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Reference and Linguistic Intuitions

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1 Introduction

The standard methodology in theoretical linguistics goes more or less as follows. The linguist interested in a particular phenomenon consults intuitions on how it is realized. These intuitions usually relate to grammaticality, for syntax or phonology, or acceptability, for pragmatics; work in semantics can reference both. If the linguist is a native (or near-native) speaker of the language of interest, she probably uses her own intuitions; if not, she uses those of a consultant. Data collection can end here, or the linguist can go on to consult additional native speakers, or even construct a survey to elicit the intuitions of a larger group of people. After considering a data set of some size, she constructs a hypothesis, and then formalizes it in some theoretical framework. This is the traditional model.

It is also the traditional model in philosophy. Recently, the role of intuition has been highly controversial in that field, with the advent of experimental philosophy. The aim of this movement has been (on the dominant interpretation at least) to solidify the data used in philosophical work: this means, essentially, less reliance on the 'armchair' intuitions of philosophers, via a focus on the intuitions of what are (somewhat patronizingly) referred to as 'the folk,' i.e. laypeople, whose intuitions are usually gathered by means of surveys or questionnaires. This larger amount of data can then be analyzed for statistically significant patterns. This movement has many critics (see e.g. Williamson 2008, 2012; Devitt 2012), but also many adherents.

For researchers in linguistics, the interest of the movement lies both in

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some of the results it claims and also in the way in which it is justified. A good deal of work in experimental philosophy focuses on issues relating directly to language, such as the interpretation of deontic or teleological modals like *should* (cf. von Fintel and Iatridou 2007), or the truth presupposition associated with knowledge (and, by extension, the factive verb *know*: see Karttunen and Peters 1979; Beaver 2001). In this paper I want to focus on a more recent line of experimental work which has yielded extremely controversial results, which begins with Machery et al. (2004).

Machery et al. (2004) examined two theories of *reference* with respect to two groups of participants. It is generally accepted in linguistics and philosophy that proper names such as *John Locke* or *Ricardo Villalobos* directly pick out individuals in the world; in terms of semantic composition, they directly contribute a referent for the individual, of which properties can be predicated. But how are individuals picked out by proper names? Theories of reference, in the sense that is of interest here, attempt to answer this question. Reference can thus be thought of as a metasemantic property, because it works to determine what the semantics of a given proper name turns out to be.

The two most widely accepted theories of reference (or families of such theories) are the *description theory* and the *causal theory*. Roughly, according to the description theory, names are shorthand for definite descriptions, and refer to the individuals which satisfy those descriptions (e.g. Searle 1958). The name *Gödel*, then, might be shorthand for 'the individual who discovered the incompleteness of arithmetic,' and therefore refers to the individual who discovered that incompleteness. On the causal theory, names pick out those individuals who are related to them by some causal chain (Kripke, 1972). For instance, the individual which is picked out by my use of the name *Gödel* will depend on the way I learned this name, and the way the name was learned by the source from which I learned the name, and so on, back to the 'baptismal event' by which Gödel himself became called by the name

Gödel. Which of these theories is correct is a vexed issue; there are arguments and bad predictions on both sides. It is not clear that the introspective method is able to resolve this dispute.

From this starting point, Machery et al. applied the experimental method. Inspired by claims of Nisbett (2003, i.a.) about "Asian" vs "Western" styles of thought and reasoning, to the effect that "East Asians" reason holistically, meaning that they have "a preference for explaining and predicting events on the basis of [relationships between a focal object and the field]", while "Westerners" have a "preference for using rules about...categories to explain and predict" the behavior of objects (Nisbett et al. 2001, p. 293).[1] It's not really clear to me what exactly these preferences amount to in terms of actual behavior, especially for the holistic case, but, for Machery et al., they were enough to spur the construction of a hypothesis: that Westerners are likely to use a causal theory of reference, while East Asians are likely to use a descriptive theory. They tested this hypothesis via questionnaires administered to undergraduate students at Rutgers University and the University of Hong Kong. On examination, it turned out that the Rutgers (Western) students did indeed answer the questions in a way consistent with a causal theory of reference, and the Hong Kong (East Asian) students in a way consistent with a descriptive theory. Within philosophy, furor ensued. The results seem to have been intended by the authors to call into question the method of introspective examination of cases as a source of philosophical data, at least to the extend that the intuitions thereby elicited belong to professional philosophers. I will return to this point later in the paper.

