255). He adds a note: “Soames (2003: 366) is one rare case in which a Millian externalist even mentions ‘Madagascar’” (255 n. 20). Yet, in the very work of mine that Sullivan cites, there is a long section on reference change showing that, far from being counterexamples to the causal theory, cases like ‘Madagascar’ actually confirm the theory (1981a: 138–52). The section concludes:

A strength of the account offered here, making use of multiple groundings and partial reference, is that it enables a plausible account of the gradual transition from one convention with a name to another. (151)<sup>29</sup>

This discussion is accompanied by a note that does indeed mention the example of ‘Madagascar’ (289 n. 14), as does an earlier note (285 n. 23).<sup>30</sup>

So what is the causal theory’s explanation of this designation change? Briefly, *change in the pattern of groundings*. There are three stages in the history of ‘Madagascar.’ (A) Before Marco Polo, the pattern was of grounding ‘Madagascar’ in the portion of the mainland by locals (overlooking that this name is a corruption) with the result that users of the name conventionally designated and, typically, speaker-designated that portion. (B) Then came the period of confusion following Marco Polo’s misidentification. There was a change in the pattern of groundings, some being in the island. During this period, Marco Polo and those influenced by him typically do not speaker-designate nor, after a while, conventionally designate either the island or the portion of the mainland with the name: they partially designate them both. The period may be short or long. (C) However long the period of confusion, the time came when the pattern of groundings in the designating-chains underlying our ancestors’ uses of ‘Madagascar’ were in the island. A new designation convention with the name had been born.<sup>31</sup>

<sup>29</sup> My earliest discussions (1972; 1974) addressed the problem of designation *confusion* using the ideas of multiple groundings and partial designation. I did not use these ideas to address the problem of designation *change* until later (1981a), after Evans had pressed that problem so effectively.

<sup>30</sup> That example, and another of Evans’ examples of reference change, are also discussed in both editions of the textbook, *Language and Reality* (Devitt and Sterelny 1987: 62–3; 1999: 75–6).

<sup>31</sup> This talk, and the talk of a name *changing* its designation, implicitly take ‘name’ to refer to a sound or inscription type, what I once called a “physical type.” But we might also, or instead, take ‘name’ to refer to what I once called a “semantic type.” On the latter usage, physically identical tokens with different designata are of different names (1981a: 9–10). So the phenomenon of ‘Madagascar’ would be described, as Marco Santambrogio (ms) insists it should be, not as a case of “a name changing its designation” but of “a new name being created.” I see only a verbal issue here. Nothing of theoretical interest hinges on whether we say that the name, qua physical type, has changed its reference or that a new name, qua semantic type, has been created. I shall (mostly) continue with the former way of talking.

The phenomena we are discussing are at odds with the following: “At some point the name ‘Aristotle’ entered our practice and then its semantics was finished. Passing it from one to another is like passing the salt . . . The chain is of interest in various ways, but it’s not an externalist link of name to referent. That link was whole and complete; if not there was nothing to pass along” (Wettstein 2012: 118 n.). Because of the possibility of misidentifications in groundings (and misunderstandings in reference borrowing), there is no interesting respect in which the semantics of a name is ever “finished.”

Stage (B) is, of course, the interesting one. The most helpful technical notion for explaining this period is a development of Field's idea of partial designation: we talk of *degrees of designation* (and, hence, *degrees of truth*). Thus we see (B) as a period of change in the degrees to which Marco Polo and those associating with him speaker-designate the island and speaker-designate the portion of the mainland, change brought about, of course, by the change in pattern of groundings.<sup>32</sup> How do we settle these degrees? "The decision on [the degree to which a token designates one object not another] is based on the relative importance of groundings in that object in the causal explanation of the token" (1981a: 148). The key theoretical claim is that *any explanation of the degree to which a name token designates an object is entirely causal*, based on what the designating-chains underlying the token are grounded in and on their causal role. This leaves plenty of room for negotiation about degrees. And there is, of course, a lot of indeterminacy in this period of confusion.

Just how long the period of confusion lasted depended on developments. An important factor is whether those influenced by Marco Polo and those influenced by the locals continued to mingle. Suppose that they did not. Then the period of confusion would have been short. Members of Marco Polo's group would have continued grounding the name only in the island leading to a rapid increase in the degree to which they speaker-designated the island rather than the portion of the mainland. This pattern of groundings in the island would quickly establish a new convention of designating the island with the name. Meanwhile, the old convention of designating a portion of the mainland would doubtless have lived on with the locals. The name would have become "ambiguous." If, on the other hand, we suppose that the two groups did mingle, the period of confusion would have likely been long and troublesome. For quite a while, each use of the name in both groups would probably have underlying it some designating-chains grounded in the portion of the mainland and some others grounded in the island. In that time, the convention for the name is not for it to fully designate either the portion of the mainland or the island but rather for it to only partially designate each to a high degree. And, to repeat, there would be much indeterminacy about the length of the period of confusion and about the situation during that period.<sup>33</sup>

This explanation of designation change is a straightforward corollary of the causal theory I have proposed. Groundings fix designation. From the causal-perceptual account of groundings we get the likelihood of multiple groundings. From multiple groundings we get the possibility of confusion through misidentification. From

<sup>32</sup> The result, on this Fieldian approach, is that any token of 'Madagascar is in Africa' will come out fully true, any token of 'Madagascar is in Australia,' fully false, and many other token sentences like 'Madagascar is an island,' *o.n*-true, where *o.n* is a number between 0 and 1.

<sup>33</sup> Kripke discusses 'Madagascar' in the "Addenda" to *Naming and Necessity*. He remarks that "a present intention to refer to a given entity [the island] . . . overrides the original intention to preserve reference in the historical chain of transmission" (1980: 163). Setting aside my qualms about talk of intentions to refer (6.2.1), one should see my theory as explaining *what makes* the present intention one to refer to the island despite that original reference-borrowing intention.



confusion we get the possibility of designation change through change in the pattern of groundings.<sup>34</sup>

What is the alternative to a causal theory of designation change along these lines? Those who present designation change as a problem for the causal theory may see it as some sort of vindication of descriptivism. But it is not. What, after all, could the description be that determines that ‘Madagascar’ now designates the island? Presumably it must include ‘island.’ Yet, given the general level of geographical ignorance, we can’t be confident that all competent users of the name even know that Madagascar is an island. And that is just the start of the problem for descriptivism. We need a description that is rich enough to identify *which* island is Madagascar. So we will be tempted by a description that includes ‘Africa.’ But that is another name that also needs an identifying description. The problems of “ignorance and error” (as I call them) that Kripke made familiar are as present with ‘Madagascar’ as with ‘Cicero’ and ‘Einstein’: if descriptivism were right, many names out of just about every mouth would fail to designate. Descriptivism about names is a lost cause.

### 6.2.7 *Descriptive Names*

Paradigm proper names have their reference fixed by causal–perceptual links in groundings. But some names are not paradigms and get their reference fixed by what Donnellan (1966) calls an “attributive” use of a description.<sup>35</sup> In such a use the speaker refers to whatever uniquely fits the description, to whatever the description denotes. As a result, in my usage, the name “denotes” that object. Two famous examples are ‘Jack the Ripper,’ the name for whoever brutally murdered certain prostitutes in 1888; and Evans’ ‘Julius,’ the name for whoever invented the zip (1979). I called such names “attributive” (1974: 195–6; 1981a: 40–1), but they are now more usually called “descriptive” and that is what I shall call them here. In contrast, I call the paradigm names “designational.”

A descriptive name differs from a designational one in the way its reference is fixed. Does it differ also in the way its reference can be borrowed? I argued not, with the following consequence:

<sup>34</sup> Multiple grounding is also vital in explaining reference change in “natural kind terms” (1981a: 190–5). Arthur Fine (1975: part 4) criticized Putnam’s causal theory of these terms on the grounds that it made reference change impossible. But, as I pointed out (1981a: 291–2 n. 1), Putnam could easily add multiple grounding to his theory. And later he did: “As Devitt rightly observes, such terms are typically ‘multiply grounded’” (2001: 497).

