Speaker's Reference, Semantic Reference, and Intuition*

Richard Kimberly Heck

Department of Philosophy, Brown University

One of the many arguments that Saul Kripke gives against the description theory of names involves a now famous example about Kurt Gödel and the unfortunate Schmidt:

> Let's suppose someone says that Gödel is the man who proved the incompleteness of arithmetic.... In the case of Gödel that's practically the only thing many people have heard about him—that he discovered the incompleteness of arithmetic. Does it follow that [for such people] whoever discovered the incompleteness of arithmetic is the referent of "Godel"? ... Suppose that Gödel was not in fact the author of this theorem. A man named "Schmidt", whose body was found in Vienna under mysterious circumstances many years ago, actually did the work in question. His friend Gödel somehow got hold of the manuscript and it was thereafter attributed to Gödel. On the view in question, then, ... since the man who discovered the incompleteness of arithmetic is in fact Schmidt, we [who have heard nothing else about Gödel], when we talk about 'Gödel', are in fact always referring to Schmidt. But it seems to me that we are not. We simply are not. (Kripke, 1980, pp. 83–4)

The judgement Kripke reports here is often regarded as a paradigmatic case of an appeal to 'philosophical intuition',[1] and such appeals have

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[1] As Timothy Williamson (2004; 2007; 2016) keeps complaining, it is none too clear what 'intuitions' are supposed to be. (See also Cappelen (2012).) I'll assume here, minimally, that they are relatively spontaneous responses and, most importantly, are to

been the subject of much recent debate. This particular one attracted the attention, some years ago, of Edouard Machery, Ron Mallon, Shaun Nichols, and Stephen Stich (MMN&S), who were then at the leading edge of the emerging 'experimental philosophy' movement. Their paper "Semantics, Cross-Cultural Style" reported the results of experiments that show, or so they claimed, that such intuitions vary cross-culturally. In particular, although 'Westerners' tend to agree with Kripke, 'East Asians' tend to disagree.[2]

More precisely, MMN&S presented experimental subjects with the following version of Kripke's Gödel–Schmidt case:

> Suppose that John has learned in college that Gödel is the man who proved an important mathematical theorem, called the incompleteness of arithmetic. John is quite good at mathematics and he can give an accurate statement of the incompleteness theorem, which he attributes to Gödel as the discoverer. But this is the only thing that he has heard about Gödel. Now suppose that Gödel was not the author of this theorem. A man called "Schmidt", whose body was found in Vienna under mysterious circumstances many years ago, actually did the work in question. His friend Gödel somehow got hold of the manuscript and claimed credit for the work, which was thereafter attributed to Gödel. Thus, he has been known as the man who proved the incompleteness of arithmetic. Most people who have heard the name "Gödel" are like John; the claim that Gödel discovered the incompleteness theorem is the only thing they have ever heard about Gödel. (Machery et al., 2004, p. B6)

MMN&S then asked their subjects the following question:

> When John uses the name "Gödel", is he talking about:
>
> (A) the person who really discovered the incompleteness of arithmetic? or
>
> (B) the person who got hold of the manuscript and claimed credit for the work?
>
> (Machery et al., 2004, p. B6)

be distinguished from the sorts of conclusions one reaches as a result of argument. This is in the ballpark of how Jennifer Nagel (2012, p. 498) characterizes intuitions.

[2] The difference here is supposed to be cultural, not geographical, but I shall use these labels, as they are common in the literature.

What they found was that 58% of Westerners gave Kripke's preferred answer (B), whereas only 29% of East Asians did.[3] MMN&S take this to show that East Asians tend to have 'descriptivist intuitions' whereas Westerners tend to have 'Kripkean intuitions', a fact that is supposed to "raise[] questions about the nature of the philosophical enterprise of developing a theory of reference" (Machery et al., 2004, p. B1).

Kirk Ludwig seems to have been the first to observe (in print) that the question MMN&S asked their subjects—namely, whom John is "talking about"—appears to be ambiguous:

> For anyone at all familiar with work in the philosophy of language, it is immediately evident that the question does not clearly distinguish between two things: whom John intends to be talking about (or speaker's reference) and who the name John uses refers to, taken literally in the language he intends to be speaking (semantic reference). (Ludwig, 2007, p. 150)

A similar point is made by Max Deutsch (2009, pp. 453–7),[4] and this sort of worry tends, in my experience, to be very widespread among working philosophers of language.

The distinction in question is perhaps most familiar from Kripke's paper "Speaker's Reference and Semantic Reference" (Kripke, 1977). But, while that paper does introduce this now common terminology, and although the distinction is only developed in detail there, it appears already in a footnote in *Naming and Necessity*, which was first published in 1972. Kripke there gives the following example:

> Two men glimpse someone at a distance and think they recognize him as Jones. "What is Jones doing?" "Raking the leaves." If the distant leaf-raker is actually Smith, then in some sense they are *referring* to Smith, even though they both use "Jones" as a name of Jones. (Kripke, 1980, p. 25, fn. 3, emphasis original)

As Kripke would later put it: When the men use the name "Jones" in this case, the *semantic* referent is Jones, but the *speaker's* referent is Smith. Suppose, however, that we were to ask, in ordinary colloquial English, whom these men are "talking about". Both the answers "Smith"

[3] Machery (2012, p. 40) reports the results this way in a later paper. MMN&S reported their results somewhat differently.

[4] Deutsch (2015) has since published a book-length treatment of these issues.

and "Jones" seem reasonable. The same is true of the probe question in MMN&S's experiment: Both answers seem reasonable. I can see why someone might answer either way. Indeed, I can see why I myself might be inclined to answer either way.

If that is right, then MMN&S's results do not show that East Asians are more likely to have 'descriptivist intuitions' about *semantic* reference. The difference may be due to East Asians' being more likely to interpret the probe question as asking about *speaker's* reference. If so, however, MMN&S's results do not bear at all upon the judgement Kripke makes about his example, which concerns only what the semantic reference of "Gödel" is.

Machery and his collaborators have offered a number of different replies to this objection:

1. The probe question isn't ambiguous.

2. Even if the probe question is ambiguous, it can be rephrased to eliminate the ambiguity.

3. Even if the ambiguity can't be eliminated, the vignette can be rephrased so as to neutralize the effect of the ambiguity.

I will be arguing here, in corresponding sections, that these replies are ineffective. Then, in section 4, I'll argue that 'intuition' plays no significant role in Kripke's argument.

The larger lesson, however, will concern the role that first-order philosophy should, but also should *not*, play in the design of such experiments and in the evaluation of their results. As we shall see, Machery *et al*. sometimes seem to be supposing that their subjects will appreciate philosophical subtleties that were unknown just a few decades ago.

1 Is the Probe Question Ambiguous?

Machery and Stich (2012) have argued that the probe question is not ambiguous in the way Ludwig and others have claimed it is. They first make the following two points:

(i) One can only ask about the speaker's reference of particular *uses* of a given expression, not about the speaker's reference of an expression as such.

(ii) What the speaker's reference is, on a given occasion of use, depends upon the speaker's intentions.

That much should be uncontroversial. They then argue as follows:

> At the end of the vignette, participants are asked who John is talking about "when he uses the name 'Gödel'". Since no specific utterance is mentioned and no contextual information is provided that would enable participants to determine John's communicative intention, it is hard to see how participants could understand the question to be about the speaker's reference of John's utterance of "Gödel". Rather, we submit, on the only plausible interpretation of the question it is asking about the reference of "Gödel" *qua* type—that is, it is asking about the semantic reference of the term in John's language. If this is right, then. . . the question in the probes is *not* ambiguous. (Machery and Stich, 2012, p. 506, emphasis original)

Machery and Stich's argument in no way depends upon the fact that the expression in question, "Gödel", is a proper name.[5] Rather, it is supposed to be because "no specific utterance is mentioned and no contextual information is provided" that the probe question has to be read as concerning "Gödel" *qua* type. If so, however, then wording that shares these two features should force the corresponding reading in the case of other sorts of expressions. As we are about to see, however, it does not.

Recall Keith Donnellan's distinction between 'referential' and 'attributive' uses of definite descriptions. Suppose you are at a party and see someone sipping bubbly liquid from a tulip-shaped glass. "Who is the person drinking champagne?" you ask your friend. As it happens, the person you noticed is not drinking champagne but just sparkling water. Nonetheless, Donnellan (1966, p. 287) insists, you may still have "asked a question about a particular person", the person you noticed, "a question it is possible for someone to answer". And this is so even if there is someone else at the party who really is drinking champagne. The mere fact that this other person satisfies the condition you specified—being a person who is drinking champagne—does not make your question one about them.Does Donnellan's example itself invite an appeal to 'intuition'? Certainly people are often happy just to describe the example and

[5] Of course, this *could* be relevant, but we would need to be told how and why. One cannot simply say that the argument was only intended to apply to cases involving proper names.

then to say something like: It seems reasonable to think that the speaker has referred to the man drinking sparkling water. And it is reasonable to think that. But we need not, and should not, rest there. What's really driving Donnellan are readily observable facts about how communication in such circumstances proceeds, facts he mentions several times in his paper. If someone says, "The man drinking champagne is a famous actor", then it's easy to see how you could come to believe, and even to know, that he is a famous actor. (That's especially true if you are under the same misimpression as the speaker, but it is also true if you know that your interlocutor wrongly believes (or thinks you wrongly believe) that the man is drinking champagne.) The simple thought is then that, if there is no sense in which the speaker has referred to that man, and said of him that he is a famous actor, then it would be irrational for you now to believe that *that* man is a famous actor. (Kripke's response is—to use Grice's terminology—that it's enough for the speaker to have *meant* that he is a famous actor.) By contrast, if you had merely smelled champagne and asked the same question—maybe you want to ask whoever is drinking champagne to share—then you would not have been asking about any particular person but just about whoever it was who was drinking champagne. This is what Donnellan calls an 'attributive' use. The first is what he calls a 'referential' use.

