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REFERENCE, INTUITION, AND INTUITION ABOUT REFERENCE – NOTES ON THE EXPERIMENTAL PHILOSOPHY OF LANGUAGE DEBATE

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ABSTRACT. The introduction of experimental techniques as a tool of philosophical investigation has created quite a stir in analytic philosophy in the last two decades. Experimental results have shed a new light on traditional debates in various fields and have led to a reappraisal of the use and merits of various methods and types of arguments in philosophy. This paper provides an overview of the central debate regarding Machery et al.'s results and conclusions about the interplay between theory and various methods that are supposed to provide evidence for (a certain) theory. The debate is connected with the initial setting of the question of reference and, more generally, with the main staples of the philosophy of language. A large part of the paper is devoted to a discussion of the most important arguments and perspectives that have been developed or reformulated as a result of the debate on experimental philosophy. The final aim of the discussion is to provide a basic description of the contemporary landscape of philosophy of language and related epistemology, to inform future research designs and developments.

Keywords: *philosophy of language, experimental philosophy, reference, philosophical expertise.*

1. Introduction

The paper discusses recent work in experimental philosophy of language, focusing on some of the main criticisms that were proposed against the possible conclusions that the Machery et al. experiments appeared to lead to, in what regards reference, but also the importance of intuitions, the distinction between expert and layman intuition in the philosophy of language, and the role that experimental philosophy may play in the dynamics of philosophical inquiry. To

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some extent, the paper may be regarded as a case study of the most important debate in current experimental philosophy that attempts to provide a primary assessment of the perspectives of extending the experimental method to other issues or types of discourse in the philosophy of language.

We begin with presenting a part of the philosophical background of the experiments by summarizing the famous arguments of Kripke against the descriptivist theory of names, which is followed by a description of the now famous Machery et al. experiment and the most significant (in my view) replies to that paper. These replies are categorized and discussed, along with their rebuttals by Machery and their collaborators. The final chapter aims to offer a more encompassing perspective of the debate and its significance for epistemology and the philosophy of language. Some of the newer Machery et al. – inspired experimental research and the theoretical discussions of the experiments from the literature are examined on the larger canvas of (dominating and rival) perspectives on reference, intuition, and intuitions about reference.

2. Kripke against descriptivism

Kripke's arguments in *Naming and Necessity* are regarded nowadays almost unanimously as a turning point in analytic philosophy.[2] In his famous lectures, Kripke discusses various issues that are central for the philosophy of language, metaphysics or the philosophy of mind, having to do with the role and nature of referential terms (especially proper names) and with modal notions and arguments. Kripke's critique of the descriptivist theory of proper names is widely regarded as the most powerful and influential part of *Naming and Necessity*. In order to refute and displace reigning descriptivist theories, Kripke relies significantly on modal notions, but modal notions themselves are also discussed and redefined in the lectures. Kripke's logical contributions, his views on meaning and reference in the philosophy of language, and his metaphysically oriented stances regarding modality support each other in a successful example of what Gutting calls *persuasive elaboration*.[3] As such, *Naming and Necessity* may also be regarded as the last step in the philosophical rehabilitation of strong (metaphysical) modal notions. Contrary to previous widespread suspicion about modal notions, most post-Kripkean philosophers and logicians have fully embraced modality and have developed and put to use a wide range of modal logics for various theoretical purposes.

[2] Kripke (1980).
[3] Gutting (2009).

Kripke's refutation of descriptivist theories of proper names was (and still is) considered by many analytic philosophers as definitive. The causal-historical view, which is the alternative that Kripke proposed, is the dominant perspective in contemporary philosophy of language pertaining to the analytic tradition. As a starting point to this paper, I will examine briefly Kripke's arguments against descriptivism by making use of the taxonomy of Devitt and Sterelny.

Ignorance and Error.[4] Strong descriptivism takes the meaning of a name in a language to consist in the description associated by competent speakers of that language with that name (e.g., the meaning of "Augustus" is given by the description "the first Roman emperor"). Weak descriptivism makes the more modest claim that while one may admit that the meaning of a name is not exhausted by a certain description, associated descriptions or clusters of descriptions do determine the referents of names. Kripke uses multiple cases of names of famous people ("Cicero", "Moses", "Jonah", "Feynman", "Einstein", "Peano") to argue against weaker forms of descriptivism. The cases considered seem to show that descriptions are neither necessary, nor sufficient for identifying the referents of proper names.[5]

Although they know and use names of famous people like "Cicero", "Einstein" or "Feynman", ordinary people aren't usually able to tie any identifying belief to those names. They may know, for instance, that Cicero was a famous Roman orator or that Einstein was an important physicist. But even when they are able to identify, say, Cicero as "the man who denounced Catilina" or Einstein as "the author of the theory of relativity", language users are often not able to supply an independent description for the terms purportedly employed to identify the referents (Catilina will be described as "the man denounced by Cicero" and the theory of relativity will be "the theory discovered by Einstein"). Moreover, people often entertain false beliefs about famous people; many think that Einstein was the inventor of the atomic bomb, for instance. Yet, even though they may ignore or be mistaken about the unique properties of Cicero or Einstein, people still refer to Cicero or Einstein when using their names (and not, for instance, to Julius Caesar or Oppenheimer). Of course, everyone may be in the position of the speakers who falsely believe that Einstein was the inventor of the atomic bomb. That is one of the take-home messages of Kripke's discussion of the case of Moses (an example borrowed from Wittgenstein). We might learn that none of the deeds the Old Testament attributes to Moses are true of him, but this is not to say that Moses didn't exist at all.[6] Kripke proposes the alternative example of Jonah, about whom

[4] Devitt & Sterelny (1999: 54-57).

[5] Cf. Kripke (1980: 80-83); Devitt and Sterelny (1999: 55-57).

[6] The case of Moses is actually discussed at various places in Kripke's *Naming and Necessity.* Kripke (1980: 31-34, 58-61, 65-67).

we suppose (or know) that he couldn't have done the deeds attributed to him in the Old Testament, and yet we don't conclude that he never existed. Instead, it may be the case that there had been a historical character named Jonah and he became the subject of the myths that are written down in the Book of Jonah. Even if there is no truth to these stories, we can still hold that Jonah existed.[7] To sum up, all these examples are put forth by Kripke in order to show that descriptions are not *necessary* for identifying the referents of names.