Antonio Rauti (2012) objects that the prevalence of mistaken groundings of natural kind terms should, on my theory, lead to much more instability in reference than there actually is. This leads him to introduce a notion of *deference* to explain reference stability. (He carefully and rightly distinguishes deference from reference borrowing; see section 6.2.4.) But I think that his objection overlooks that not all groundings are equal: they differ greatly in their causal role. Where that role is causally insignificant, it does not threaten the stability of reference.

<sup>35</sup> We can prescind here from the issue of whether a description has a referential *meaning* as well as the Russellian quantificational meaning exemplified in an attributive use; see note 14.

Though [a descriptive] name was associated with an identifying description at its introduction, many of those who now use it may not know this description. Indeed, it is possible, though not likely, that nobody now using the name should know it. (1981a: 41)<sup>36</sup>

So underlying a token descriptive name that denotes an object are causal chains starting with a description denoting the object, likely continuing through various reference borrowings, and culminating in the speaker's ability to denote that object with that name. Let's call this chain a "denoting-chain."<sup>37</sup>

It is tempting to think that a name that starts life as descriptive can become designational: the object denoted by the name is discovered and the name is thereafter frequently grounded in the object (1981a: 57). This would have happened to 'Jack the Ripper' had we ever discovered who murdered those prostitutes. And it did happen to 'Neptune.' This was originally a descriptive name introduced by Le Verrier for whatever planet was causing certain perturbations in the orbit of Uranus. Then that planet was sighted and the name became designational. And, of course, there is unlikely to be a determinate point at which this happened, at which the designational convention was established.

We shall see that this tempting story may be too simple when we say a lot more about descriptive names in section 6.3.4.

### 6.2.8 *Biconditionals for Bianchi*

It is finally time to try to meet Bianchi's challenge of producing a biconditional. In fact, we need several biconditionals, starting with two for designational names, one for speaker-designation, and one for conventional-designation.

The initial biconditional from *Designation* would lead us to the following biconditional for speaker-designation: a designational name token speaker-designates an object if and only if underlying the token is a designating-chain grounded in the object. But our discussion of designation confusion and change has brought out how inadequate this is: there may be many designating-chains underlying a name, not all of them grounded in the same object. So, the biconditional we need is:

*Speaker-Designation:* A designational name token speaker-designates an object if and only if all the designating-chains underlying the token are grounded in the object.<sup>38</sup>

<sup>36</sup> So we might say that a description theory of reference is half-true of descriptive names.

<sup>37</sup> Many think that descriptive names yield a priori knowledge, for example, the knowledge that Jack the Ripper is a murderer. I must disagree because I have argued that there is no a priori knowledge (1996, 2011c). Still, it is obvious that knowledge of semantic facts can yield knowledge of the world. Thus, suppose it really is the case that the reference of 'Jack the Ripper' is fixed by 'whoever brutally murdered certain prostitutes in 1888.' Then anyone who knows that will know that Jack the Ripper is a murderer. But knowledge of that referential fact is both hard to obtain, because Cartesianism is false, and as empirical as could be (2011c: 23–4).

<sup>38</sup> Similarly, of course, a designational name token *partially* speaker-designates an object if and only if *at least one* designating-chain underlying the token is grounded in the object. (So a token that designates *x* partially designates *x*.)

A consequence of this, I have argued (6.2.5), is that Ralph's use of 'Jones' in

(\*) Jones is raking the leaves

probably does not speaker-designate Smith or Jones. Although a designating-chain grounded in Smith clearly underlies this utterance, designating-chains grounded in Jones probably do too, because background beliefs about Jones are likely playing a causal role. Similarly, Marco Polo's early uses of 'Madagascar' probably do not speaker-designate the island or part of the mainland.

The biconditional for conventional-designation is relatively easy to come by:

*Conventional-Designation:* A designational name token conventionally designates an object if and only if the speaker, in producing the token, is participating in a convention of speaker-designating that object, and no other object, with name tokens of that type.

We are working with a distinction between two causal processes involved in the use of a name: one, *underlying*, that features in *Speaker-Designation* and the other, *participating in a convention*, that features in *Conventional-Designation*. The distinction warrants more discussion. *Underlying* concerns the process of a speaker using the name to express a thought grounded in a certain object. *Participating in a convention* concerns the process of a speaker using the name because she has a disposition, dependent on the dispositions of others, to use it to express thoughts grounded in a certain object. Typically these two groundings are in the same object; for example, when Ralph says, before sighting Smith, "Jones has been acting strangely lately," both the underlying thought and the convention for 'Jones' are grounded in Jones. So Ralph both speaker- and conventionally designates Jones. But it is a different story when he later says (\*). The convention is still grounded in Jones, but the underlying thought is grounded in Smith (and, probably, Jones). So Ralph still conventionally designates Jones but fails to speaker-designate him.

Irony provides another example of the distinction (1996: 225–7). Thus, a cynical journalist, Bruce, observing General Westmoreland at his desk during the Vietnam War, comments: "Napoleon is inventing his body count." The thought underlying Bruce's use of 'Napoleon'—the thought he expresses—is grounded in Westmoreland. So he speaker-designates Westmoreland. But the journalist is participating in a convention grounded in Napoleon: he uses 'Napoleon' because he has a dependent disposition to use it to express thoughts grounded in Napoleon. So he conventionally designates Napoleon. Expanding on the latter story a little, the journalist uses 'Napoleon' because he supposes that the hearer, who also participates in the convention, will infer that he does not mean Napoleon but rather Westmoreland.

Bruce deliberately participates in a convention grounded in one object to express a thought grounded in another. In the following story (1981a: 139–40, 146), Spooner does so accidentally. Spooner is reported to have once given a sermon that included many uses of 'Aristotle.' He was leaving the pulpit when he suddenly stopped,

returned, and announced to the congregation, “When in my sermon I said ‘Aristotle’ I meant St. Paul.” The thought underlying Spooner’s uses of ‘Aristotle’—the thought he expresses—is grounded in St. Paul, but because of “crossed wires” he ends up participating in a convention grounded in Aristotle.

We turn now to descriptive names, names that have their reference fixed not by a causal grounding but by a description: these names denote their referent, not designate it. The biconditionals we need are:

*Speaker-Denotation:* A descriptive name token speaker-denotes an object if and only if all the denoting-chains underlying the token start with a description that denotes the object.

*Conventional-Denotation:* A descriptive name token conventionally denotes an object if and only if the speaker, in producing the token, is participating in a convention of speaker-denoting that object, and no other object, with name tokens of that type.

A speaker might ironically speaker-designate one object whilst conventionally denoting another. Suppose that Tom is at a police station confessing to some gory murders. A skeptical detective observes Tom’s weird behavior and remarks: “Jack the Ripper is crazy.” She speaker-designates Tom but conventionally denotes Jack the Ripper.

One further phenomenon has to be contemplated. In responding to Evans’ (1973) example of ‘Ibn Kahn,’ I argued (1981a: 157–60) that the names of authors—my example was ‘Shakespeare’—can have a double life. In claims about where “Shakespeare” lived, was educated, and so on, the name seems to function as a designational name. In critical assessments of “the works of Shakespeare,” however, it often seems to function as a descriptive name, so that it would not matter to the truth of these assessments if the work was actually written by Bacon. And there may often be no determinate fact of the matter which meaning a token of the name has: we may have to take it as only partially designating and partially denoting. Suppose we do have to take it that way. Then we need further biconditionals for these names:

*Speaker-Designation/Denotation:* A name token that is neither determinately designational nor determinately descriptive speaker-refers to an object if and only if both the designating-chains underlying the token are grounded in the object and the denoting-chains underlying the token start with a description that denotes the object.

*Conventional-Designation/Denotation:* A name token that is neither determinately designational nor determinately descriptive conventionally refers to an object if and only if the speaker, in producing the token, is either participating in a convention of speaker-designating or speaker-denoting that object (but there is no fact of the matter which convention), and no other object, with name tokens of that type.

