Semantic intuitions, conceptual analysis, and cross-cultural variation[1]

1. Introduction

While philosophers of language have traditionally relied upon their intuitions about cases when developing theories of reference, this methodology has recently faced a number of challenges. Some have argued that semantic externalism gives us particularly *philosophical* reasons to worry about the value of our intuitions about reference,[2] but a specifically *empirical* attack on the use of intuitions in the theory of reference has recently been developed by Edouard Machery, Ron Mallon, Shaun Nichols, Stephen Stich (hereafter "MMNS").[3] According to these authors, the standard philosophical methodology presupposes that philosophers' intuitions about their thought experiments are (more or less) universally shared, and that this universality is what underwrites the assumption that their intuitions about reference reflect the nature of reference itself (rather than simply philosophers' idiosyncratic conception of it). MMNS then purport to show that intuitions about reference, far from being universal, show significant cultural variation, thus undermining their relevance for semantic theory.

In what follows, I'll present MMNS's arguments, and then attempt to demonstrate that they do not, in fact, undermine the traditional philosophical methodology to the extent that they suggest. Further, I'll argue that a single account of reference can accommodate the culturally variable intuitions that MMNS point to without having to treat either culture's intuitions as mistaken.[4] That said, this defense of the role of intuitions in philosophy may not be as robust as some might hope, since it involves two important assumptions/concessions. First of all, the explanation for why intuitions remain relevant in philosophical inquiry will turn out to presuppose a degree of 'constructivism' about the topics that these intuitions are about. While, I'm independently sympathetic with such constructivist views, it seems clear that both critics and defenders of the use of intuitions often presuppose a more straightforwardly 'realistic' attitude

[2] Capppelan and Winblad, 1999. (For a response to this line of attack, see Jackman 2005.)
[3] Machery, Mallon, Nichols & Stich (2004).
[4] MMNS do not go into much detail about just what they take "intuitions" to be, and a similar lack of concern will be shown here. The question we are all focusing on is whether philosophers judgments about various thought experiments (the so-called "method of cases") should be allowed to play a central role in philosophical inquiry. Whether such judgments are properly seen as part of a psycholocially/epistemically natural kind that is properly

towards the semantic questions that these intuitions are supposed to be about. Secondly, the reliability of our semantic intuitions will turn out not to obviate the importance of cross-linguistic data when trying to develop an adequate semantic theory. Since it is this defense of the relevance of empirical studies, not their criticism of intuitions themselves, that I take to be at the heart of MMNS's paper, I don't think that this defense of intuitions is ultimately contrary to the spirit of writers engaged in "experimental philosophy".

2. Semantic Intuitions and Cultural Variation

MMNS focus on our intuitions about how proper names pick out their reference, and they present the 'descriptivist' and 'causal/historical' views as the two primary competing accounts in this area. They characterize the two views as follows:

The descriptivist view of reference

D1. Competent speakers associate a *description* with every proper name. This description specifies a set of properties.

D2. An object is the referent of a proper name if and only if it *uniquely or best satisfies* the description associated with it. An object uniquely satisfies a description when the description is true of it and only it. If no object entirely satisfies the description, many philosophers claim that the proper name refers to the unique individual that satisfies most of the description (Searle 1958, Lewis 1970). If the description is not satisfied at all or if many individuals satisfy it, the name does not refer.

The causal-historical view

C1. A name is introduced into a linguistic community for the purpose of referring to an individual. It continues to refer to that individual as long as its uses are linked to the individual *via a causal chain* of successive users: every user of the name acquired it from another user, who acquired it in turn from someone else, and so on, up to the first user who introduced the name to refer to a specific individual.

C2. Speakers may associate descriptions with names. After a name is introduced, the associated description *does not play any role* in the fixation of the referent. The referent may *entirely* fail to satisfy the description.[5]

As MMNS note, philosophers have moved away from descriptivist theories and towards causal/historical ones because the latter seemed to fit better with our intuitions about scenarios such as Kripke's well-known 'Gödel' case, in which we are asked to imagine that the description typically associated with the name (say, "discoverer of the incompleteness of arithmetic") is entirely false of the man whose friends and colleagues call "Gödel" (person *a*), and true of a

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called "intuition" is a less central concern. (Indeed, I suspect that "intuition" does not pick out such a unified or relevant kind.)

different individual (person *b*), whose friends and family all refer to as "Schmidt". (As it turns out, *a* stole *b*'s manuscript and took the credit for it.) Descriptivism seems to entail that our use of "Gödel" would refer to *b* in such a case, while the causal-historical view would suggest that the name would refer to *a*. Kripke's intuition, which is shared by most philosophers, is that we would be referring to *a* in such a case, so our intuitions seem to support the causal-historical over the descriptive view.[6]

Not only do most philosophers share Kripke's intuition about the Gödel case, but they also presuppose that such intuitions are (more-or-less) universal. Consequently, they are seen as a "data point" that any theory of reference must accommodate. Indeed, as MMNS note, most critics of the causal theory still accept the standard judgments about the Gödel case; they just insist that they can be explained within a descriptivist framework.[7] It would thus seem to be a serious problem if, rather than being universal, these intuitions varied systematically from one group to another. Such a result "would raise questions about whose intuitions are going to count, putting in jeopardy philosophers' methodology."[8] Unfortunately for the armchair philosopher, the possibility that our semantic intuitions show such cultural variation is a very live one.

Just as empirical investigation has shown initially surprising variation in our ethical and aesthetic intuitions, recent work in cultural psychology gives us reason to have doubts about the universality of our semantic intuitions as well. In particular, there is evidence of large and systematic differences between East Asians and Westerners on (1) basic cognitive processes such as perception, attention and memory, (2) how they describe, predict and explain events, (3) the way they categorize objects, and (4) the way they revise beliefs in the face of new arguments and evidence.[9] As MMNS put it:

> According to Nisbett and his colleagues, the differences between [East Asians] and [Westerners] "can be loosely grouped together under the heading of holistic vs. analytic thought." Holistic thought, which predominates among East Asians, is characterized as "involving an orientation to the context or field as a whole, including attention to relationships between a focal object and the field, and a preference for explaining and predicting events on the basis of such relationships." Analytic thought, the prevailing pattern among Westerners, is characterized as "involving detachment of the object from its context, a tendency to focus on attributes of the object in order to assign it to categories, and a preference for using rules about the categories to

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[5] MMNS pp. B2-B3.

