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EXPERIMENTAL SEMANTICS  
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**1. Introduction**

In their delightfully provocative paper, “Semantics, Cross-Cultural Style,” Edouard Machery, Ron Mallon, Shaun Nichols, and Stephen Stich (2004),<sup>1</sup> make several striking claims about theories of reference. First, they claim:

(I) Philosophical views about reference “are assessed by consulting one’s intuitions about the reference of terms in hypothetical situations” (p. B1).

This claim is prompted by their observations of the role of intuitions in Saul Kripke’s refutation of the descriptivist view of proper names in favor of a causal-historical view (1980). The particular intuitions they attend to are those aired in discussing Kripke’s cases of Gödel and Jonah. This prompts the next claim:

(II) Those particular cases are “central” to Kripke’s refutation (p. B1).

Indeed, Machery *et al* describe these cases as “some of the most influential thought experiments in the philosophy of reference” (p. B8). Inspired by recent work in psychology (e.g., Nisbett *et al* 2001) that shows “systematic cognitive differences between East Asians and Westerners” (p. B1), Machery *et al* predicted that there would be cultural differences in referential intuitions. They conducted some ingenious experiments on Gödel and Jonah cases to test this predication. The results in the Gödel cases, although not in the Jonah cases, confirmed their prediction, leading them to conclude: “Westerners are more likely than East Asians to report intuitions that are consistent with the causal-historical view” (p. B1). And, implicitly, they claim:

(III) These results raise serious doubts about Kripke’s refutation, which relies solely on the intuitions of Westerners.

They are explicit about the following bolder claim:

(IV) The fact of these cultural differences “raises questions about the nature of the philosophical enterprise of developing a theory of reference” (p. B1); it points to “significant philosophical conclusions” (p. B8).

Four preliminary points. First, Machery *et al* are surely right in claiming that theories of reference are assessed by consulting intuitions: this practice does seem to be *the* method of semantics. However, claim (I) exaggerates the role of intuitions about the *reference* of terms in *hypothetical* situations. I shall describe the exaggeration in a moment. Second, one should wonder, as Stich (1996, 2009) and I (1994, 1996, 2009a)

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<sup>1</sup> All unidentified references are to this paper.

both have, about the appropriateness of semantics' apparently total reliance on intuitions for evidence. I shall address this issue later. Third, I agree with Machery *et al* that, insofar as theories of reference rest on intuitions, it is appropriate to test those intuitions. Indeed, as a card-carrying naturalist (2009b), I am as concerned as they are to put semantics in a scientific setting. Fourth, I accept that the Gödel experiments provide puzzling evidence against Kripke's refutation and I shall make no attempt to show otherwise. But I will argue that the evidence is far less significant than Machery *et al* suppose.

I start by arguing, in section 2, that Machery *et al*'s claim (II) exaggerates the importance of Gödel and Jonah cases to Kripke's refutation of descriptivist theories. This is doubtless related to the just-mentioned exaggeration in claim (I). Certainly *some* intuitions used to support theories of reference are about reference in hypothetical situations – intuitions about Gödel cases are examples – but many intuitions used are not of this sort. The most important intuitions about reference in Kripke's refutation are about *actual* not hypothetical situations; and other key intuitions are not semantic ones about reference but metaphysical ones about *modal properties*. The Gödel case is certainly famous among philosophers – it is the sort of fantasy that charms us – but it is not very important to Kripke's refutation. So Machery *et al* did not choose the best intuitions to test. Next, in section 3, I shall apply a general view of intuitions and their evidential role to argue that although the intuitions of the folk do have weight for theories of reference we should prefer those of philosophers. Finally, in section 4, I shall propose a way of testing theories of reference that does not rest on intuitions about reference. Each of these three sections provides an argument that serves to diminish the significance of Machery *et al*'s findings and to respond to claims (III) and (IV).

## 2. Intuitions in Kripke's Refutation

The first step in seeing that claim (II) exaggerates the significance and influence of intuitions about Gödel and Jonah cases is to note that Kripke's refutation of descriptivist theories of names consists in *three* arguments. Kim Sterelny and I have called these three, "Unwanted Necessity," "Lost Rigidity," and "Ignorance and Error."<sup>2</sup> Intuitions are certainly the basis of all these arguments. And intuitions about Gödel cases do feature in the Lost Rigidity and the Ignorance and Error argument. But, as we shall see, they are not the dominant intuitions in those arguments and they do not feature at all in the Unwanted Necessity argument.