That is my best shot at stating my theory of reference for names in the biconditional form that Bianchi insists on. But I do wonder if the search for strictly true biconditionals here is fruitless. Perhaps, as noted in Part 1, social reality is too messy and complicated for that.

### 6.2.9 Conclusion

The theory of reference for proper names that I have presented is a relatively straightforward development of Kripke's revolutionary idea within a naturalistic and anti-Cartesian framework. In light of it, my conclusion is that the reference of proper names should not still seem so problematic. This is not to say, of course, that the theory is complete. I have emphasized that any theory of reference at this time must look to future psycholinguistics for more details. And it is not to say either that the details already provided are certainly right. The point is simply that we have good reason now to think that this theory is more or less right, so far as it goes, and it goes as far as it is reasonable to expect at this time. And we can see that such adjustments as may be necessary will not be large and will be in terms of the same reality of designating- or denoting-chains.

I turn now to the matter of a name's meaning.

## 6.3 Meaning

### 6.3.1 Direct Reference

As noted in Part 1, I have argued for the apparently shocking view that a (designational) name's meaning<sup>39</sup> is its causal mode of referring (1972, 1974, 1981a).<sup>40</sup> This view stands in stark contrast to two standard views. First, the Fregean view that a name's meaning is a descriptive mode of referring: its meaning is expressed by a definite description that competent speakers associate with the name and that determines its reference.<sup>41</sup> Second, the direct reference view that a name's meaning is simply its bearer. Kripke refuted the Fregean view. But what about direct reference?

<sup>39</sup> Or "semantic value," "semantic content," or . . . Take 'meaning' as standing in for any of these other technical terms. I address the issue of what we should mean by such terms in section 6.3.2.

<sup>40</sup> It follows that we should say of a name like 'Aristotle' that has more than one bearer—and so of almost all names—that it is "ambiguous" (1974: 188; 1981a: 32). Kaplan agrees: "proper names really are ambiguous" (1989a: 562). Kent Bach, in contrast, thinks that it is "absurd" to say this (1987: 34). There are two ways to accommodate theoretically the fact that reference of a name in a context is determined by a certain sort of causal connection. (i) My way is to take a name as having an unusual sort of ambiguity: a meaning for each object to which it is thus causally connected. (ii) Bach's way is to take a name as having just one meaning, that of a special sort of indexical, with reference fixed in context by that causal connection. So, just as the pronoun 'he' demands "saturation" in context by a male connected in a certain way, the name 'Bruce' demands saturation in context by a bearer of 'Bruce' connected in a somewhat different way. There is nothing "absurd" about my preference for (i), but nothing hangs on it: the theory could adapt to (ii). The issue seems to be largely verbal.

<sup>41</sup> So it is strange indeed that Abbott should place my view with the descriptivist views of Searle and Lewis under the heading "Causal description" (2012: 312). This is a bad misunderstanding. "Causal description" refers to some post-revolutionary description theories that are *parasitic on* causal theories like mine: where a causal theory says that a name designates what it is causally related to in a certain way, its causal-descriptivist parasite says that *speakers associate with the name a description of that very causal relationship* and the name designates what that description denotes. For a criticism, see Devitt and Sterelny 1999: 61.

Direct reference, as I am understanding it,<sup>42</sup> is simply a resurrection of the “Millian” theory. So it faces the familiar, and apparently overwhelming, problems that led Frege and Russell to abandon Millianism long ago in favor of description theories. Because of these problems, I did not, for many years, entertain direct reference as even a *candidate* theory of the meaning of a name. And because of these problems, presumably, Kripke has never embraced direct reference. But then, by the mid-’80s, direct reference’s popularity made it impossible to ignore.<sup>43</sup> Indeed, by that time the received view, even among counter-revolutionaries, seemed to have become that direct reference was a *consequence* of a Kripkean nondescriptive causal theory of a name’s reference.<sup>44</sup> The history of this is curious.

An early statement of the view that direct reference was indeed part of the Kripkean revolution was by Brian Loar in a paper that was *critical* of the revolution (1976: 355). Loar went further, claiming that direct reference was “the chief philosophical interest of causal theories of referring” (368). Yet, as I pointed out in a response (1980: 272–3), direct reference is not to be found in the only papers that Loar cites, Kripke (1972) and Donnellan (1972). And we should note that Stephen Schwartz, writing at roughly the same time as Loar, lists the “main features” of what he calls “the new theory” of reference without even mentioning direct reference (1977: 20–34). Loar’s claim led me to say that direct reference was “too implausible to be attributed to anyone without evidence” (1980: 273). Other *critics* of the revolution, Stephen Schiffer (1979) and Felicia Ackerman (1979), soon joined Loar in misattributing direct reference to Kripke.<sup>45</sup> But they are on much firmer footing in finding the view in Donnellan 1974.<sup>46</sup> Schiffer also claims that David Kaplan’s famous “Dthat” (1978) “clearly implies” the view (1979: 73 n. 5). I see no such clear implication in “Dthat” (but see later for more on Kaplan). Was there anyone else back in the ’70s who was led by the revolution to *embrace* direct reference?<sup>47</sup> The only publication I could find is by Michael Tye (1978). A little later, there was one by Tom McKay (1981). Both, like the critics, misattribute the view to Kripke (Tye 1978: 220; McKay 1981: 301 n.).<sup>48</sup> And

<sup>42</sup> “Direct reference” is often understood in other ways. I have summarized the various “direct reference” theories and their histories elsewhere (1989: 206–12; see also 1996: 170 n.).

<sup>43</sup> See Barwise and Perry (1983: 165); Almog (1984, 1985); Salmon (1986); Soames (1985, 1987); Wettstein (1986). For criticisms, see my (1989, 1996, 2013a).

<sup>44</sup> See, for example, McGinn (1982: 244); Baker (1982: 227); Lycan (1985); Block (1986: 660, 665); Lepore and Loewer (1986: 60); Wagner (1986: 452).

<sup>45</sup> Schiffer claims, without argument, that it is “difficult, if not impossible, to see how [Kripke] could reject” direct reference (1979: 63). Ackerman cites two passages to support her attribution to Kripke, while allowing that it is “less clear-cut” than the attribution to Donnellan (1979: 68 n. 6). The citations are not convincing, in my view.

<sup>46</sup> I have earlier (1989: 209) doubted that direct reference was to be found in Donnellan (1974), but I now think I was wrong about this. In a later work, Donnellan certainly embraces direct reference (1989: 275–6).

<sup>47</sup> Ruth Barcan Marcus (1961) had embraced the view before the revolution.

<sup>48</sup> The misattributions continue to this day: “But Kripke still goes a long way with Mill. For the name user down the chain, the name is fully Millian—no descriptions or modes of presentation, not even in a reference-fixing capacity” (Wettstein 2012: 117).

McKay claims that the view “has quickly become a commonplace” (287). So, by the end of the ’70s direct reference was well and truly in the air, despite a mostly mistaken view of where precisely it had come from.<sup>49</sup>

But what about Kaplan? This is a fascinating story. Although I do not think that the direct reference theory of names is clearly in “Dthat,” it is clearly in the influential paper “Demonstratives” (1989a) that circulated widely in the ’70s, long before it was published. This paper introduced the *term* “direct reference” but used it primarily in the discussion of indexicals and demonstratives. Only in “the most hastily written section” (558) does Kaplan, somewhat tentatively, adopt the direct reference theory of names (562).<sup>50</sup> And, in a final footnote, Kaplan briefly raises the possibility of a view with some similarities to mine (563 n.). Such a view is further explored in his “Afterthoughts,” although his considered position is direct reference (1989b: 574–7, 598–9). Yet recently, and remarkably, Kaplan (2012) has urged much the same view as mine, and for much the same reason as I did, apparently unaware that the view has been around for 40 years<sup>51</sup> and that it is generally regarded as shocking. I shall discuss his new view in the next section.

Direct reference faces the familiar problems that drove us out of the Millian paradise more than a century ago. Aside from these apparently insuperable problems, direct reference is theoretically unmotivated; or so I have argued (1989, 1996, 2013a). The motivation for my own view that I am about to summarize contains some passing mention of those criticisms.