[6] See Kripke 72/80, pp. 83-84.

[7] MMNS, p. B3. They give as examples of this, Evans 1973, 1982, Jackson 1998. For another instance of this, see Searle 1983.

[8] MMNS, p.B4.

[9] See Nisbett et al 2001, Nisbett, 2003, Norenzayan et al. 2002; Watanabe 1998 & 1999.

explain and predict the object's behavior" (Nisbett et al. 2001, 293).... One range of findings is particularly significant … The cross-cultural work indicates that [East Asians] are more inclined than [Westerners] to make categorical judgments on the basis of similarity; [Westerners], on the other hand, are more disposed to focus on causation in describing the world and classifying things. (MMNS p. B5)

These facts about the difference in 'cognitive style' between East Asians and Westerners, particularly the Western preference for causation-based judgments, lead MMNS to predict that "when presented with Kripke-style thought experiments, *Westerners would be more likely to respond in accordance with causal-historical accounts of reference, while East Asians would be more likely to respond in accordance with descriptivist accounts of reference*."[10]

To test their hypothesis MMNS set up an experiment with 40 undergraduates at Rutgers University and 42 undergraduates from the University of Hong Kong.[11] The participants were presented with 4 probes, two of which were modeled on Kripke's Gödel case (i.e.: the descriptions associated with a name pick out the 'wrong' person), and two were modeled on Kripke's Jonah case (i.e.: the descriptions associated with a name -- in this case the biblical figure who was swallowed by a whale -- pick out no one at all). One of each type of probes used names and situations that were familiar to the Chinese participants, while the other used names and situations more familiar to the American participants.[12]

The result of the two 'Gödel' probes partially supported MMNS's hypothesis. Answers consonant with causal-historical accounts of reference were given a score of 1, and answers consonant with the descriptive account of reference were given a score of 0. The scores were

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[10] MMNS p. B5 (italics theirs, but contractions for "Westerner" and "East Asian" removed).

[11] They note: "The University of Hong Kong is an English speaking university in Hong Kong, and the participants were all fluent speakers of English. A standard demographics instrument was used to determine whether participants were Western or Chinese." (MMNS, p. B6.)

[12] One of the Gödel probes was closely modeled on Kripke's own example, and ran as follows:

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

then summed, so the cumulative score could range from 0 to 2. Means and standard deviation (in parentheses) for summary scores were as follows.

Gödel cases
Western participants: 1.13 (.88)
Chinese participants: .63 (.84)

While MMNS recognize that one can only draw conclusions tentatively from such a limited range of data, they take this experiment to give at least *prima facie* evidence for some significant (meta) philosophical conclusions. The two sets of responses to the Gödel case seem to show not only that our intuitions about reference are culturally variable, but also that in the 'Western' sample, the 'causal' intuition was *far* from universal. Indeed, the high standard deviation for both groups suggests that semantic intuitions vary considerably *within* cultural groups, so even without evidence of inter-cultural variation, the surprising amount of intra-cultural variation is itself enough to put the suggestion that philosophers' semantic intuitions are 'universal' in jeopardy. The existence of such variation seems to force upon us the question of "whose intuitions are going to count?"[13] How then, should the philosopher who relies on semantic intuitions still take them to be probative in the face of such data?

3. Three types of Semantic Theory

Before answering this question, we need to first answer the larger question of what *sort* of theory we understand a semantic theory to be. MMNS canvas the possibility that philosophers rely on their own intuitions because they see themselves as engaged in a project modeled on Chomskyan linguistics, with their intuitions about reference being used to develop "an empirically adequate account of the implicit theory that underlies ordinary uses of names."[14]

This suggestion has various degrees of plausibility depending upon what we take a "semantic theory" to be.   There are at least three sorts of project that fall under the heading of "semantic theory", and intuitions may seem to play a different role in each. These are:

1. A theory of "Logical Form" (hereafter "LF").

2. A theory stating *what* our terms refer to.

3. A theory explaining *why* our terms refer to what they refer to.

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[13] MMNS, p. B4.
[14] For an example of such an approach, MMNS cite Segal 2001.

I'll refer to these as "type-1", "type-2" and "type-3" semantic theories with the intuitions directly relevant to them as "type-1", "type-2" and "type-3" semantic intuitions.

To the extent that one is engaged in a semantic theory of the first sort, the analogy with Chomskyan linguistics has a good deal of plausibility. Our semantic intuitions in these cases can plausibly be understood as reflecting underlying realities about the formal properties of the sentences we grasp. However, it is not over such formal features that the causal and the descriptive theorist are disputing. In particular, the question of who, say, "Gödel" refers to in the case described above is not such a formal question. The issue might have come up if one defended a type of generative semantics in which names were actually disguised definite descriptions, or possibly demonstratives, at some deeper processing level,[15] but barring that, the issue of proper name reference isn't dealt with at LF. Further, if there were cultural differences in intuitions about logical form, the claim that two varieties of English simply embodied different underlying logical forms would not be particularly hard to defend, so social non-uniformity does not impugn the validity of the intuitions in question. Finally, LF is the only semantic level where it does seem plausible to think that every speaker knows the semantics for his or her own language in a full blooded, if implicit, way comparable to the way in which he or she can be said to know his or her own syntax.

The second sort of semantic project, in which the terms of a language are assigned their extensions, seems to be the one most relevant to the sorts of intuitions MMNS consider. The probes focus on, after all, intuitions about what particular terms refer to in particular contexts. However, such "referential" questions should not be understood as reflecting implicit knowledge of precisely the sort that our syntactic intuitions do. In particular, one of the main consequences of semantic externalism is that speakers needn't know in the relevantly full-blooded sense what their terms refer to. An "empirically adequate account of the implicit theory that underlies ordinary uses of names" in *this* sense would explain what we *take* our terms to refer to, but since this sort of semantic theory is supposed to describe what our terms *actually* refer to, the syntactic model seems to miss out on the 'normative' character of such semantic theories.[16]

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[15]   And it does seem that it is precisely such a 'formal' question that is the focus of Segal 2001.