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<sup>2</sup> Devitt and Sterelny 1999: 48-59. The following discussion of Kripke's arguments draws on the more detailed discussion in those pages. Kripke's Ignorance and Error argument, and a similar argument by Keith Donnellan (1972), were totally novel. Kripke's Lost Rigidity argument was near enough so. The Unwanted Necessity argument against the "cluster" descriptivist theory was novel but the one against the classical theory was already familiar and was one of the motivations for the cluster theory; see Searle 1958. Two other arguments against the classical theory – what we called "Principle Basis" and "Unwanted Ambiguity" – were known from the beginning, as Kripke notes (1980: 30).

Consider Ignorance and Error first. Descriptivist theories are naturally construed strongly as theories of meaning: the *meaning* of a proper name is expressed by the description that competent speakers associate with the name, a description that identifies the referent. A weaker construal makes no claim about meaning: the associated description simply identifies the referent. Kripke showed, with a range of *actual* cases, that most users of a lot of names are not able to supply descriptions that are adequate to identify what are intuitively the referents. So associated descriptions do not determine the referent of a name. So they do not express a meaning that determines the referent. Thus, most users of the names of Cicero, Catiline, Feynman, and Einstein are too ignorant to give identifying descriptions of these people (1980: 80-3). Furthermore, many users of the names of Peano, Einstein, and Columbus associate descriptions that are false of those people (pp. 84-5). Yet Kripke's intuitions – indeed, the intuitions of just about all philosophers - are that these users nonetheless succeed in designating these people with their names.

It is important to note that these cases are *humdrum*: they are not counterfactual, hypothetical, fictional, or in the least bit fanciful. Inspired by them it is *very easy* to come up with countless cases, each just as humdrum and each yielding the intuition that a speaker's use of a name designates an object despite the speaker's ignorance or error about the object.<sup>3</sup> These intuitions are very powerful because *to reject them is to rule that many names out of just about every mouth fail to designate what they should.*

Kripke also produced some cases where error is at least *possible*. These range from cases where the error is very likely actual – Jonah (p. 67, 87) – through cases where it may well be actual– Moses (pp. 66-7) – to cases where it certainly is not actual – Gödel (pp. 83-6).<sup>4</sup> But the key point is that our intuitions about these cases are not as powerful as those about the humdrum cases. The further one gets away from actual error toward hypothetical cases that may seem a bit fanciful, the less confident we should be about our own intuitions and, foreshadowing a bit, *especially, those of the folk*. Thinking about fanciful hypothetical cases is hard, particularly for the folk. As Stich has aptly remarked, with cases like Twin Earth in mind, “nonphilosophers often find such cases so outlandish that they have no clear intuitions about them” (1983: 62n.).

So, my claim is that, although intuitions about Gödel and Jonah cases play a role in the Ignorance and Error argument, intuitions about the humdrum cases play a much more important role. And that argument rests more on intuitions about actual cases than about hypothetical ones. I turn next to the other two arguments. These are aimed only at the strong theory-of-meaning construal of descriptivism. The dominant intuitions in these arguments

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<sup>3</sup> Donnellan has a nice example that shows just how easy it is to come up with humdrum cases. A child is gotten up from sleep at a party and introduced to a person called ‘Tom’. “Later the child says to his parents, ‘Tom is a nice man’...nothing the child possesses in the way of descriptions, dispositions to recognize, serves to pick out in the standard way anybody uniquely” (1972: 364). Yet the child is talking about that very person he was introduced to.

<sup>4</sup> Donnellan's case of Thales is similar (1972: 374).

are not ones about reference in Gödel and Jonah cases, indeed they are not semantic intuitions at all: they are metaphysical intuitions about modal properties.

This is obvious with the Unwanted Necessity argument. As John Searle says, the modern cluster version of the descriptivist theory has the consequence “that it is a necessary fact that Aristotle has the logical sum, inclusive disjunction, of properties commonly attributed to him” (1958: 172). But, as Kripke points out, this is “very implausible.... Most of the things commonly attributed to Aristotle are things that Aristotle might not have done” (1980: 61). Kripke’s claim rests on metaphysical intuitions that each of those things – for example, being taught by Plato – are not essential to being Aristotle.