### 6.3.2 *Should We be Shocked by Meanings as Causal Modes of Reference?*

The view that a name’s meaning is its causal mode of referring immediately prompts a question about the nature of this mode. I shall summarize my theory’s answer in a moment. In Part 1, I mentioned that my view is shocking to traditional ways of thinking. Nathan Salmon, for example, is positively aghast, describing the view as “ill conceived if not downright desperate . . . wildly bizarre . . . a confusion, on the order of a category mistake” (1986: 70–1). I think that we can infer that most philosophers of language find the view too shocking to contemplate.<sup>52</sup> For, how else are we to explain the just-noted general acceptance of an inference from the revolution in the theory of reference to the direct reference theory of meaning? It is important to see that the cause of this shock is not so much the *particular* causal mode that I propose, to be

<sup>49</sup> Thanks to Nathan Salmon for scholarly help with this paragraph.

<sup>50</sup> I have discussed the argument that leads him to this (1989: 213–15).

<sup>51</sup> Aside from my own proposal, Panu Raatikainen has drawn my attention to a note in which Kripke briefly mentions something like this idea: “Hartry Field has proposed that, for some of the purposes of Frege’s theory, his notion of sense should be replaced by the chain which determines reference” (1972: 346 n. 22; the note does not reappear in the 1980 book). (Field and I attended Kripke’s 1967 lectures together. In the months that followed, we talked countless times about the issues raised by the lectures. The idea of a name’s meaning as a causal mode of referring surely first emerged in those discussions.)

<sup>52</sup> Scott Soames, who is certainly well aware of the view, does not even mention it as a possibility in *Beyond Rigidity* (2002).

described soon, but the *very idea* of a meaning as a causal mode. So it is worth discussing this very idea briefly.

We should start the case for this very idea by reminding ourselves of a major reason why the description theory of names was so appealing. Consideration of the role of names in identity statements, negative existence statements, and propositional attitude ascriptions made it seem irresistible that there was more to a name's meaning than its role of referring to something.<sup>53</sup> The description theory provides the extra: the name's meaning is its descriptive mode of reference. Kripke's refutation of the description theory does not remove the need for the extra. Where are we to find it? Well, if a name's reference is not determined by associated descriptions it must be determined in some "nondescriptive" way, a way that links the name to its bearer not via associated descriptions but more directly.<sup>54</sup> We seem to have found what we need: the name's meaning is its *nondescriptive* mode of reference, whatever that may be. And the only candidates for this nondescriptive mode seem to be causal, whether along the lines of Kripke's picture or not. So the meaning is a causal mode.

Why does this very idea, then, seem so shocking? I have two suggestions.

(i) The idea is clearly alien to the semantic tradition that dominates the philosophy of language, a tradition that has been heavily influenced by formal semantics. That tradition structures the debate over meaning (or content) in general, and over the meaning of a name in particular, as a choice between two sorts of abstract "propositions," Fregean ones containing descriptive concepts and Russellian ones containing objects. The tradition leaves no room for nondescriptive modes. The Kripkean revolution should have led us to question this tradition and resist this forced choice.<sup>55</sup>

I am suggesting, in effect, that people may find the meaning of a name so problematic partly because they adhere to this misguided tradition of propositions.<sup>56</sup>

(ii) The idea of causal modes runs counter to a ubiquitous Cartesianism about meaning. It is a truism that competent speakers of a language "know the language."

<sup>53</sup> However, we should not go along with the Fregean idea that the differing "cognitive values" of '*a=a*' and '*a=b*' show that '*a*' and '*b*' differ in meaning. This popular idea wrongly presupposes that we have Cartesian access to meanings (1996: 172–3). The reason for thinking that these two identity statements differ in meaning can be found by considering why we are interested in meanings in the first place; see the discussion to follow.

<sup>54</sup> Putnam finds my notion of a term's "mode of reference" "extremely unclear" (2001: 495). The notion simply refers to that property of a term, *whatever it may be*, in virtue of which the term has its reference. If a term refers it must have such a property and so the notion of its mode of reference is no less clear than the notion of its reference.

<sup>55</sup> If one must take meanings to be abstract propositional *objects*, then we should take them to be sets of modes of reference. But I think that it is much better not to. It is hard to see how such objects, "outside space and time," could be relevant to explaining the causal role of language in the natural spatio-temporal world. Better to take meanings to be the *properties* of concrete mental and linguistic representations in virtue of which those representations play certain causal roles. It is then no more mysterious in principle that the meaning of 'Aristotle' plays a causal role in the natural world than that, say, the intelligence of Aristotle does (1996: 208–15).

<sup>56</sup> I argue that the received view that there is something particularly problematic about thoughts *de se* and their ascription is similarly an artifact of this tradition (2013b).



The Cartesian assumption is that this involves (“tacitly”) knowing *facts about* meanings: if an expression has a certain meaning in the language, then speakers know *that* it does.<sup>57</sup> Then, since the typical speaker knows nothing about causal modes of reference, those modes cannot be meanings. Yet this popular Cartesianism is almost entirely unsupported and is, I have argued, undermined by the revolution. We should embrace the much more modest view that linguistic competence is an ability or skill, a piece of knowledge-how not knowledge-that (1981a, 1996, 2006a, 2011a).<sup>58</sup>

The case for the idea of causal modes that I briefly presented earlier rests on claims about the meanings of, for example, identity statements. Such claims have strong intuitive support, as strong as any claims in the philosophy of language. Still, intuitions can always be resisted by the iron-jawed. What is insufficiently recognized is that there are powerful theoretical considerations in favor of the idea that a name has a meaning that is a nondescriptive causal mode of referring (1989, 1996, 2001, 2013a).

To see this we need to start with a very basic question: What are languages and why do we posit them?<sup>59</sup> Languages are parts of the natural world that are of theoretical interest because of their causal interactions with other parts of that world. They are representational systems that play a large causal role in, at least, communicative behaviors. Thus, Karl von Frisch won a Nobel Prize for discovering that the bees’ “waggle dance” is a language communicating messages about food sources. Another scientist, Con Slobodchikoff (2002), discovered that the “barks” of Gunnison’s prairie dogs form a language that communicates messages about predators. And it is a truism that humans have languages that communicate messages that are the contents of thoughts: as already noted (6.2.2), language expresses thought. So, just as the bees and the prairie dogs have representational systems used for communicating messages to each other, so do we.<sup>60</sup>

In light of this, the properties of languages that we are theoretically interested in are those we have to ascribe to messages to explain their causal roles. In the case of the bees, the properties are of indicating the direction and distance of the food source. In the case of the prairie dogs, the properties are of indicating the sort of predator that is threatening and the characteristics of the particular predator of that sort. In the case of humans, our question about language comes down to the question of which theoretically interesting properties of *thoughts* are communicated by language. Consider proper names, in particular. It is often the case that the only property of a name that concerns us is (near enough) its property of conveying what

<sup>57</sup> An analogous Cartesianism is rife in linguistics. My 2006a is an extended critique.

<sup>58</sup> Jason Stanley and Timothy Williamson (2001) have argued ingeniously for the surprising thesis that knowledge-how is really a species of knowledge-that. I have responded (2011b).

<sup>59</sup> I draw on my 2013c.