Kripke also uses some of these cases and similar ones to show that descriptions are not *sufficient* for identifying the referent of a proper name. Error is also, as Devitt and Sterelny note, an argument against sufficiency.[8] If descriptions were indeed sufficient for referring to a certain object, then everyone using a description such as "the inventor of the atomic bomb" would refer to Oppenheimer (the actual inventor).[9] But, Kripke's argument goes, people who think that Einstein was the inventor of the atomic bomb do not appear to refer to Oppenheimer when using the name "Einstein", rather they refer to Einstein himself.

Kripke's Gödel-Schmidt mental experiment illustrates the same point. Many people know a single fact about Gödel, namely that he discovered the incompleteness of arithmetic. So, according to descriptivism, such an ordinary man would refer to whoever discovered the incompleteness of arithmetic when using the name "Gödel". In order to show that this tenet is wrong, Kripke considers a possible, though implausible, turn of events in which Gödel stole the proof of incompleteness from one of his friends named Schmidt. So, Schmidt would be the real discoverer of the incompleteness of arithmetic. But then, according to the descriptivist view, whenever an ordinary man uses the name "Gödel", he actually means to refer to Schmidt, since Schmidt is the real discoverer of the incompleteness of arithmetic. This seems to be wrong, according to Kripke. If someone uttered the statement "Gödel is a fraud", he would seem to be right, but not about Schmidt, the actual man who discovered the incompleteness of arithmetic.[10] Kripke uses another example to support his claim and avoid the possible misunderstandings generated by the implausibility of the Gödel-Schmidt case. The supplementary Peano-Dedekind example serves to make the same point. Although the Peano axioms are/were attributed

[7] Kripke (1980: 67-68).

[8] Devitt & Sterelny (1999: 57).

[9] Oppenheimer was the coordinator of a team of researchers who developed the atomic bomb. It can be argued that he is not the inventor of the atomic bomb, in the traditional sense of "inventor", because the atomic bomb is the result of a team effort. But this would also spell trouble for the descriptivist, as the name "Einstein" (and any name tied to the "inventor of the atomic bomb" description) would simply fail to refer, given that there is no actual inventor of the atomic bomb.

[10] Kripke (1980: 83-84).

to Peano, they were actually formulated by Dedekind. Yet, people who believe Peano was the author of the Peano axioms do not refer to Dedekind by the name "Peano".[11]

Unwanted necessity.[12] According to strong descriptivism, it is obviously necessary that the bearer of a certain name has the property picked out by the associated description, so for instance, in any possible world Aristotle would be the author of the *Nicomachean Ethics* (the statement "Aristotle is the author of the *Nicomachean Ethics*" should have the same content, according to descriptivism, with the tautology "The author of the *Nicomachean Ethics* is the author of the *Nicomachean Ethics*"). But this is wrong for Kripke. Any such property seems to be detachable in various counterfactual situations from the object that actually possesses it.[13] Strawson and Searle proposed a more modest version of descriptivism called the 'cluster theory'. According to this theory, the object must have most (but not necessarily all) of the properties that are usually attributed to it. These properties are designed by various descriptions. On such a view, it will only be necessary that the object has the logical sum, i.e. inclusive disjunction, of the properties from the cluster.[14] Kripke also objects to this weaker form of descriptivism on similar grounds.[15, 16]

Lost Rigidity.[17] A term is rigid if and only if it designates the same object in every possible world in which that object exists. If the meaning of names is given by descriptions, then names cannot be rigid. But according to Kripke, this shouldn't be right – names should refer to the same individual in every possible world. To take one of Kripke's examples, 'Nixon' should refer to the same man (the one we call 'Nixon' in this world) in every possible world. However, if the meaning of 'Nixon' is expressed by some description, say, 'the man who won the 1968 U.S. presidential elections', the name will refer to other individuals in other possible worlds. That is because in other possible worlds different people are the

[11] Kripke (1980: 84-85).

[12] Kripke (1980: 84-85).

[13] Cf. Kripke (1980: 30)

[14] A weighting of properties from the group may be included in the theory. Some properties may be more important than others, which may give rise to some complications.

[15] Kripke (1980: 31-34)

[16] There is, however, the case of essential properties which gives some credit to descriptivist theories. Cluster theorists would presumably treat essential properties as properties with a large weight. In any case, essentialism appears to provide us with properties that are inseparable from the objects that have them and thus with descriptions that are successful. The simplest answer that the Kripkean can level against this point is that people do not need to know any essential property in order to refer to a certain object when using a certain name. One may not know any essential property of Aristotle and still refer to him in virtue of being part of the relevant chain of reference.

[17] Devitt and Sterelny (1999: 51-52).

winners of the 1968 elections. E. g., in some other possible state of affairs, the Democrat candidate (Humphrey) wins the election. Then, according to descriptivism, the name 'Nixon' should pick out Humphrey in that world.[18] While it is true that descriptions can be 'rigidified' (by operators such as "actually" or *that*) in order to refer to the same individual in every possible world, we don't need to apply the same maneuver to names, which seems to show that names and descriptions have a different nature.

Devitt argues that one of Kripke's Aristotle examples, discussed in the preface to *Naming and Necessity*, serves the same argument (Lost Rigidity).[19] According to Devitt, while some of Kripke's cases (like the Nixon example or the Hesperus and Phosphorus case) are based on modal intuitions, this example (much like the Gödel-Schmidt mental experiment) rests on intuitions about reference. That is, the reference of a name stays the same, whether or not the descriptions we associate with that name are true of the name's bearer in a world. For Kripke, it makes no difference if a statement such as "Aristotle is fond of dogs" describes an actual or a counterfactual situation as far as its truth conditions are concerned. The same paradigm is applied in every case: the statement is true in this world or in any other world if and only if there is a certain man – the same one in every possible world – who is fond of dogs. That man is, of course, the one we call "Aristotle" in this world.

3. The experiments of Machery et al. and the ensuing debate about their meaning

A. The initial experiment

The significant development of the field of experimental philosophy in recent years is partly due to the interesting and controversial work of Machery et al. and the ensuing debate surrounding their results.[20] While Kripke has notoriously appealed to his own intuitions and to what he deemed as natural (and presumably generalizable) intuitions about the nature of some concepts or expressions, he and the overwhelming majority of philosophers of language continued to favour mental experiments in their work. The field of experimental (philosophical) semantics is still in a pioneering stage; however, the results of Machery et al. have led to an increase in philosophers' appreciation and interest concerning the significance of experimental results.

[18] Cf. Kripke (1980: 48-49).
[19] Devitt (2011: 422-423).
[20] Machery et al. (2004).