[16]   One might, of course, question the type of externalism upon which this assumption is based, but I think that the general point that what we actually apply our words to and what they actually refer to may not be the same does not rest on any externalist assumptions. Rather, externalism rests on this more basic assumption.

Finally there is the third 'foundational' or 'meta' semantic project which hopes to explain *why* our terms have the referents/extensions that they are taken to have in the second sort of theory. The rival causal and descriptive theories that MMNS consider are theories of this sort. However, the sorts of intuitions that are *directly* relevant to this third sort of theory are not considered. Intuitions about whether or not, say, the descriptive theory is a plausible one are not the topic here. Such intuitions are more like the intuition that a particular syntactic rule is plausible rather than the intuitions about the grammaticality of particular sentences, and they seem to carry less weight than the intuitions relating directly to the second sort of semantic theory. That said, these intuitions do play a role in philosophical theorizing, and, say, the decision to try to explain the judgments of the second sort within broadly 'naturalistic' frameworks (which both the descriptive and causal theories could claim to be) could be seen as driven by the assumption that non-naturalistic (or 'magical') theories of reference are 'unintuitive'.[17] Philosophers certainly have such intuitions, and one reason some will stick to roughly 'internalist' accounts of meaning is that they simply find it implausible that meaning would be anywhere other than 'in the head'.[18] Still, the level 3 intuitions, while they motivate many things, are less likely to be taken as universal.

It is not surprising that MMNS focus on intuitions relating directly to the second type of theory while discussing philosophers working on theories of the third sort. This approach is typical, and entirely appropriate. While type-2 intuitions bear less directly on type-3 theories than type-3 intuitions, type-2 intuitions are generally considered stronger (and possibly less theory driven), and thus more probative than type-3 intuitions. Just as syntacticians evaluate a theory of, say, binding by their intuitions about what co-reference assignments are possible for a given sentence, and not, say whether it seem intuitive to think that C-command would be a constraint on anaphoric relations, philosophers typically test a type-3 theory by evaluating its compatibility with type-2 intuitions. In both meta-semantics and syntax, the most relevant intuitions are about the cases themselves, not about how best to systematize them. This is why the types of intuitions involved when philosophers construct semantic theory of the third sort are

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[17] See, for instance, Putnam 1981. For Lewis's reply to Putnam's related criticism that aspects of his theory of intentionality were "spooky" and "medieval-sounding", see Lewis 1984, p. 67.

not intuitions to the effect that something like the descriptive theory of reference is correct. Rather than being the direct result of type-3 intuitions, type-3 theories like the causal and descriptive theory are attempts to systematize and account for our type-2 intuitions.

Nevertheless, while analogies between syntactic and semantic theories are often enlightening, they can also lead one astray, and the assumption that intuitions are justified the same way in both cases is a mistaken one. In particular, semantic intuitions of the sort we are here concerned with need not get their justification from *reflecting* the "implicit theory that underlies ordinary uses of names". Syntactic intuitions are justified on the assumption that they reflect the underlying mechanisms that produce them, and syntactic theories are needed to explain these intuitions as a theory fits its data.[19] There is certainly a wide-spread view that semantic intuitions, and with it conceptual analysis, should be understood in the same way. Such a view is guided by the 'Platonic' assumption that "we all know the 'right' necessary and sufficient conditions [for the application of our concepts]… but this knowledge is buried very deep down in our minds and is therefore hard to make explicit."[20] So understood, we all have tacit knowledge of our own concepts and conceptual analysis is just a matter of making this tacit knowledge explicit. Intuitions about possible cases are guided by such tacit knowledge, so they are an ideal way of making the structure of these concepts explicit. Just as grammaticality judgments are taken to reliably reflect implicitly known linguistic rules, philosophers' intuitions are taken to reliably reflect their implicitly known concepts.[21] It is precisely this general framework for understanding conceptual analysis that allows cultural variation among our type-2 semantic intuitions to present the problems that MMNS to suggest they do.

Nevertheless, rather than presenting a problem for intuitions themselves, I think that the variation in our intuitive judgments points to a limitation in the conception of conceptual analysis outlined immediately above. The methodology of syntax is essentially 'detective'. That is to

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[18] See, for instance, Searle 1983.

[19] The performance/competence distinction provides some wiggle room, but not of the sort that I will argue can be found with semantic theories of the second and third type.

[20] Cappelan & Winblad 1999b, p. 205. They are discussing, but not endorsing the assumption in question. Cappelan & Winblad's views are discussed in more detail in Jackman 2005.

[21] This analogy has been recently defended Miscevic 2000, and for a criticism of this defense, see DePaul 2000. See also Ramsey 1998 (p. 165) for an unusually clear exposition of the psychological picture purportedly presupposed by classical conceptual analysis.

say, syntactic intuitions are taken to be produced by underlying rules and thus can be used as evidence for them. Independently of the sorts of variation that MMNS point to, it should be clear that the psychological data on the mechanisms underlying classification shows the 'detective' model of conceptual analysis to be in need of serious revision. Philosophers have traditionally looked for conceptual analysis to yield necessary and sufficient conditions for the concepts in question,[22] but it seems increasingly evident that whatever is tacitly producing our classifications does not have this sort of 'definitional' structure, and that the mechanism in question are often more exemplar and prototype driven.[23] If concepts themselves are taken to have sharp boundaries in spite of this, then conceptual analysis is better understood as partially a *constructive* and *normative* process.[24] Rather than thinking that there are coherent concepts lying *behind* our usage, philosophers should assume that a coherent concept can be constructed *out of* our usage, and when conceptual analysis is understood this way, the reliability of intuitions becomes easier to defend.[25]

On the constructive model of conceptual analysis, semantic intuitions of the second (and to a certain extent the third) type are better understood as contributing to rather than reflecting the semantic facts that they are taken to be about. In this way, the relation between semantic intuitions and semantic facts may be more like the way Rawls conceives of the relation between ethical intuitions and ethical truth than it is with anything familiar from syntax.[26] There may be underlying mechanisms that produce our intuitions, but just as ethical theory is supposed to make our ethical intuitions coherent, not just describe them, type-3 semantic theory is supposed to systematize our type-2 semantic intuitions, not just describe the mechanisms that produce them.[27]

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[22] And I think that there are good reasons for thinking that we are rationally committed to viewing our concepts in this way (see Jackman 2004, 2006a).