<sup>60</sup> Strangely, this view of human language is rejected by Chomskians; see e.g. Chomsky (1986, 1996); Dwyer and Pietroski (1996); Laurence (2003); Collins (2008a, 2008b); Antony (2008). They see a human language as an internal state, not a system of external symbols that represent the world. I have argued against this view: 2003, 2006a: chs. 2 and 10, 2006b, 2008a, 2008b, 2008c, 2009.

the thought is *about*: these are cases where we are trying to explain how a language helps humans learn from each other, just as a language helps bees and prairie dogs to. For this purpose, only the reference of the name is of theoretical interest. To this extent, direct reference is right.<sup>61</sup> But often we are interested in explaining or predicting a speaker's behavior. Then we are interested in the *way* a person thinks about something. So, we are interested in the name's mode of reference. Thus, suppose Abigail has a neighbor called "Samuel Clemens" and reads books by someone called "Mark Twain." On hearing someone report, "Mark Twain is at the Town Hall," she rushes there, saying, "I'd love to meet Mark Twain." It makes all the difference to our explanation of Abigail's behavior that she said this, not, "I'd love to meet Samuel Clemens." For, the former expresses a thought that causes her to rush to the Town Hall whereas the latter expresses one that might not have (because she may not know that Mark Twain is Samuel Clemens). It matters to the explanation of her behavior that her utterance refers to Twain/Clemens *under the mode of 'Mark Twain.'* Our theoretical interest in languages in general leads to an interest in 'Mark Twain's' property of referring to Twain/Clemens in a certain way. I call this mode of referring a "meaning" because, given its role, it is the sort of property that should motivate our theoretical talk of "meanings" in the first place. But, whether it is called a "meaning," "semantic value," or whatever, the mode is as theoretically interesting a property of a name as its having a certain referent. To this extent, direct reference is quite wrong.<sup>62</sup>

On my view, we have good theoretical reasons for counting as meanings *both* a name's property of referring to its bearer *and* its property of referring to the bearer in a certain causal way. For, both properties are causal-explanatory. So, the main objection to direct reference is what it omits from meaning, not what it includes. The new Kaplan expresses a similar sentiment. In urging a semantic place for nondescriptive ways of referring, he does not want to deny a place to "Millian" meanings: "I only wish to resist the term 'semantics' being hijacked for one kind of content" (2012: 168 n. 28). Bravo! And Kaplan's reason for urging a place for nondescriptive ways of referring is also similar to mine:

<sup>61</sup> But, we should note, this theoretical concern with what the thought is *about* also motivates an interest in what a Russellian definite description refers to, independent of how it refers to it. So, the direct reference identification of a term's meaning with its referent is as true of those descriptions as it is of proper names (1996: 144–6).

<sup>62</sup> The following "snappy summary" captures another way of putting the argument that a name's causal mode of reference is a meaning:

1. A name in a [that]-clause of an apparently opaque attitude ascription conveys information about a mode of referring to the name's bearer.
2. A name's mode of referring to its bearer is causal not descriptive.
3. Apparently opaque attitude ascriptions explain behavior in virtue of what they convey.
4. So, the causal mode [explains behavior and hence] is the name's meaning. (2013a: 75)

if we are concerned with the sort of meaning that we grasp when we understand and communicate through language, the sort of meaning that figures into our ability to reason and act on the basis of what we take to be true . . . . . then Russellian singular propositions will not do. (128)

And the nondescriptive way of referring that Kaplan seems to have in mind, explained ultimately in terms of a perceptual link to the bearer, is also similar to mine.

In sum, I accept that a name's property of referring to its bearer is one of its meanings. For convenience, however, I will henceforth largely ignore this meaning. What I insist on is that a name has a meaning that is a nondescriptive causal mode of referring. This view is theoretically well motivated and should not be shocking. No argument has been produced against it. (As Lewis so delightfully indicated, an incredulous stare is not an argument.) It is time now to summarize my theory's account of this meaning.

### 6.3.3 *Meanings as Causal Modes of Reference*

Starting with Kripke's leaf-raking example, we have noted several cases where the speaker- and conventional-reference of a name token differ. If a name's meaning is its mode of reference, then these must be cases where its speaker- and conventional-meaning differ. I shall discuss only conventional meanings.

The basic idea in explaining the conventional meaning of a name, *N*, as a causal mode is simple: *N*'s causal mode of designating an object is its property of designating that object by the type of designating chain that constitutes a certain network of chains. In virtue of what are chains of that type? There are three requirements.<sup>63</sup>

- (i) Obviously, designating chains of that type must be grounded in the object designated. I take it I have said enough about what it is for the chains to be so grounded. Clearly we need to say more to distinguish the meanings of names like 'Mark Twain' and 'Samuel Clemens' that are grounded in the same object.
- (ii) The designating chains for these two names are distinguished by the physical forms of the name tokens that partly make up the chains. This points to the next requirement. The name tokens in the meaning-constituting designating chains for *N* must have one of the conventional physical forms of the name: a certain sound in speech, shape in writing, and so on through other media. We should expect some indeterminacy here. Thus, there is surely no fact of the matter about exactly how much variation is allowed in, say, inscriptions of 'Mark Twain' (2008a: sec. 5).
- (iii) Finally, we require that the designating-chains for *N* be linked together to form one particular network. The mechanism by which links are established is, of course, that of reference borrowing (6.2.4). But why do we need this linking requirement? Well, we already know that an object can have more than one name; for example, one object had the names 'Mark Twain' and 'Samuel Clemens.' And, we have established, such names have different meanings. But now consider an object which, by chance, is

<sup>63</sup> I draw on my 1974: 203–4; 1981a: 153–5; 1996: 165–70.

given the one name in two totally different circumstances; thus, I once imagined the case of a person living a double life, one as a burglar, the other as a wealthy aristocrat, but, by chance, known as 'George' in both lives (1996: 167). Given our theoretical interests in explaining behavior, we *should* distinguish the conventional meanings of these two uses of 'George' just as much as we *do* distinguish the meanings of 'Samuel Clemens' and 'Mark Twain.' We do that with the requirement that the designating chains for *N* be linked together in the one network. So the meaning of *N* is its property of designating by chains of a type that requires them to be linked together in that particular historical network. Since 'George' has two networks it has two meanings.

More needs to be said to cope with some difficult cases, particularly two ingenious ones of Kripke (1979b): first, whereas related names in different languages, like 'London' and 'Londres,' mostly count as having the same meaning, occasionally they do not,<sup>64</sup> as puzzled Pierre showed; second, there is the example of 'Paderewski.' I have discussed such cases elsewhere (1996: 228–40) and will say no more here, beyond noting this. We have already seen that we have a theoretical interest in more than one semantic property of a name, sometimes its reference, sometimes its mode of reference (6.3.2). These difficult cases show that the modes we are interested in for explanatory purposes are sometimes finer-grained than "normal." A name has more than one meaning as mode of reference. The key theoretical point to be made about all cases, whether easy or difficult, is that just as any referential property of a name token is determined solely by its designating-chains, so too is any meaning property: "there are no semantic differences between names without differences in networks" (1981a: 153).<sup>65</sup>

I do not pretend that the theory of a name's meaning that I have summarized, any more than the theory of its reference, is a complete account. However, I do claim that it is good so far as it goes. The meaning of a designational name is a nondescriptive causal mode of reference of the sort indicated.

#### 6.3.4 *Descriptive Names*

In my view, the meaning of a descriptive name, just like the meaning of a designational name, is its mode of referring. For a designational name I have proposed a causal theory of both reference fixing and borrowing: a totally causal mode of reference.

<sup>64</sup> Similarly, 'chrysos,' Putnam's ancient word for gold (2001: 496), and our word 'gold.' Putnam would disagree, as he claims simply that these words "have the same meaning" (497). I would say, for reasons of the sort indicated in the text, that they count as having the same meaning (mode of reference) for some explanatory purposes *but not for others*.

<sup>65</sup> The theory claims to explain all theoretically needed differences in the meanings of names, but what about differences in their "cognitive value" or informativeness? Once Cartesianism is abandoned, this epistemic matter is not something that any theory of names must explain (Devitt and Sterelny 1999: 70–1). (I was wrong about this in earlier works; e.g. 1989.) What about empty names? In brief, an empty name is meaningful because it has a causal network. It is empty because the network is not appropriately grounded. We might say that its meaning is its causal mode of *purportedly* referring.

For a descriptive name, however, I have proposed a descriptive mode of fixing and a causal mode of borrowing (6.2.7): a hybrid mode of reference, part descriptive, part causal.<sup>66</sup> The reason for thinking that this hybrid mode is a descriptive name's meaning is the same as the reason, presented in section 6.3.2, for thinking that a causal mode is a designational name's meaning: briefly, to explain the name's causal role.