Turn next to the Lost Rigidity argument. A term is “rigid” iff it designates the same object in every possible world in which that object exists. And the argument is that names are rigid but the descriptions that, according to descriptivism, express their meanings are not. So descriptivism is wrong. We can distinguish four versions of this argument, the first three of which rest simply on modal intuitions. (A) Kripke claims that “although someone other than the U.S. President in 1970 might have been the U.S. President in 1970...., no one other than Nixon might have been Nixon.” So, ‘Nixon’ passes “the intuitive test” for rigidity while ‘the U.S. President in 1970’ fails (p 48). (B) Kripke claims that since Hesperus is Phosphorus, necessarily Hesperus is Phosphorus. But substituting descriptions, say, ‘the planet seen in the evening’ for ‘Hesperus’ and ‘the planet seen in the morning’ for ‘Phosphorus’, turns this truth into a falsehood. The names, unlike the descriptions, are rigid (pp. 3-5, 98-102). (C) Modal sentences like the following have a scope ambiguity:

- (a) It might have been the case that the last great philosopher of antiquity was not a philosopher.
- (b) It might have been the case that Aristotle was not a philosopher

The scope of ‘the last great philosopher of antiquity’ in (a) and ‘Aristotle’ in (b) can be wide or narrow. But, Kripke claims, the two readings of (b) are equivalent but those of (a) are not. This equivalence is a mark of the rigidity of ‘Aristotle’ (pp. 10-12, 62).

(D) Finally, however, we must allow that one version of the Lost Rigidity argument *does* rest on intuitions about reference in a case like Gödel. This version first appeared in the Preface of *Naming and Necessity*. Consider

- (1) Aristotle was fond of dogs.

Kripke says,

Presumably everyone agrees that there is a certain man – the philosopher we call ‘Aristotle’ – such that, as a matter of fact, (1) is true if and only if *he* was fond of dogs. The thesis of rigid designation is simply...that the same paradigm applies to the truth conditions of (1) as it describes *counterfactual* situations. That is, (1) truly describes a counterfactual situation if and only if the same aforementioned man would have been fond of dogs. (p. 6)

This version of the argument, like the possible error argument about Gödel, rests on the intuition that it makes no difference to the reference of a name in a counterfactual situation whether or not the descriptions we associate with the name are true, in that situation, of the object we call by that name. In general, the possible error argument of the Gödel sort is, as I have noted elsewhere, “quite close” to this version of the Lost Rigidity argument (2005: 161, n. 8).

In light of this discussion, we can see that Machery *et al*’s claim (II) exaggerates the centrality and significance of intuitions about reference in Gödel and Jonah cases. These intuitions certainly play a role in the Ignorance and Error argument, but their role is not central and not nearly as significant as that of intuitions about humdrum actual cases. The intuitions also play a role in the Lost Rigidity argument but the dominant intuitions there are metaphysical ones about modal properties. And the intuitions play no role at all in the Unwanted Necessity argument.

This has obvious consequences for an assessment of the significance of the experiments of Machery *et al* for Kripke’s refutation of descriptivism. Machery *et al* do not think, of course, that the experiments *settle* the case against the refutation: “we have no illusions that our experiment is the final empirical word on the issue” (p. B8). But it is important to see how far the experiments are from the final word. Machery *et al* leave untouched the referential intuitions about humdrum actual cases and the metaphysical ones about modal properties. These intuitions together are massively more important to the refutation than the intuitions about Jonah and Gödel cases that Machery *et al* tested. And whereas their test of Gödel intuitions counts against the refutation, *their test of Jonah intuitions confirmed the refutation*. Yet, it seems to me, the Jonah intuitions are more trustworthy than the Gödel ones because the Jonah case is less fanciful than the Gödel one.

I have accepted that the findings of Machery *et al* are evidence against Kripke’s refutation. The discussion in this section is my first step in diminishing the significance of this evidence and hence resisting claims (III) and (IV). It is the first step in resisting the idea that these results raise serious doubts about Kripke’s refutation in particular and about the philosophical enterprise of developing a theory of reference in general.

Finally, in section 1 I criticized Machery *et al*’s claim (I) that theories of reference are assessed by consulting *referential* intuitions about *hypothetical* situations. Such intuitions do play a role but so do many others. To a considerable extent Kripke’s refutation of descriptivism rests on intuitions about actual situations not hypothetical ones. And to a considerable extent the refutation case rests on intuitions about modal properties not about reference. Doubtless the mistake in claim (I) is related to the exaggeration of the significance of intuitions about Gödel and Jonah cases in claim (II).

### **3. Intuitions and Their Evidential Role**

So far we have been considering the actual role of intuitions in Kripke's refutation of descriptivism. What view of the semantic task would make this role appropriate? *Prima facie* semantics is concerned with the nature of meaning, reference, and the like. If so, why would it be appropriate to put all this weight on people's intuitions about these matters? We don't do physics, biology, or economics simply by consulting people's intuitions. Why should semantics be different? Philosophers tend not to address this question explicitly but implicitly seem to endorse the view that the semantic task *simply is* the systematization of our ordinary intuitions about meaning, reference, and the like. Now if this common view were right, so too would be Machery *et al*'s claim (IV): their findings would indeed raise questions about the philosophical enterprise of developing a theory of reference. For, their findings would strike at the very subject matter of semantics. But the common view of the semantic task is deeply misguided; or so I have argued (1996). No biologist thinks that the systematization of our ordinary intuitions about living things is the way to do biology. There is no good reason to think that semantics is different.<sup>5</sup> Perhaps the idea is that it is different because intuitions about meaning and reference are a priori. In my view (2009b), and I suspect in that of Machery *et al*, this is not an idea that we should accept.