It remains controversial how we should theorize this phenomenon. Kripke (1977) famously argued that Donnellan's distinction was just a special case of the distinction between speaker's reference and semantic reference: The speaker's reference, in the first of my examples, is the person you noticed; the semantic reference is the person actually drinking champagne. Other philosophers (e.g. Stalnaker, 1970, §IV; Devitt, 1981, §2.7) have argued that this is a genuine ambiguity: that the *semantic* reference, in that same example, is the person you noticed. But we do not need to resolve this dispute here. For our purposes, the crucial point is just that Donnellan's distinction concerns *uses* of descriptions. All sides are agreed that it would make no sense to say that the *expression* (qua type) "the person drinking champagne" refers, in the language you speak, to the person you noticed.[6]

Consider, then, the following story:

[6] Thus, Devitt (2004, p. 281) writes that "the core of the referential meaning of a description token is its reference-determining relation to the particular object that the speaker has in mind in using the description". So different *tokens* will refer to different objects, depending upon which object the speaker has in mind when uttering that token.

> Grace is a ten-year old girl who lives at the Laughing Pines apartments with her family. Grace is *obsessed* with baseball. And all summer long now, her neighbor Bob has been regaling her and some of the other kids with stories about how he used to be a professional baseball player. In fact, however, and unbeknownst to Grace, Bob never even played amateur baseball. He just enjoys the company of the children and is, perhaps, a bit delusional. By coincidence, however, there is an elderly woman, Lily, who also lives at Laughing Pines and who played for several years in the All-American Girls Professional Baseball League.[7] Grace, though, has never met Lily.
>
> Suppose that we now ask the following question:
>
> When Grace uses the phrase "the baseball player who lives at Laughing Pines", is she talking about:
>
> (A) Bob, who never played professional baseball? or
>
> (B) Lily, who did once play professional baseball?

It seems to me that answer (A) is completely reasonable. If so, however, then the probe question in this case does not have to be heard as being about the reference of the mentioned phrase *qua* type. If it did, then the only reasonable answer would be (B), since it is only of a particular *use* of the mentioned phrase that it would make sense to say that it referred to Bob. And yet, not only do this story and question have the two features that Machery and Stich emphasize—no specific use is mentioned, and no contextual information is provided—but the probe question uses, in relevant respects, exactly the same wording as does MMN&S's probe question.

One might object that it is an empirical issue how ordinary speakers would interpret the question just mentioned. Haven't we learned not to rely upon 'intuition' in such cases? Shouldn't we instead do an experiment? I myself think it is just obvious that answer (A) is reasonable—at least as reasonable as answer (B). Moreover, the argument from Machery

[7] The league was founded in 1943, in large part because so many mens' teams had been disbanded after the United States entered into the Second World War. The league was quite successful for a time but was itself disbanded in 1954. There is now an exhibit at the Baseball Hall of Fame dedicated to the women who played in *A League of Their Own*, that being the title of a documentary and then a feature film about the league and the women who played in it.

and Stich to which I'm responding was entirely a priori. Nonetheless, I did conduct an experiment along these lines.[8] I presented the above story to thirty-nine subjects. All but one of them chose answer (A): Bob. Only one subject, that is to say, answered in a way that is compatible with their having interpreted the probe question as "asking about the reference of ['the baseball player who lives at Laughing Pines'] *qua* type" (Machery and Stich, 2012, p. 506).

Machery and Stich, I submit, make far too much of fine details of how their story and probe question are phrased.[9] The mere *presence* of the features they emphasize cannot force the question to be heard as one concerning an expression *qua* type. One has, minimally, to notice those features—and then one has to appreciate their alleged significance. But for someone who is just reading a story and trying to answer questions about it, there's no reason to suspect that such features would even register. Indeed, neither Ludwig nor Deutsch would seem to have picked up on these features—or, at least, made of them what Machery and Stich insist they must. Ludwig (2007, p. 150), one might recall, regards the claim that the probe question is ambiguous as one that "anyone at all familiar with work in the philosophy of language" will find "immediately evident". And I agree with him, even still.

It is really not that difficult to see how speakers might come to hear this sort of question as concerning uses (and so, potentially, as concerning speaker's reference). It is certainly true that no specific utterance is mentioned in the probe question in my experiment. But the question does ask to whom Grace refers when she *uses* a certain phrase.[10] The natural understanding of the question thus seems to be as a sort of generic: It invites us to consider typical uses of the phrase that Grace might make and to report to whom she would then be referring. And there is actually quite a lot of information available to someone attempting to imagine such a use. It's easy to imagine what kind of thing Grace might say to her friends, and why. Try it.

Something similar is true of MMN&S's version of the Gödel–Schmidt

[8] See the appendix to original paper (Heck, 2018) for the details.

[9] Compare DeRose (2011, pp. 93–4) on 'shallow processing'.

[10] We'll discuss re-wordings of the question in the next section. But note that the question actually *has* to ask about uses: The phrase in question is context-sensitive—it uses the present tense—so the phrase itself refers to no particular object. Even if we were asking about *semantic* reference, then, we would have to be asking about (potential) uses of the phrase.

case.[11] MMN&S's probe question generalizes over uses and very much has the feel of a generic. It would thus be entirely natural for someone trying to answer that question to imagine a typical use John might make of the name and to respond on that basis. But the only thing John has ever heard about Gödel, we are told, is that he proved the incompleteness theorem. So when I try to imagine John saying something about 'Gödel', the sorts of things that come to mind are very often 'about' the person who proved that theorem, e.g., "Gödel must have studied really hard".[12]

So, I submit, it's easy to see why someone might want to say that, when John uses the name "Gödel", he will (in a typical case) be 'talking about' Schmidt: the person who actually proved the incompleteness theorem. To borrow Donnellan's language, that is who he will 'have in mind'.[13] But anyone who answered the probe question that way, and did so for those sorts of reasons, would be making a claim about speaker's reference, not semantic reference.

There is another phenomenon that is relevant here. In a footnote in *Naming and Necessity*, Kripke notes that there is a certain sort of use of proper names that one might think conforms to the description theory. He reports some people as having wanted to make the following objection:

> . . . [I]f we say, "Gödel proved the incompleteness of arithmetic", we are, of course, referring to Gödel. But, if we say, "Gödel relied on a diagonal argument in this step of the proof", don't we here, perhaps, refer to *whoever proved the theorem*? . . . By analogy to Donnellan's usage for descriptions, this might be called an 'attributive' use of proper names. If this is so,

[11] And it is at least arguable that, even if we want to ask a question about the *semantic* reference of "Gödel", we must still ask about uses of this expression. It is widely held nowadays that proper names are general terms (see e.g. Gray, 2014, Fara, 2015, and references contained therein): that proper names do not denote one particular individual but many individuals, e.g., all the people named "Gödel". On this view, which seems seems to originate with Burge (1973), it is only certain uses of "Gödel" that refer, even in the semantic sense, to particular, though different, Gödels. If that is the correct view of names, then the probe question must be about uses of the name, not about the name itself, even if it concerns semantic reference.

[12] As Sytsma and Livengood (2011, pp. 320–1) note, such judgements might vary depending upon exactly we imagine John saying. We'll explore the significance of this point in section 3.

[13] Vignolo and Domaneschi (2022, pp. 767–8) rightly point out that Donnellan uses this phrase only when the speaker is acquainted with the object. This does not, however, affect the argument here.

> then assuming the Godel–Schmidt story, the sentence "Gödel proved the incompleteness theorem" is false, but "Gödel used a diagonal argument in the proof" is (at least in some contexts) true, and the reference of the name "Gödel" is ambiguous. (Kripke, 1980, p. 85, fn. 36, emphasis original)

There really are attributive uses of names,[14] just as there really are attributive uses of descriptions. So it may be that some subjects regard John as 'talking about' Schmidt because they are construing his use of "Gödel" as attributive. It is an empirical question, of course—and one I do not know how to answer—how many, if any, subjects do suppose that John's typical uses of "Gödel" would be attributive. But it does not seem to me to be implausible that some should.

It's a different question, of course, how we philosophers should explain attributive uses of names. Kripke suggests we do so in terms of the distinction between speaker's reference and semantic reference:

> It is, perhaps, true that sometimes when someone uses the name "Gödel", his main interest is in whoever proved the theorem, and *perhaps*, in some sense, he 'refers' to him. I do not think that this case is different from the case of Smith and Jones. . . . If I mistake Jones for Smith, I may *refer*. . . to Jones when I say that Smith is raking the leaves; nevertheless I do not use "Smith" ambiguously, as a name sometimes of Smith and sometimes of Jones, but univocally as a name of Smith. (Kripke, 1980, p. 86, fn. 36, emphasis original)

On the other hand, one might think—as some people do in the case of definite descriptions—that the difference between 'referential' and 'attributive' uses of proper names is not merely pragmatic but semantic (e.g. Devitt, 1981, pp. 157–60).

Once again, however, it does not matter who is right here. Subjects who regard John's (typical) uses of "Gödel" as attributive, and give the non-Kripkean answer for that reason, are not thereby disagreeing with Kripke's judgement about the Gödel–Schmidt case: If the distinction

[14] Devitt (2011, p. 428, n. 9) mentions this sort of phenomenon, too, but he ties it specifically to names of authors. Clearly, however, the phenomenon is more general. One could, e.g., use the name of a warrior attributively when talking about the plans for a certain battle. Just how widespread the phenomenon might be is not so clear, but I'd speculate that it can arise whenever a certain act is associated strongly enough with a given agent.

between referential and attributive uses of names is, indeed, to be understood as a sort of ambiguity, then Kripke's theory applies only to referential uses. If, on the other hand, Kripke is right, as I would argue he is, then subjects who regard John's typical uses of "Gödel" as being attributive are construing the probe question as being about speaker's reference.

The question MMN&S asked their subjects is thus readily interpreted as concerning speaker's reference—or, at least, Machery and Stich have given us no reason to believe otherwise. The reason there is a difference between the responses of Western and East Asian subjects may, therefore, have more to do with how those subjects are interpreting the question MMN&S asked them than with their intuitions about semantic reference.

2 Can the Probe Question Be Rephrased?

The obvious strategy for dealing with this problem is to try to rephrase the probe question so as to force the appropriate reading. This is precisely what Machery, Justin Sytsma, and Deutsch (MS&D) attempt to do in their paper "Speaker's Reference and Cross-Cultural Semantics". Instead of asking their subjects the question mentioned above (see p. 2), they instead asked them this question:[15]

> When John uses the name "Gödel", *regardless of who he intends to be talking about*, he is *actually* talking about:
>
> (A) the person who really discovered the incompleteness of arithmetic;
>
> (B) the person who got hold of the manuscript and claimed credit for the work.
>
> (Machery et al., 2015, p. 69, emphasis original)

Re-phrasing the question this way had little effect on the results.