This view of a descriptive name's meaning, like the earlier view of a designational one's, stands in stark contrast to two standard views, nicely described in papers by Marga Reimer (2004) and Robin Jeshion (2004). The two views are the Evans view (1979), that a descriptive name's meaning is that of a definite description; and the direct reference view, that its meaning is simply its bearer. I shall consider these in turn.

The appeal of the Evans view comes, of course, from the fact that the reference of a descriptive name, unlike that of a paradigm name, is fixed by an attributively used definite description; for example, the reference of 'Julius' is fixed by the description 'the person who invented the zip.' Nonetheless, we must not take the reference-fixing description as expressing the meaning because Kripke's ignorance and error arguments apply to these names just as much as to designational names like 'Cicero' and 'Einstein' (as Jeshion (2004: 597) illustrates). We have already noted (6.2.7) that those who get the name by reference borrowing may not know the description that fixes the name's reference. Indeed, they may not know that its reference was so fixed. A descriptive name is fundamentally different in meaning from the description that introduces it.<sup>67</sup>

The direct reference view of descriptive names has much the same appeal for some as the direct reference view of paradigm names. And it has the same problems—the familiar problems for Millianism and a lack of theoretical motivation—together with one more: a commitment to the dubious thesis that descriptive names are rigid.

The rigidity of a designational name is explained by its nondescriptive causal mode of reference. What could explain the rigidity of a descriptive name? The rigidity could come from the name's association with a description which is *thereby rigidified*. Thus, assume, as we surely should, that 'Jack the Ripper' was introduced by a nonrigid description. We could explain the name's rigidity by taking it to be equivalent to a rigidified version of that description; for example, 'whoever, as a matter of actual fact, brutally murdered certain prostitutes in 1888.' So, it is easy to explain how a descriptive name *could be* rigid. The problem is finding a persuasive reason for thinking that it *is* rigid.

Reimer (2004) finds the rigidity of descriptive names intuitive. She seems to have Kripke himself on her side (1980: 79). Yet, when we submit them to the standard tests for rigidity, they do not *clearly* pass, even if they pass at all. Consider, for example, how 'Jack the Ripper' fares on what Kripke describes as an "intuitive test" of rigidity (1980: 48–9, 62). The following should be false:

<sup>66</sup> For a discussion of such hybrid modes of reference, see Devitt and Sterelny (1999: sec. 5.5).

<sup>67</sup> Still we might say that a description theory of meaning is half-true of descriptive names just as we earlier noted that we might say that a description theory of reference is half-true of them (note 36).

Jack the Ripper might not have been Jack the Ripper.

Yet it seems not to be so, at least not clearly so. Similarly,

It might have been the case that Jack the Ripper was not a murderer,

(with ‘Jack the Ripper’ having narrow scope) should be true, but it seems not to be. And suppose that Prince Alfred was Jack the Ripper. Is it really necessary that he was? If ever there was a thesis in the philosophy of language that needs more than intuitive support—and I think they all do (2012a, 2012b)—the thesis that descriptive names are rigid is surely one.

This having been said, I have no firm commitment on whether descriptive names are rigid and it is of no consequence to my theory whether they are. My point is that the direct reference view, along with its many other woes, depends on these names being rigid and it is dubious that they are.

In general, it seems to me that rigidity is thought to be much more interesting than it really is. Kripke introduced it in an argument against description theories of a name’s mode of reference. The rigidity of (paradigm) names also gives support to the causal theory of their mode of reference. So rigidity provides evidence against, and for, some theories of reference. I certainly think this role for rigidity is important (2005). But, beyond that, rigidity strikes me as of little theoretical interest. In particular, rigidity achieved by rigidifying a description strikes me as being of very little interest.

We were tempted by the story that ‘Neptune’ started life as descriptive and then became designational when the planet was “discovered” (6.2.7). So, on my view, its meaning changed from being a hybrid, part-descriptive and part-causal, mode of referring to being a totally causal mode. Reimer (2004) also thinks that there was a change of meaning (which she calls “semantic content”). But if my argument is right, she is wrong about the meanings both before and after the change. For, she takes the Evans view of the name’s meaning before the change and the Millian direct-reference view of it after the change.

The story of ‘Neptune’ that I have told is of the death of its old denotational convention and the birth of a new designational convention (with probably a short period of indeterminacy during which there was no fact of the matter whether the old or new convention was in play). Reimer has a nice example which shows that things may not be that simple:

Physical anthropologists introduce the name ‘Eve’ and stipulate that it is to refer to the first (female) human being. Suppose that anthropologists find the skeletal remains of what they believe [to] be the first (female) human being. For decades, they refer to the remains (*e*) as ‘Eve’ . . . However, the remains of an earlier female human (*e1*) are subsequently discovered, and anthropologists throughout the world declare that they have now found Eve—and that the remains of the other female previously called ‘Eve’ are not in fact those of Eve. (2004: 627)

The response of the anthropologists to the discovery of *e1* is good evidence that the old denotational convention for ‘Eve’ was alive. And it does not seem plausible that it has been suddenly resurrected. So it looks as if it continued to live even after the discovery

of *e*. Yet, the decades of groundings of ‘Eve’ in *e*, leading to many discussions of *e*’s properties using that name, provide good evidence that the name had a designational convention before the discovery of *e*1. So ‘Eve’ seems to have both a denotational and designational meaning at the same time.

Reimer herself thinks that there may be meaning indeterminacy with names like this (2004: 628). I think that she is right. These names are reminiscent of the names of authors like ‘Shakespeare,’ discussed earlier (6.2.8). An author’s name seems to have a double life so that there may often be no determinate fact of the matter which meaning a token of the name has. Reimer’s nice example seems to show that this indeterminacy is more widespread than the names of authors.

### 6.3.5 Conclusion

I have presented a theory of the meaning of proper names that rests on my earlier theory of their reference. I claimed that that theory of reference was a relatively straightforward development of Kripke’s revolutionary idea (set in a naturalistic and anti-Cartesian framework). In the ’70s, my theory of meaning also struck me as a straightforward development, but it seems that I was wrong: the received view is that *direct reference* is the straightforward development. Yet, as I have argued elsewhere (1989, 1996, 2013a), direct reference faces seemingly overwhelming problems and is theoretically unmotivated. In contrast, my theory of meaning is theoretically motivated and faces no such problems. Furthermore, its case for meanings as causal modes has never been answered. This is not to say, of course, that the theory has all the details right. But, as with the theory of reference, we have good reason to suppose that such adjustments as may be necessary will not be large and will be in terms of the same reality of designating- or denoting-chains.

I conclude that the meaning of proper names should not still seem so problematic. I earlier concluded that the reference of proper names should not still seem so problematic. So proper names should not still seem so problematic.<sup>68</sup>

## References

- Abbott, B. 2012. “Names.” In G. Russell and D. G. Fara, eds, *The Routledge Companion to Philosophy of Language*. New York: Routledge.
- Ackerman, F. 1979. “Proper Names, Propositional Attitudes and Non-Descriptive Connotations.” *Philosophical Studies* 35: 55–69.

