Semantic Intuitions and the Theory of Reference

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There are aspects of semantics that are indisputably empirical. For instance, it is an empirical question what a particular name, such as ‘Gödel’, refers to (as used by a given speaker or group of speakers). And it is an empirical question whether only H2O is in the extension of ‘water’. To settle such questions, then, we need empirical evidence. No doubt, the evidence does not determine the correct answer, evidence rarely does. We also need general theoretical considerations (relating to issues of simplicity and explanatory power, etc.) as well as more specific considerations deriving from theoretical commitments in philosophy of language. But no one can deny that answering these very basic questions of semantics requires empirical investigations.

What type of investigations? Here, too, there are some obvious answers. Clearly, evidence has to come from speaker use, one way or the other. Not only from speaker use, of course. There is every reason to cast the net wider than that. For example, it is plausible that we should consider all sorts of psychological evidence concerning cognitive processing, etc.\(^1\) Whatever stand one takes on this issue, however, no one will question the central evidential importance of facts about speaker use. But what type of use? Should we simply ask speakers about their linguistic intuitions? Or should we make people use their terms, in production tasks or categorization judgments, rather than ask them to reflect on this use? And what do we do if it turns out that there are in fact great individual differences in the speaker responses?

\(^1\) For a discussion of the importance of further empirical evidence see Gross 2012.
These questions have come to be much discussed as a result of some important experimental work on the theory of reference; in particular, the famous 2004 paper by Machery, Mallon, Nichols and Stich (MMNS), “Semantics, cross-cultural style”. The paper presents experiments that purport to test the semantic intuitions of Western and East Asian lay speakers concerning the reference of proper names, suggesting that there is considerable variation when it comes to these intuitions. According to MMNS the experiments show that Westerners tend to have intuitions consistent with Kripke’s causal-historical view whereas East Asians have more descriptivist intuitions, but they also show that there is considerable variation within cultures.

How should the semanticist respond to these results? MMNS (2013) suggest three ways of accommodating the variation in intuitions: We can deny that the intuitions tested are reliable guides to reference; we can adopt referential pluralism and grant that names refer differently in different cultures (and across individuals); or we can deny the value of non-expert intuitions, hoping that the variation will disappear if we were to test merely expert intuitions.² Philosopohers of language have tended to endorse either the first or the third option. For instance, in support of the first option it has been argued that the type of intuitions tested by MMNS do not provide real evidence for the theory of reference, since they do not in fact tell us anything about the actual semantics of the terms involved.³ In defense of option three it has been suggested that lay speaker intuitions are not sufficiently reliable when it comes to semantics since semantic theories, like all theories, require expertise.⁴

I have argued elsewhere that the third option, the so called ‘expertise defense’, fails in the case of semantics (Häggqvist & Wikforss 2015). In this paper I shall examine the

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² See also Machery (2012).
³ See for instance Ichikawa et. al. 2012, Marti 2009 and Sytsma and Livengood 2011. Another version of the first option is the claim, defended by Deutsch 2009, that it is a mistake to think that semantics needs intuitions in the first place. For a critical discussion of Deutsch see Häggqvist and Wikforss 2015, 114.
first option, that of dismissing the evidential value of the MMNS intuitions to the theory of reference. In particular, I shall address one important source of skepticism about the results, due to Genoveva Martí 2009. According to Martí the 2004 experiments (as well as subsequent ones) carried out by Machery et. al. test the wrong kinds of intuitions since what is being tested are people’s meta-linguistic intuitions. The great variation in the results, she suggests, merely reflects the fact that people are not very good at theorizing about language, not that they actually use proper names in different ways. I shall argue that although Martí is quite right to stress that MMNS are not really testing people’s use of proper names, the intuitions tested are not meta-linguistic in a sense that undermines their evidential value. I shall argue, moreover, that the proper response to the variation in intuitions is not to dismiss their evidential value but the second option: referential pluralism. This option has not been much discussed in the literature but it has generally been assumed (also by MMNS) to be a non-starter. I will argue that, on the contrary, semantic pluralism is a live option and need be taken seriously by philosophy of language.

The paper is divided into three parts. In part 1 I give a brief presentation of the original MMNS experiments and spell out what it may mean to test ‘the theory of reference’. In part 2 I discuss Martí’s criticisms of the experiments, including her criticisms of some later experiments carried out by Machery, Olivola & De Blanc (2009). In part 3 I discuss what conclusions to draw from this and argue that referential pluralism is more plausible than commonly assumed.