It is far from obvious, however, that this re-phrasing "should lead subjects to read the question as asking about semantic reference" (Machery et al., 2015, p. 69). MS&D concede, in fact, that "[i]t is sometimes possible to understand 'actually talking' as bearing on speaker's reference. . . " (Machery et al., 2015, p. 71). In response, they argue as follows:

[15] It is not clear from MS&D's text whether the emphasis was included in the question itself or whether thay have added it to mark the changes. It was presumably the latter, but it won't matter.

> Although it is true that asking whom a person is actually talking about might sometimes lead to a judgment about speaker's reference, there are cases in which it is quite unlikely to do so. In the Clarified Gödel Case in particular, if lay people grasp the distinction between speaker's reference and semantic reference and if our participants paid sufficient attention to the probe question at the end of the Clarified Gödel Case, then it seems unlikely that this probe question could have been understood as being about anything else but semantic reference. One feature of the probe question that supports this assessment is the contrastive nature of the probe question, along with the facts presented to participants in the background story: Our Clarified Gödel Case did not simply ask participants to say who they thought John was actually talking about in using "Gödel," but asked participants to say whom they thought John was "actually talking about" *in contrast to* whomever he may be "intending to talk about". (Machery et al., 2015, p. 71, emphasis original)

In the same spirit, Machery (2015, p. 75) elsewhere insists that the changes MS&D made to the probe question "... make it clear that participants should ignore the speaker's communicative intention and thus, by contrast, focus on whoever the proper name refers to according to the rules of English". What Machery means is presumably that the changes make it clear *to* participants that *they* should do these things. But do they?

MS&D's entire discussion seems to be insufficiently attentive to what the distinction between speaker's reference and semantic reference actually is. The distinction is not between the person about whom one *intends* to be speaking and the person about whom one *is* speaking ("actually", if you like). It is between the person about whom *someone* is speaking and the person to whom a particular *expression* refers, in the language of that same person. To put it differently, the relations have different terms: What we call "speaker's reference" is a relation between a *speaker* and an object; what we call "semantic reference" is a relation between a *linguistic expression* and an object.[16]

It therefore seems to me that remarks Deutsch once made about MMN&S's original example apply just as well to MS&D's modified exam-

[16] Both relations are probably more complicated. The latter makes reference also to a language: that of the speaker. But the gist of the point is nonetheless correct.

ple:

> To my ear, the vignette question. . . nearly forces a speaker's reference interpretation; it is a question about what *John* is doing with the name—making speaker's reference to the man (Schmidt) who actually discovered the proof—not a question about what *the name itself* is doing, which is, on a Kripkean causal–historical theory, semantically referring to the man who stole the proof. (Deutsch, 2009, p. 454, fn. 7, emphasis added)

The addition of the remark about intentions, and the emphatic use of "actually", do not affect this basic point.[17]

When this sort of situation arises in experimental psychology, the appropriate response is not just to modify the probe question but to verify experimentally that the modified question really does force the reading one wants. Thus, we might consider the following variant of Kripke's Smith–Jones case:

> One day, Alex and Toni were hanging out on their deck when they saw a person next door doing something in the yard. "What's Smith doing?" Alex asked. "I think he's skimming the pool", Toni said. Unbeknownst to Toni and Alex, however, it wasn't Smith at all but someone else, Jones, whom Smith had hired, and who just happened to look a lot like Smith.

We can then ask the obvious question:

> When Alex says "What's Smith doing?", regardless of whom Alex might intend to be talking about, whom is Alex actually talking about?
>
> (A) The Hired Pool Person (B) Their Neighbor

It seems to me, once again, that either answer would be reasonable: just as reasonable as in Kripke's original example. But, just to be sure, I presented forty-three subjects with the story just told and asked them

[17] One might object that Deutsch's claim can't be true: Many participants do answer "the person who got hold of the manuscript", and this can't plausibly understood as an answer to a question about speaker's reference. I'll argue below, however, that it can be so understood. See p. 18.

for their responses.[18] Not only did they not tend to choose answer (B), as they should have if they were intepreting the question as concerning semantic reference, but slightly more than half—twenty-three of the forty-three—chose answer (A). Obviously, however, that preference is not statistically significant.

Those of us who regularly teach this sort of material know from experience how easy it is to motivate the distinction between speaker's reference and semantic reference. The phenomenon is striking once one notices it. But getting students to make this distinction reliably is an entirely different matter. Initially, students are just puzzled—much as Donnellan's early readers were.[19] There is really no reason to believe that "lay people *grasp the distinction* between speaker's reference and semantic reference" (Machery et al., 2015, p. 71, emphasis added). To the contrary, experience teaches that "people without training in philosophy do not spontaneously grasp the distinction between speaker's reference and semantic reference" (Machery et al., 2015, p. 72), a suggestion MS&D attribute to Ludwig.

Of course, in some sense, ordinary speakers are 'sensitive' to this distinction: The same examples that serve to motivate it also serve to illustrate one of the ways in which we are all sensitive to it. But MS&D require more of their subjects. They need them first to notice "the contrastive nature of the probe question" and then to use it to resolve a potential ambiguity. Minimally, that is to say, MS&D need their subjects to appreciate that speakers' intentions are irrelevant to questions about semantic reference.[20] But do ordinary speakers appreciate this philo-

[18] These were students who visited the philosophy department's table at a college event for first-year students in October 2015 (and who, therefore, had little if any prior exposure to philosophy, and certainly not to philosophy of language). Those who were willing to participate were given a printed version of the story and were asked to circle their preferred answer. Thanks to Zachary Barnett and Tatiana Spottiswoode for helping me with this. It was quite the ice-breaker.

[19] This was illustrated by how students reacted to my survey question. There were several quizzical smirks. A few thought it was a trick question; a couple thought it was a riddle; several asked if there was a right answer. (They were told there wasn't and that we were just interested in how people responded to the question.) Many people thought for a long time before answering. Some even felt compelled to justify their answers. These were usually people who gave the 'speaker's reference' answer, and they usually said something like, "Well, they're *talking about* Jones".

[20] And what makes this all the more complex is that only certain sorts of intentions are irrelevant. The 'ambiguity' of proper names—the existence of lots of Gödels—suggests that the speaker's intentions *are* relevant to determining which Gödel is the reference of any particular use of "Gödel", though exactly what role these intentions are playing

sophical point? The mere fact that the subjects are, somehow or other, sensitive to the distinction between speaker's reference and semantic reference does not imply that they must be; hence, their being told that they should not consider whom John intends to talk about need not lead them to hear the probe question as asking about semantic reference.[21] And the experiment reported above shows that it does not so lead them. I conclude, therefore, that MS&D, like Machery and Stich, are making far too much of fine details of phrasing.

Extrapolating, I am inclined to be somewhat skeptical that *any* rephrasing of the probe question will both (i) resolve the ambiguity between speaker's reference and semantic reference and (ii) be intelligible to the ordinary speakers whose 'intuitions' such experiments are supposed to reveal. Although the distinction between speaker's reference and semantic reference is, as I have noted, grounded in everyday linguistic phenomena, it is deeply theoretical. It is not at all obvious exactly what distinction those phenomena illustrate. Kripke (1977), one will recall, essentially accuses everyone who had then contibuted to the literature on the referential–attributive distinction of having overlooked *his* distinction. And Kripke's distinction remains controversial. Its nature (how it should be drawn), its location (where it should be drawn), and its significance (why it should be drawn) are all hotly contested. Indeed, much of the controversy can be understood as concerning whether we even *need* the notion of semantic reference. But, whether we theorists need such a notion or not, it may well be that ordinary speakers really do not have much of a grasp of semantic reference at all.[22]

Of course, further ingenuity might produce an appropriate probe question in this case. One might suggest, for example, that it should be phrased in something like the way Deutsch (2009, p. 454) implicitly suggests:

> To whom does the name "Gödel" refer when John uses it?

depends upon how we resolve the issues mentioned in footnote 11.

[21] Related points have been made by both Bach (2002) and Lam (2010, p. 326) and are even noted by MS&D themselves (Machery et al., 2015, p. 72). Indeed, the slipperiness of the semantics–pragmatics distinction has been one of the overarching themes of philosophy of language in the twenty-first century.

[22] Similar remarks might be made about the distinction between saying and meaning, originally due to Grice (1989), of which Kripke's distinction is really just a special case. Deutsch (2009, pp. 460–4) has many sensible things to say about the relevance of this more general distinction to experimental philosophy. Much of it, as Deutsch is well aware, is strikingly similar to things Grice said when first introducing his distinction, though his target was ordinary language philosophy.

Or, incorporating MS&D's amendment:

> Setting aside any concern with whom John might be intending to talk about when he uses the name "Gödel", to whom does the name "Gödel" refer when John uses it?

But the latter question strikes me as beyond confusing,[23] and the former question still seems to be more about John's use of "Gödel" than it is about the name itself. In any event, the mere fact that these questions use the same sort of language we philosophers use does not imply that ordinary speakers will understand them the same way we do, nor even that they can understand them at all. Meta-linguistic discourse does not come naturally to people.

3 Can the Ambiguity in the Probe Question Be Neutralized?

MS&D are ultimately prepared to concede that their re-phrasing of the probe question may be ineffective (Machery et al., 2015, pp. 71–2). In the later parts of their paper, then, they report the results of two more experiments (Machery et al., 2015, §§3.7–3.8). The story itself is amended with the following material:

> One night, John is sitting in his room, reviewing for his mathematics exam by going over the proof of the incompleteness theorem. After a while, he says to his roommate, "Gödel probably got a huge number of awards from mathematical societies [for the proof of the incompleteness theorem]!"

The two experiments differed depending upon whether the bracketed material was included; this apparently made no difference. The probe question was then that from the original experiment. Presumably, using the modified one discussed in section 2 would have made little difference, as well.