So, what role *should* intuitions have in semantics? I agree with Machery *et al* that any intuition that has a role is open to being tested. But how should they be tested? By testing folk intuitions? Or in some other way? I shall start answering these questions in this section. Drawing on other work (2006a,b,c), I shall summarize a view of intuitions in general (of the sort that concern us), and then apply this to the referential and modal intuitions that play a role in Kripke's refutation. Finally, I will contrast my position on intuitions with that of Machery *et al*. In the next section, I shall apply a certain methodology for semantics to recommend ways of testing those intuitions.

I urge that intuitions "are empirical theory-laden central-processor responses to phenomena, differing from many other such responses only in being fairly immediate and unreflective, based on little if any conscious reasoning" (2006a: 103). From this perspective, we should trust a person's intuitions

to the degree that we have confidence in her empirically based expertise about the kinds under investigation. Sometimes the folk may be as expert as anyone: intuitions laden with "folk theory" are the best we have to go on. Perhaps this is the case for a range of psychological kinds. For most kinds, it clearly is not: we should trust

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<sup>5</sup> Machery *et al* briefly mention what may seem to be a reason, a way of justifying the common view (p. B9). (The way is suggested in Stich 1996 and urged by Segal 2001.) Folk semantics might be viewed the way Chomskians standardly view folk linguistics. Their standard view is that speakers derive their grammatical intuitions about their language from a representation of the grammatical principles of the language. We might then take a similar view of referential intuitions: speakers derive them from a representation of referential principles. So, just as, according to the linguists, the true grammar that they seek to discover is already stored in the mind of every speaker, so too, according to this view, is the true theory of reference. I have argued against this standard view (2006a,b,c).

intuitions laden with established scientific theories. Consider, for example, a paleontologist in the field searching for fossils. She sees a bit of white stone sticking through grey rock, and thinks “a pig’s jawbone”. This intuitive judgment is quick and unreflective. She may be quite sure but unable to explain just how she knows. We trust her judgment in a way that we would not trust folk judgments because we know that it is the result of years of study and experience of old bones; she has become a *reliable indicator* of the properties of fossils. Similarly we trust the intuitions of the physicist over those of the folk about many aspects of the physical world where the folk have proved notoriously unreliable. And recent experiments have shown that we should have a similar attitude to many psychological intuitions. Thus, the cognitive psychologist, Edward Wisniewski, points out that “researchers who study behavior and thought within an experimental framework develop *better* intuitions about these phenomena than those of the intuition researchers or lay people who do not study these phenomena within such a framework. The intuitions are better in the sense that they are more likely to be correct when subjected to experimental testing (1998: 45).” (2006a: 104-5)

Although we may often be right to trust an intuition in the short run, it is crucial to see that nothing rests on it in the long run. We can look for more direct evidence in scientific tests. In such a scientific test we examine the reality the intuition is *about*. These scientific examinations of reality, not intuitions about reality, are the primary source of evidence. The examinations may lead us to revise some of our initial intuitions. They will surely show us that the intuitions are far from a complete account of the relevant bit of reality.

What then should we make of referential intuitions? And whose intuitions should we most trust? All these intuitions arise from reflection on linguistic data. The ordinary competent speaker is surrounded by expressions that refer and so is in a good position to have well-based opinions about reference by reflecting on these expressions. This is not to say that she will reflect. Indeed, a totally uneducated person may reflect very little and hence have few if any intuitive judgments about her language. Still it is clear that the normal competent speaker with even a little education *does* reflect on linguistic reality just as she reflects on many other striking aspects of the world she lives in. And this education will usually provide her with the terms and concepts of folk semantics, at least. As a result she is likely to be able to judge in a fairly immediate and unreflective way what an expression refers to. Such intuitive opinions are empirical central-processor responses to linguistic phenomena. They have no special authority: although the speaker’s competence gives her ready access to data it does not give her privileged access to the truth about the data.