1. Testing the theory of reference

MMNS explicit aim is to test ‘theories of reference’ which they describe as theories explaining “how terms pick out their referents” (2004, B2). They suggest there are two main contenders in the case of proper names: ‘the descriptivist view of reference’, associated with
Frege and Searle, and ‘the causal-historical view of reference’, associated with Kripke. In the original experiments MMNS use a probe based on Kripke’s Gödel-case, where information associated with the name ‘Gödel’ (‘the man who proved the incompleteness of arithmetic’) turns out not to be true of the original bearer of the name, a, but of someone else entirely, b. The subjects in the experiment are asked to determine whether a speaker, John, who uses the name ‘Gödel’ is talking about the person who really discovered the incompleteness of arithmetic (b) or the person who got hold of the manuscript and claimed credit for the work (a). Descriptivism predicts that the subjects will take ‘Gödel’ to refer to b, whereas Kripke’s theory predicts that they will take it to refer to a (i.e. to the person who has been called ‘Gödel’). And the experiments showed that Western participants were more likely than the Chinese to have Kripkean intuitions: 1.13 versus 0.63. Put in terms of percentages, 58% of the Westerners had intuitions in accordance with Kripke’s theory, while only 28% of East Asians had such intuitions (Machery 2012).

What precisely is it that is being tested here? It is important at the outset to note that ‘the theory of reference’ can be understood in two quite different ways: As a theory in descriptive semantics or as a metasemantic theory (sometimes called “foundational semantics”). Descriptive semantic theories provide an account of the semantic content of a term – for instance, traditional descriptivism, the cluster theory, and Millianism are all versions of descriptive semantics. Metasemantic theories, by contrast, provide an account of the facts in virtue of which a term has a certain semantic value – examples are dispositional theories of meaning, causal theories, social externalism and internalism. There is no one-one relation between semantic theories and meta-semantic theories. Thus, although descriptivist theories are typically combined with internalism it is possible to combine a descriptivist

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5 See Stalnaker 1997 for the distinction between descriptive semantics and foundational semantics. In what follows I shall refer to descriptive semantics simply as ‘semantics’, in order to avoid conflating descriptive semantics with one particular such theory – descriptivism.
semantics with an externalist metasemantics, and the causal-historical theory is not the only metasemantic theory compatible with Millianism. Moreover, as argued in Häggqvist & Wikforss 2015, precisely because there is not a one-one relation between semantics and metasemantics the latter are one step further removed from the empirical evidence. Evidence that S uses ‘Gödel’ to refer to Gödel rather than Schmidt, is in the first place evidence that ‘Gödel’ refers to Gödel in S’s language, not evidence that Kripke’s causal-historical theory is correct. It may be that Kripke’s metasemantics is the best way of accounting for the fact that ‘Gödel’ has this reference (assuming that it does) but that, again, requires arguments going beyond what the empirical evidence provides, such as arguments appealing to metaphysical and epistemological considerations.6

MMNS are not clear on the distinction between semantics and metasemantics, and assume that what they are testing is Kripke’s causal-historical picture.7 This has caused confusion in the debate and made it hard to assess the evidential value of their experiments. What the experiments primarily test is not Kripke’s metasemantics but the semantic thesis that ‘Gödel’ (in the context) refers to Gödel rather than Schmidt. This, in turn, provides a test of descriptivism: If there is evidence that a speaker (of group of speakers) use ‘Gödel’ to refer to Gödel, then there is evidence that (traditional) descriptivism is false (for this speaker or group of speakers). If traditional descriptivism is false there are many alternative semantic

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6 As usual, there is a form of confirmation holism here. If well established theoretical considerations throw doubt on the causal-historical theory then the proper conclusion may be that the suggested semantic theory is not sustainable – despite the evidence provided from speaker intuitions. In Häggqvist & Wikforss (forthcoming) we argue that this is indeed the case when it comes to natural kind terms.

7 Machery at one point mentions metasemantics and suggests that theories of reference ”belong squarely to metasemantics” (2012, 38). The trouble is that classical descriptivism of the sort attacked by Kripke (and defended by Frege and Searle) does not belong with metasemantics. Marti, similarly, does not distinguish between descriptive semantics and metasemantics and accepts MNNS’s assumption that ’old-style descriptivism’ and ‘the causal-historical picture’ are theories of the same type. It would be possible to put forth a metasemantic version of descriptivism (for linguistic meaning, but not for mental content) but, again, this is not the standard version.
theories, including Kripke’s Millianism, and these, in turn, need be coupled with a
metasemantic theory. The empirical evidence is therefore relevant to the metasemantics but
not in the very direct way suggested by MMNS. Indeed, it is hard to see how one could test
metasemantic theories directly, by trying to elicit lay speaker intuitions. The question of what
determines meaning and content is a highly theoretical question in metaphysics and there is
little reason to think that ordinary speaker intuitions about these matters would have any
evidential value. In this case, it would seem, the expertise defense would be legitimate.8