The impetus for Machery et al.'s research was given by Nisbett et al.'s experimental work in cultural psychology.[21] Nisbett et al. found significant differences between Westerners' and East Asians' basic cognitive processes. The main conclusion of the research was that while Westerners tend to be more analytic, holistic thought is dominant in East Asian cultures.[22] The results of Nisbett et al. had already occasioned Weinberg et al.'s similar findings in epistemology (e.g., an experimental treatment of the famous Gettier cases).[23] Machery et al. started from the (experimentally established) fact that East Asians are typically inclined to make judgments on the basis of similarity, while Westerners favour causal connections. This led them to posit the working hypothesis that East Asians should be more prone to a descriptivist outlook on names, while Westerners would keep to a causal-historical pattern, in the same vein as Kripke.

Machery et al. devised an experiment that was based largely on Kripke's mental experiments from *Naming and Necessity*. The subjects were undergraduate students from Rutgers University in New Jersey and Hong Kong University. The students from Hong Kong were Chinese, but fluent in English, which was the language in which probes were administered in both locations of the experiment. Participants were confronted with different probes, with the following one being notably modeled on Kripke's Gödel-Schmidt story:

> 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?[24]

[21] Nisbett et al. (2001).
[22] See also Nisbett (2003).
[23] Weinberg et al. (2001).
[24] Machery et al. (2004: B6).

Another vignette used a similar story, but with the name of a Chinese astronomer (Tsu Chung Ch'ih) instead of Gödel. The results confirmed Machery et al.'s hypothesis. About 60% of Westerners had causal-historical intuitions in relation to the probes, compared to only about 30% of Hong Kong students; about 40% of Rutgers students and 70% of Hong Kong undergraduates reported descriptivist intuitions about the case.

The conclusion Machery et al. drew from the experiments was that philosophers of language cannot assume that their semantic intuitions are universal. The results of the experiments were naturally interpreted as showing that there is significant cultural variation in semantic intuitions. The authors suggested that the variation might be explained by different dominant patterns in the thought of Westerners and East Asians that might have led each group to appraise differently the cases.[25] As one can clearly see from the short description of the experiments given here, variation exists not only between different cultures; it is also an intra-cultural phenomenon. While there is a dominant tendency inside each group (causal-historical for Westerners and descriptivist for East Asians), it is not at all manifested by an overwhelming majority of users, so as to allow us to posit that there is one correct understanding (and therefore a correct theory or semantics) of how people from a cultural group use names. As far as we know, differences might even exist at the individual level, so that the same individual reports descriptivist intuitions in some cases and causal-historical intuitions in other cases.[26]

B. The objections and some experimental and non-experimental answers

Machery et al.'s results were met with considerable interest. Naturally, the paper received some critical replies. The criticism of the first paper and of subsequent ones authored by some or all of the members of the research group can be broadly construed as *methodological* or *philosophical*. This is not an either-or distinction: some of the papers written in response included both methodological and philosophical rejoinders. We will now review some of the foremost replies to Machery et al.'s work, the subsequent replies from the authors and the debate they engendered.

B1. Methodological replies

i. Linguistic and metalinguistic intuitions

Marti argues that Machery et al.'s research does not appeal to the right kind of intuitions, i.e. intuitions that would allow one to tell if the subjects use the

[25] Cf. Machery et al. (2004: B8-B9).
[26] Machery et al. (2004: B8).

name descriptively or as a directly referential term.[27] She argues that there is a difference between metalinguistic intuitions and the way we actually use terms (and our intuitions about that use). According to Marti, Machery et al.'s experiments test the subjects' preferred theory of reference instead of testing (intuitions about) the use of names, and these are two different things. While eliciting metalinguistic intuitions may be the right way to go in some cases (Marti proposes the example of syntacticians testing people's intuitions about grammaticality), it is not appropriate for the issue of reference. Marti argues that we should be interested in reference itself, not users' preferred theory of reference. So, the experiment may prove that East Asians are more inclined towards a descriptive theory of reference, but not that East Asians use names descriptively.[28] Marti proposes a different kind of question at the end of the Gödel-Schmidt experiment:

> One day, the fraud is exposed, and John exclaims: 'Today is a sad day: we have found out that Gödel was a thief and a liar'.
> What do you think about John's reaction?[29]

Machery has answered Marti's challenge in a paper published in 2011.[30] He puts forth several arguments against Marti's claims:

1. Metalinguistic intuitions provide evidence about use. Intuitions about synonymy or antinomy (e. g. "reply" and "response" are synonymous) are useful in determining or discussing the meaning of terms. The metalinguistic intuitions elicited by syntacticians provide information about grammaticality. The same should apply to metalinguistic intuitions about meaning.[31]

2. Metalinguistic intuitions are reliable. Machery considers two possible explanations of the relation between theory and the way we use names. One is that a (possibly implicit) theory about proper names underpins use. This theory must be correct, since we use proper names correctly. The other explanation is that use of names is a skill that doesn't presuppose an accompanying theory (Devitt's point of view). This last view should be more problematic, but Machery argues that a theory *must result* from reflection on the way we use proper names, and this theory would be naturally adjusted in practice, therefore leading to reliable metalinguistic intuitions. Even if the theory is incorrect, it may still lead to some reliable intuitions.[32]

[27] Marti (2009).
[28] Marti (2009: 44-46).
[29] Marti (2009: 47).
[30] Machery (2011).
[31] Machery (2011: 122-124).
[32] Machery (2011: 124-125).

3. Linguistic intuitions don't seem to provide better evidence than metalinguistic ones. The underlying idea behind Marti's criticism of Machery et al.'s experiments was that linguistic and metalinguistic intuitions may be divergent. In response, Machery and some of his collaborators devised a pair of experiments they called "the linguistic Tsu Chung Ch'ih case" and the "metalinguistic Tsu Chung Ch'ih case." The linguistic Tsu Chung Ch'ih case is a variation on the first Tsu Chung Ch'ih experiment (which is itself, as already pointed out, a culturally sensitive variation of the Gödel-Schmidt case). There is a protagonist who knows only one thing about Tsu Chung Ch'ih (the famous-deed description), but Tsu Chung Ch'ih had actually (unbeknownst to our protagonist and other people) stolen the discovery from another scientist. The only difference is that instead of questioning who the protagonist of the story refers to by the name 'Tsu Chung Ch'ih', the final question of the probe is the following:

> Is the statement "Tsu Chung Ch'ih is a great astronomer" true or false?