Still, are these referential intuitions likely to be right? I think we need to be cautious in accepting them: semantics is notoriously hard and the folk are a long way from being experts. Still it does seem to me that their intuitions about “simple” situations are likely to be right. This having been said, we should prefer the intuitions of semanticists, usually philosophers, because they are much more expert (which is not to say, *very* expert!). Just as the intuitions of paleontologists, physicists, and psychologists in their respective domains

are likely to be better than those of the folk, so too the intuitions of the semanticists.<sup>6</sup> This is the second of three steps in diminishing the significance of Machery *et al*'s findings and hence resisting claims (III) and (IV).

On the view of intuitions I am urging, there is nothing inappropriate about Machery *et al* checking the referential intuitions of philosophers against those of the folk. However, for reasons already indicated, the most important ones to check are the intuitions about humdrum cases of actual ignorance and error: (1) these intuitions are more important to Kripke's refutation; (2) the folk are likely to have more reliable intuitions about these cases than about hypothetical and rather fanciful cases like Gödel. It would not be hard to construct experiments to check these intuitions about humdrum cases and I think it would be quite fruitful to do so. But, in the first instance at least, this is not what I would most recommend, for reasons that will soon become apparent.

What about the modal intuitions that support the Unwanted Necessity argument and play the dominant role in the Lost Rigidity argument? We could conduct experiments to check these intuitions against those of the folk too, of course. But I predict that we would have problems arising from the fact that the most folk have given little thought to such metaphysical matters as the essence of individuals. And it is for that very reason that the experiment would not be a good idea. The intuitions we need are ones from people with some expertise in these matters, presumably metaphysicians and other philosophers.

To say that intuitions, whether those of philosophers or of the folk, are evidence is not to say that they are the only, or even the primary, evidence. Indeed, we can look for more direct, less theory-laden, evidence by studying what the intuitions are *about*, the reality itself; in particular, we can study the actual referential relation between names and their bearers.<sup>7</sup> But how do we do that? I shall address this in the next section. The experiments I shall propose test Kripke's refutation without checking it against the referential intuitions of the folk. These experiments are to be preferred because those folk intuitions are not ones in which we should put a great deal of trust.

But first, we should compare my position on intuitions with that of Machery *et al*. In contemplating possible responses to their results, they say:

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<sup>6</sup> **Objection:** The analogy does not hold. Paleontologists and other scientists are trained to have good intuitions by being corrected by their teachers but there is no similar training of semanticists. **Response:** Philosophy produces theories just as do the sciences. One trains in philosophy by doing philosophy courses just as one trains in the sciences by doing science courses. And the training in both cases involves the application of technical terms to situations. The training leads to an improved reliability of intuitions expressed using those terms.

<sup>7</sup> Stich is independently gloomy about this project (1996: 37-51). The theory of reference is certainly hard but I argue that Stich's gloom is excessive (2009a). Stich has responded (2009).



One possibility is that philosophers of language would claim to have no interest in unschooled, folk semantic intuitions, including the differing intuitions of various cultural groups. These philosophers might maintain that, since they aim to find the *correct* theory of reference for proper names, only *reflective* intuitions, i.e., intuitions that are informed by a cautious examination of the philosophical significance of the probes, are to be taken into consideration.

We find it *wildly* implausible that the semantic intuitions of the narrow cross-section of humanity who are Western academic philosophers are a more reliable indicator of the correct theory of reference...than the differing semantic intuitions of other cultural or linguistic groups....In the absence of a principled argument about why philosophers' intuitions are superior, this project smacks of narcissism in the extreme. (pp. B8-B9)

I certainly think that we should have *some* interest in folk referential intuitions. However, I claim to have given “a principled argument,” summarized above, that the philosophers’ intuitions are indeed superior.

How should we respond if the folk’s referential intuitions differ from the philosophers’? To say that the philosophers’ intuitions are superior is not to say that they must be correct nor that the folk’s intuitions can be ignored. We simply have to seek more evidence whether from intuitions or, more directly, from the reality that is the concern of these intuitions; see below. This is not very helpful, of course, but we have sadly little in the way of guides to the epistemic life *in general*.

But then what about the results of Machery *et al*’s experiments? In the Gödel experiments, the referential intuitions of the Hong Kong undergraduates differed from those of the Rutgers undergraduates. Nothing I have said gives any basis for preferring the intuitions of one folk group over another. So what should we say about those results? One possibility is that the conventions for what appear to be names differ in the two groups: the descriptivist theory is true of “names” in the Hong Kong dialect,<sup>8</sup> the causal-historical theory true of “names” in the Rutgers dialect. So, the intuitions of both groups are correct. This is certainly *possible* – even the most ardent causal theorist should allow that there *could be* “names” covered by a descriptivist theory<sup>9</sup> – but it seems to me *extremely* unlikely. So

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<sup>8</sup> Genoveva Marti surmises that the *point* of the experiment is to see whether this is in fact so: “to determine whether the East Asian participants *use* names descriptively” (2009: ###). As a result, she concludes that the experiment “does not prove what [Machery *et al*] purport it to prove” (###). This is a misunderstanding of what Machery *et al* intend. Their explicit point is to establish claim (IV): to raise questions about the enterprise of philosophers developing theories of reference by consulting their referential intuitions. Marti does not show that the experiment fails to prove that point.