This does pose a problem for MMNS given that the original idea behind their
experiments was that documented cognitive differences between Westerners and East Asians
would have implications for the theory of reference. In particular, their hypothesis was that
because Westerners are more disposed to focus on causation in describing the world, and East
Asians more inclined to make judgments on the basis of similarity, they would be more
inclined towards the causal-historical theory. This requires the problematic assumption that,
somehow, speakers have implicit metasemantic theories and, moreover, that these theories are
influenced by their implicit metaphysical theories. While it may be plausible that lay speakers
have implicit theories about the reference and truth-conditions of their terms, it is far less
plausible to think that they have implicit metasemantic theories and even less plausible that
they have implicit metaphysical theories that precede and inform their use of language.9 The
connection between Western ways of thinking and Kripkean semantics would therefore seem
rather contentious.10

8 See Ostertag 2013 for a related discussion. He suggests that whereas who or what ‘Gödel’
refers to is a reasonable experimental question, how ‘Gödel’ refers is a foundational question
that it would make little sense to ask an experimental subject (2013, 71).
9 There is of course the claim, explored within cognitive science, that language influences
cognition (for a recent discussion see Lai & Boroditsky 2013). However, here the influence is
in the other direction. For instance, there is evidence that in languages written from the right
to the left the flow of time is construed as going from the right to the left.
10 It is contentious in any case, as noted by Marti, since descriptivism is a distinctively
individualistic semantics and East Asians tend to have a less individualistic, ’socio-centered’
However, even if this undermines the larger project that MMNS were involved in, the fact remains that the experiments did establish a surprising degree of variation in intuitions about the use of proper names, both across cultures and across individuals within a culture.\textsuperscript{11} My focus, therefore, shall be less on the question whether the intuitions tested by MMNS provide evidence for the causal-historical theory and more on whether they provide evidence for the first step. That is, do these speaker intuitions provide evidence about the reference of ‘Gödel’? If they do, then the fact of variation in intuitions does provide a challenge that need be addressed.

It has been much discussed in the recent literature precisely what intuitions are and, in particular, whether intuitions are distinct from beliefs.\textsuperscript{12} In the case of MMNS, it is clear that intuitions do not constitute a particular category of psychological states, but are merely to be understood as direct, unreflective judgments, i.e. beliefs. Semantic intuitions are assumed to provide evidence, Machery writes, in just the same way that ordinary judgments are often taken to provide evidence for particular facts: “If I judge of an object that it is a chair, my judgment that it is a chair is evidence that it is a chair because I am reliable at sorting chairs from things that are not chairs” (2011: 39).

This needs to be spelled out with some care since in itself, clearly, my belief that \( p \) does not provide evidence for my belief that \( p \) – that would mean that there is evidence for anything I happen to believe. Rather, the idea must be that if a third party has reason to think that people are reliable when it comes to \textit{certain types of judgments}, such as judgments about middle-sized objects in front of them, then the fact that someone judges that \( x \) is a chair is

\begin{footnotesize}
\begin{enumerate}
\item Further experiments have shown that the results are robust and that the variation in intuitions across speakers and cultures remains. For a summary and discussion see Machery 2015.
\item See for instance Boghossian 2014.
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evidence (for the third party) that \(x\) is a chair.\(^{13}\) Similarly, if a third party has reason to think that people are reliable when it comes to judgments about who someone is speaking about, such as the judgment that John is speaking about Gödel, then the fact that someone judges that \(S\) is speaking about \(x\) provides evidence (for the third party) that \(S\) is speaking about \(x\).\(^{14}\) This, at any rate, seems to be the idea underlying the appeal to intuitions in MMNS and in later work carried out by Machery and collaborators. Next, let us consider Marti’s challenge to MMNS’s appeal to intuitions.

## 2. The evidential value of the MMNS intuitions

### 2.1 Meta-linguistic intuitions?

According to Marti 2009 the MMNS experiments test the wrong type of intuitions and hence they fail to prove what they purport to prove. By asking *what the term refers to*, she argues, what is being tested is not how the participants use their names, but which theory of reference they *think* is correct. A subject may think descriptivism is correct and still use proper names in accordance with Millianism. Indeed, Marti suggests, “the experiment does not provide any evidence at all about name use” (2009, 46). What the variation in intuitions shows, according to Marti, is merely that East Asians are better disposed towards a descriptivist *theory* of proper names, not that they actually use their terms in accordance with descriptivism. The important question is whether people in fact use their terms descriptively or not, and to determine this we would need an experiment that posed a very different kind of question, prompting people to use their terms in ordinary first-order judgments, not to produce meta-linguistic judgments. For instance, Marti suggests, we might ask people what they think about

\(^{13}\) For a related point see Williamson 2016.