What is the amendment supposed to accomplish? Suppose that some people are indeed interpreting the probe question as being about speaker's reference. But suppose further that we can force people who are interpreting it that way to give Kripke's answer. Then anyone who gives the other answer must be understanding the question as asking about semantic reference, in which case they must be expressing a genuine

[23] If it is irrelevant whom John might be intending to talk about, why do we care to whom the name refers when John uses it?

disagreement with Kripke. So MS&D's modified case is supposed to be one in which a subject can only give the non-Kripkean answer if they are interpreting the probe question as being about semantic reference:

> . . . [T]he speaker intends to be talking about the man who stole the theorem: Given the information provided in the vignette, only the man who stole the theorem can be viewed as having won a huge number of awards from mathematical societies. (Machery et al., 2015, p. 72)

That is: The speaker's reference is the person who stole the theorem. If so, then anyone who gives the other answer must understand the probe question as asking about semantic reference and so, again, must be disagreeing with Kripke.

MS&D report that, in response to their 'Award Winner Gödel Case', 74% of American subjects gave the Kripkean answer, whereas only 56% of Chinese subjects did so (Machery et al., 2015, p. 73). So a difference remains. It's worth noting, though, in passing, that the proportion who agree with Kripke in this case is quite a bit higher than it was in the original experiment, where the split was 58% vs 29%. If MS&D are correct that the change they made to the story has the effect they claim, then quite a few people in previous experiments were apparently understanding the question as being about speaker's reference, which is an interesting consequence in its own right.

In fact, however, it seems doubtful that the changes MS&D made actually do have the effect they claim. MS&D seem to expect their subjects to reason roughly as follows:

> When John said "Gödel probably got a huge number of awards from mathematical societies for the proof of the incompleteness theorem", he cannot have intended to refer to the person who actually proved the theorem, because that person was dead before it was ever published. He must have intended to refer to the person who stole and published the theorem,[24] since only that person can have won any awards for it.

[24] Or, perhaps better, to the person who is (wrongly) known as the author of the theorem. I'll speak, though, of the person who published it, for ease of exposition. (Note, by the way, that if the description theory were correct, Gödel would *not* be *wrongly* known as the author of the theorem. Schmidt would be correctly so known, though under the name "Gödel". Which is part of Kripke's point.)

This strikes me as really quite complicated reasoning. Should we really expect MS&D's subjects to register all these details of the story and to think though their implications in this way?[25] What's more worrying, though, is that the reasoning in question is fallacious. Even if it is true that John intends to refer to the person who published the theorem, it simply does not follow that John does not *also* intend to refer to the person who proved it. John, after all, has no reason to believe that the person who published the theorem is different from the person who proved it, and John does not know that the person who proved the theorem died before it was published by someone else. To be sure, it should be obvious to subjects that John does not intend to refer to *Schmidt*, but, once again, it just doesn't follow that John doesn't intend to refer to the person who proved the theorem. He may and, in fact, does.

Of course, MS&D may have expected their subjects to make one or more of the mistakes just mentioned. If so, however, they do not say why.

This case is in some ways analogous to David Kaplan's famous Carnap–Agnew case:

> Suppose that without turning and looking I point to the place on my wall which has long been occupied by a picture of Rudolf Carnap and I say: [That] is a picture of one of the greatest philosophers of the twentieth century. But unbeknownst to me, someone has replaced my picture of Carnap with one of Spiro Agnew. I think it would simply be wrong to argue an "ambiguity" in the demonstration, so great that it can be bent to my intended demonstratum. I have said of a picture of Spiro Agnew that it pictures one of the greatest philosophers of the twentieth century. (Kaplan, 1978, p. 239)

Kaplan is here arguing against the view that the referent of an uttered demonstrative should be identified with the object to which the speaker intended to refer.[26] His claim is that, when he points behind him and utters "that", he thereby refers to the picture of Agnew, even if it was his intention to refer to the picture of Carnap.

But Kaplan *does* intend to refer to the picture behind him. That is why he points back there. If that seems wrong, then that is probably because one is confusing the question whether Kaplan intends to refer

[25] And if they did, would they still be reporting their 'intuitions'? Or would they be making reasoned judgements?

[26] Kaplan (1989, §II) would later change his mind about this issue.

to *the picture behind him* with the question whether he intends to refer to *the picture of Agnew*. The fact that these happen to be the same picture does not imply that the intentions are the same unless one is interpreting the ascription of intention *de re*. If we are interested in the details of Kaplan's psychology, however, the ascription needs to be read *de dicto*. And, for the same sort of reason, Kaplan's intending to refer to the picture behind him does not preclude his also intending to refer to the picture of Carnap. He can and does have both of these intentions. Indeed, I think we can say more. Kaplan not only has both of these intentions, but they are intimately related: He intends to refer to the picture of Carnap *by* referring to the picture behind him; he thinks that he can do this because he thinks that the picture behind him *is* the picture of Carnap. Facts unknown to Kaplan, of course, frustrate this intention, and in that sense his intentions conflict. Such is life as a finite being.[27]

Similarly, then, in MS&D's case. It's just false that John doesn't intend, *de dicto*, to refer to the person who discovered the theorem. He does. John may also intend, *de dicto*, to refer to the person who published it. Even if he does, however—and even if we modify the story so that John makes some remark about Gödel winning lots of awards for *publishing* the theorem—that in no way undermines the fact that John also intends, *de dicto*, to refer to the author of the theorem. The relation between these intentions seems less clear in this case and might well differ depending upon how exactly it was elaborated. But, once again, the two intentions are in no way contradictory, even though, given facts not known to John, they cannot both be satisfied. Indeed, it should now be obvious that John may have more intentions still, such as to refer to the person who answers to the name "Gödel".

It follows that there is no reason at all to assume that someone who gives the non-Kripkean answer in the 'Award Winner Gödel Case' must be understanding the probe question to be asking about semantic reference. Someone who understood the question to be asking about speaker's reference but who took John to intend, *de dicto*, to refer to the author of the theorem—something it would be correct to do—would also give the non-Kripkean answer. MS&D's modification is thus ineffective.

One might object that it is just implausible that, when John says "Gödel probably got a huge number of awards from mathematical societies for the proof of the incompleteness theorem", subjects would not

[27] I've discussed such cases in more detail elsewhere (Heck, 2014b, pp. 351–2).

take him to intend to refer to the person who published the result.[28] Here, however, it seems worth reflecting on the 'epistemic ambiguity' that Sytsma and Jonathan Livengood (2011) uncover in MMN&S's experimental design. They show that subjects' answers to the probe question will vary depending upon whose perspective the subjects adopt in answering it: John's or the narrator's. More precisely, when subjects were asked whom John *thinks* he is talking about, they were much more likely to give the 'descriptivist' answer: Schmidt, roughly.

But now, if someone has heard the probe question as being about speaker's reference, then they really ought to adopt John's perspective in answering it. There is very little room, in this sort of case, between whom John *thinks* he is talking about and whom he *intends* to be talking about. And the changes MS&D made to the vignette make me, anyway, not a whit less inclined to say that John *thinks* he is talking about the person who discovered the theorem, i.e., Schmidt. The really crucial point, though, is that, from John's perspective, there is no significant difference between talking about the person who published the theorem and talking about the person who proved it. It thus does not seem unreasonable, either, in this case, to say that John thinks he is talking about the person who published the theorem. Indeed, I would suggest that John probably thinks he is talking about the person who discovered *and then* published the theorem—and, for that matter, who answers to the name "Gödel".

There is a more general lesson here that it is worth making explicit. Commentators have generally supposed that anyone who understood the original probe question to be asking about speaker's reference should give the non-Kripkean answer. What we have just seen is that this is a mistake. John's intentions in these cases are multiple and conflicting, and which of them one regards as most important may vary depending upon how the cases are developed or interpreted (see e.g. Mount, 2008; Heck, 2014b; King, 2014; Speaks, 2016). Indeed, this may be why more subjects gave Kripke's preferred answer in the 'Award Winner Gödel Case': Perhaps the material that MS&D added to the story made John's intention to refer to the person who published the theorem more salient, to some subjects, than his intention to refer to the author of the theorem.

Indeed, it is an open possibility that *all* of the subjects in the experi-

[28] The probe question does not actually ask about this particular utterance, but since this is the one utterance that has been mentioned, one might expect it to be particularly salient to the subjects.

ments we have been discussing are interpreting the probe question as asking about speaker's reference: They just happen to fasten on different of the speaker's intentions when answering the question. As I mentioned earlier, it is far from clear to me that ordinary speakers have much of a grip at all on the concept of semantic reference.

4 Does Kripke's Argument Depend Upon 'Intuition'?

It would be foolish, absent further investigation, to be too confident that there are no cross-cultural differences in people's 'intuitions' about the Gödel–Schmidt case. I have argued that we have no reason to believe that there are; that is different from having good reason to believe that there are not.

Suppose, then, that there are such differences. MMN&S think that should lead us to be skeptical about the conclusion Kripke draws from consideration of the Gödel–Schmidt case—indeed, of the entire enterprise of theorizing about reference—the reason being that Kripke's argument,[29] as they read it, rests upon our intuitions about that case: The description theory predicts that "Gödel", in John's mouth, refers to Schmidt, but we have the 'intuition' that it does not. What MMN&S are really questioning, of course, is whether 'we' really have such intuitions—and whether it is wise to rely upon intuitions that vary in the way that these ones, we are now assuming (for the sake of argument), do (Mallon et al., 2009, pp. 338–40).

But Kripke's argument doesn't depend, essentially, upon an appeal to anyone's intuitions about the Gödel–Schmidt case. Kripke doesn't simply announce his 'intuition' that "Gödel" refers in John's mouth to Gödel and then leave matters there. Rather, as Deutsch (2009, pp. 451–3) emphasizes, Kripke gives several arguments in favor of this claim, and the arguments are what carry the weight. Unfortunately, however, neither Kripke nor Deutsch makes it as clear as they might what insight lies behind these arguments. So let me try to do so myself.[30]

[29] When I speak in what follows about 'Kripke's argument', I mean the argument in which he employs the Gödel–Schmidt example. This is the argument against what Kripke calls 'Thesis (3)'—that a name denotes whatever best fits the associated cluster of descriptions. That argument spans pp. 82–5 of *Naming and Necessity*. The Gödel–Schmidt example is also mentioned several times later, sometimes for different purposes, though some of the later discussions reinforce the earlier argument.

[30] What follows might be regarded as an elaboration of an example about Newton that Kripke (1980, p. 95) very briefly sketches.