[23] For a discussion of this, see Lakoff 1987, Rorsch 1975.

[24] For a possible example of this approach to analysis, see the discussion of "knowledge" in Williams 2001. For a discussion of how the empirical data on the role of prototypes in classification makes conceptual analysis as traditionally understood difficult, see Ramsey 1998.

[25] One might think that this is simply to replace conceptual analysis with something like Carnap's notion of 'explication' (Carnap 1950) Indeed, DePaul (1998) seems to suggest that a move to something like explication is the most plausible way to continue with something like conceptual analysis when our concepts are prototype driven. However, while Carnap takes the explicative process to be one of replacing older 'confused' concepts with new and improved ones, I'm more inclined to view the constructive process as often (though certainly not always) one of getting clear on what our concepts have committed us to all along. (See, Jackman 1999, 2004, 2006a.)

[26] Rawls 1971. For an instance of this approach to semantics, see, for instance, Devitt 1994, 1996.

[27] Also, as in ethics, but unlike syntax, type-3 semantic intuitions can carry some weight.

A semantic theory bears a 'normative' relation to intuitions of a sort that syntactic theory does not. Of course, syntactic intuitions can be mistaken, but such mistakes are explained in terms of performance errors that don't change the fact that the 'correct' intuitions can be understood as directly reflecting the underlying syntactic reality.[28] Semantic intuitions, on the other hand, even when correct, are correct in virtue of their relations to other intuitions, not to some underlying concept, and because of this, it is easier to think of semantic intuitions (like ethical or epistemic intuitions) as mistaken. The process of systematizing them may be best understood as revisionary.

This constructive model of conceptual analysis will point, I hope to show, to at least one reason why philosophers should be able to remain confident about their intuitions even in the face of the sorts of cross-cultural data that MMNS point to.

4. Reflective Intuitions

One possible response to the non-universality of our semantic intuitions that MMNS consider is that philosophers need not assume the universality of semantic intuitions because they are not interested "unschooled, folk semantic intuitions, including the differing intuitions of various cultural groups",[29] and it is only the *reflective* intuitions of philosophers that are relevant in finding the *correct*, rather than merely folk, theory of proper name reference.[30] MMNS are, to say the least, unsympathetic with this sort of reply. As they put it:

> 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. (MMNS p. B9)

While I agree with MMNS that we shouldn't simply dismiss ordinary intuitions, it needn't follow that there can't be good reason to favor reflective over pre-reflective intuitions. Indeed, MMNS never deny that this might turn out to be the case, they just ask that any philosopher who proceeds as if this were so provide some "principled argument about why philosophers' intuitions

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[28] Further, if a syntactic intuition represents a "performance error" there should be an account of what interfered with the underlying competence, while 'incorrect' semantic intuitions may be no more 'polluted' than the correct ones, They just may turn out to be outweighed by other intuitions or fit badly with the actual environment.

[29] MMNS p. B8.

[30] For something like this defense of philosophical intuitions, see Hales 2006, chapter 4 (esp. pp. 171-72).

are superior", and that is precisely what I'll be trying to do here.[31] In particular, it will be argued that philosophers' reflective intuitions are often superior because they are largely shaped by the need to make our intuitions about various cases *consistent*.[32]

After all, one thing that comes up in MMNS's data is that philosophers' intuitions are noticeably different from not only most Chinese informants, but also with a good deal of the Western informants as well. How should one explain this difference? One might try to do so in terms of 'disciplinary' pressures which, say, prevent students who don't have 'causal' intuitions from being admitted to graduate programs, and thus into the profession.[33] However, while this might explain the intuitions of philosophers who are, say, 45 and younger, it doesn't explain the initial reception of Kripke's and Putnam's work in the mid-seventies. If philosophers' intuitions were the result of professional indoctrination, one would have expected most philosophers in the early 70's to have descriptivist intuitions, but that does not seem to be how things played out at all. While Kripke's and Putnam's work was not universally accepted, most critics focused on just what could be taken to follow from the 'causal' intuitions; they did not, as mentioned earlier, deny the intuitions themselves. The 'indoctrination' explanation thus seems, at least for the case of semantic intuitions, suspect.

A more charitable explanation of why (at least some) philosophers have intuitions about the Gödel cases (where the descriptions seem to 'misidentify' the referent) that are in line with the causal theory is that this seems to be what is needed to make them consistent with their intuitions about the Jonah cases (where the descriptions associated with a name pick out nothing at all).[34] As MMNS note, the intuitions of *both* Western and East-Asian groups about the Jonah case are roughly the same, and in both cases they support the causal account over the descriptive (and to a greater extent than either group supported the causal account in the Gödel case).   These results were:

Jonah cases
Western participants: 1.23 (.96)
Chinese participants: 1.32 (.76)

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[31]   Though given the view ultimately defended, it might be somewhat misleading to say that I take philosophers' intuitions to be a "reliable indicator of the correct theory of reference", since such "indicator" talk suggests that intuitions are tracking something which is independent of them.

[32]   As the earlier comparison with Rawls should make clear.

[33]   MMNS suggest as much themselves (p. B9). For a similar suggestion about our epistemic intuitions, see Cummins 1998.

[34]   Or, more commonly in the case of names, no *unique* person at all.

This may not be surprising. It seems to be a deeply entrenched assumption with our use of names that we take them to pick out unique individuals, and if the descriptive theory suggests that our names don't uniquely refer, or (as in the Jonah case) don't refer at all, that is a serious bullet for most speakers to bite.[35] Indeed, the move from the 'simple' description theory (where the name picks out whatever satisfies *all* of the descriptions associated with it) to the 'cluster' theory (where the name picks out whatever satisfies *most* of the associated descriptions) is motivated by the realization that the simple theory would make for extremely widespread reference failure.

Further, it should also be noted that when MMNS present the Jonah case to their informants, what they treat as the 'descriptivist' doesn't posit reference failure as much as it does the speaker referring to a fictional character.[36] When MMNS outline the descriptive theory it involves a clear commitment to the effect that "If the description is not satisfied at all or if many individuals satisfy it, the name does not refer", but when they present the case, this commitment disappears, and the name is taken to refer to "a fictional person who does not really exist".[37] The switch to

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[35] As MMNS note on p. B7.