\(^{14}\) If one wishes to avoid a purely reliabilist story here, the same point can be put in terms of propositional evidence: The relevant evidential relation would then be that between the proposition ‘\(S\) believes that John is speaking about Gödel’ and the proposition ‘John is speaking about Gödel’ — i.e. the idea would be that truth of the former is indicative of the truth of the latter.
John’s reaction when he exclaims “Today is a sad day: we have found out that Gödel was a thief and a liar” (after it has become known that Schmidt discovered the theorem). The descriptivists should balk at John’s exclamation, she points out, since they simply use ‘Gödel’ to refer to whoever discovered the theorem (in this case Schmidt).

Marti’s criticisms illustrate that there is an important difference between studying the ordinary, first-order use of a term, and studying semantic intuitions in MMNS’s sense. Ordinary first-order use, such as a speaker assenting to ‘Gödel was a thief and a liar’ express judgments but these are not about the semantic properties of any linguistic items at all, but about the world. The terms are used, not mentioned. Consequently, they do not qualify as semantic intuitions. Of course, some of these judgments may qualify as intuitions nevertheless, in virtue of being unreflective and not result of conscious reasoning, but they are not semantic intuitions.15

So Marti is quite right to point out that what MMNS are testing is not first-order use, non-semantic judgments, but judgments that are ‘meta-linguistic’ in the sense that they concern linguistic items. She is also quite right to suggest that non-semantic judgments would provide evidence for the theory of reference. This, of course, is a familiar idea within the philosophy of language. For instance, it plays a central role in Davidson’s account of radical interpretation. According to Davidson the speaker’s pattern of holding sentences true and false (i.e. ordinary first-order use), provides evidence for the semantic theory (given certain assumptions about her beliefs and desires, i.e. the principle of charity): If a speaker holds ‘Es regnet’ true whenever it rains this provides evidence that ‘Es regnet’ is true if and only if it is raining.16 Naturally, the speaker may be confused or deluded in some way, and so the evidence is defeasible, but the fact that a subject holds a sentence true under certain

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15 As Cohnitz & Haukioja (2015) put it: “That Gödel should not have claimed credit for the incompleteness theorem is perhaps a moral intuition, but it is certainly not a linguistic intuition”.

circumstances provides prima facie evidence that the sentence is true under these circumstances, and hence prima facie evidence for the truth conditions of the sentence. So, first-order use judgments provide evidence, just as Marti suggests, and these judgments cannot be characterized as ‘semantic intuitions’.

However, even if this is so, it is a mistake to dismiss the evidential value of the intuitions elicited by MMNS’s experiments. First, it is important to note that MMNS are not really inviting the individuals to reflect on ‘what they think the correct theory of reference determination is’, as Marti puts it. After all, they are not asked to consider whether descriptivism is correct. And they are certainly not asked to make a judgment on the metasemantics of proper names – on whether they have an causal-historical or internalist metasemantics. The subjects are just asked to consider who John is talking about when using the name ‘Gödel’. The relevant intuitions, therefore, do not require knowledge in semantics or possession of any semantic concepts beyond that of ‘speaking about’ and can therefore not be considered problematic from the point of view of the (cognitively mature) layperson. Indeed, the notion of ‘speaking about’ is a pre-theoretical notion, one that cannot be equated with the semanticist’s concept of reference. The latter concept has a specific role in semantic theory and it is not plausible to take ordinary speakers to possess it. The intuitions elicited by MMNS’s experiments therefore belong to the less theoretical end of the spectrum, even if they do involve the notion of ‘speaking about’.

Moreover, there are reasons to think that these basic judgments concerning who someone is talking about are reliable. The capacity to use a proper name in communication would seem closely linked with the capacity to determine what or who another person is talking about when using these terms.\(^{17}\) One should therefore expect the first-order use of a name such as ‘Gödel’ to cohere with beliefs about who someone else is talking about when

\(^{17}\) For a related point see Ichikawa et. al. 2012, note 3.
using the name (on the assumption that the subject and the other person speak the same language).\(^{18}\) To the extent, therefore, that one takes the first-order judgments to be reliable, as Marti does, there is reason to expect the aboutness judgments to be equally reliable. As Machery puts it in a reply to Marti: “People have extensive experience with the use of proper names, they give and get feedback when names are misused, they observe others using proper names and getting feedback, and so on” (2014: 14).

There is one further issue here deriving from the fact that the notion of ‘speaking about’ cannot be equated with the semanticist’s notion of reference. As pointed out in the literature, the experiments fail to distinguish between semantic reference and speaker reference (Deutsch 2009). That a subject takes John to be ‘speaking about’ Gödel, does not yet tell us whether the subject takes ‘Gödel’ to refer to Gödel, since it is possible that the subject is simply thinking about who the speaker, John, is referring to when using ‘Gödel’.