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1   In the sequel I will eliminate the quotation marks I have put around 'Westerner' and 'East Asian' above. I doubt that these are really coherent categories, especially given the actual results of Nisbett (et al.), which show what look like significant differences between people from different European countries with respect to the claimed cognitive factors, and also given the ill-defined and vague definitions to be had here. I will suspend skepticism on this issue for the purposes of the present discussion, though see Devitt 2012 for additional discussion.

In the rest of this paper, I wish to take a closer look at this debate and what it means for linguistics. In section 2, I will take a linguistic perspective on these issues. I'll first briefly discuss what might be the consequences for purely linguistic research, focusing on semantics, if Machery et al.'s results are indeed correct, and what implications we might expect this to have elsewhere. As we'll see the results are pretty disastrous for many standard linguistic assumptions, and cause serious problems for the usual semantic methodology. Is there any way to avoid these consequences? It seems that there may well be. In sections 3 and 4, I'll examine some of the criticisms that have been leveled against the Machery et al experiment and its follow-ups, adding some worries of my own. The conclusion will be that major changes in our semantic methodologies are likely to be premature.

2   Consequences for linguistics

If the results Machery et al. report are correct, there are substantial implications for theoretical linguistics, both in fundamental assumptions and in the range of possibilities for analysis. Some predictions are also made about what we should expect to find in areas such as cross-cultural communication. This section examines some of these consequences.

The standard assumption in semantics is that meanings are essentially identical across languages, in the sense that the same basic constructs can be used to model or describe meanings cross-linguistically. One could view this position as a version of a 'universal grammar' assumption: just as all languages (as far as we know) make use of the same syntactic categories, all languages can be assumed to make use of the same semantic objects. On this picture, what we might call the universality assumption is basically a Chomskyan position about how languages are structured, perhaps relating to innatist views on the possibility of language acquisition. This more or less corresponds to the view on philosophical intuitions on which they indicate something about our cognitive structures, rather than something about the external world.

For linguistics, this is clearly the default position.

But what happens if we take Machery et al's results at face value? Suppose that one's native culture influences, or even determines, the metasemantics of reference. The mechanism of reference is a highly fundamental one. Researchers with a strongly externalist perspective on meanings might even believe that the semantics of all lexical items is determined by referential mechanisms. But if this is so, then, if metasemantic factors like how reference works can depend on factors as unexpected as cultural heritage, it seems that the languages used by individuals can vary metasemantically in diverse and idiosyncratic ways. Then the assumption laid out in the previous paragraph seems unmotivated. There is no longer any reason to believe that there is a constrained set of semantic objects from which meanings can be drawn; rather, we should be prepared for anything in examining empirical facts. Similarly, the domain of analysis is thrown wide open. Semantically relevant differences could follow from any number of sources. It would be possible that one group of speakers uses a standard semantics for a first-order metalanguage, and that another group uses a completely different semantics which validates a different set of inferences; it would be possible that some language uses a completely different set of type-theoretic entities than another, or all the others. In a sense, we would validate the conclusions of Whorf (1956), which otherwise do not seem empirically well supported.[2]

I do not think that many semanticists would be happy with this result. Theory evaluation in that area, as in other fields, makes use of parsimony as a methodological principle; a theory is to be preferred to the extent that it is simpler and makes use of less machinery, all else being equal. But if parameters are allowed to vary even a bit wildly, this kind of

2   However, there is a sense in which some of the possibilities raised here fall into
     line nicely with Wittgenstein's rule following argument (see e.g. Kripke 1982), in
     that it might well be that such variation exists in a hitherto unnoticed, or even
     completely inaccessible, domain. This is an interesting line, but I won't pursue it
     further in this paper.

evaluation becomes very difficult, for the theories to be compared may well lack a common language or be set at different levels. The comparison between theories comes to look like a multidimensional one, with a correspondingly complex evaluation metric (van Rooij, 2011). The consensus among linguists about exactly how certain theoretical entities should be understood is already imperfect. If the languages which are the objects of linguistic analysis themselves can disagree, it seems we have little hope in the end for a proper, universal, theory.