<sup>9</sup> And causal theorists have *already* allowed that some names are covered by a description theory of reference *fixing*. These are “descriptive” or “attributive” names like ‘Jack the Ripper’ that have their reference fixed by attributive descriptions (rather than causally in ostension via a demonstrative, by another nondescriptive name, or by a referential description). Still these names, like any other, are thought to be covered by a causal

the intuitions of at least one of these folk groups are almost certainly wrong and we must seek more evidence to see which. Perhaps the different intuitions do indeed arise from cognitive differences of the sort described by Nisbett *et al* (2001). If so, we must look for evidence of which style of thought is leading to the truth here.

#### 4. Testing Referential Intuitions

In the last section I claimed that we can find more direct evidence for Kripke's intuitions than consulting folk referential intuitions: we can study what all these intuitions are *about*. How? I shall set aside Kripke's modal intuitions – too hard to test! – and focus on his referential ones. I shall propose experiments that test the referential intuitions without checking them against somewhat dubious folk intuitions.

The referential intuitions that we most want to test are those about the humdrum cases. The intuitions were that speakers succeed in designating objects by name in these cases despite being ignorant or wrong about the objects. The experiments I have in mind arise from applying the methodology argued for in *Coming to Our Senses* (1996: ch. 2; also 1994).

Here is a brief summary of that methodology. It is natural and appropriate to think that the basic task in semantics is to explain the *meanings* of thoughts and utterances. But this talk of meanings is sadly vague, as many have noted: it is far from clear what counts as a meaning that needs explaining.<sup>10</sup> We need to be much more precise in identifying the subject matter of semantics. In my view, we should identify the meaning of a thought or utterance with a certain property of the thought or utterance that is crucial to its causal role.<sup>11</sup> Of particular interest here are the roles of thoughts and utterances in causing intentional behavior and informing us about the world. So let us take meanings to be the properties of thoughts and utterances in virtue of which they play those causal roles. How then are we to investigate the nature of these meanings? We look to ordinary opaque attitude ascriptions. Day in and day out, folk, and social scientists, use “content clauses,” usually ‘that’ clauses, to ascribe properties to thoughts and utterances for the very

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theory of reference *borrowing* and so are not covered by classical or modern description theories (Devitt and Sterelny 1999: 109). It should be noted, however, that the names like ‘Gödel’ or ‘Shakespeare’ that are the names of famous authors are a bit problematic for the causal theory. They seem to have a double life, functioning sometimes as a normal “referential” name covered by the causal theory and sometimes as an attributive name. They seem to have the latter role when our only interest is in the author's works. Thus, the truth value of a claim about the literary merits of “Shakespeare's sonnets” might be unaffected by the discovery that they were written by, say, Bacon (Devitt 1981: 157-80).

<sup>10</sup> “The chief problem about semantics comes at the beginning. What is the theory of meaning a theory of?” (Higginbotham 1991: 271). “Meaning is notoriously vague” (Block 1986: 615). Lycan has brought out the problem wittily with his “Double Indexical Theory of Meaning”: “MEANING =<sub>def</sub> Whatever aspect of linguistic activity happens to interest *me now*” (Lycan 1984: 272).

<sup>11</sup> Paul Horwich has a similar idea (1998: 6).

purpose of explaining behavior and learning about the world. In thus applying their “folk psychology,” they are, in effect, trying to identify the meanings that play the causal roles. Now if the folk and scientists are right in their ascriptions, then what they are ascribing are meanings. And we have good reason to believe that the folk and social scientists are mostly right because their ascriptions are mostly *successful*: they really do explain behavior and guide us to the world. The center of the theorist’s task of explaining meanings then becomes the discovery of what is common and peculiar to the thoughts and utterances that are ascribed the *same* meaning by a content clause. And the main conclusion I came to in the book is that what is common and peculiar to the meaning we ascribe to a word is a property of *referring to something in a certain way*; a *mode of reference*.