This is no doubt a valid point but it does not undermine what I have said above. Just as it would be a mistake to equate the ordinary notion of ‘speaking about’ with that of reference, it would be a mistake to equate it with the semanticist’s notion of ‘speaker reference’, making it a clearly pragmatic notion. The more plausible conclusion is that there is an ambiguity in the notion of ‘speaking about’, one that speakers do not usually have to worry about since semantic reference and speaker reference normally coincide. When the two come part, disambiguation is required: In one sense the speaker is speaking about Smith, since he just said ‘Smith is raking the leaves’, but in another sense he is speaking about Jones (since he mistook Jones for Smith).\(^{19}\) These cases typically involve a misunderstanding or mix-up and clues about the context, taken together with knowledge about the ordinary semantic reference of the proper name, allows the hearer to infer whom the speaker intends to speak about. They are therefore parasitic on the ordinary cases, where the speaker speaks about a person by

\(^{18}\) This is an important assumption and I shall return to it below.

\(^{19}\) See Kripke 1980, 25, n. 3.
using a name that refers to that person. This is just an instance of how pragmatic interpretation in general depends on semantics. Even if, on occasion, ‘aboutness judgments’ concern speaker reference and not semantic reference, there is therefore reason to think that these are the exceptions: In general, speaker reference and semantic reference coincide which means that, in general, ‘aboutness judgments’ track semantic reference.  

2.2 ‘Linguistic intuitions’

I have argued that the aboutness judgments tested by MMNS do provide evidence for semantics: The judgments are not theoretically demanding and they play a role in communication which suggests that they are sufficiently reliable to function as evidence for propositions about semantic properties, such as ‘Gödel’ refers to Gödel in the language of speaker S. This is corroborated by further experimental evidence.

In a response to Marti’s criticisms, Machery, Olivola & De Blanc (2009) have carried out new experiments testing whether the intuitions of the 2004 experiments are in agreement with more direct evidence from use, what they call ‘linguistic intuitions’. The latter are intuitions about truth values of sentences. For example, subjects are asked whether they consider the following sentences true or false: ‘When Ivy says, ’Tsu Ch’ung Chih was a great astronomer’, do you think that her claims is: (A) true or (B) false?’ (2009, 690).  

The conclusion is that the two types of intuitions are in agreement and that the variation between cultures and within cultures remains when the test questions are not metalinguistic in Marti’s sense.

20 Indeed, in recent experiments carried out by Machery, Sytsma and Deutsch (2015) probes that eliminate the ambiguity between speaker reference and semantic reference are used, and the cross-cultural variation remains.
21 The Tsu Ch’ung Chih case is parallell to that of Gödel, where the role of Gödel is taken over by Tsu Ch’ung Chih who stole an important discovery from an astronomer who died soon after making the discovery.
It has been objected that the questions asked in the new experiments are still too theoretical, encouraging speakers to consider the properties of sentences rather than to use these sentences. For instance, Hansen (2015) writes that since the ‘linguistic prompts’ also are metalinguistic “this result doesn’t address the question whether metalinguistic intuitions and ‘linguistic’ intuitions are ‘largely congruent’ (as Machery et.al. claim it does).”\(^\text{22}\)

Similarly, Marti (2013) objects that the questions asked are still too theoretical. She grants that there is a difference between the linguistic question asked in the new experiments, and the meta-linguistic asked in the earlier experiments, since the new question is one about truth values, and hence testing subjects’ intuitions about truth conditions. And, she suggests, “it is undeniable that competent speakers of a language have by and large correct intuitions about the truth conditions of the sentences they use” (2013, p. 75). This much is required in order for speakers to interact with one another and communicate. However, she argues, it is one thing to have these intuitions and act on them, as in the interaction with others, quite another to reflect on them and verbalize them. Moreover, she argues, there are general reasons to distrust the meta-level beliefs since there are often “wide differences between how people do things and how they think they do them” (2013, 71). For instance, there is the striking evidence concerning implicit bias, which seems to suggest a discrepancy between our explicit beliefs and our intuitive reactions. Hence, we still need evidence that is more directly supplied by the actual use: we need the subjects to use ‘Gödel’, not to reflect on someone’s practice using the name.

Now, there is no doubt that the distinction between ‘linguistic’ and ‘metalinguistic’ intuitions is not a very sharp one. After all, both types of intuitions involve judgments about semantically relevant properties of linguistic items – ‘aboutness’ and truth. So Marti is quite right to suggest that there is not a very great difference between the types of

\(^{22}\) Hansen 2015, section 3.2.1.
judgments tested in the two experiments. She is also quite right to stress that it is one thing to have an intuition, and act on it, quite another to *report* this intuition: Things can go wrong when we try to explicitly report on our implicit beliefs (as evidenced by experiments on implicit bias). However, none of this shows that the reported linguistic intuitions do not provide evidence for the semantic theory. As Marti herself notes, there are reasons to expect people to be reliable in their judgments about the truth values of sentences and reliability is all that is required for the judgments to play an evidential role. Moreover, although one does need to pay attention to the potential gap between the beliefs a subject has, and the beliefs she reports, it seems unlikely that a very large gap would open up in this case: If one believes that Kurt said something true when uttering ‘Gödel was a thief and a liar’, and one is asked to report on one’s belief, it does not seem as if there are any very obvious psychological mechanisms that could come in the way (unlike the case where people are asked to judge the truth value of sentences such as ‘Women are equal to men in intellectual capacities’).