The natural reaction for a semanticist invested in the status quo is to look for a more conservative solution. I'll try to work toward this in the following sections by looking for alternate explanations of the experimental data. But before turning to this project, one might wonder whether there are other ways that one could try to determine whether the conclusion that different cultural backgrounds bias people toward different theories of reference is otherwise supported. Are there predictions that it makes that might be empirically tested in ways outside those already explored? Does it have consequences that one might expect to have already been noticed? For the first, I am not sure; but it does seem to have clear consequences for areas such as second language acquisition and cross-cultural communication. If different cultures bias those raised in them toward particular reference mechanisms, we would expect miscommunications to occur fairly frequently between individuals from cultures with different 'theories of reference,' especially given that situations where such differences might be problematic are not uncommon, as emphasized by Devitt (2012). For instance, in conversation between East Asians and Westerners, there should be fairly frequent errors of reference identification based on the different theories at work. If we do not find such errors, this would seem to be pretty strong negative evidence against the proposed difference. Similarly, given the quantity of research on the acquisition of second languages and cross-cultural communication, one would expect that someone would have noticed all the acquisition problems and errors in reference of lan-

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guage learners, or the reference errors made in communication between people of different cultures. The lack of discussion in the relevant literature strikes me as suspicious. I find it surprising that the first people to notice the (alleged) difference in referential theories come from outside these disciplines.

Thus, outside the experimental results, it does not seem that there is strong evidence for differences in referential mechanisms of the kind proposed by Machery et al. (2004). In the following section, I'll turn to some worries raised by the experiment itself.

3 Semantic worries

Machery et al. (2004) take their results to show that standard philosophical methods must be abandoned, as they do not take into account the intuitions of non-philosophers and in particular those from different cultural backgrounds than most philosophers have. Of course, for this to follow, it must be that the results of the experiment(s) carried out cannot be given alternate explanations. Criticisms of their work offer alternate explanations along two main lines: (i) the experimental design is weak, and allows confounding factors to enter the picture, or (ii) the intuitions of the subjects of the experiment do not carry as much weight as those of experts. In the rest of the paper, I will discuss each type of criticism briefly, adding some new factors: (i) in this section, (ii) in the next.

The first and most obvious problem with the study carried out by Machery et al. (2004) is the form of the task given to the subjects. They were asked to read relatively long and complex stories and then to answer questions about the reference of names used in them. I'll reproduce one such story here, involving the so-called 'Gödel case' of Kripke (1972) (the others are roughly similar in character):

> 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 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; they 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 completeness of arithmetic? or

(B) the person who got hold of the manuscript and claimed credit for the work?   (Machery et al. 2004:B7)

Answering (A) implies a descriptivist theory of reference, and answering (B) a causal theory — if the task yields reliable results. A number of authors have claimed that it does not, for a variety of reasons; for example, that it is not sufficiently controlled as it allows subjects to inject pragmatic reasoning into their responses, for John may mean (in the sense of speaker meaning, cf. Grice 1975) something different by the name 'Gödel' than the name actually refers to. I find this a legitimate and serious criticism. However, the line of criticism I wish to take up here is different.

Systma and Livengood (2011) note a perspectival ambiguity in the question used in the task. Is the subject meant to use a first-person perspective, which would mean that the information about Schmidt can be used in determining reference, or to use the perspective of John, who has no idea about the existence of Schmidt, and so can only mean Gödel

by his use of the name? Without resolving this question, the task is underdetermined, and it is impossible to tell exactly what factors are involved in the patterns of answers that Machery et al. (2004) found. Systma and Livengood (2011) ran several versions of it with the problematic questions replaced with (relatively) unambiguous ones. None of these studies showed the pattern of results that Machery et al. (2004) found; rather, a large majority of the subjects, 'Western' and 'Eastern,' responded in ways consistent with a causal theory. This fact at least suggests that the Machery et al. results are due to the ambiguous nature of the questions.