John is taking intermediate logic. On Monday, his teacher Toni tells the class, "It was Kurt Gödel who proved that arithmetic is incomplete". So John comes to have a belief he would express as "Gödel proved that arithmetic is incomplete". Tuesday, Toni tells the class, "Gödel was Austrian", and now John acquires another new belief, one that he would express as "Gödel was Austrian"; on Wednesday, she tells them, "Gödel studied at the University of Vienna"; Thursday, "Gödel was born in 1906". We may assume that all of these other claims are also true of Schmidt. We may also assume that Toni knows a great deal about Gödel, enough that, by anyone's lights, the name "Gödel", in her mouth, refers to Gödel.

Consider now the various beliefs that John thus acquires (in a world in which Schmidt proved the incompleteness theorem). According to the description theory, none of these beliefs even concern the same person as did Toni's beliefs. If John's beliefs aren't about Gödel at all, however, but are instead about Schmidt, then they can't constitute knowledge, even if they are true.[31] Kripke, by contrast, would regard John as having acquired a few pieces of knowledge about Gödel from Toni—he was an Austrian who was born in 1906 and studied at Vienna—but also as having acquired one piece of misinformation: that he proved the incompleteness theorem. Which of these interpretations should we prefer?

Suppose that the following Monday Toni says to the class, "It was just discovered that Gödel did not, in fact, prove the incompleteness theorem! Gödel did a lot of important mathematical work, but the incompleteness theorem was actually proved by Schmidt. . . ", etc, etc. If the description theory is correct, then John should regard this statement as self-contradictory. But that is not what actually does happen in real-life cases of this kind, such as the Peano and Einstein cases that Kripke (1980, pp. 84–5) mentions. Unless John were very unusual indeed, he would not insist that he knows that Gödel (the person *he* calls "Gödel") proved the theorem, whatever anyone else might say, and cite the description theory of names in his defense. Rather, he would appreciate that he might have acquired some misinformation from Toni along with some correct information. That, again, is how people actually respond in cases like the Peano case.

A different way to approach the same point is by thinking of the

[31] I hope this claim seems uncontroversial: If you misunderstand someone to such an extent that you don't even get the references of their words right, then the belief you acquire as a result of that transaction cannot constitute knowledge. Note that this is much weaker than the claims deployed in Heck (1995, 2002).

argument from error (the one in which the Gödel–Schmidt example is used) as building on the argument from ignorance (the one in which the Feynmann example is used). Suppose Toni had instead said, on Monday, "Gödel was a mathematician". Then, by the end of the week, John associates no information with "Gödel" that is sufficient to identify anyone uniquely: All John knows about 'Gödel' is that we was an Austrian mathematician who was born in 1906 and studied at Vienna. The argument from ignorance shows that we should nonetheless regard John's uses of "Gödel" as referring to Gödel, and this is true even if John happens to have acquired some misinformation from Toni, say, that Gödel was Jewish (a common misconception). So John's ability to refer to Gödel using "Gödel" does not require that he be able to identify Gödel uniquely, and it is compatible with his having some false beliefs about Gödel. That much, again, is established by the argument from ignorance.

Now suppose that Toni tells the class on Friday, "Gödel proved that arithmetic is incomplete". So John now has another false belief about Gödel, or so one might reasonably suppose. But, according to the description theory, he does not: John now uses "Gödel" to refer to Schmidt; what he used to believe about Gödel he now instead believes about Schmidt. But that seems absurd. It simply isn't plausible[32] that the mere fact that this new belief happens to be uniquely identifying should deprive John of the ability he already had to use "Gödel" to refer to Gödel.

As Kripke (1980, p. 91) notes, it is of course possible for John hardheadedly to decide to use "Gödel" as a descriptive name of the person who proved the incompleteness theorem, whoever that may be. But then "Gödel" in his mouth is just a homonym of the name "Gödel" that Toni uses. What Toni says about 'Gödel' will then not bear directly upon John's beliefs about 'his' Gödel. It will do so only if he can justify the auxiliary premise that $\text{Gödel}_{\text{John}} = \text{Gödel}_{\text{Toni}}$. But yinter-personal communication using proper names does not, in the usual case, depend upon such auxiliary premises. Rather, the way John communicates with Toni—e.g., his using "Gödel" to ask her questions about (the person he calls) 'Gödel'—implicitly presupposes that "Gödel" in his mouth and

[32] As Walter Sinnot-Armstrong emphasized to me, some philosophers would regard appeals to plausibility as appeals to 'intuition'. But that is intuition in a different sense: A kind of intellectual intuition, not an intuition about 'cases'. And, for my own part, I'd prefer not to talk that way, as it runs together two very different phenomena. In any event, if one wants an argument for the claim made in the text, the kinds of considerations Evans (1985b, §IV) marshalls when discussing 'cognitive dynamics' should suffice.

"Gödel" in Toni's mouth as *not* mere homonyms, i.e., that no such auxiliary premise is required for him to acquire information about 'Gödel' from Toni.[33]

Here's another example of the same sort. Suppose the only thing Jones has heard about Peano is that he discovered certain axioms for arithmetic. Now suppose that he wants to learn more about Peano. How will he proceed? He might ask someone; he might look on the internet. Both of these ways of proceeding in effect involve the assumption that, when other people use the name "Peano", they are talking about the same person he calls "Peano". If that were not true, then neither of these ways of proceeding would make any sense at all. If Jones now reads that Peano did not discover the axioms, he may decide that he's no longer so interested in Peano. But he will not think that, as a result of his reading, he has acquired a lot of misinformation about Dedekind. He knows that other people, when they talk about 'Peano', are not talking about Dedekind.

Kripke (1980, pp. 90–7) goes on to suggest that we should regard the reference of the name "Gödel", as John uses it, as having been inherited from the reference of "Gödel" as Toni uses it. That is the first step towards what we now know as the causal-historical theory of reference. It is a plausible enough claim that one might be content to rest the argument there. But the argument from error doesn't even need that much. All that's required are the weaker claims (i) that "Gödel" in John's mouth refers to the same person as it does in Toni's mouth and (ii) that this fact could survive the discovery that Gödel did not prove the incompleteness theorem.[34] What most fundamentally underlies that claim, or so I have been arguing, is just the fact that we use language to talk to each other, to engage rationally with each other. The crucial insight that underlies Kripke's argument is thus that using language to engage rationally with one another presupposes a certain constancy of reference:[35] If "Gödel" as I use it did not refer to the same person as when you use it, then our conversations about 'Gödel' would be comedies of errors, if not tragedies.

[33] Elsewhere, I appeal to a similar principle in trying to explain how a broadly idiolectical approach to language might account for some of the normative features that fans of 'common' or 'public' languages emphasize (Heck, 2006). A similar idea is found in Schroeter (2012) and Schroeter and Schroeter (2016).

[34] Ostertag (2013) rightly emphasizes the difference between Kripke's arguments against descriptivism and his arguments for the causal–historical theory.

[35] I argue in Heck (1995, 2002) that, in fact, more than just constancy of reference is presupposed. But we do not need any stronger claim here.

Of course, such errors do occur, if only because there are so many people named "Gödel". But it is now almost universally agreed that such errors do not occur for the sorts of reasons a naïve descriptivist interpretation of the Gödel–Schmidt case would imply that they might. Every sensible view must somehow come to terms with the fact that language is, as Hilary Putnam (1975a, p. 146) famously put it, less "like a hammer or a screwdriver which can be used by one person [than] like a steamship which require[s] the cooperative activity of a number of persons to use". And, so far as I know, all extant heirs of the description theory do indeed strive to respect this insight.[36]

That is one of the great ironies here. Although there are plenty of philosophers nowadays who defend some sort of descriptivism, I know of none who would disagree with Kripke's judgement about the Gödel–Schmidt case. This makes it all the more questionable, it seems to me, whether subjects who give the non-Kripkean answer are reporting 'descriptivist intuitions'. Not even descriptivists have such 'intuitions'.[37]

We can now see what is right about an objection that Michael Devitt (2011, §2) brings against MMN&S.[38] This so-called 'Expertise Defense' suggests that we should not be so concerned with the intuitions of ordinary speakers but should pay more attention to, or at least give more weight to, the intuitions of 'experts': speakers with an appropriate sort of education and training.[39] I have already said what is wrong about

[36] Thus, Kroon (1987) not only argues that argues that causal descriptivism does respect this insight but that the causal-historical theory, in the end, does not fully respect it. That is why he thinks we should prefer causal descriptivism.

[37] MMN&S (2004, p. B3; 2009, p. 339) note this fact, but seem to think it reveals nothing but the biases of Western philosophers. In fact, what it reveals is how compelling Kripke's claims about the Gödel–Schmidt case really are (though, as I shall mention below, it took some time for it to become clear how right he was). It is the *significance* of Kripke's claims about the Gödel–Schmidt case that is controversial.

[38] Related ideas are expressed by Ludwig (2007, pp. 148ff) and Williamson (2007, pp. 190ff). Neither of them, however, is out to defend the role that intuitions play in arguments like Kripke's, as Devitt explicitly is. It is a different question whether philosophers' *judgements* about e.g. the Gödel–Schmidt case should be trusted. But, like Williamson (2011), I find it difficult to see what, other than a general distrust of abstract reasoning, might justify broad skepticism about philosophers' capacity to evaluate philosophical arguments. The fact that undergraduates are less reliable is irrelevant (not to mention unsurprising).

[39] It is interesting to read the exchange between Mates (1958) and Cavell (1958) against this background. Mates argues that ordinary language philosophers are not entitled to the claims they characteristically make about what 'we' are inclined to say without empirical investigation. Cavell argues that they are, in large part because they are themselves competent speakers. As has often been pointed out, MMN&S seem to think

this: Intuitions just don't play the sort of role in Kripke's criticism of the description theory that it has often been supposed that they do. Kripke gives us, or at least gestures at, *arguments* that (a certain extreme form of) descriptivism sometimes yields the wrong result. But Devitt's central point survives this corrective.