I therefore agree with Machery et. al. that we have reasons to think that both the intuitions in the original experiments, and those in the new experiments, provide evidence for the semantic theory. There is one complication, however, which I suspect is the real issue underlying the debate between Machery et.al. and Marti. In both the original 2004 experiments and in the 2009 experiments, the test subjects are asked to make a judgment about someone else’s use of language: They are asked to determine who *John* is talking about when using a proper name, or whether *Ivy* says something true when making a certain assertion. The assumption, clearly, is that judgments of this sort provide evidence for the subject’s *own* language, the semantics that holds true of her language: If someone is inclined to judge that John is using ‘Gödel’ to speak about Schmidt, then this is evidence that she herself uses proper names in accordance with descriptivism. Arguably, this is not a very problematic assumption – we normally use our own linguistic competence to interpret other
people’s speech. But it need not always be true. As Davidson has stressed, we tend to adjust our theories of interpretation depending on who we are talking to. For instance, we may know of Mrs. Malaprop that she is an unreliable user of the common language.\(^\text{23}\) This may not be a very big worry when it comes to the probes used in either the 2004 or 2009 experiments. After all, the test subjects know very little about John and would therefore have every reason to assume that John uses proper names in the way the speaker herself uses them. Nevertheless, it is a worry that MMNS have not paid attention to and it may well be the main worry that underlies Marti’s objections to testing meta-linguistic intuitions.\(^\text{24}\)

To avoid this complication it would be better simply to design experiments that do not require the subject to make judgments about the language of another speaker, but simply about her own language. This should minimize the risk of prompting the subject to theorize that worries Marti. What is of importance then, is not so much that linguistic (or meta-linguistic) questions are avoided, as that the questions directly trigger the subject’s own linguistic competence without involving the task of interpreting others. For instance, instead of asking about the truth value of Ivy’s assertion the question could simply be about the truth value of sentences about the world: “In this scenario (as described by Kripke) would you say that the following is true or false

(i) ‘Gödel was a liar and a thief’

(ii) ‘Gödel was a great mathematician’”

\(^{23}\) See Davidson 1986. This is related to the worry that the probes are ambiguous between the epistemic perspective of the narrator (or reader) of the vignette, who knows the descriptive information associated with ‘Gödel’ is false, and the perspective of the character in the vignette, John. See Systma & Livengood 2011 and Genone 2012: 157.

\(^{24}\) Indeed, in the 2013 paper Marti stresses that it is problematic that the test subjects are asked for intuitions about others’ use of names (2013, 75). Notice, though, that the probe suggested by Marti (2009), still involves reflection on another speaker’s language, since the question asked to the test subjects is what they think about John’s reaction when he exclaims “Today is a sad day: we have found out that Gödel was a thief and a liar”.

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This would prompt the subject simply to apply her own linguistic competence, without the complications introduced by thinking of the language of another subject. And it should be clear that even if the intuitions tested are in some sense meta-linguistic (since they concern the truth-values of sentences) they do provide important evidence for the semantic theory since, as suggested above, it is very plausible that facts about what sentences a subject holds true and false, provide evidence for facts about the truth conditions of these sentences.

3. Accommodating variation

I have argued that MMNS’s experimental results cannot be dismissed. If indeed a speaker (or a group of speakers) has the intuition that John is speaking about Schmidt, not Gödel, then this is important evidence for the semantic theory. The evidential role, here, involves two steps: First, it is evidence that John is speaking about Schmidt (since competent speakers are reliable in judging who someone is speaking about) and therefore that ‘Gödel’ refers to Schmidt in John’s language (since speaker reference normally coincides with semantic reference). Second, it is evidence that the speaker herself uses proper names in accordance with a descriptivist semantics (since speakers typically apply their own semantic theory when interpreting others). Again, intuitions of this sort are not the only type of empirical evidence that is of relevance. Moreover, other intuitions are arguably of relevance as well, such as the modal intuitions appealed to by Kripke. However, that there is more evidence that is of relevance, does not show that the basic ‘aboutness intuitions’ (or the truth value intuitions) lack evidential value.

If, therefore, the variation in intuitions documented by MMNS cannot be written off as lacking evidential value, the question is what conclusion the semanticist should draw.

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25 This is stressed by Devitt 2011, 2012, and by Ostertag 2013.
As noted above, MMNS suggest that there are three ways to accommodate this variation. They spell them out as follows (2013: 620-621):

(i) Rejection of the method of cases: Intuitions are not reliable guides to the reference of names – names refer in the same way in all languages.