[36] The Jonah case is presented by MMNS as follows:

> In high school, German students learn that Attila founded Germany in the second century A.D. They are taught that Attila was the king of a nomadic tribe that migrated from the east to settle in what would become Germany. Germans also believe that Attila was a merciless warrior and leader who expelled the Romans from Germany, and that after his victory against the Romans, Attila organized a large and prosperous kingdom.
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> Now suppose that none of this is true. No merciless warrior expelled the Romans from Germany, and Germany was not founded by a single individual. Actually, the facts are the following. In the fourth century A.D., a nobleman of low rank, called "Raditra", ruled a small and peaceful area in what today is Poland, several hundred miles from Germany. Raditra was a wise and gentle man who managed to preserve the peace in the small land he was ruling. For this reason, he quickly became the main character of many stories and legends. These stories were passed on from one generation of peasants to the next. But often when the story was passed on the peasants would embellish it, adding imaginary details and dropping some true facts to make the story more exciting. From a peaceful nobleman of low rank, Raditra was gradually transformed into a warrior fighting for his land. When the legend reached Germany, it told of a merciless warrior who was victorious against the Romans. By the 8th century A.D., the story told of an Eastern king who expelled the Romans and founded Germany. By that time, not a single true fact remained in the story.
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> Meanwhile, as the story was told and retold, the name "Raditra" was slowly altered: it was successively replaced by "Aditra", then by "Arritrak" in the sixth century, by "Arrita" and "Arrila" in the seventh and finally by "Attila". The story about the glorious life of Attila was written down in the 8th century by a scrupulous Catholic monk, from whom all our beliefs are derived. Of course, Germans know nothing about these real events. They believe a story about a merciless Eastern king who expelled the Romans and founded Germany.
> When a contemporary German high school student says "Attila was the king who drove the Romans from Germany," is he actually talking about the wise and gentle nobleman, Raditra, who is the original source of the Attila legend, or is he talking about a fictional person, someone who does not really exist?
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> > (A) He is talking about Raditra.
>
> > (B) He is talking about a fictional person who does not really exist.

[37] Unless, of course, MMNS are following Rorty 1979 in making a strong distinction between "referring" and "talking about". I'd be surprised if that was the case, especially since, if the distinction was intended, it wouldn't

the Jonah cases already tilts the intuitions of both groups towards the causal theory, and I expect that they would tilt considerably more so if actual *reference failure* was postulated rather than lapsing into fiction. Indeed, quasi-mythic characters like Jonah are not the best examples of reference failure precisely because the thought that talk of them involves lapsing into fiction has some appeal. This appeal is consequently less with, say, the speaker who only believes of "Einstein" that he was "the man who invented the atomic bomb".[38] Since there was no single man who invented the atomic bomb, the name would on the descriptive view, fail to refer, and the alternative explanation that the speaker is talking about a fictional character has far less appeal in the "Einstein" case than it does with names like "Moses", "Homer" or "Jonah".

Speaking from my own (undergraduate) past, my intuitions about cases of 'misidentification' were initially in line with the descriptive theory, and it was only on reflection about what to say about cases where the descriptions picked out no unique person at all that they shifted more towards the causal/historical account. This, in turn, made descriptivism about the kind-terms focused on by Putnam seem less appealing, which eventually lead to accepting the sorts of cases focused on by Burge.[39] A theory of reference is constructed to systematize our intuitions, and our intuitions, in turn, may be modified when we understand what they seem to commit us to.[40] The theory thus needn't be understood as already implicitly there producing our intuitions. Consequently, the preference for philosophers' intuitions is not because philosophers are especially good at detecting what is already there,[41] but rather because they are the only ones actively concerned with the sort of consistency-driven construction that conceptual analysis involves. Because of this, it isn't obvious that the philosopher's preference for reflective intuitions is *simply* an instance of narcissism.

That said, the possibility still remains that, just as we bring our intuitions about the Gödel cases into line with our causal-theory-friendly intuitions about the Jonah cases, reflective Chinese speakers might bring their Jonah intuitions into line with their descriptive-theory-

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be clear why cultural variation in our intuitions about "talking about" should be relevant to the question of whether philosopher's intuitions about *reference* were representative.

[38] See Kripke 1972, p. 85.

[39] Putnam, 1975, Burge 1979. This line of thought could ultimately be taken to include the contribution of *future* use as well, and Jackman 1999 outlines quite clearly how these commitments in one of these areas make taking on similar commitments in others much less conceptually costly.

[40] Indeed, Nesbit 2003 describes how the intuitions of both Easterners and Westerners can be 'primed' to run in a characteristically Eastern or characteristically Western fashion, and having an awareness of the reference-failure cases may 'prime' ones intuitions about the misidentification cases towards the causal account.

friendly intuitions about the Gödel cases.   Consequently, the mere fact that we have reason to privilege reflective intuitions over unreflective ones, doesn't show that cultural variance in unreflective intuitions will disappear at the reflective level. After all, that we may ultimately end up with conflicting sets of *reflective* intuitions is just what many suspect with the case of our ethical intuitions.   Admittedly, I find it highly unlikely that any group would systematize their semantic intuitions in a fashion that allowed for comparatively wide-spread reference failure. Nevertheless, lets assume for the sake of argument that the differences between Eastern and Western type-2 semantic intuitions would persist through the process of each group's reflections. If disagreement persists even among people's "reflective" intuitions, would that show that intuitions aren't a trustworthy starting point in philosophical inquiry?