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- Almog, J. 1984. "Semantic Anthropology." In P. A. French, T. E. Uehling, Jr., and H. K. Wettstein, eds, *Midwest Studies in Philosophy Vol. 9, Causation and Causal Theories*. Minneapolis: University of Minnesota Press.
- Almog, J. 1985. "Form and Content." *Noûs* 19: 603–16.
- Almog, J. 2012. "Referential Uses and the Foundations of Direct Reference." In J. Almog and P. Leonardi, eds, *Having in Mind: The Philosophy of Keith Donnellan*. New York: Oxford University Press.
- Almog, J. and P. Leonardi, eds. 2012. *Having in Mind: The Philosophy of Keith Donnellan*. New York: Oxford University Press.
- Antony, L. 2008. "Meta-Linguistics: Methodology and Ontology in Devitt's *Ignorance of Language*." *Australasian Journal of Philosophy* 86: 643–56.
- Bach, K. 1987. *Thought and Reference*. Oxford: Clarendon Press.
- Baker, L. R. 1982. "Underprivileged Access." *Noûs* 16: 227–42.
- Barwise, J. and J. Perry 1983. *Situations and Attitudes*. Cambridge, Mass.: MIT Press.
- Bianchi, A. 2011. "Reference and Descriptions." In M. Sbisà, J. O. Östman, and J. Verschuereen, eds, *Philosophical Perspectives for Pragmatics*. Amsterdam: John Benjamins.
- Bianchi, A. 2012. "Two Ways of Being a (Direct) Referentialist." In J. Almog and P. Leonardi, eds, *Having in Mind: The Philosophy of Keith Donnellan*. New York: Oxford University Press.
- Blackburn, T. 1988. "The Elusiveness of Reference." In P. A. French, T. E. Uehling, Jr., and H. K. Wettstein, eds, *Midwest Studies in Philosophy Vol. 12, Realism and Antirealism*. Minneapolis: University of Minnesota Press.
- Block, N. 1986. "Advertisement for a Semantics for Psychology." In P. A. French, T. E. Uehling, Jr., and H. K. Wettstein, eds, *Midwest Studies in Philosophy Vol. 10, Studies in the Philosophy of Mind*. Minneapolis: University of Minnesota Press.
- Burge, T. 2012. "Referring *De Re*." In J. Almog and P. Leonardi, eds, *Having in Mind: The Philosophy of Keith Donnellan*. New York: Oxford University Press.
- Chomsky, N. 1986. *Knowledge of Language: Its Nature, Origin, and Use*. New York: Praeger Publishers.
- Chomsky, N. 1996. *Powers and Prospects: Reflections on Human Nature and the Social Order*. Boston: South End Press.
- Collins, J. 2008a. "Knowledge of Language Redux." *Croatian Journal of Philosophy* 8: 3–43.
- Collins, J. 2008b. "A Note on Conventions and Unvoiced Syntax." *Croatian Journal of Philosophy* 8: 241–7.
- Devitt, M. 1972. "The Semantics of Proper Names: A Causal Theory." Harvard PhD dissertation.
- Devitt, M. 1974. "Singular Terms." *Journal of Philosophy* 71: 183–205.
- Devitt, M. 1980. "Brian Loar on Singular Terms." *Philosophical Studies* 37: 271–80.
- Devitt, M. 1981a. *Designation*. New York: Columbia University Press.
- Devitt, M. 1981b. "Donnellan's Distinction." In P. A. French, T. E. Uehling, Jr., and H. K. Wettstein, eds, *Midwest Studies in Philosophy Vol. 6, The Foundations of Analytic Philosophy*. Minneapolis: University of Minnesota Press.
- Devitt, M. 1989. "Against Direct Reference." In P. A. French, T. E. Uehling, Jr., and H. K. Wettstein, eds, *Midwest Studies in Philosophy Vol. 13, Contemporary Perspectives in the Philosophy of Language II*. Notre Dame: University of Notre Dame Press.
- Devitt, M. 1996. *Coming to Our Senses*. Cambridge: Cambridge University Press.



- Devitt, M. 2001. "A Shocking Idea about Meaning." *Revue Internationale de Philosophie* 208: 449–72.
- Devitt, M. 2003. "Linguistics is not Psychology." In A. Barber, ed., *Epistemology of Language*. Oxford: Oxford University Press.
- Devitt, M. 2004. "The Case for Referential Descriptions." In M. Reimer and A. Bezuidenhout, eds, *Descriptions and Beyond*. Oxford: Clarendon Press.
- Devitt, M. 2005. "Rigid Application." *Philosophical Studies* 125: 139–65.
- Devitt, M. 2006a. *Ignorance of Language*. Oxford: Clarendon Press.
- Devitt, M. 2006b. "Responses to the Rijeka Papers." *Croatian Journal of Philosophy* 6: 97–112.
- Devitt, M. 2007. "Referential Descriptions and Conversational Implicatures." *European Journal of Analytic Philosophy* 3: 7–32.
- Devitt, M. 2008a. "Explanation and Reality in Linguistics." *Croatian Journal of Philosophy* 8: 203–31.
- Devitt, M. 2008b. "A Response to Collins' Note on Conventions and Unvoiced Syntax." *Croatian Journal of Philosophy* 8: 249–55.
- Devitt, M. 2008c. "Methodology in the Philosophy of Linguistics." *Australasian Journal of Philosophy* 86: 671–84.
- Devitt, M. 2008d. "Reference Borrowing: A Response to Dunja Jutrović." *Croatian Journal of Philosophy* 8: 361–6.
- Devitt, M. 2009. "Psychological Conception, Psychological Reality." *Croatian Journal of Philosophy* 9: 35–44.
- Devitt, M. 2011a. "Linguistic Knowledge." In J. Bengson and M. Moffett, eds, *Knowing How: Essays on Knowledge, Mind and Action*. Oxford: Oxford University Press.
- Devitt, M. 2011b. "Methodology and the Nature of Knowing How." *Journal of Philosophy* 108: 205–18.
- Devitt, M. 2011c. "No Place for the A Priori." In M. J. Shaffer and M. L. Veber, eds, *What Place for the A Priori?* Chicago and La Salle: Open Court Publishing Company.
- Devitt, M. 2012a. "Whither Experimental Semantics?" *Theoria* 27: 5–36.
- Devitt, M. 2012b. "The Role of Intuitions." In G. Russell and D. G. Fara, eds, *The Routledge Companion to Philosophy of Language*. New York: Routledge.
- Devitt, M. 2013a. "Still Against Direct Reference." In R. Schantz, ed., *Prospects for Meaning*. Berlin: De Gruyter.
- Devitt, M. 2013b. "The Myth of the Problematic *De Se*." In N. Feit and A. Capone, eds, *Attitudes De Se: Linguistics, Epistemology, Metaphysics*. Stanford: CSLI Publications.
- Devitt, M. 2013c. "What Makes a Property 'Semantic'?" In A. Capone, F. Lo Piparo, and M. Carapezza, eds, *Perspectives on Pragmatics and Philosophy*. Champaign, Ill.: Springer.
- Devitt, M. 2014. "Lest Auld Acquaintance Be Forgotten." *Mind & Language* 29: 475–84.
- Devitt, M. forthcoming. *Overlooking Conventions: The Trouble with Linguistic Pragmatism*.
- Devitt, M. and K. Sterelny 1987. *Language and Reality: An Introduction to the Philosophy of Language*. Oxford: Blackwell Publishers.
- Devitt, M. and K. Sterelny 1999. *Language and Reality: An Introduction to the Philosophy of Language*. 2nd edition. Oxford: Blackwell Publishers.
- Dickie, I. 2011. "How Proper Names Refer." *Proceedings of the Aristotelian Society* 101: 43–78.
- Donnellan, K. S. 1966. "Reference and Definite Descriptions." *Philosophical Review* 75: 281–304.