The metalinguistic case is a version of the initial Tsu Chung Ch'ih vignette with a more explicit final question. Machery and his collaborators tested subjects from India, Mongolia and France. The answers given in the three settings were fairly similar: the percentages of subjects from each country that reported causal-historical intuitions were very close (from 60% to about two-thirds of the subjects opted for the causal answer, while the rest chose the descriptivist one). More importantly, the linguistic and metalinguistic vignette received the same type of response. The proportion of causal-historical answers was slightly higher in the linguistic case, but there was no significant difference in answers between the two framings of the Tsu Chung Ch'ih story. This was interpreted by Machery as a clear indication of the convergence of linguistic and metalinguistic intuitions.[33, 34]

ii. Speaker's reference and semantic reference

Ludwig and Deutsch have both argued that the final question of the original vignette does not distinguish between speaker's reference and semantic reference.[35] It is not clear if the question asks what John's token of "Gödel" refers to or what the semantic reference of the name "Gödel" should be. To take an example, Jane may see a look-alike of Cher on television and say, without knowing that it is not really Cher she is seeing, "Cher has a beautiful necklace." Now, the referent of the token of "Cher" used by Jane is the look-alike, but the semantic

[33] Machery (2011: 125-128).
[34] The experiments that addressed Marti's objection are documented in Machery et al. (2009).
[35] Ludwig (2007); Deutsch (2009).

reference of the name (the name as used in Jane's linguistic community) is Cher, the singer and actress. For Ludwig and Deutsch, Machery et al.'s probe fails to take this distinction into account.

Machery has replied that the question is clearly about the semantic reference of the name "Gödel", as subjects are asked what John is talking about when he uses the name (as opposed to what John intends to refer to). No contextual information about the speaker's intentions is given.[36] Another point made by Machery is that there is an *ad hoc* assumption underlying the arguments of Deutsch and Ludwig. That is to say, the gist of their argumentation is that there is no cross-cultural variation in such semantic intuitions. People have uniform intuitions about reference. So, if these intuitions are, say, causal-historical, the critics account for the difference between Westerners and East Asians by assuming that the former understand the question as bearing upon semantic reference, while the latter understand it as being about speaker's reference. But such an assumption is unwarranted, Machery argues.[37]

Finally, Machery and a new team of researchers, including Max Deutsch, added a clause asking subjects to ignore the intentions of the speaker in the probe and conducted a new series of experiments with similar results, therefore concluding that inter- and intracultural variations in intuitions about reference are genuine.[38, 39]

B2. Philosophical replies

iii. The Expertise Defense

Ludwig and Devitt have advanced another line of argument against the significance of Machery et al.'s findings.[40] The two philosophers argue that even if we accept that intuitions about reference are variable, the intuitions of lay people are irrelevant, or at least not as relevant as experts' intuitions for establishing what the correct theory of reference is.

[36] Machery (2011: 128-129).
[37] Machery (2011: 129-130).
[38] Machery (2011: 130-131).
[39] Sytsma and Livengood (2011) raise a similar concern about an epistemic ambiguity in the original probe. That is, according to Sytsma and Livengood, the question does not specify if the subjects should adopt the narrator's epistemic perspective (as expected by Machery et al.) or John's. The epistemic asymmetry between the two should lead to different answers from the subjects, but not for the right reasons. Experiments reported on by Sytsma et al. (2014) show that the variation in intuitions is preserved even after removing the epistemic ambiguity.
[40] Ludwig (2007); Devitt (2011).

Machery has levelled several arguments against what he calls 'the Expertise Defense'. First, it is not clear that experts' theories about reference are better than the views of lay people. All our ideas about reference, expert or not, may be wrong, much like ancient views about the origins of diseases. Disagreement between philosophers over the right theory of reference is another problem for asserting confidence in the superiority of experts' intuitions.[41]

Second, experiments have shown that expertise does not improve intuitions about language. If anything, experts may be biased by the theories they hold. This is what another experiment from Machery and his collaborators seems to have found. The Tsu Ch'ung Chih case has been presented to various categories of experts in language: semanticists, philosophers of language, historical linguists, sociolinguists, terminologists, etc. The findings show that expertise has an inconsistent influence on intuitions about the reference of names. The intuitions of experts (philosophers of language and semanticists) that are presumably familiarized with Kripke's view are different from the intuitions of linguists (e.g., sociolinguists, historical linguists, anthropological linguists) that are sensitive to the informational load of words. Furthermore, philosophers of language and semanticists are more likely than comparatively educated lay people to endorse Kripkean views, while sociolinguists, historical linguists, and anthropological linguists are more likely than comparatively educated people to have descriptivist intuitions (the differences do not reach significance, however). Machery interprets the results as a refutation of the Expertise Defense: experts' intuitions are not more reliable than lay people's, which means that we should take experimental results about variations in semantic intuitions seriously.[42]

Devitt has responded to Machery's arguments from the paper just cited and from another one Machery co-authored with Stephen Stich.[43] The case Devitt builds is very complex and nuanced. He argues again in favour of the Expertise Defense and for a different methodology for testing theories of reference, which takes into account different kinds of evidence (including the experts' and the laymen's intuitions), but focuses on testing usage rather than metalinguistic intuitions. An interesting example of the advances that looking into usage might bring is that, as Marti has also noticed, Machery et al.'s initial probe is flawed, that is, the name "Gödel" is used in a causal-historical manner, thus possibly favouring one type of response.[44] If the semantic content of "Gödel" is exhausted by the

[41] Machery (2012a: 37-38).
[42] Machery (2012a: 44-50).
[43] Devitt (2012); Machery & Stich (2012)
[44] Marti (2009: 46).

description "the discoverer of the Incompleteness theorems", then a descriptivist cannot make sense of the change in view that the uncovering of the authorship theft should bring. I cannot give a detailed account of Devitt's reply here, but I will touch important parts of his answer in the discussion from the following chapter.

iv. On the role of Ignorance and Error

More recently, Flanagan has proposed a criticism of the Ignorance and Error arguments, and, consequently, of Machery et al.'s initial probe and all subsequent experiments. Flanagan's criticism is two-fold:

(1). *Against ignorance.*[45] Kripke holds that people can identify the referent of some name without knowing any unique property of the name's bearer. Flanagan accepts Kripke's point but maintains that name users have to associate some property with a name on pain of not using that name properly. For instance, we expect people who talk about Einstein to know at least that he was a scientist, even though they may not know anything about his particular achievements; if someone describes Einstein as 'a famous conductor', then we should not count this as an actual use of the name. To see what Flanagan has in mind, think about the fact that no name is in principle unique. Even if a causal-historical theory is true, we need some background information to know who/what we are talking about, what chain of reference we aim to enter. When someone describes Einstein as "a famous conductor", it should not count as a use of the physicist's name, as it doesn't allow us to discriminate correctly between bearers of the "Einstein" name.