We can now apply this methodology to the problem in hand. Take one of the names, ‘*a*’, in the humdrum cases of ignorance and error. Then gather some subjects who are sufficiently expert about the bearer of the name for there to be no doubt that they are competent with it: if anyone participates in the convention that relates that name to its bearer, these subjects do. For example, if ‘*a*’ were ‘Cicero’, the subjects would be knowledgeable about Cicero’s life and times. If necessary the subjects can be supplied with some of this information. Then tell the subjects some stories, each involving a different character. The stories reveal that these characters vary greatly in their knowledge of *a*, one being knowledgeable, one being largely ignorant and wrong, and others being in between. Yet all of the characters use ‘*a*’ several times to make statements ask questions, and so on. Thus, if the name were ‘Cicero’, part of each story might include its character stating, “Cicero was an orator.” And part of the story for the largely ignorant character might include her asking, “Didn’t Cicero denounce someone?” and concluding, “He was an ancient Greek, I think.” Then, the experiment seeks to discover what thoughts and utterances the subjects attribute to these characters using ‘*a*’ in content clauses. Do subjects assert that a character believes that...*a*..., said that...*a*..., wonders whether ...*a*..., and so on? The experiment might simply ask the subjects their opinions on these matters. However it would be better to try to elicit the opinions by asking the subjects to say how they explain the behaviour of the characters and what, if anything, they have learnt from the characters.<sup>12</sup> These attributions by the subjects are significant because we theorists can then reason as follows. If the subjects are right in the contents they ascribe, then those contents will all be *about a*: the attributions won’t be right unless the thoughts and utterances of the characters co-refer with the subjects’ ‘*a*’ and the subjects’ ‘*a*’ refers to *a*. And we have good reason to suppose that the subjects will mostly be right in their attributions: first, we can expect the subjects to be, like the rest of us, *generally* successful in ascribing contents to explain behavior and gain information about the world; second, given their indubitable competence with ‘*a*’, there is no reason to suppose that this general success will not be exemplified in this particular context. And note that, although these attributions reflect folk psychology, they do not deploy any semantic concepts like REFERENCE and so do not draw on a folk semantics in which we should not put a great deal of trust.

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<sup>12</sup> The model here is “the technique of elicited production” that some psycholinguists have used so effectively on children; see Crain *et al* 2005, section 4, for a nice summary.

So we have very good reason to believe that any thought or utterance of a character that the subjects describe using ‘that...*a*...’ refers to *a*. So we can look to what is common and peculiar to these thoughts and utterances to discover what is required for referring to *a*. The Kripkean intuitions predict that a character’s associating true descriptions of *a* is not required: ignorance and error should make no difference to the readiness of the subjects to ascribe thoughts and utterances about *a* to characters. Thus the prediction is that subjects will be as ready to ascribe, without qualification, the true belief that Cicero was an orator to the ignorant character as to the knowledgeable. The subjects will say of the ignorant character that she wonders whether Cicero denounced someone and that she said, wrongly, that Cicero was an ancient Greek. Ignorance and error about the bearer of a name will make no difference to the capacity of a character to use the name to refer to the bearer and to have thoughts about the bearer that she would express with the name. The confirmation of these predictions would confirm Kripke’s intuitions and thus count heavily against the description theory of names.

We can even test the intuitions about the Gödel case, although not as neatly and directly. The subjects in this experiment are sufficiently aware of the facts about Gödel for there to be no doubt that they refer to the famous logician by his name. The subjects are presented with a story about the son of a logician called “Schmidt” who died in mysterious circumstances. The son is obsessed with the idea that his father proved the incompleteness of arithmetic and has been badly wronged. He persuades several others of this and starts a movement to right this perceived wrong. Members of this movement make many statements like: “Schmidt proved the incompleteness of arithmetic”; “Gödel did not prove the incompleteness”; “Gödel is a plagiarist and a fraud.” The members behave accordingly, sending statements to logic journals, demonstrating at logic conferences, and so on. The experiment then attempts to elicit the opinions of the subjects about the thoughts and utterances of the members of this movement. The crucial ascriptions are ones prompted by members’ uses of ‘Gödel’ or by their thoughts that would be expressed by uses of ‘Gödel’; for example, those prompted by the utterance, “Gödel is a plagiarist and a fraud.” For, if the subjects rightly describe such utterances and thoughts using content clauses including the name ‘Gödel’ – for example, “The members believe that Gödel is a plagiarist and a fraud” - then those thoughts and utterances must be *about Gödel*, the famous logician (because the subjects’ use of ‘Gödel’ are about him). Yet if descriptivism were right, these thoughts and utterances could not be about Gödel because the members do not associate ‘the person who proved the incompleteness of arithmetic’ with the name ‘Gödel’.<sup>13</sup> And, as before, we have good reason to suppose that the subjects are right in their ascriptions.<sup>14</sup>

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<sup>13</sup> Of course, if descriptivism were right, the members might well be referring to Gödel by ‘Schmidt’ (because the description they associate with ‘Schmidt’ is actually true of Gödel), but the experiment is not testing for that.