- Donnellan, K. S. 1968. "Putting Humpty Dumpty Together Again." *Philosophical Review* 77: 203–15.
- Donnellan, K. S. 1972. "Proper Names and Identifying Descriptions." In D. Davidson and G. Harman, eds, *Semantics of Natural Language*. Dordrecht: D. Reidel.
- Donnellan, K. S. 1974. "Speaking of Nothing." *Philosophical Review* 83: 3–31.
- Donnellan, K. S. 1989. "Belief and the Identity of Reference." In P. A. French, T. E. Uehling, Jr., and H. K. Wettstein, eds, *Midwest Studies in Philosophy Vol. 13, Contemporary Perspectives in the Philosophy of Language II*. Notre Dame: University of Notre Dame Press.
- Dummett, M. 1973. *Frege: Philosophy of Language*. London: Duckworth.
- Dwyer, S. and P. Pietroski 1996. "Believing in Language." *Philosophy of Science* 63: 338–73.
- Evans, G. 1973. "The Causal Theory of Names." *Proceedings of the Aristotelian Society*, Supplementary Volume 47: 187–208.
- Evans, G. 1979. "Reference and Contingency." *Monist* 62: 161–89.
- Field, H. 1973. "Theory Change and the Indeterminacy of Reference." *Journal of Philosophy* 70: 462–81.
- Fine, A. 1975. "How to Compare Theories: Reference and Change." *Nous* 9: 17–32.
- Fodor, J. A. 1975. *The Language of Thought*. New York: Thomas Y. Crowell.
- Fodor, J. A., T. G. Bever, and M. F. Garrett 1974. *The Psychology of Language: An Introduction to Psycholinguistics and Generative Grammar*. New York: McGraw-Hill Book Company.
- Glezakos, S. 2009. "Public Proper Names and Idiolectal Identifying Descriptions." *Linguistics and Philosophy* 32: 317–26.
- Jeshion, R. 2004. "Descriptive Descriptive Names." In M. Reimer and A. Bezuidenhout, eds, *Descriptions and Beyond*. Oxford: Clarendon Press.
- Jeshion, R. 2010. "Singular Thought: Acquaintance, Semantic Instrumentalism, and Cognitivism." In R. Jeshion, ed., *New Essays on Singular Thought*. Oxford: Oxford University Press.
- Jutronic, D. 2006. "Is Reference Borrowing a Causal Process?" *Croatian Journal of Philosophy* 6: 41–9.
- Jutronic, D. 2008. "Reference Borrowing and the Role of Descriptions." *Croatian Journal of Philosophy* 8: 349–60.
- Kaplan, D. 1978. "Dthat." In P. Cole, ed., *Syntax and Semantics 9: Pragmatics*. New York: Academic Press.
- Kaplan, D. 1989a. "Demonstratives: An Essay on the Semantics, Logic, Metaphysics, and Epistemology of Demonstratives and Other Indexicals." In J. Almog, J. Perry, and H. Wettstein, eds, *Themes from Kaplan*. Oxford: Oxford University Press.
- Kaplan, D. 1989b. "Afterthoughts." In J. Almog, J. Perry, and H. Wettstein, eds, *Themes from Kaplan*. Oxford: Oxford University Press.
- Kaplan, D. 2012. "An Idea of Donnellan." In J. Almog and P. Leonardi, eds, *Having in Mind: The Philosophy of Keith Donnellan*. New York: Oxford University Press.
- Kripke, S. A. 1972. "Naming and Necessity." In D. Davidson and G. Harman, eds, *Semantics of Natural Language*. Dordrecht: D. Reidel.
- Kripke, S. A. 1979a. "Speaker's Reference and Semantic Reference." In P. A. French, T. E. Uehling, Jr., and H. K. Wettstein, eds, *Contemporary Perspectives in the Philosophy of Language*. Minneapolis: University of Minnesota Press.
- Kripke, S. A. 1979b. "A Puzzle about Belief." In A. Margalit, ed., *Meaning and Use*. Dordrecht: D. Reidel.

- Kripke, S. A. 1980. *Naming and Necessity*. Cambridge, Mass.: Harvard University Press.
- Laurence, S. 1996. "A Chomskian Alternative to Convention-based Semantics." *Mind* 105: 269–301.
- Laurence, S. 1998. "Convention-based Semantics and the Development of Language." In P. Carruthers and J. Boucher, eds, *Language and Thought: Interdisciplinary Themes*. Cambridge: Cambridge University Press.
- Laurence, S. 2003. "Is Linguistics a Branch of Psychology?" In A. Barber, ed., *Epistemology of Language*. Oxford: Oxford University Press.
- Lepore, E. and B. Loewer 1986. "Solipsistic Semantics." In P. A. French, T. E. Uehling, Jr., and H. K. Wettstein, eds, *Midwest Studies in Philosophy Vol. 10, Studies in the Philosophy of Mind*. Minneapolis: University of Minnesota Press.
- Lewis, D. K. 1969. *Convention: A Philosophical Study*. Cambridge, Mass.: Harvard University Press.
- Loar, B. 1976. "The Semantics of Singular Terms." *Philosophical Studies* 30: 353–77.
- Lycan, W. G. 1985. "The Paradox of Naming." In B. K. Matilal and J. L. Shaw, eds, *Analytical Philosophy in Comparative Perspective*. Dordrecht: D. Reidel.
- McGinn, C. 1982. "The Structure of Content." In A. Woodfield, ed., *Thought and Object*. Oxford: Clarendon Press.
- McKay, T. 1981. "On Proper Names in Belief Ascriptions." *Philosophical Studies* 39: 287–303.
- Marcus, R. B. 1961. "Modalities and Intensional Language." *Synthese* 13: 303–22.
- Mercier, A. 1999. "On Communication-Based De Re Thought, Commitments De Dicto, and Word-Individuation." In K. Murasugi and R. Stainton, eds, *Philosophy and Linguistics*. Boulder: Westview Press.
- Putnam, H. 1973. "Meaning and Reference." *Journal of Philosophy* 70: 699–711.
- Putnam, H. 1975. *Mind, Language and Reality: Philosophical Papers, Volume 2*. Cambridge: Cambridge University Press.
- Putnam, H. 1978. *Meaning and the Moral Sciences*. London: Routledge & Kegan Paul.
- Putnam, H. 2001. "Reply to Devitt." *Revue Internationale de Philosophie* 208: 495–502.
- Rauti, A. 2012. "Multiple Groundings and Deference." *Philosophical Quarterly* 62: 317–36.
- Reimer, M. 2004. "Descriptively Introduced Names." In M. Reimer and A. Bezuidenhout, eds, *Descriptions and Beyond*. Oxford: Clarendon Press.
- Salmon, N. 1986. *Frege's Puzzle*. Cambridge, Mass.: MIT Press.
- Santambrogio, M. ms. "Paderewski's Two Names."
- Schiffer, S. 1979. "Naming and Knowing." In P. A. French, T. E. Uehling, Jr., and H. K. Wettstein, eds, *Contemporary Perspectives in the Philosophy of Language*. Minneapolis: University of Minnesota Press.
- Schwartz, S. P. 1977. "Introduction." In S. P. Schwartz, ed., *Naming, Necessity, and Natural Kinds*. Ithaca, NY: Cornell University Press.
- Searle, J. R. 1983. *Intentionality: An Essay in the Philosophy of Mind*. Cambridge: Cambridge University Press.
- Slobodchikoff, C. N. 2002. "Cognition and Communication in Prairie Dogs." In M. Bekoff, C. Allen, and G. M. Burchardt, eds, *The Cognitive Animal: Empirical and Theoretical Perspectives on Animal Cognition*. Cambridge, Mass.: MIT Press.
- Soames, S. 1985. "Lost Innocence." *Linguistics and Philosophy* 8: 59–71.
- Soames, S. 1987. "Direct Reference, Propositional Attitudes, and Semantic Content." *Philosophical Topics* 15: 47–87.

- Soames, S. 2002. *Beyond Rigidity: The Unfinished Semantic Agenda of Naming and Necessity*. New York: Oxford University Press.
- Soames, S. 2003. *Philosophical Analysis in the 20th Century*, vol. 2. Princeton: Princeton University Press.
- Stanley, J. and T. Williamson 2001. "Knowing How." *Journal of Philosophy* 98: 411–44.
- Strawson, P. F. 1959. *Individuals: An Essay in Descriptive Metaphysics*. London: Methuen.
- Sullivan, A. 2010. "Millian Externalism." In R. Jeshion, ed., *New Essays on Singular Thought*. Oxford: Oxford University Press.
- Tye, M. 1978. "The Puzzle of Hesperus and Phosphorus." *Australasian Journal of Philosophy* 56: 219–24.
- Wagner, S. J. 1986. "California Semantics Meets the Great Fact." *Notre Dame Journal of Formal Logic* 27: 430–55.
- Wettstein, H. 1986. "Has Semantics Rested on a Mistake?" *Journal of Philosophy* 83: 185–209.
- Wettstein, H. 2012. "On Referents and Reference Fixing." In R. Schantz, ed., *Prospects for Meaning*. Berlin: De Gruyter.