Flanagan argues that the division of linguistic labour allows name users to identify referents without knowing any unique property thereof. The idea is analogous to Putnam's explanation of the division of linguistic labour concerning natural kinds. Lay people know some interesting fact about the referent that allows them to characterize "the relevant expertise" (e.g., Cicero was a famous roman orator, Feynman was a physicist). Then, experts possess the relevant knowledge; they attribute unique achievements to the name bearers and more ignorant users defer to these attributions. If someone is completely ignorant about, say, Feynman's achievements, then according to Flanagan, this does not count as a use of Feynman's name.

(2). *Against Error.*[46] Flanagan proposes a subtle argument against Kripke's claim that name users (even the most competent ones) may be mistaken in every property they attribute to the referent of a name and still use that name properly.

[45] Flanagan (2014: 88-90).
[46] Flanagan (2014: 90-93).

As we have seen, the Gödel-Schmidt story is a part of this argument. Flanagan argues that attribution of an achievement is based on some kind of evidence of that achievement. We believe that Gödel proved the incompleteness of arithmetic because we have learned it from some book. So, even as we find out that someone else proved incompleteness, we are still left with a description of Gödel: "the man who was wrongly credited (by …) with having proven the incompleteness of arithmetic." Similarly, for the case of Jonah we have the description "the subject of the Book of Jonah from the Bible." If we come to believe that Jonah was a purely mythical character, we will not say "Jonah never lived in a whale's belly", but rather "Jonah was not a real person". Returning to Gödel's case, the decision on how to treat Gödel in the hypothetical case where we find out that Schmidt has actually proven incompleteness depends on how much information is available about Gödel. If this achievement is all we can attribute to Gödel, then we may simply apply the moral of the Jonah story in an adjusted variant and declare that "Gödel is Schmidt." If we have substantial independent information about Gödel (such as that he showed that the continuum hypothesis cannot be disproved from the axioms of set theory), then we may conclude that "Gödel was a fraud."

To sum up, any knowledge of an achievement is doubled by knowledge of the attribution of that achievement. So, even if we have reasons to doubt that the bearer of a name was indeed the author of some achievement, we still have knowledge of another property of that name's referent, that is, *being attributed that certain achievement by a certain source*. Flanagan concludes that Machery et al.'s experiments (and, for that matter, any experiment stemming from Kripke's story) should make the evidential status of the property clear.

v. The importance of the Gödel-Schmidt story

Devitt and Ichikawa et al. have argued at length that Machery et al.'s experiments fail to challenge Kripke's argument against descriptivism and, on a larger scale, his view on reference.[47] They point out that the Gödel-Schmidt story plays a small role in Kripke's refutation, which is based rather on considering actual cases of error. According to Devitt, modal intuitions of a metaphysical nature are also very important for Kripke's case.

Machery et al. have replied to these critiques in a paper that is devoted to specifying their stance.[48] They argue that the point of their experiments was not to support or refute one theory of reference or another, but rather to challenge

[47] Devitt (2011); Ichikawa et al. (2012).
[48] Machery et al. (2013).

the method used by Kripke and other philosophers of language, that is, appealing to "the intuitions of competent speakers about the reference of proper names (or other kinds of words) in actual and possible cases"[49], i. e. *the method of cases*. Variation in referential intuitions among different cultures and inside linguistic communities shows, according to Machery et al., that the method of cases is inappropriate.

4. Discussion

In what follows I will explore the significance of Machery et al.'s experiments and the significant arguments and distinctions that the experiments helped engender. The aim of the discussion is to cut across some of these arguments and distinctions by starting from some general points about reference that are situated at the intersection of epistemology and philosophy of language.

At the end of their somewhat apologetic paper, the four authors of the Machery research group examine the alternatives that the results of their experiments leave for the reference theorist.[50] These are: (a) giving up theories of reference and arguments from reference altogether; (b) endorsing a certain theory of reference on the basis of some metaphysical commitment (and ignoring the main conclusion of the experiments regarding the variation in intuitions about reference); (c) endorsing referential pluralism, but finding a way to accommodate the fact that significant variation in intuitions cross cuts significant criteria like language, culture or profession, which is a hard thing to do.[51] In the absence of a way of knowing which intuition group a person adheres to, referential pluralism leads – according to Mallon et al. – to "the absurd view that we do not know when people agree, when they disagree and when they speak at cross-purposes."[52] The presupposition that underpins this conclusion is, of course, that each intuition group (descriptivist or causal) has its own type of reference relation (best, but not faultlessly, described by their intuitional judgments).

It has been suggested that referential pluralism is not such an unwanted alternative as Mallon et al. may hold. Andow has proposed that the pluralist take an assessor relative view, as opposed to a speaker relative one.[53] In this sense, we

[49] Machery et al. (2013: 620).
[50] Mallon et al. (2009).
[51] Mallon et al. (2009: 352).
[52] Mallon et al. (2009: 352).
[53] Andow (2014).

can hold that people agree or disagree even if we don't know what type of reference they subscribe to: the context of assessment is the one that matters in establishing what proposition they express, and not the speakers' context. Ichikawa et al. also argue that (partially) successful communication is possible in the conditions of different semantic content (it is a rather trivial fact that actual referents are the same in most cases for both causalists and descriptivists).[54] Finally, Genone and Lombrozo have used a similar strategy to Machery et al.'s in experiments concerning natural and nominal kind terms, and the results exhibited a similar variation in intuitions. Genone and Lombrozo have found that the same individuals often apply mixed criteria, that is, sometimes they rely on causal information and sometimes on descriptive information, which suggests that a hybrid theory is more appropriate for explaining reference than a purely descriptive or a purely causal one.[55]