It might initially seem as if such reflective disagreement doesn't produce any real problems for defenders of the intuitions who endorse the 'constructive' understanding of intuitions and their role in conceptual analysis. After all, the constructive conception can be contrasted with the realism implicit in the 'indicator model' of the role of intuitions in conceptual analysis in which semantic facts are independent of our intuitions about them, and so there is always a live question of why our intuitions should be taken to be reliable indicators of the truths about this independent semantic reality (reflective intuitions just being our most considered take on these independent facts).   On this realist understanding of semantic facts, if different groups have different intuitions about semantics, this is itself a good reason to think that *neither* set of intuitions can be trusted. By contrast, on the constructive model, semantic truths are simply what are produced by the best systematization of our intuitions, and this sort of constructivism about semantic facts has the following beneficial consequence for the epistemology of semantic theory. Since they are partially constitutive of what they are about, semantic intuitions are *prima facie* justified and can only be mistaken if they conflict with more deeply held sets of judgments. Consequently, if it turned out that there were competing sets of consistent systematization of two cultures' type-2 semantic intuitions, then rather than saying that neither set is reliable, we can say that the two groups are picking out different objects with the names in their versions of English. There is nothing incoherent in assuming that two people could ultimately turn out to mean different things by terms like "person", "knowledge" or "Gödel", and the two cultures' ability to

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[41] By contrast, see MMNS p. B3, note 3.

reach non-convergent equilibria relating to these terms would suggest that we are dealing with such a case. Disputes about what, say, "Gödel" refers to would be the product of speakers not realizing that they mean different things by the same term.[42]

However, this may treat the conflicts of intuitions too lightly, since what seems to be on the table here is not simply a kind of 'semantic pluralism' where we refer to *Gödel* by "Gödel" while the Chinese refer to *Schmidt* by "Gödel". Rather we are considering a type of 'meta-semantic pluralism' whereby, given what they mean by "refer", speakers of Hong Kong English are *correct* to say that *our* use of "Gödel" refers to *Schmidt*. Since different conceptions of reference would lead to different reference assignments, and thus different assignments of truth-conditions, it would seem that people with different conceptions of reference would disagree over which claims are assertable, and that this difference is a *practical* one. Like our ethical vocabulary, semantic vocabulary may, at bottom be difficult to be pluralistic about if different groups are expected to interact. One might still remains the case that we could just insist that there is more than one type of assertional practice we can take part in,[43] but the pluralistic view seems to be that we can legitimately be interpreted as engaging in both, even if we clearly intend not to participate in one of the two.[44]

Perhaps there is a way to make sense of a pluralism about our type-3 semantic theories, but it seems clearly preferable to have a unified type-3 theory, especially one that would allow for the possibility of pluralism at the type-2 level. For instance, just as a utilitarian can be an 'ethical pluralist' in that he or she allows that different ethical claims may be true in different cultures, and still be a 'meta-ethical monist' in that he or she takes these different ethical systems to each by justified by the fact that they are the systems that maximize utility in their particular cultural

[42] For a discussion of the possibility that this may be the norm in metaphysical and meta-semantic debates, see Lynch 1998, Jackman 1996. The pessimism about intuitions would thus ultimately stem from a semantic analog of something like the "epistemological realism" criticized in Williams 1991, 2001. For a dissenting view about the possibility of such pluralism, see Kornblith 1998.

[43] While it might be easier to view ourselves as being able to switch between the two sorts of assertional practice without undermining the objectivity of either than it would be to make a similar move with our ethical practices, it should be stressed that this is not the view that the type-3 pluralist seems committed to. He is committed to the stronger view that we can *always* be justly evaluated in terms of *either* practice. This would point to a type of indeterminacy, and thus a skepticism about meaning and truth.

context, we would like a type-3 semantic theory that remains meta-semantically monistic, but semantically pluralistic in the sense that it will allow the considered type-2 semantic judgments of the member of each culture to be correct about their own language. Fortunately, I think that there is a fairly well known meta-semantic theory that does just this.

5. Charity

Both the descriptive and the causal views that MMNS present are extremes, and if they were the only options available, then we might doubt that our (collective) type-2 intuitions could converge on a single type-3 theory. However, there are more nuanced accounts of reference that incorporate aspects of both the causal and the descriptive theory, and these may be able to accommodate cross-cultural variation without forcing us to endorse just one culture's set of intuitions. In particular, I believe that the most plausible account of reference, one associated with what is commonly referred to as the "Principle of Charity", can accommodate, and even explain, the results presented by MMNS. I should note here that the presentation and defense of Charity presented here will hardly seem adequate for someone not already sympathetic to the principle. However, considerations of space prevent me from providing a fuller defense,[45] and the principle serves here mainly as an example of how a single type-3 account of reference could accommodate (rather than just dismiss or explain away) the diversity of type-2 intuitions across cultures.

The Principle of Charity, very roughly, ties the referents of the terms in a speaker's language to those objects (or sets of objects) that maximizes the (weighted) total number of truths among the speaker's commitments. Since Charity maximizes the truth of our commitments in a way that seems reminiscent of theories that tie the referents of names to clusters of descriptions, the cases that seem (to philosophers) to be counterexamples to descriptive theories (e.g.: "Gödel") would also seem to be counterexamples to Charity.

However, the principle is comparatively resistant to such purported counterexamples provided that we remember that:

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[44] So, for instance, we would have to, in the Schmidt scenario, treat Easterner's "Gödel proved the incompleteness of arithmetic" utterances as false even when they became appraised of all the non-semantic facts and still insisted that *Schmidt* is the proper reference for " Gödel ".

[45] For a much more detailed presentation and defense of the principle, see Jackman 2003

(1) Some of our commitments carry greater weight than others.
(2) Charity applies holistically to all of our commitments at once.
(3) Many of our most deeply held commitments are rarely made explicit.

Understood in the light of these reminders, Charity can accommodate the 'causal' intuitions that most philosophers associate with the "Gödel" case.

For instance, as Nisbett and MMNS suggest, we (at least we in the 'West') have a deep, if usually implicit, commitment to perception, memory and testimony being *causally* structured. While we may not *explicitly* think that, say, our memories are about those events that are causally responsible for them, such an understanding is manifested in our practices, and thus forms part of the larger set of commitments against which our thoughts acquire their contents. If we discover that the person of whom we took a memory of ours to be a memory about could not have been causally responsible for that memory, we typically conclude that we must have been thinking of someone else. We 'know' that our memories must be causally connected to what they are memories of, even if this knowledge is not explicitly represented.

The thought experiments that Kripke musters in support of his 'causal' account of reference can be understood as illustrating our implicit commitment to testimony having this causal structure. If we did not already have such manifesting commitments, we would not find it 'obvious' that we could not be referring to, say, *Schmidt* by "Gödel", even if most of our 'Gödel-beliefs' would have been true of him. Indeed, such reactions are partially *constitutive* of our commitment to testimony's causal basis. Since the commitment to testimony's causal structure is heavily entrenched, it would not be charitable to give it up just to preserve the truth of a small set of Gödel-beliefs. Cases like that of Gödel are thus not incompatible with the Principle of Charity; rather, they only highlight how the commitments that must be taken into account extend beyond those that the speaker might explicitly manifest in his utterances.