<sup>14</sup> This experiment is complementary to a nice one proposed by Marti (2009) in her critique of Machery *et al.* She proposes this experiment because she rightly insists that theories of reference for proper names should be tested against how the names are actually used. This is much the same idea as mine that we should study directly the actual

These experiments would show that the claims of the Ignorance and Error argument are open to straightforward empirical test without appeal to intuitions about reference. This is the third and last step in diminishing the significance of Machery *et al*'s findings and hence resisting their claims (III) and (IV). The experiments do not raise serious doubts about Kripke's refutation in particular nor about the philosophical enterprise of developing a theory of reference in general.

## 5. Conclusion

I accept that Machery *et al*'s ingenious Gödel experiments provide evidence against Kripke's refutation but I have presented three considerations that diminish the significance of this evidence and hence count against claim (III). First, I argued that the referential intuitions about somewhat fanciful Gödel cases of possible error that were tested are much less important than referential intuitions about humdrum cases of actual error. Furthermore, various intuitions about modal properties that remain unchallenged also carry considerable weight. And, Machery *et al*'s Jonah experiments confirmed Kripke's refutation and yet its evidence is arguably more trustworthy than the Gödel evidence because the case is less fanciful (sec. 2). This argument shows also that Machery *et al*'s claims (I) and (II) are mistaken. The intuitions used to assess theories of reference are not all about reference in hypothetical situations. And referential intuitions about Gödel and Jonah cases are not central to Kripke's refutation. Second, I think we have a principled basis for preferring the intuitions of philosophers to those of the folk on these semantic matters because the philosophers are more expert (sec. 3). Finally, Kripke's refutation can be supported in an empirical test without appeal to the referential intuitions of folk or philosophers. This test rests on the assumption that if experts about a person ascribe thoughts and utterances about that person to others, this is very good evidence that those thoughts and utterances are really about that person (sec. 4).

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reference relation between names and their bearers. However, whereas I think that the intuitions of the sort tested by Machery *et al* are evidence for theories of reference, albeit not very significant evidence, it seems that Marti thinks that they are not evidence at all. She thinks that these intuitions are about people's "*theories of reference*" and hence of no interest (pp. ###). Yet, the intuitions are elicited by the 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?" (p. B6) *Prima facie*, these intuitions are about *what a name refers to*. And, as noted, such intuitions are of interest because they are likely to be right. In producing these intuitions a person may indeed be prompted to theorize about reference, but she may not be. Certainly, the question that prompts the intuitions does not demand such theorizing; rather, it demands the identification of instances of the reference relation. The way to test intuitions about theories of reference would surely be to present subjects with the theories themselves. That was not what Machery *et al* did. And I would agree with Marti that such intuitions would not be of much interest.

Let us sum up the actual and potential evidence for and against Kripke's refutation of descriptivism. The actual evidence *for* includes the philosophical consensus in intuitions about the Unwanted Necessity argument, the Lost Rigidity Argument, and the Ignorance and Error argument. Intuitions in the latter argument are about humdrum actual cases and some possible ones, including Jonah and Gödel cases. Actual evidence *for* also includes the results of Machery *et al*'s testing the intuitions of Rutgers' undergraduates on the Jonah and Gödel cases and Hong Kong undergraduates on the Jonah cases. Actual evidence *against* consists in the results of Machery *et al*'s testing Hong Kong students on the Gödel cases. Potential evidence *for* could come from testing the folk's intuitions about the humdrum cases in the Ignorance and Error argument. We could also test the folk's modal intuitions about cases in the Unwanted Necessity and Lost Rigidity Argument but I doubt that the folk have the necessary expertise to provide evidence. Finally, potential evidence *for* could come from experiments along the lines suggested in the last section, tests of the ascriptions that experts are prepared to make to the ignorant and wrong. I wager that if the potential tests were performed, the results would favor Kripke's refutation.

Finally, from the perspective on semantics summarized in this paper, the role of folk intuitions is simply evidential. And those intuitions do not provide the only, or even the most important, evidence for a semantic theory. Although the findings of Machery *et al* about Gödel cases are troubling they do not, from this perspective and contrary to claim (IV), raise questions "about the nature of the philosophical enterprise of developing a theory of reference."<sup>15</sup>

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