What is right about the Expertise Defense is the observation that the Gödel–Schmidt example is *addressed* to 'experts': to people who are familiar with the larger philosophical context in which claims about the nature of reference have to be assessed and who know enough philosophy to be able to appreciate and evaluate the various arguments that Kripke gives. These 'experts' may well have 'intuitions' about the 'case'. They may just be able to 'see', without working through the details, that Kripke's central claim about it—that, in John's mouth, "Gödel" still refers to Gödel—is correct. But, as Devitt (2011, p. 425) emphasizes, "nothing rests on [intuition] in the long run". It is always in order to demand the details.[40] And there were, at first, plenty of philosophers who did demand the details, since they disagreed with Kripke's judgement about the Gödel–Schmidt case (e.g. Dummett, 1973, pp. 136–8). That judgement only became uncontroversial after a great deal of time was spent, in the decade or so following the appearance of *Naming and Necessity*, arguing about the details that Kripke omitted. (That, by the way, counts as philosophical progress.) It would, it seems to me, be well worth someone's writing a history of that discussion, if only because of what it would teach us about the role that 'intuition' does and doesn't play in philosophy.[41]

What is true, then, is that, in the actual course of philosophical argument, we 'experts' often make 'snap judgements' about examples without working through the details.[42] But that doesn't distinguish philosophy from mathematics. It's not uncommon in mathematics for proofs to be omitted entirely. Sometimes they are indicated only in vague outline. Sometimes authors will just say that some point is obvious or clear. It's just assumed that the intended audience can 'see' that the claim made is correct and that filling in the details would be routine if tedious, though sometimes there are surprises and what seemed obvious isn't at all obvious.[43] But that only serves to remind us that proof remains

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we're all still ordinary language philosophers.

[40] In principle. Overdoing it might not go down well.

[41] Related work was done by Geoffrey Grossman (2019), in his dissertation.

[42] For a particularly clear discussion of this point, see Ichikawa (2014).

[43] A recent example concerns a proof given by Henryk Kotlarski (1986). Independently

the gold standard (Burgess, 2015, ch. 1): Mathematics does not rely *essentially* upon such snap judgements (though, in practice, it's hard to see how one could get by without them). The same goes, I suggest, for Kripke's argument against descriptivism. Our 'intuitions' about the Gödel–Schmidt example have no ultimate weight. What matters, in the end, is how we evaluate the arguments that Kripke gave, and others that were later added, not anyone's initial reaction to a single example, however striking it may be. And that is all the Gödel–Schmidt 'case' really is: an *example* that allows certain very abstract considerations to be made somewhat more concrete.[44]

I am of course happy to admit that the experts are divided on how Kripke's overall argument against descriptivism should be assessed—as opposed to his judgement about the Gödel–Schmidt case, about which there is, as I have said, nearly universal agreement. But I flatly deny that there is any reason to believe that further "intuitions about possible cases are likely to be needed to determine what the correct theory of reference is" (Machery et al., 2013, p. 623). To take just one example: The sorts of arguments that have been offered for and against two-dimensional semantics—the most sophisticated extant heican'tr of the description theory—do not, so far as I can see, depend essentially upon appeals to intuition, any more than Kripke's arguments do (see e.g. García-Carpintero and Fabrega, 2006). If MMN&S think otherwise—or if they think more generally that, ultimately speaking, theories of reference can *only* be assessed on the basis of intuition—then they need to give an argument for that claim, not just keep asserting it.

It has been suggested to me (by an anonymous referee) that there may be such an argument in this passage:

> A plausible justification for the method of cases might be the assumption that language users have an implicit theory of reference that produces intuitions about reference. The project

and around the same time, Albert Visser and I both realized that there was a large enough lacuna in Kotlarski's argument that it called into question the result allegedly proved (Heck, 2015, fn. 56). The hole has since been closed (Wcisło and Łełyk, 2017; Łełyk, 2022), but closing it required a very different argument from the one that Kotlarski sketched. It does not seem that his proof is fixable. So he was right about the result but wrong about why it holds.

[44] Or, as Cappelen puts it in discussing other 'cases' in which 'intuition' is alleged to play a significant role, the Gödel–Schmidt example is a "fact focuser": It is "a device for drawing our attention to some phenomenon or feature of the world that has philosophical significance" (Cappelen, 2012, p. 133; see also pp. 142 and 151–2). See also Burge (2007, pp. 27–8).

> for reference theorists can then be conceived by analogy with the Chomskyan project in linguistics. Philosophers of language use people's intuitions about reference to reconstruct the implicit theory that is part of each speaker's cognitive endowment (Segal, 2001).
>
> (Mallon et al. 2009, p. 339; see also Machery et al., 2004, p. B9)

But MMN&S are not here offering an argument that only intuitions can decide among theories of reference. Rather, they are speculating about how their opponents might try to *justify* their alleged reliance upon the 'method of cases'. They are simply assuming that this is something their opponents would want to do.

What this passage does nicely illustrate, however, is how unreliable MMN&S's scholarship in this paper is.

It is difficult to think of anyone who holds the sort of view MMN&S describe.[45] Gabriel Segal, whom MMN&S cite, certainly does not.[46] Indeed, the reference to Segal is puzzling, since 'intuitions about cases' play almost no role in the paper cited.[47] The action in that paper is in section 5, where Segal wheels in data from the study of linguistic deficits, language acquisition, and syntax. What's worse, Segal is not even discussing 'theories of reference' in the sense in which MMN&S are concerned with them.[48] Segal's topic is how proper names should be handled in natural language semantics. MMN&S seem to be conflating 'theories of reference' in the *meta*-semantic sense—theories about the *nature* of reference—with 'theories of reference' in the semantic sense: the sense in which Donald Davidson (1967) proposed that a theory of meaning should take the form of a theory of truth. It is only in this latter sense that neo-Davidsonians like Larson and Segal (1995), say, hold that semantic competence rests upon tacit knowledge of a theory of reference

[45] The one person who may have held this view, ironically, is Stich, at least at one time. This conception of the theory of reference seems to have been due to him (Stich, 1996, §6.1), and he reports having tried to explore our 'tacit theory of reference' experimentally (Stich, 1996, §7). It is odd methodology to saddle one's opponents with idiosyncratic views one has oneself abandoned.

[46] Chomsky himself is very dismissive of any role for 'intuition', especially in semantics (Chomsky, 2000a).

[47] Elsewhere in the paper, MMN&S cite Evans as a "sophisticated descriptivist" (Mallon et al., 2009, p. 339), which is even more bizarre.

[48] I can, in a way, see why MMN&S were misled. Segal describes the view of names he prefers as "quasi-descriptive". But it is just the view that names are constants, as opposed to predicates, which is the view Segal is opposing.

(i.e., of a theory of truth).[49] Gabriel Segal, whom MMN&S cite, certainly does not.[50] No one, so far as I know, thinks that semantic competence rests upon tacit knowledge of some theory of the *nature* of reference.

Moreover, MMN&S are confusing two quite different projects: a philosophical one and a psychological one (see also Williamson, 2007). On the one hand, as a philosopher, one might be interested in the nature of reference itself: the relation between a proper name and the object for which it stands. On the other hand, as a psychologist, one might be interested in people's 'implicit theory of reference': how the 'folk' think about that relation, and how such thought develops.[51] Maybe ordinary speakers' thought about reference even plays some role in their use of language.[52] Or not. But what ordinary people think about reference has no obvious bearing upon questions about the *nature* of reference, any more than people's ordinary thought about space (a topic studied by cognitive psychologists) is relevant to questions about the nature of space (a topic studied by physicists). But, if surveys of the sort done by MMN&S tell us about anything, they tell us about what ordinary people think about reference; absent further argument, the surveys tell us nothing about the nature of reference.[53]

In the end, then, we have to make do with remarks like these:

> One might reply that besides the method of cases and the appeal to the philosophical consequences of theories of reference, some other considerations might be used to justify these theories. In reply, we first note that this move would involve breaking with the dominant tradition of employing the method of cases in the philosophy of language. More important, we have no idea what other considerations philosophers

[49] That said, there is an important question to be asked about the role of speakers' 'intuitions' in linguistics itself. For much wisdom about that issue, see Ludlow (2011) and Jacobson (2018). Speaker's meta-linguistic judgements *about* grammaticality (say) are not the most important data and, in fact, are unnecessary.

[50] Chomsky himself is very dismissive of any role for 'intuition', especially in semantics (Chomsky, 2000a).

[51] The relation between these is not unlike the relation between theories of the nature of belief (that very mental state) and ordinary people's 'theory of mind'. See Heck (2014a, §3).

[52] As mentioned previously, Kroon (1987) makes just such an argument.

[53] As it happens, there is much interesting work in developmental psychology on phenomena in this vincity. See e.g. Krehm et al. (2014). The methods used in that work are vastly more sophisticated than the blunt tools typically used in 'experimental philosophy'.

> of language might appeal to. Thus, in the absence of concrete suggestions, we remain skeptical of the proposal to downplay the role of intuitions in choosing a theory of reference. (Mallon et al., 2009, p. 343)

It is certainly true that philosophers in the 'dominant tradition' often discuss cases—that is, examples—and judgements about these examples are often used to confirm or infirm theories that make different predictions about them. What we are seeking, however, is reason to believe that our *intuitions* about these examples must play some central role.[54] That is an entirely different matter.

Suppose that some theory predicts that "Gödel" refers to Schmidt. Suppose further that we have good reason to believe that "Gödel" does not refer to Schmidt. Then the theory is to that extent disconfirmed, *whatever* the source of the latter belief. It does not matter whether we 'intuited' it, reached it on the basis of a long chain of argument, or established it by experiment, but only whether our reasons for it are good ones.[55] Nor is there any restriction on the sorts of reasons we are allowed to have. They might come from everyday life, from other parts of philosophy, or from other disciplines. If the question, then, is "what other considerations philosophers of language might appeal to", besides intuitions, there is no general answer to be given, other than: whatever seems relevant. The reconstruction of Kripke's argument I gave earlier is a case in point. It appeals, among other things, to general theses about what is involved in using language to communicate with someone; to connections between language and mind; to theses about what is involved in understanding someone, and about the epistemic costs of misunderstanding; and so forth. These claims can all be questioned, obviously. They can also be argued for.

What's more, it has long been appreciated that 'intuitions' *cannot* decide between theories of reference. The earliest really clear statement of this point that I have been able to find is due to Robert Stalnaker,[56]

[54] I confess again that I do not really know what 'intuitions' are supposed to be. But what follows depends only upon the assumption that "intuition" is supposed to mean something more specific than "judgement". It must, since, if not, skepticism about intuition just exploded into general skepticism (Williamson, 2004).

[55] There is another discussion to be had about whether 'intuited' judgements are trustworthy. I don't think this question is critical here, since such judgements have no ultimate weight. But see Ichikawa (2014) for discussion.