Even with this extremely brief presentation, it should be clear how the account of reference determination based upon the principle of Charity has a type of flexibility that allows for it to accommodate the sort of cultural variation that MMNS discuss. In particular, the Charity-based account explains the fact that we refer to *Gödel* rather than *Schmidt* by "Gödel" not in terms reference being intrinsically causal, but rather in terms of our implicit commitment to our words

being part of a causally structured 'informational system.'[46] If some person, or group of people, were less committed to being part of this sort of causal/information system, then the charitable account would predict that they might refer to Schmidt rather than Gödel by "Gödel". Consequently, if, as Nisbett suggests, East Asians put less emphasis on causation in their explanations, then the charitable account might predict that the referents of some of their terms might differ from what they would be for Westerners with a similar set of 'forgrounded' beliefs associated with similar terms.[47]

However, this doesn't mean that different accounts of reference are true of (or even endorsed by) the two groups. A single type-3 semantic theory applies to both groups, it is just that this theory predicts that the names in North American and Hong-Kong English may behave differently given the differences in the higher level cognitive commitments between the two groups.[48] We thus are able to preserve the possibility of semantic pluralism within the context of a monistic meta-semantic theory.

Cultural differences in type-2 intuitions about what particular speakers refer to need not entail any sort of cultural variation in what would be our more reflective commitments about the nature of reference. Rather, the cultural variation could be explained by our tendency to project our culture's background cognitive assumptions and commitments onto the characters in the thought experiments. In the Gödel probe, East Asians and Westerners would 'fill in' the rest of the protagonist's cognitive commitments in different ways, leading them to take the subject to be referring to different things.[49] Important inputs into the reference determining function may vary from culture to culture, but it doesn't follow from that the function isn't the same. Our initial inclination may be to interpret Hong-Kong English as having the same type-2 semantic values as North-American English, but our reflective and more empirically informed judgments may require rejecting this initial assumption.

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[46]   For a discussion of this see Evans 1982.

[47] One should also note that such variations occur within the members of a culture, and quite possibly within a single person as they shift from context to context. This might also help explain some of the *intra* cultural variation in the reported data. How this sort of contextually sensitive weighting of commitments plays out is described in Jackman 2006b

[48]   While we have very few of what Evan's refers to as "descriptive names" (Evans, 1982), they might have many.

[49]   Since they may approach the question of thinking what *they* would be referring to if they were in John's position.

6. Conclusion

In conclusion, then, the cultural variation in responses to Kripke-style thought-experiments may be surprising to those not already familiar with cultural variation in 'cognitive styles.' However, since the relevant intuitions are about what we would be talking about in particular cases, not about the nature of reference *itself*, there is no reason to think that such intuitions cannot all be accommodated within a general theory of reference. Cultural variation thus doesn't prevent us from thinking that an adequate theory of reference should rely heavily on systematizing our intuitions. It only requires that an adequate theory of reference have a degree of flexibility (flexibility that the simple descriptive and causal theories lack) that would allow it to accommodate such variation.[50]

That said, the above is not meant to suggest that philosophers of language wouldn't benefit from having a look at the sort of cross-linguistic data that MMNS appeal to. Again, the analogy with Universal Grammar (UG) is instructive. While few doubt that our intuitions about grammaticality are generally reliable, it doesn't follow from this that one has much of a chance of coming up with a good model of UG without looking at a good deal of cross-linguistic data. In particular, while we may have reliable intuitions about which sentences are grammatical or not, we need not have reliable access to whether those judgments reflect aspects of UG directly, or just more contingent aspects of the parameter settings of our own language. In much the same way, our intuitions about cases may be reliable, but mistakes may come if we overgeneralize the import of these intuitions, and cross-cultural data can be a corrective to such overgeneralizations. In that respect, MMNS are correct to claim that:

> the intuitions philosophers pronounce from their armchairs are likely to be a product of their own culture and their academic training, in order to determine the implicit theories that underlie the use of names across cultures, philosophers need to get out of their armchairs.[51]

The difference is that I don't take the above to suggest that our type-2 intuitions are *unreliable*, only that they may *underdetermine* the correct meta-semantic theory.[52]

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[50] Compare this to the need for a proposed universal syntactic theory to explain why some languages allow various kinds of movement, while others do not. The conflicting intuitions about, say, the applicability of dropping PRO don't require either that we treat one of the linguistic communities as mistaken or simply give up the idea of there being a universal grammar that they both share. (Still, the analogy here shouldn't be pushed too hard, since in the semantic case, there may be more room to dismiss certain intuitions given the normative nature of the enterprise.)

[51] MMNS, B9.

Philosophers' intuitions about the Gödel and Jonah cases may be accurate, but causal theorists may be overgeneralizing from them if they take them to show that reference is *essentially* a causal relation. Once again, the mistake is not with the intuitions themselves, but the conclusions drawn from them. The 'causal' intuitions are equally compatible with the Charitable account where causation (rather than being an essential feature of reference) is simply a salient commitment that is put into the reference determining function. The charitable account is more complex, but it has the advantage of being compatible with the cross-linguistic data in a way that neither the pure causal nor the pure descriptive account are. Even if the Western data left it underdetermined which of the two is best,[53] the cross-linguistic set would favor the more flexible account.

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[52] Though MMNS's studies suggest that sufficient sensitivity to the variation that exists *within* our culture could play a similar role.

[53] Actually, I doubt that the 'Western' data leaves this underdetermined (especially given the intra-cultural variation MMNS point to).

References:

Burge, T. 1979. "Individualism and the Mental." In French, Uehling & Wettstein. *Midwest Studies in Philosophy IV: Studies in Metaphysics*. Minneapolis: University of Minnesota Press: 73-121.

Capppelan, H. and Winblad, D., 1999. "'Reference' Externalized and the Role of Intuitions in Semantic Theory" *American Philosophical Quarterly*. Vol. 36, no. 4. (Oct): 337-350.

Carnap, R. 1950. *Logical Foundations of Probability*. Chicago: University of Chicago Press (second edition 1962).

Cummins, R. 1998. "Reflection on Reflective Equilibrium." In DePaul and Ramsey 1998: 113-127.