(ii) Referential pluralism: Names refer differently in different cultures (and across individuals). Philosophers of language need to get out of their armchairs and examine the intuitions of ordinary competent speakers (across cultures).

(iii) Expertise defense: Some intuitions but not others are reliable guides to the semantic properties of names – those of philosophers of language (and linguists). \(^\text{26}\)

In the debate, it is often assumed that MMNS advocate option (ii), and that their main goal is to question Kripke’s anti-descriptivism. Marti, for instance, starts her response to MMNS by writing that “Kripkean anti-descriptivism about proper names has recently come under attack” (2009: 42). However, it is quite clear that the intention of MMNS was never to attack Kripke, and that the conclusions they draw from their experiments are rather different. Indeed, they stress that their goal has never been to challenge Kripke’s argument against descriptivism (2013: 620), and they claim that their argument undermines causal-historical theories as much as descriptivist theories insofar as causal-historical theories are justified by appeal to intuitions of the sort in question (ibid. 632).

What, then, are they up to? It is quite clear, I think, that their aim is to question the very project of developing a theory of reference. Their central claim is that all three options of accommodating the variation in intuitions are unacceptable, and that, therefore, we should abandon the attempts to provide a theory of reference. Option (i) is unacceptable since if philosophers of language reject the method of cases they owe an account of how the correct theory of reference is determined – some further considerations, some evidence, would be

\(^{26}\) See also Machery 2012, 41, and Machery 2014, 8.
required. And, MMNS add, they have “no idea what other considerations philosophers of language might appeal to” (MMNS 2009, 343). Referential pluralism, on the other hand, involves giving up the search for a general theory of reference, which would undermine the goal of the theory of reference to begin with. The result would be a ‘sea change’ in the methods of philosophy of language, forcing philosophers to work piecemeal, and engaging in empirical investigations of how the terms of individual speakers refer (MMNS 2013: 261, Machery 2011: 41 and 2014: 8). Finally, the expertise defense is unacceptable since there is no reason to expect experts to have more reliable intuitions about the reference of names than ordinary, lay speakers. Since they take all three options to be problematic they conclude that we should be skeptical of the whole project of developing a theory of reference. Indeed, in their original paper this idea is quite explicit. Noting that the results of the experiment constitute prima facie evidence that aboutness intuitions vary from culture to culture, they state: “the paper argues that this fact raises questions about the nature of the philosophical enterprise of developing a theory of reference” (2004: B 1).

The project of MMNS, therefore, is best understood as a skeptical project, where variation in intuitions is taken to throw doubt on the philosophical project in question.

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27 See also Machery 2014, 8. It should be clear that this statement is an exaggeration and that there are all sorts of other considerations which philosophers of language can and do take into account – both other empirical evidence (concerning cognitive processing, for instance) and more theoretical considerations (concerning explanatory power, the nature of communication, etc.).

28 See also MMNS 2009 where they suggest that we should give up on the idea that the search for a substantive theory of reference is a viable enterprise. Machery, at points, even seems to lean towards questioning the very idea that “there is such a thing as reference” (2012: 43). See also Machery 2015.

29 It should be noted that MMNS’s skepticism about the project of developing a theory of reference hangs together with the fact that their original interest in experimental semantics does not stem from an interest in semantics, but from a quite different source, from an interest in so called ‘arguments from reference’: Arguments that attempt to derive philosophically significant conclusions from theories of reference. For these arguments to succeed, MMNS stress, it is essential that one is able to provide independent evidence for the theory of reference. The belief that there can be no such evidence is therefore an important aspect of their criticism of arguments from reference (see in particular MMNS 2009).
Nadelhoffer & Nahimas 2007 label this version of experimental philosophy ‘experimental restrictionism’, ER. Experimental restrictionism relies on data concerning cognitive diversity to argue that philosophers should not use intuitions as evidence in their theorizing.

Nadelhoffer and Nahimas write: “ERs argue that if our intuitions about a particular topic vary cross-culturally or socio-economically and we don’t have independent grounds for privileging our own intuitions to those of others, these particular intuitions will be insufficient for philosophical theory building” (2007, 128).

I think this is all overly pessimistic – at least when it comes to semantics. I shall not pronounce on ‘experimental restrictionism’ generally, but in the case of semantics there is very little reason to take variation in intuitions (if real) to support skepticism about the project of developing a theory of reference. No doubt, if what we have is not variation but chaos, with no stable intra-individual intuitions perhaps skepticism is warranted. However, the MMNS results do not point in this direction. Indeed, I think, response (ii), semantic pluralism, is the correct one. If there are relevant differences in intuitions (across individuals and cultures) it is a live hypothesis that these reflect real semantic differences. Why have people found this type of pluralism so objectionable?