Machery, in a personal communication to the authors (quoted in their footnote 6), indicates that the presence of the term *really* in the (A) response is enough to bias toward a first-person interpretation. It does; but it does more. The term *really* is a modifier; its meaning is, roughly, to force that the content in its scope be interpreted *in the actual world or situation*. But the actual world is of course not the same as the world in the scenario; in reality (as far as we know, anyway) Gödel discovered incompleteness. This might bias the respondents toward answering in a way consistent with reference to Gödel, regardless of their 'underlying theory.' It's hard to say without further experimentation just how any noise introduced by this potential problem might have influenced the study.

Of course, the survey format used in the experiments disallows getting a clear picture of why the participants answered as they did; though they were given space to comment on their reasons, these comments are not always enough to clarify the thought processes involved. In an attempt to get clear on what might be going on, I gave the scenario and question above to 30 undergraduate students at Aoyama Gakuin University. These students have some experience with semantics and with tasks like determining truth conditions, so, while definitely not experts at the level of e.g. professional philosophers, the subjects were not complete novices either. The survey was done in a classroom setting. Interestingly, nearly

all the students chose (A), the descriptivist answer; but, when asked for their reasons, they said things like "I chose (A) because surely John could not be talking about someone he had never heard of." This reason, however, should prompt the choice of (B), for (A) means that John refers to Schmidt by his use of "Gödel". When this was pointed out to the students, they *all* changed their answer to (B). To me, this suggests that the task itself is one that was excessively difficult for non-native speakers of English, and that competency issues with a second language might be causing problems for the test (even though most students in the class are highly proficient in English). This conclusion is consistent with the results of Lam (2010), who ran a version of the Machery et al. survey in Cantonese, to which, again, most participants chose causal-type answers.

All in all, I think it is clear that the task itself is difficult and contains ambiguities; in short, the experimental prompt is flawed. This kind of difficulty is associated with the use of experimental procedures in new contexts. The experimental turn in linguistics has also produced its share of procedurally problematic experiments, which have led to improvements in procedures. The response to the work of Machery et al., to me, is in this sense very positive; progress is being made, and future work on experimental philosophy of language will benefit from it. Further experiments will also show whether the reference hypothesis is correct. For the present, though, I believe that semanticists need not reconsider all their methodological assumptions.

4   Expertise and Semantic Intuitions

In this last section I want to briefly consider a response made by Machery (2012) to a challenge leveled by a number of philosophers, notably Michael Devitt. The challenge in a nutshell is this: perhaps many naive speakers give unexpected responses to questionnaires about their intuitions, but why should we care? Philosophers presumably have better intuitions, since they have thought about the topics, and their judgements

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are sharpened by their familiarity with the kind of task they must perform, their knowledge of the area which allows them to abstract away from irrelevant features of the experiment, and so on. We should privilege the intuitions of the professionals over those of the naive. So the challenge goes.

Machery, in response, claims in brief that philosophers have no claim to infallibility in the domain of intuitions. It is enough to note the conflicting intuitions that some philosophers have about some problematic cases to see this (e.g. the "trolley problem," Gettier cases, etc; e.g. Gettier 1963; Alexander 2012).[3] He notes correctly that linguists have worried about theoretical bias in the intuitions of professionals, and some have taken steps to avoid it. I will say more about this below. His main argument, though, rests on an experiment purporting to show that the intuitions of linguists, who are at least supposed to be experts on language, do not bias them toward a causal or a descriptivist theory of names. This experiment is highly unconvincing.

The experiment solicited the participation of linguists online. Participants were invited to self-describe themselves as specializing in a particular category of linguistics: semantics, philosophy of language, historical linguistics, etc. On the basis of this self-declaration, Machery separated the linguists into groups. Machery hypothesized that semanticists and philosophers of language would show a bias toward a causal theory because people in this group were likely to be familiar with the Kripkean theory, and thus likely trained to get the relevant intuitions. For the purposes of analysis, these groups were classed together. He further put discourse analysts, historical linguists, and sociolinguists together because "they are likely to be particularly sensitive to the

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3   The 'trolley problem' involves the following kind of thought experiment: suppose that there are five workers on a train track which will be hit by a train if nothing is done. The train can be diverted to another track, thereby killing another worker. You have the power to divert the train or not. Should you? Interesting biases can be found in the solutions depending on the identity of the sixth worker, etc. See Alexander (2012) for details.