DePaul, M. and Ramsey, W. (eds.). 1998. *Rethinking Intuition : The Psychology of Intuition and its Role in Philosophical Inquiry*. Lanham, Md.: Rowman & Littlefield.

DePaul, M. 2000. "Linguistics is not a Good Model for Philosophy." In Henderson & Horgan 2000: 113-120.

Devitt, M. 1994. "The Methodology of Naturalistic Semantics", *The Journal of Philosophy*, Oct. 94, 545-572.

Devitt, M. 1996. *Coming to Our Senses*, New York: CUP.

Evans, G. 1973. "The Causal Theory of Names". *Proceedings of the Aristotelian Society*, Supp. 47, 187-208. Reprinted in G. Evans, 1985, *Collected Papers¸* Oxford: OUP, and P. Ludlow, ed., 1997.

Evans, G. 1982. *The Varieties of Reference,* New York: Oxford University Press.

Hales, S. 2006. *Relativism and the Foundations of Philosophy*, Cambridge: MIT Press.

Henderson, D. & Horgan, T. (eds.), 2000. *The Role of the Empirical (and of the A Priori) in Epistemology*, *Southern Journal of Philosophy* Vol. XXXVIII, Supplement.

Jackman, H. 1996. *Semantic Norms and Temporal Externalism*. Ph.D. Thesis, University of Pittsburgh. (Available at www.jackman.org)

Jackman, H. 1999. "We Live Forwards but Understand Backwards: Linguistic Practices and Future Behavior", *Pacific Philosophical Quarterly* 80 (1999) pp. 157-177.

Jackman, H. 2003. "Charity, Self-Interpretation, and Belief" *Journal of Philosophical Research*, Volume 28 (2003), pp. 145-170.

Jackman, H. 2004. "Temporal Externalism and Epistemic Theories of Vagueness" *Philosophical Studies*, Vol. 117, No.1-2, (January 2004), pp.79-94.

Jackman, H., 2005. "Intuitions and Semantic Theory" *Metaphilosophy*. Vol. 36, no. 3 (April 2005) pp. 363-380.

Jackman, H. 2006a. "Temporal Externalism, Constitutive Norms, and Theories of Vagueness" In Tomas Marvan (ed.), *What Determines Content? The Internalism/Externalism Dispute*, Newcastle: Cambridge Scholars Press.

Jackman, H. 2006b. "Externalism, Holism and Contextualism" Talk delivered at the Institute Jean Nicod, Paris, May 2006.

Jackson, F. 1998. "Reference and Description Revisited". *Philosophical Perspectives, 12, Language, Mind, and Ontology*, 201-218.

Kornblith, H. 1998. "The Role of intuition in Philosophical Inquiry: An Account with No Unnatural Ingredients." In DePaul, M. and Ramsey, W. (eds.). 1998a. *Rethinking Intuition : The Psychology of Intuition and its Role in Philosophical Inquiry*. Lanham, Md.: Rowman & Littlefield: 129-141.

Kripke, S. 1972/1980. *Naming and Necessity*, Cambridge, MA: Harvard UP.

Lakoff, G. 1987. *Women, Fire and Dangerous Things*, Chicago: The University of Chicago Press.

Lewis, D. 1970. "How to Define Theoretical Terms". *The Journal of Philosophy*, *67*, 427-446.

Lewis, D. 1984. "Putnam's paradox", reprinted in his *Papers in Metaphysics and Epistemology*, (New York: Cambridge University Press, 1999).

Lynch, M. 1998. *Truth in Context*. Cambridge: MIT Press.

Machery, E., Mallon, R., Nichols, S., Stich, S. P. 2004. "Semantics, Cross-Cultural Style", *Cognition* 92 (2004), B1-B12.

Miscevic, N. 2000. "Intuitions as a Second Window." In Henderson & Horgan 2000: 87-112.

Nisbett, R. E., Peng, K., Choi, I., & Norenzayan, A. 2001. "Culture and systems of thought: Holistic vs. analytic cognition." *Psychological Review*, *108*, 291-310.

Nisbett, R. E. 2003. *The geography of thought: How Asians and Westerners think differently … and why*, New York, Free Press.

Norenzayan, A., Smith, E., & Kim, B., 2002. "Cultural Preferences for Formal versus Intuitive Reasoning". *Cognitive Science*, *26*, 653-684.

Putnam, H. 1975. "The Meaning of 'Meaning'". Reprinted in H. Putnam, 1975, *Philosophical papers: Vol. 2, Mind, language, and reality*, New York: Cambridge University Press.

Putnam, H. 1981. *Reason, Truth and History*, New York: Cambridge University Press.

Ramsey, W.1998. "Prototypes and Conceptual Analysis" in DePaul, M. and Ramsey, W. (eds). 1998. *Rethinking intuition : the psychology of intuition and its role in philosophical inquiry* Lanham, Md. : Rowman & Littlefield, 1998.

Rawls, J. 1971. *A Theory of Justice*, Cambridge: Harvard University Press.

Rorty, R. 1979. *Philosophy and the Mirror of Nature*. Princeton: Princeton University Press.

Rosch, E. 1975. "Family resemblances: studies in the internal structure of categories", *Cognitive Psychology*, 7.

Searle, J. 1958. "Proper Names". *Mind*, *67*, 166-173. Reprinted in P. Ludlow, ed., 1997.

Searle, J. 1983. *Intentionality : An Essay in the philosophy of mind*, Cambridge: Cambridge University Press.

Segal, G. 2001. "Two Theories of Names". *Mind and Language*, *16*, 5, 547-563.

Watanabe, M. 1998. "Styles of reasoning in Japan and the United States: Logic of Education in two cultures". Paper presented at the American Sociological Association Annual Meeting, San Francisco, August, 1998.

Watanabe, M. 1999. *Styles of reasoning in Japan and the United States: Logic of education in two cultures*. Unpublished Ph.D. thesis, Columbia University.

Weinberg, J., Nichols, S., & Stich, S. 2001. "Normativity and Epistemic Intuitions". *Philosophical Topics*, *29*, 1&2, 429-459.

Williams, M. 1991. *Unnatural Doubts*. Cambridge: Blackwell.

Williams, M. 2001. *Problems of Knowledge*. New York: Oxford University Press.