[56] Stalnaker's discussion is focused on the Twin Earth examples (Putnam, 1975a) rather than the Gödel–Schmidt case, but his discussion applies (and is intended to apply)

though I suspect the point was widely appreciated previously:[57]

> The issue is a highly theoretical one concerning the nature of mind [or language], and the nature of intentionality. But the externalist case... was made with examples and thought experiments, examples *and arguments* that focused attention on a range of phenomena. The case is not based on any theoretical explanation for the phenomena, or on any general theory of mind or of intentionality. It is thus open to the internalists, who have general theoretical reasons to resist the externalist picture, to accept the phenomena—the ~~intuitive~~ judgments about the examples—but argue that they rest on superficial facts about the way we happen to talk, and the way we happen to describe our mental states. (Stalnaker, 1993, p. 301, my emphasis, my deletion)

That is: Internalists need not, and by then often did not, reject the 'intuitions' on which externalists rely; they just accommodate them in a different way. But then intuitions can't possibly decide the issue.

Indeed, by the time Stalnaker was writing, a quite general strategy for reconciling internalism with the agreed judgements about the examples had emerged.[58] It first appears in a footnote in *Naming and Necessity*:[59]

> As Robert Nozick pointed out to me, there is a sense in which a description theory must be trivially true if any theory of the reference of names, spelled out in terms independent of the notion of reference, is available. For if such a theory gives conditions under which an object is to be the referent of a name, then it of course uniquely satisfies these conditions. (Kripke, 1980, p. 88, fn. 28)

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much more broadly.

[57] See footnote 58 for some evidence. For a more recent expression of much the same thought, see Schroeter and Schroeter (2016, p. 196).

[58] The way Stalnaker describes this view makes me think that he has Loar (1988) in mind, though Stalnaker does not cite him or, really, any opponents of externalism (not in this connection, at least). That makes it plausible that Stalnaker thought that this strategy had by then become well known. Indeed, Kroon (1987, p. 1, fn. 1) cites several authors as holding this sort of view, including Loar (1976). Proposals in this vicinity can also be found in Dummett (1973, Appendix to Ch. 5).

[59] Lewis (1984, p. 227) makes essentially this same point, writing: "When causal theories work, causal descriptivism works too". But, in that paper, Lewis is arguing for a different sort of view.

This is what I call 'internalizing the external' or, more colorfully, "Nozick's Gambit":[60] Take whatever externalists claim the correct conditions on the reference of a name are, and then build them into the descriptive condition itself. Causal descriptivism, which is nowadays *the* alternative to externalism (see e.g. Kroon, 1987; Chalmers, 2002, 2012), makes precisely this move. You say that the referent is what's at the end of a suitable causal chain? Then let's just have the description be: The thing that's at the end of the causal chain.

Now, obviously, if that's the alternative to externalism, then not even the *facts* about what refers to what can decide between internalism and externalism. The issue is, indeed, "a highly theoretical one", and what externalists need, as Stalnaker (1993, p. 301) goes on to say, is

> a theoretical account of intentionality that explains the externalist phenomena, and that justifies the claim that the phenomena show something about the nature of intentionality, and do not just reflect an accidental fact about the way we happen to talk. . . .

It's on that ground that the dispute must be conducted, and it's on that ground that the argument nowadays *is* conducted. The big issues concern, e.g., whether externalism is compatible with the demands of intentional explanation (e.g. Fodor, 1980; Loar, 1988; Burge, 1989; Peacocke, 1993; Fodor, 1994) and with the kind of self-knowledge we ordinarily suppose ourselves to have of our own mental states (e.g. Davidson, 1987; Burge, 1988; McKinsey, 1991; Stalnaker, 2008; Parent, 2017). Certainly my own discussion of these sorts of issues, in the rest of this book, makes no serious appeal to anyone's 'intuitions' about 'cases'.

If there ever was a "dominant tradition of employing the method of cases in the philosophy of language" (Mallon et al., 2009, p. 343), then, it died a long time ago. It certainly does not characterize the discipline now. To claim otherwise is just to reveal one's ignorance.

I suspect that MMN&S confuse using examples—which is something I, at least, insist that my students do—with restricting oneself to 'intuitions' about those examples when evaluating them, i.e., to foolishly and arbitrarily restricting one's evidence.[61] One sees just this confusion at

[60] In chess, a 'gambit' is, roughly, a Trojan horse: an offer of material to one's opponent in exchange for some other advantage.

[61] Compare Chomsky (2000b) on the claim, made by some philosophers (e.g. Soames, 1984), that only certain limited sorts of data are are relevant to linguistic theory. Chom-

work in MMN&S's remarks about a well-known example due to Gareth Evans:

> . . . [A]ccording to Evans. . . , people have the intuition that nowadays the proper name "Madagascar" refers to the large island near the south of Africa, even when they learn that the term was historically used to refer to a region on the mainland of Africa. (Mallon et al., 2009, p. 338)

But, as Deutsch (2009, pp. 448–9) notes, Evans does not mention intuition in this connection, and it's not clear why people's intuitions would be relevant.[62] What drives Evans's argument are what he takes to be three simple facts (Evans, 1973, p. 195–6):

(i) "Madagascar" nowadays refers to a certain island.

(ii) "Madagascar" (or, at least, a form of this name) originally referred to a portion of the African mainland (specifically, of Somalia, near Mogadishu).

(iii) The conditions Kripke specifies for what constitutes a reference-transmitting causal chain—specifically, that each person intended to use the name with the same reference as the person from whom they acquired it—are met in this case.

It follows from (ii) and (iii) that, on Kripke's view, "Madagascar" should still refer to a portion of the African mainland, which contradicts (i).

None of Evans's three claims is justified by an appeal to 'intuition'. Any decent dictionary will tell you what "Madagascar" refers to; Evans cites a reference on geographical nomenclature (Taylor, 1898, pp. 181–2) in support of (ii) and (iii). And it matters what the etymological facts actually are: John P. Burgess (2014) has argued that, once we pay careful attention to the details, it becomes clear how Kripke should respond to Evans's challenge: By tweaking the conditions mentioned in (iii). What matters for our purposes, however, is not whether Burgess is right about this. What matters is that 'intuition' plays no more role in his discussion than it plays in Evans's.

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sky, by contrast, insists that *everything* is in principle relevant. This point, of course, goes back to Chomsky (1969), at least.

[62] The word "intuition" occurs just twice in Evans's paper, on p. 193. Evans rests nothing on intuition. Indeed, his attitude towards intuitions seems quite dismissive. Like Kripke, he mentions certain 'intuitions' and then immediately produces an argument to back them up.

MMN&S insist elsewhere that "... the fine details of Kripke's arguments against descriptivism... are largely irrelevant to [their] central concerns...". But if their goal is to raise questions about "the use of intuitions about reference to identify or justify the right theory of reference" (Machery et al., 2013, p. 621), then it is not just *relevant* what role intuition plays in such arguments, it is *crucial*. There's no point raising questions about the wisdom of relying upon intuitions in theorizing about reference if no one relies upon intuitions. And, as Herman Cappelen (2012, esp. Ch. 1) has made clear, one cannot decide what role intuitions play in such arguments simply by looking at what the authors of those arguments *say* about the role it plays.[63] One has to look at the arguments and do the hard work of determining what role intuition actually does play.[64]

To do that for all the various arguments that have been given concerning theories of reference is not a task for one person, let alone for one chapter. Here, I have looked only at the argument in which Kripke deploys the Gödel–Schmidt example, and I have argued that intuition plays no significant role in that argument. Cappelen (2012, §8.2) has offered a similar analysis of the influential arguments against individualism due to Tyler Burge (1979), and Geoffrey Grossman (2019, ch. 3) has examined the Twin Earth argument due to Putnam (1975a, pp. 139–42). Since Grossman's discussion has not been published, however,[65] and since it is still common to see the Twin Earth case cited as a paradigmatic case of an argument that appeals to intuition, let me give a brief summary of

[63] It is, in particular, irrelevant what role Kripke thinks intuition plays in his argument; citations of Kripke's off-the-cuff (and, to my ear, somewhat tongue-in-cheek) remarks about intuition (Kripke, 1980, pp. 41–2) show nothing. No one has first-person authority over what assumptions *actually* play a significant role in their arguments: not in mathematics and not in philosophy, either.

[64] Just to be clear, there certainly are papers in the literature from which one might get the misleading impression that arguments about reference depend upon intuition. In "Cognitive Science and the Twin Earth Problem", for example, Fodor (1982) talks repeatedly of "Putnam's intuitions" about the references of certain expressions. But Fodor does not simply record his disagreement with Putnam's 'intuitions'. Instead, he offers a whole battery of arguments that Putnam's *claims* (as I would prefer to call them) are false. Indeed, so far as I can see, little in Fodor's paper would have to change if one were simply to replace "intuition" throughout by "claim". Similarly, in "Social Content and Psychological Content", Loar (1988) often describes certain claims as 'intuitive', but he never rests anything upon their intuitiveness. He backs his claims up with arguments (like philosophers are supposed to). In his case, the crucial question is one mentioned earlier: whether 'wide' content can do the right sort of explanatory work in psychology.

[65] The dissertation is available online from the Brown library.

Grossman's discussion.

Here's the basic structure of Putnam's argument:[66]

(1) As it is used today, the word "water" refers to H_2O on Earth and to XYZ on Twin Earth.

(2) The reference of the word "water" has not changed since 1750 (either here or on Twin Earth).

(3) So, as it was used in 1750, "water" referred to H_2O on Earth and to XYZ on Twin Earth. (From (1) and (2))

(4) The descriptions that people on Earth and Twin Earth associated with "water" in 1750 were the same.

(5) So, the descriptive conditions (or 'operational definition') that people associate with a term (at a given time) do not determine its reference (at that time). (From (3) and (4))

Putnam takes (1) to be uncontroversial. More precisely, even a descriptivist should accept it. We know enough chemistry, and the Twin Earthlings know enough chemistry, that the descriptions we and they associate with "water" will secure different references for that word.[67] Premise (4) is equally uncontroversial: It's just built into the thought experiment that what people believed about 'water' in 1750, before the dawn of modern chemistry, was the same in both locales.