Now, these points lead us to some more important insights. Genone and Lombrozo's moral may be easily generalized, which is to say that we may find the explanation for the variation in referential intuitions less in cultural particularities, and more in the unstable nature of our referential practices. Remember Flanagan's points against the argument of ignorance and error: famous deeds descriptions are doubled by descriptions of the attribution of that property by some source, and we need to determine some field of expertise for a certain name so that we may use that name properly. The first idea is mentioned explicitly in Machery et al.'s discussions of descriptivism: deferential descriptivism is still not refuted.[56] These considerations rely on the fact that linguistic expressions such as names may be used to refer to multiple objects, that is to say, they don't have any type of reference built in. One answer to the predicaments of theorizing reference would be to follow the view of Chomsky that the expressions of language don't refer to anything at all independently.[57] Two general points underwrite Chomsky's idea: (a) reference is a technical notion that doesn't characterize language, at least not in an irreplaceable manner; (b) reference must be at least a tetradic relation, that takes place between a language user, an object, a name (or some other expression) and a context; names don't refer to anything by themselves, without a certain user and a specified context of use. Naturally, if we adopted a view similar to Chomsky's, we would give up the search for an adequate theory of reference as, for obvious reasons, we would deem it futile. The documented variation in referential intuitions may be interpreted then as an argument for this conclusion. Devitt believes this is

[54] Ichikawa et al. (2012).
[55] Cf. Genone & Lombrozo (2012: 734).
[56] Machery et al. (2013: 625).
[57] Chomsky (1995).

one of the main tenets that may be taken out of Machery et al.'s work, and has reacted critically thereto, but Machery et al. refused to endorse this view. They maintain that their aim is to question the viability of the method of cases as a tool for establishing or supporting theories of reference, and not to reject theories of reference altogether.[58]

Be that as it may, the admitted lack of an adequate explanation of the variation in referential intuitions appears to make such a radical conclusion at least worthy of attention. As discussed above, Machery et al. admit that language and culture do not provide us with a criterion for marking off and explaining reference, because groups of intuition crosscut language and culture. Differences in epistemological features between communities may give us some clues about the distribution of referential intuitions inside them, but they are not strong enough to form the basis of a full-fledged theory. Moreover, if Genone and Lombrozo's interpretation of their results is correct, it seems that even at the personal level, there is an inescapable variation in determining reference. This seems to support Chomsky's tenet that the personal and contextual components cannot be factored out of reference. Yet, one is not forced to treat reference strictly as a phenomenon of pragmatics – *the experimental results do not show that a semantic component of reference may not be delimited and characterized, even though such an explanation will not be able to account for the entirety of our referential practices*. It is clear, however, that an extension of Genone and Lombrozo's findings to proper names[59] will make the distinction between speaker's reference and semantic reference hard to determine. If language users do not typically favour either causal or descriptivist uses of names or natural kind terms, then there is no *correct* type of use of such an expression in a language (semantic pluralism). A radical interpretation of these results is that the uniqueness and determinability of semantic reference should be disputed, even if we agree that we succeed in referring and reference has a semantic component. Future research, more or less experimental in nature, should address issues related to this matter. Can we specify a form of semantic reference, regardless of the experimental findings or interpreting them in such a way that semantic reference is not affected? If there is no semantic reference of a word to speak of, how can we account for something more than speaker's reference? How do we achieve collective reference, as it seems that something like this can/must exist? Specifically, how does the interplay between causal and descriptive reference

[58] Machery et al. (2013: 620).

[59] There are salient reasons to think that the case of natural kind terms is different from the case of proper names. See Jackson (2010), for an account that contrasts the two types of expressions.

work, both at a personal and an intersubjective level? How do we tune in to our companions' expectations and which type of use normally takes precedence (and in what contexts)? How does theoretical bias affect our referential practices?

One theoretical distinction that has been discussed in relation to experimental work in philosophical semantics is the one between *descriptive* and *normative* semantic theories. I would parse it as less of a distinction between types of semantic theories, and more of a distinction between different aspects of a semantic theory. Jackman mounts an expertise defense that is based on a distinction between what he calls 'type-2' intuitions and theories and 'type-3' intuitions and theories.[60] A type-2 theory is about *what* our terms refer to, and corresponding type-2 intuitions are therefore semantic ones, in a classic sense (i.e., intuitions about the extensions of particular terms, such as "Gödel" or "Jonah"). A type-3 theory explains, according to Jackman, *why* our terms refer to what they refer to. One could think that type-3 intuitions, that is, meta-semantic intuitions about the nature and role of our terms, would be the most widely used in a type-3 theory, but this is not the case. Even if the main concern is a meta-semantic one, the type of intuitions used in type-3 theorizing and discussions (as is the case of Machery et al.'s experiments and debate) is semantic (type-2), as they are about actual linguistic practice, and thus more reliable and less vulnerable to theoretical bias. That is why I prefer to talk about these distinctions as separating various aspects of semantic theories, and not different classes of semantic theories. Pretty much all important theories in the philosophy of language are in a large part meta-semantic (type-3), but they are supported by semantic (type-2) intuitions (and some inescapable semantic theorizing). This fact illuminates, for instance, why Genone takes *descriptive* theories to be theories that determine "how it is that referring terms pick out their referents" or "why they refer to the particular things that they do", and at the same time separates them from *normative* theories, that determine "how reference should work as a maximally systematic and rational phenomenon".[61] If we take this to be a robust distinction in the field of the philosophy of language, it should make Jackman wrong, but to be sure, this is not the case. There are, however, some supplementary clarifications to be made. A meta-semantic account should be composed of both descriptive and explanatory parts. The philosopher of language both describes how certain expressions work and explains the reason why they work that way. Take, for instance, Frank Jackson's account of proper names. Jackson argues that the Kripkean picture of

[60] Jackman (2009: 164). Obviously, there are also type-1 intuitions and theories, but their characteristics are not important for this discussion.

[61] Genone (2012: 161).

our use of names is correct – names are rigid designators and their meaning is not given by a definite description or a cluster of descriptions. Proper names work this way because, for one, they are marks that allow us to distinguish between otherwise indistinguishable things (streets, towns, persons, etc.), i.e. things whose identifying properties we do not know. Also, names figure in counterfactual hypotheses where many of our beliefs about the bearers of the names are "thrown away".[62]

Jackson's account is a model of how a semantic theory is put forward. Not only in his account, the "because" is strongly connected with an "ought", that is, with more or less explicit norms for the use of certain expressions, which are derived from the understanding and explanation of their role in language. Names work in this or that way, because they are this or that, therefore we should drop possible usage that doesn't conform to this construal.[63]