descriptions associated with proper names" (Machery 2012: 48). The reason for this last point is not clear; I doubt many linguists would endorse the claim that historical linguists are deeply concerned with descriptivism as it relates to proper names. It looks a bit like saying that metaethicists are sensitive to logical aspects of the first-order theory of arithmetic because their work often requires them to compare numerals in the context of the trolley problem. Putting that point aside, Machery analyzed the data and found that individuals in Group 1 gave Kripkean intuitions more than those in Group 2; but, he concluded, since both groups are experts on language, clearly expertise does not consistently bias one way or the other, and expert intuitions cannot be considered reliable. This is an interesting way of interpreting the data. Suppose I assembled a group of brewers and asked them to specify the proper quantity of hops needed for brewing an IPA. Suppose further that the beer brewers nearly all gave one answer, and the sake brewers gave another answer. It seems improbable that one would therefore wish to argue that expertise in brewing does not help in knowing how much hops to use in brewing IPA. The expertise of the sake brewers simply does not lie in the right area, though they are still experts. Similarly, though workers in fields like historical linguistics are obviously experts on language, their expertise might well not be of the right type to help with judgements about reference.

This is not to say that the expertise defense is quite out of the woods. Returning to linguistics, researchers have certainly worried about the evidential status of intuitions there as well. The worries here tend to center around the objectivity of linguistic judgements. It does seem as if one's theoretical positions can bias one's intuitive judgements; most working linguists are also familiar with what one might call 'intuition bleaching,' where one's intuitions become unreliable after some period of working with data of a particular type. Worries like these have prompted a great deal of methodological reflection (see e.g. Schutze 1996 for an overview, now slightly dated), and have led to a good deal of

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experimental research. If expert intuitions come packaged with theoretical bias, there are serious problems for the experts.

However, the standard method of intuition consulting has not been abandoned, and recent work suggests that its track record is not too bad. Indeed, Sprouse and Almeida (2012a) examined ten years of articles from *Linguistic Inquiry*, a cutting-edge journal in the field dedicated to work in the Chomskyan tradition. Such work is precisely that where judgements are alleged to be unreliable. However, Sprouse and Almeida found that the intuitions reported by syntacticians employing the traditional method align very closely (with near 95% accuracy) with those reported by participants in a large-scale study carried out using Mechanical Turk.[4] These authors conclude that (while experimental evidence is obviously useful and good to have) the traditional method of data collection is quite reliable and sufficient for many or most purposes. Phillips (2009) reaches similar conclusions, pointing out that any data which makes its way into the peer-reviewed literature must have passed a gauntlet of experts, making it, in a sense, already crowd-sourced. I conclude, again, that we need not take any extreme position at present.

5 Conclusion

Some have concluded on the basis of the results that have come out of experimental philosophy that traditional philosophical methods should be abandoned. Does linguistics face the same kind of worries? It's not obvious that it must: the intuitions in question are not precisely the same. Philosophical intuitions are often used to decide issues about the external world, for instance in metaphysics. Linguistic intuitions are used only to decide issues about facts within language; depending on how one conceptualizes language, these intuitions might well only involve matters internal to the mind. For instance, if one takes the Chomskyan idea of i-language at face value, language is purely a mental object, and can even

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4   See also Sprouse and Almeida (2012b) on this issue.

differ from individual to individual (cf. the talk of 'dialects' in much formal linguistics). We end up with something close to the 'conceptual' interpretation of work on intuitions in experimental philosophy, where intuitions are taken to exhibit something about how people conceptualize the external world (cf. Alexander 2012 on this distinction). This understanding of how intuitions should be used has been argued to be of little philosophical interest. But this worry doesn't arise for linguistics, where we are interested in how internal representations of language work. I take this to be a reason that linguists should not be concerned about making use of intuitions, though raw intuitions still should be backed up with experimental work.

In this paper I have examined briefly some arguments made within experimental philosophy about the status of intuitions, and the degree to which standard universalist views of semantic content can be supported. I argued that linguistic intuitions at least are, in general, reliable, even those of experts, and that we are not yet in a position to require a deep shift in our view of semantics. I believe that at this point it is premature to say that the standard picture must be abandoned.

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