Here, again, it is of importance to be clear about what type of theory the ‘theory of reference’ is supposed to be. For instance, if by ‘the theory of reference’ one has in mind not (descriptive) semantics but a theory concerning the nature of reference, then individual (and cultural) variation would seem to be problematic. Indeed, this seems to underlie MMNS’s rejection of semantic pluralism: The nature of reference, clearly, cannot vary from one culture to another, from one individual to another. I also think that one reason philosophers of language have felt unease concerning option (ii), is that they too have taken the theory of reference to be concerned with the nature of reference. For instance, Michael Devitt (2011) suggests that “semantics is concerned with the nature of meaning, reference,
and the like” and argues that semantics therefore is best left to the experts. And Marti suggests that we should resist the conclusion that in some cultures descriptivism is true, since that would undermine the project of providing a general theory of reference (2009: 43).30

However, as noted above, it is far from clear how intuitions concerning Kripke’s Gödel-case and the like could speak to this very general project of providing a theory about the nature of reference. If Kripke is right, then proper names and natural kind terms have a Millian semantics, and if indeed such a semantics requires a causal-historical metasemantics, then we have reasons to adopt a certain metasemantics for proper names and natural kind terms. However, it hardly follows that this metasemantics applies to all types of terms. For instance, there is widespread skepticism about Putnam’s proposal that artifact terms can be treated like natural kind terms and that an externalist metasemantics applies to them. No doubt, some philosophers of language are concerned with the nature of meaning and reference. In particular, there are reductionist, metasemantic projects that claim to explain the reference relation in terms of underlying naturalistic facts.31 But these projects are highly theoretical, only loosely connected with the types of intuitions relied on in the debate over Kripke’s anti-descriptivism.

If, instead, one construes the ‘theory of reference’ simply as a (descriptive) semantic theory, then there is no principled reason why the theory should have to be general. Semantic theories in this sense, as stressed above, are clearly empirical theories, true of a particular language at a particular time. Such a theory need not be general in the sense that it need be true of all speakers, not even of all speakers within a given community. On the contrary, it is an open, empirical question what language any particular individual (or group of individuals) speaks. MMNS, again, suggest that taking this seriously, and adopting

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30 See Jackman 2009 for a theory about the nature of reference (drawing on Davidson) that is intended to accommodate cultural variation in descriptive semantics.
31 For example, Fodor’s version of the causal theory and Millikan’s teleosemantics are plausibly construed as general theories about the nature of reference in this sense.
semantic pluralism, would lead to a ‘sea change’ in the methods of philosophy of language, forcing philosophers to engage in empirical investigations of how the terms of individual speakers refer. To the extent that the philosopher of language wishes to provide a semantics of a given language, this is a sea change that would seem required. As should be clear from above, this is not to suggest that all questions that is the concern of philosophy of language require empirical investigations or that there is no place for philosophical expertise. It is to say, however, that certain parts of philosophy of language need be more empirically oriented – in particular, those that are concerned with finding the correct semantic theory of a given language.32

Another worry concerns the possibility of communication. If indeed we draw the conclusion that people use proper names in different ways, so that some speakers use them in accordance with descriptivist theories and others in accordance with Kripke’s theory, would we not also have to conclude that communication fails? Surely, it might be argued, no respectable semantic theory can allow for such a conclusion. Consequently, semantic pluralism is not a live option. This raises complex questions about the nature of communicative success. In particular, one would need to determine whether indeed communicative success requires sameness in the content of the message sent and the message received, or whether some weaker relation will do, such as sufficient similarity.33 For now, however, it is worth noting that the objection is dialectically odd. Consider the original MMNS experiments. If indeed people have very different views concerning who John is speaking about when using ‘Gödel’, then the plausible conclusion would seem to be precisely

32 A vivid illustration of the need for empirical methods is provided by the case of natural kind terms. Philosophers have largely accepted Kripke’s account of these, without much (or any) empirical evidence. However, it is a very good question whether people in fact use natural kind terms to pick out underlying essences. Evidence from experimental psychology would suggest the opposite (see Hampton et.al. 2007), as does evidence from the history of science. For a discussion, see Hansen 2015, Häggqvist & Wikforss 2015 and Nichols et. al. 2016.

33 For a discussion, see Pagin 2016.
that communication will fail. For instance, assuming John was an actual speaker who takes himself to be talking about Gödel, a subject who takes him to be talking about Schmidt will fail to understand John. No amount of a priori reasoning can take away this threat to communication between the subject and John.

In conclusion, the findings of MMNS 2004 should be taken seriously. What is being tested are basic ‘aboutness’ judgments that are not theoretically demanding and that there’s every reason to expect people to be able to make reliably. If there is a great deal of variation in these judgments, this is not something that can be dismissed by appealing to armchair considerations, but something that need be taken into account as important empirical evidence when constructing the semantics of the language(s) of the speakers in question.34

References


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