Thus, what happens is that the normative aspect becomes, this time in Jackman's terms, *constructive*, so that it not only explains, but also builds up and circumscribes a new usage, based on the systematization of our semantic and meta-semantic intuitions in order to maximize consistency.[64] Jackman proposes a theory that acknowledges variations in semantic intuitions (so, he accepts both causal and descriptive answers), but attempts to unify the responses at the meta-semantic level, where the differences can be explained away and one type of intuition can be favoured in order to maximize truth. At least for Westerners, Jackman argues that the causal view should be privileged meta-semantically, because it conforms to our dominant causal cognitive habits and it allows us to avoid failure of reference in the Jonah cases, but other theoretical answers can be put forward following the same pattern. Philosophers' interest for meta-semantically driven unification is what supports the expertise defense, according to Jackman, as philosophers of language are "the only ones actively concerned with the sort of consistency-driven construction that conceptual analysis involves".[65]

Logic itself may be regarded as a highly normative, and to a certain extent constructive, form of theorizing. Possible worlds semantics, for instance, have greatly influenced our understanding of modal notions. Not only that, but as modal notions became easier to handle formally, similar treatments were proposed and accepted on a large scale for other important notions, such as knowledge or moral obligation. Fundamental notions in the philosophy of language, such as the

[62] Jackson (2010: 136-139).
[63] See Jackson's view that people who don't treat names as rigid designators are plain wrong – Jackson (2010: 137).
[64] Cf. Jackman (2009: 166).
[65] Jackman (2009, p. 170).

concept of *proposition*, received modal definitions. As we have seen, the modal aspect is very important in the discussion surrounding the theory of reference, and this is due both to Kripke's logical work and to his philosophical contribution. Although they are not interdependent, the logical and the philosophical modal developments supported each other at least at a certain moment in the history of the disciplines. For one, it is not far off the truth to say that if modal notions hadn't become palatable again (as a result of the work of Kripke, Hintikka, Prior, and other important logicians of the mid-twentieth century), Kripke's modal (or modality-using) arguments in the philosophy of language would probably have been first regarded with less trust or interest.

The need for a separation between the various aspects of theories in the philosophy of language has been better construed and evinced following Machery et al.'s contributions, but this concern is older. Føllesdal, for instance, argues that rigidity is not guaranteed by the introduction of a singular term (the initial baptism) and its diffusion across the linguistic community. In other words, there may be a *change of reference* at a certain step of the causal chain. One speaker may misapprehend a certain term, although his intentions are the right ones, and the new usage may spread to the entire community.[66] Føllesdal sees rigidity as akin to Kant's *regulative ideas*, a prescriptive ideal for the use of singular terms like names.[67] But, as his simple example shows, although names should work as the causal theory prescribes, this doesn't happen always (and anywhere). What the original and the derived experimental results show is that the exceptions to the norm are plentiful, and moreover it seems that the alternative usage may be itself proposed as a norm, if it really correlates better with a certain epistemological perspective on the world.

5. Concluding remarks

Føllesdal's example may also help us understand a certain limit of purely experimental work regarding concepts in the philosophy of language. What is often ignored or, at least, not sufficiently discussed in theories, arguments or debates on

[66] There are situations where such a change of reference has more chances of occurring, that is, situations where the body of evidence concerning a certain bearer of name is slim, e.g., remote areas, mountains, or rivers; people in the past who don't have significant achievements to their name, or cases where authorship of some famous deeds is difficult to determine; things that have been recently baptized, and so on. This may be interpreted as showing that the information (codified in descriptions) we associate with a certain name is at least very important in placing users in the correct chain of reference.

[67] Føllesdal (1998: 111-112).

the subject matter is that names – as well as many other symbolic "tools" of human languages – are part of very complex dynamics and transactions in human communities. Hintikka & Sandu cite the case of a name-selling practice in Japanese history that seems to speak against necessary identity statements and the Kripkean stance on proper names.[68] Names figure in various types of transactions (not only linguistic, but also economic and cultural) and are an important part of various types of practices (not only linguistic, but also religious, juridical, political, etc.). Consequently, there may be important variations in the use – and thus, in the role – of names that are a result of their being part of such diverse practices as the ones mentioned. Their semantic role as referential marks may be construed as their primary/normative behaviour, but it is not to be regarded as a strict limit. The possibility that names are "contaminated" by information transmitted through the information channels they are part of, and thus become tied to some sort of descriptive content, is also an important one to consider. But this complex interplay of factors that bear upon the role and nature of names inside a language or cultural community is hard (if not outright impossible) to pin down in an artificial setting. Machery et al.'s experiments show us important limits of the method of cases, expert intuitions, and – some would want it, at least – the causal theory of reference. Nevertheless, if our understanding of what a semantic theory should be like is roughly correct, there are fundamental aspects that seem difficult to capture even in an ingenious experiment. As I have just remarked, names and naming have an inherent intersubjective dimension, which is hard to capture in settings similar to Machery et al.'s which focus on a single agent's perspective and cognitive status. Not only this, but there is an important temporal (or perhaps historical) dimension, which links to other spatial, evidential or cultural factors, and together they determine how certain expressions are learned and used, and what type of information matters or how the different patterns (causal and descriptive) mix.[69] These aspects should be very difficult to characterize in experimental designs, no matter how complex. Genone and Lombrozo have proposed some guidelines for testing how our intuitions about names change in light of new information which corrects previous beliefs.[70] Devitt similarly promotes the application of the *technique of elicited production* to the case of reference, the advantage being that instead of metalinguistic intuitions, it induces actual usage of terms.[71] However

[68] Hintikka & Sandu (1995: 273).

[69] It seems to me that distance, both temporal and spatial, from an object is very important for the way we refer to it and for the linguistic exchanges wherein the name(s) of the object is/are contained.

[70] Genone & Lombrozo (2012: 736).

[71] Devitt (2012: 29-30).

successful these experimental methods may be, they can hardly substitute observation. As Devitt argues, "[w]e should add to this informal practice the systematic and scientific study of the corpus for evidence."[72] There still seems to be very little work on the issues discussed here that is based on observation and recording, outside experimental settings. Experimental work appears to be able only to provide a rough outline or a limited answer to some issues in the philosophy of language. Future empirical studies based on observation and discussions thereof that complete the experimental findings seem to be the next step forward.

Devitt proposes a quasi-hierarchical list of methods to be further used in the theory of reference. It includes, in this order: philosophers' intuitions, observation, elicited production, and on "a relatively small place", testing folk intuitions.[73] I agree that all these methods are worthy of attention, but I have reservations regarding the relative importance Devitt assigns to these methods.