The key to Putnam's argument is thus premise (2). Arguments for it appear throughout "The Meaning of 'Meaning'". Indeed, immediately after describing the Twin Earth example, Putnam (1975a, p. 141) writes: "But, it might be objected, why should we accept it that [*sic*] the term 'water' had the same extension in 1750 and in 1950 (on both Earths)?" The answer is not that we all have the 'intuition' that it does.[68] The

[66] Twin Earth is meant to be a planet as much like Earth as possible except that, on it, the stuff in the rivers and lakes, etc, has a very different chemical structure (XYZ) from what we call 'water'. As has often been pointed out, that may well be impossible, given the central role water plays in human biology. But Putnam himself mentions other versions of the example that do not have that problem.

[67] Putnam never says exactly which speakers his argument concerns. But these remarks should make it clear that it could concern scientists, not ordinary speakers. If so, then this argument does not, as the elm–beech argument does, suggest that there is a 'division of linguistic labor'. It would some time before Burge (1979) disentangled these two arguments.

[68] Even if we did, that would not be an 'intuition about cases'. See footnote 32.

answer Putnam offers is that this is a consequence of the fact that "water" is a natural kind term.[69] But this premise comes into sharpest focus in the section entitled "Let's Be Realistic" (Putnam, 1975a, pp. 153–7), where Putnam argues for the claim that "'gold' has not changed its extension (or not changed it significantly) in two thousand years", so that "gold" in our mouths denotes the same thing as the corresponding Greek word did in Aristotle's. Putnam's main argument is that (2) is required by any plausible account of scientific progress.[70] Whether one agrees with that argument or not, it should be clear that appeals to intuition play no role in it whatsoever.[71] Moreover, one could reject Putnam's reasons in favor of (2) and defend it on other grounds; one could even reject (2)[72] and argue for 3 on other grounds. Indeed, as already noted, Putnam himself offers another argument for (3), based upon the fact that "water" is a natural kind term, and Burge's arguments for the same conclusion take a quite different form still.[73]

[69] Why is "water" a kind term? Putnam does not answer that question, but I would guess his answer would have been something like: Because people are prepared to generalize from instances (i.e., to do empirical induction) and to treat many generalizations about 'water' as counterfactual supporting, and that's only acceptable where kind terms are concerned.

[70] Note that, if one does so argue, that does *not*, as MMN&S (2009, p. 343) seem to suggest, preclude one from then appealing to one's theory of reference to draw conclusions in other areas of philosophy (e.g., in an argument for eliminativism about race, which seems to be Mallory's main concern). One might think it would preclude one from appealing to it in offering an account of scientific progress, but even that is not true.

[71] Putnam does use the word "intuition" a few times in the paper—thank goodness for searchable PDFs—and he uses it once in the section mentioned, where he writes: "... [P]eople tend either to be strongly anti-realistic or strongly realistic in their intuitions" (Putnam, 1975a, p. 154). Here, the word seems to mean something like "philosophical predilections". He does not appeal to 'intuitions about cases' in the argument for (2).

[72] Wilson (1982) criticizes this claim explicitly. So at least one philospher understood, early on, what the structure of Putnam's argument was. If that has been forgotten, it is largely because Putnam's claim that "water" has different references on Earth and Twin Earth, even in 1750, is no longer as controversial as it once was. It is, again, the *significance* of that claim that's disputed.

Note, by the way, that Putnam's argument for externalism depends *only* upon the mentioned claim:

(D) The word "water" had different references on Earth and Twin Earth even in 1750.

The argument does not depend upon the claim that "water" refers to H_2O. As Sarah-Jane Leslie (2013) has argued, that's misleading at best. But again, as I read it, Putnam's argument simply doesn't depend upon essentialist, let alone 'microessentialist', premises.

[73] For what it's worth, I'm myself inclined towards an argument more like Burge's: one that draws upon resources very much like those on which Kripke draws, as I've reconstructed his arguments above.

I see no reason, therefore, to think that contemporary theorizing about reference depends, to any significant extent, upon appeals to 'intuition'.

5 Closing Remarks

Whatever cross-cultural variation there may be in subjects' responses to (various versions of) the Gödel–Schmidt case, there is no reason (so far) to believe that such variation should have any bearing upon philosophical discussions about the nature of reference—even if we assume, contrary to what I've just argued, that 'intuitions' do play a role in such discussions. Not only is MMN&S's probe question ambiguous, but its most natural reading is arguably not the one they require. Attempts to eliminate the ambiguity have been unsuccessful and are arguably futile, since the notion of semantic reference is deeply theoretical and may not even be available to ordinary speakers. Attempts to neutralize the ambiguity have also failed due to the variety and complexity of speakers' intentions in such circumstances.

There are, then, several different sorts of confusion to which the subjects of the experiments we have been discussing might be vulnerable.[74] But what is more interesting is which confusions these are: ones that philosophers both before and after Kripke have labored to dispel. For a subject's response to the Gödel–Schmidt case to be so much as relevant to the issues Kripke meant to be discussing, they need to understand it as being about semantic reference rather than speaker's reference; they need to appreciate the difference between having uniquely identifying information about a person and regarding that piece of information as identificatory;[75] and they need to be careful not to treat the relevant uses

[74] It is not a problem if some of these sources of confusion conflict with one another. Different subjects could be liable to different sorts of confusion, and a single subject could even be subject to conflicting confusions.

[75] Lam (2010) once suggested that some subjects might think that "Gödel" is a so-called descriptive name, like "Jack the Ripper": one whose reference is, by stipulation, the unique object satifying some description. Machery et al. (2010, p. 364) have replied that to regard "Gödel" as a descriptive name "just is to have descriptivist intuitions about" it. But this is confused. Someone who says that, had some other person committed all those grisly murders, the name "Jack the Ripper" would have referred to them, is not reporting a 'descriptivist intuition'. They are simply registering their appreciation of the fact that "Jack the Ripper" is a descriptive name, i.e., that, as a matter of the specific meaning this name has in our language, it refers to the person who committed certain grisly murders, whoever that may be. Someone who made a similar claim about "Gödel"

of the name as attributive. Why should we suppose that naïve subjects will be able to make all the relevant distinctions reliably when students who are explicitly taught them often struggle to do so?

Of course, nothing I have said here explains why Machery and others have consistently found cross-cultural differences in the responses to their surveys.[76] Maybe there is something interesting to be said about that. On the other hand, as Simon Cullen (2010, §3.4) notes, the phenomenon may not have much to do with philosophy at all, let alone with reference, but rather be a consequence of known differences between how people from 'individualist' and 'collectivist' cultures respond to surveys. Particularly interesting is Cullen's suggestion that members of collectivist cultures are more sensitive to pragmatic factors when answering survey questions. But my goal here was not to explain the experimental results. It was simply to question their relevance to the theory of reference.

A The Baseball Experiment

Students from my Fall 2016 introductory logic class were invited to participate in what was described to them as an experiment connected with my research. They were assured that no identifying information would be collected and that no one would ever know whether they chose to participate. Those who did choose to participate were directed to a webpage hosted at Survey Monkey where they found the following story:

> Grace is a ten-year old girl who lives at the Laughing Pines apartments with her family. Grace is *obsessed* with baseball. And all summer long now, her neighbor Bob has been regaling her and some of the other kids with stories about how he used to be a professional baseball player. In fact, however, and unbeknownst to Grace, Bob never even played amateur baseball. He just enjoys the company of the children and is, perhaps, a bit delusional. By coincidence, however, there is an elderly woman, Lily, who also lives at Laughing Pines and who played for several years in the All-American Girls Professional Baseball League. Grace, though, has never met Lily.

would just be registering their *mis*understanding of it.

[76] Sytsma and Livengood (2011, p. 323) failed to replicate at least part of those results, however.

They were then asked:

> When Grace uses the phrase "the baseball player who lives at Laughing Pines", is she talking about:
>
> (A) Bob, who never played professional baseball? or
>
> (B) Lily, who did once play professional baseball?

Students were also asked whether they had previously taken a course in philosophy of language. Four of the forty-three respondents said they had, and their answers were discarded, since their prior experience with philosophy of language might be thought to distort their 'intuitions'.[77]

Of the remaining thrity-nine students, only one chose answer (B); the other thirty-eight chose answer (A). It is sufficiently clear that this is statistically significant. A two-sided binomial test confirmed the fact, with $p < 10^{-9}$, the null hypothesis being that subjects would have no preference between the answers.

One might worry that various details of the vignette, which could easily be changed, might have encouraged students to prefer answer (A).[78] That, however, is the point. What Machery and Stich (2012, p. 506) claim, recall, is that the fact that "no specific utterance is mentioned and no contextual information is provided" should force subjects to prefer answer (B). But the story and question I presented to my students have those features, which therefore cannot by themselves be sufficient to prevent the speaker's reference interpretation.

I also asked my subjects how they felt about the answer they gave, offering them three choices:[79]

1. I could just as easily have given the other answer. Both seemed pretty good to me.

2. Although I am confident in my own answer, I can easily see why someone else might have wanted to give the other one.

[77] In fact, all four of these students chose answer (A).

[78] Cullen expresses a number of concerns about the way surveys are used in experimental philosophy. His main conclusion, that "what has been regarded as evidence for the instability of philosophical intuitions is, at least in some cases, better accounted for in terms of subjects' reactions to subtle pragmatic cues contained in the surveys" (Cullen, 2010, p. 275), is obviously compatible with what has been argued here.

[79] These were on a second page, so this question was asked after the answer to the first question had been recorded.

3. My own answer seems completely right to me. I can't really see why anyone would give the other one.

I expected most students to give answer (1) or (2). That is, I expected students at least to be *aware of* the ambiguity in the probe question. In fact, however, the thirty-eight students who chose answer (A) answered the follow-up question this way:[80]

(1)	1
(2)	15
(3)	22

Most students, then, not only interpreted the probe question in a way at odds with Machery and Stich's prediction but could not understand why anyone else would interpret it the other way. This might be regarded as some confirmation of Deutsch's suggestion, quoted earlier, that "the vignette question. . . nearly forces a speaker's reference interpretation" (Deutsch, 2009, p. 454, fn. 7).[81]

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[80] The one student who chose answer (B) answered (2) on the follow-up question.

[81] As I argued in section 3, in the case of the Gödel–Schmidt vignette, the speaker's reference interpretation is compatible with both answers. That is not plausible here.

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