To end this discussion, I will make a small point regarding one of the analogies Devitt makes in support of the Expertise Defense. Devitt argues that a better background related to the subject matter makes the intuitions of the philosophers of language, even affected by bias, more reliable than the laymen's. He draws an analogy with sports where the calls of a biased professional umpire should be preferred to the calls of an equally biased fan. There are two issues that Devitt seems to ignore here. One is that theoretical bias may be more dangerous than the sources of unreliability of folk intuitions; their ignorance of the theory may make lay people more open-minded, and their answers more genuine, even if flawed. Second, as sports fans should know, the bias of a professional umpire is much harder to detect and prove than the fan's bias. The latter is much more blatant, which makes it less efficient.[74]

A direction of research philosophers of language should look into is determining how much the normative aspect of our activity changes the way we look at language and – why not? – the way we use language. The possibility that experts use different idiolects than lay people is acknowledged by both camps of the debate, but it could be interpreted either way: laymen's intuitions are preferable because they are about the idiolect spoken by the largest part of the community, or experts' intuitions should be preferred because their idiolect is better (that is, more reliable). In the third chapter of this paper, I mentioned a reply by Machery concerning the viability of people's metalinguistic intuitions. More precisely, Machery

[72] Devitt (2012: 31-32).

[73] Devitt (2012: 31-32).

[74] See also Machery and Stich (2012), and Machery (2012b), where it is argued that laymen's intuitions should be preferred.

was arguing that our use of language must be accompanied by a theory, however imperfect: either usage is already underpinned by a theory (namely, the theory is already present in the use of language), or the theory arises from usage. Now, this is a rather controversial point. What can be said is that a more in-depth look at the profile of each of the subjects in the test group may prove interesting. Even if they may be equally or similarly competent with language, have similar intellectual skills, etc., some of them may have developed (or borrowed) some form of semantic theory, while others may have not. Or, simply, some could be more reflexive or thoughtful than others in their responses. All these aspects deserve more attention.

BIBLIOGRAPHY

Andow, J. 2014. Intuitions, Disagreement and Referential Pluralism. *Review of Philosophy and Psychology* 5: 223-239.

Chomsky, N. 1995. Language and Nature. *Mind* 104: 1-61.

Deutsch, M. 2009. Experimental Philosophy and the Theory of Reference. *Mind & Language* 24(4): 445-466.

Devitt, M. 2011. Experimental Semantics. *Philosophy and Phenomenological Research* 82(2): 418-435.

Devitt, M. 2012. Whither Experimental Semantics?. *Theoria* 73: 5-36.

Devitt, M. & Sterelny, K. 1999. *Language and Reality. An Introduction to the Philosophy of Language. Second edition.* Oxford: Blackwell.

Flanagan, B. 2014. Rereading the Kripkean Intuition on Reference. *Metaphilosophy* 45(1): 87-95.

Føllesdal, D. 1998. Essentialism and Reference. Expanded edition. In: L. E. Hahn & P. A. Schilpp, eds. *The Philosophy of W. V. Quine.* Chicago and La Salle, IL: Open Court, 97-113.

Genone, J. 2012. Theories of Reference and Experimental Philosophy. *Philosophy Compass* 7(2): 152-163.

Genone, J. & Lombrozo, T. 2012. Concept possession, experimental semantics, and hybrid theories of reference. *Philosophical Psychology* 25(5): 717-742.

Gutting, G. 2009. *What Philosophers Know. Case Studies in Recent Analytic Philosophy.* Cambridge: Cambridge University Press.

Hintikka, J. & Sandu, G. 1995. The Fallacies of the New Theory of Reference. *Synthese* 104(2): 245-283.

Ichikawa, J., Maitra, I. & Weatherson, B. 2012. In Defense of a Kripkean Dogma. *Philosophy and Phenomenological Research* 85(1): 56-68.

Jackman, H. 2009. Semantic intuitions, conceptual analysis, and cross-cultural variation. *Philosophical Studies* 146: 159-177.

Jackson, F. 2010. *Language, Names, and Information.* Malden, MA: Wiley-Blackwell.
Kripke, S. A. 1980. *Naming and Necessity.* Cambridge, MA: Harvard University Press.
Ludwig, K. 2007. The Epistemology of Thought Experiments: First Person versus Third Person Approaches. *Midwest Studies in Philosophy* 31: 128-159.
Machery, E. 2011. Variation in Intuitions about Reference and Ontological Disagreements. In: S. D. Hales, ed. *A Companion to Relativism.* Oxford: Wiley-Blackwell, 118-136.
Machery, E. 2012a. Expertise and Intuitions about Reference. *Theoria* 73: 37-54.
Machery, E. 2012b. Semantic Epistemology. A Brief Response to Devitt. *Theoria* 74: 223-227.
Machery, E., Mallon, R., Nichols, S. & Stich, S. P. 2004. Semantics, Cross-Cultural Style. *Cognition* 92: B1-B12.
Machery, E., Mallon, R., Nichols, S. & Stich, S. P. 2013. If Folk Intuitions Vary, Then What?. *Philosophy and Phenomenological Research.* 86(3): 618-635.
Machery, E., Olivola, C. & De Blanc, M. 2009. Linguistic and Metalinguistic Intuitions in the Philosophy of Language. *Analysis* 69: 689-694.
Machery, E. & Stich, S. 2012. The Role of Experiments in the Philosophy of Language. In: G. Russell & F. D. Graff, eds. *The Routledge Companion to the Philosophy of Language.* New York: Routledge, 495-512.
Mallon, R., Machery, E., Nichols, S. & Stich, S. 2009. Against Arguments from Reference. *Philosophy and Phenomenological Research* 79(2): 332-356.
Marti, G. 2009. Against Semantic Multi-culturalism. *Analysis* 69: 42-48.
Nisbett, R. E. 2003. *The Geography of Thought. How Asians and Westerners Think Differently and Why.* London: Nicholas Brealey Publishing.
Nisbett, R. E., Peng, K., Choi, I. & Norenzayan, A. 2001. Culture and Systems of Thought: Holistic vs. Analytic Cognition. *Psychological Review* 108: 291-310.
Sytsma, J. & Livengood, J. 2011. A New Perspective Concerning Experiments on Semantic Intuitions. *Australasian Journal of Philosophy* 89(2): 315-332.
Sytsma, J., Livengood, J., Sato, R. & Oguchi, M. 2014. Reference in the Land of the Rising Sun: A Cross-cultural Study on the Reference of Proper Names. *Review of Philosophy and Psychology* 6(2): 213-230.
Weinberg, J., Nichols, S. & Stich, S. 2001. Normativity and Epistemic Intuitions. *Philosophical Topics* 1&2: 429-459.