

If Folk Intuitions Vary, Then What?

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We have recently presented evidence for cross-cultural variation in semantic intuitions and explored the implications of such variation for philosophical arguments that appeal to some theory of reference as a premise. Devitt (2011) and Ichikawa and colleagues (forthcoming) offer critical discussions of the experiment and the conclusions that can be drawn from it. In this response, we reiterate and clarify what we are really arguing for, and we show that most of Devitt's and Ichikawa and colleagues' criticisms fail to address our concerns.

In a pair of recent articles, we have presented evidence for cross-cultural variation in semantic intuitions (Machery, Mallon, Nichols, & Stich, 2004) and explored the implications of such variation for philosophical arguments that appeal to some theory of reference as a premise—a class of arguments we called “arguments from reference” (Mallon, Machery, Nichols, & Stich, 2009). Michael Devitt (2011) and Jonathan Ichikawa, Ishani Maitra, and Brian Weatherson (forthcoming) offer critical discussions of the experiment and the conclusions that can be drawn from it. Ichikawa and colleagues go on to suggest that variation in how words refer would not undermine arguments from reference (forthcoming, 10 ff). While we are grateful for the critical

attention that these authors have devoted to our work, we believe that they have quite seriously misunderstood our critical aim in Machery et al. (2004) and Mallon et al. (2009), perhaps because of one or two insufficiently careful sentences in these two articles. As a result, many of their arguments and just about all of their careful scholarship about Kripke's *Naming and Necessity* are largely irrelevant to the project we were pursuing. Our main task in this response is to reiterate and clarify what we are really arguing for, and to show that most of Devitt's and Ichikawa and colleagues' criticisms fail to address our concerns.

Here is how we will proceed. First, in §1, we will clarify what our claims and arguments really are. In §2, in light of these clarifications, we consider the argument, proposed both by Devitt and by Ichikawa and colleagues, claiming that we have exaggerated the role of the intuitions elicited by the Gödel case in the dialectic over the correct theory of reference. In §3 and §4, we consider Devitt's argument that experts' intuitions are more theoretically informative than lay people's intuitions, and very briefly discuss Devitt's general approach to semantics. In their paper, Ichikawa and colleagues endorse a number of objections offered elsewhere in the literature. We have already responded to most of these, often at some length. In §5, we will briefly take up one objection that we have not previously addressed, and indicate where interested readers can find detailed responses to the others. Finally, in §6, we discuss Ichikawa and colleagues' response to our article "Against Arguments from Reference."

1. Against Intuitions

Both Devitt and Ichikawa and colleagues assume that our goal in our two previous articles (Machery et al., 2004; Mallon et al., 2009) was to challenge Kripke's argument against descriptivism. Deploying a great deal of subtle scholarship about the intricate argumentative structure of *Naming and Necessity*, they argue that this alleged challenge fails. Devitt and Ichikawa and colleagues insist that most of Kripke's arguments against descriptivism do not depend on people's intuitions about the Gödel case and that, if it were the case that one cannot appeal to these intuitions to undermine descriptivism (which they doubt), Kripke's arguments still suffice to show that descriptivism is false. They conclude that the philosophical significance of the findings reported in Machery et al. (2004) and other papers is limited. More specifically, Devitt claims that "the evidence [in Machery et al. 2004] is far less significant than Machery et al. suppose" (419), while Ichikawa and colleagues assert that "the Gödel/Schmidt example plays a different,

and much smaller, role in Kripke's argument for the causal-historical theory than MMNS assume" (forthcoming, 1).

Though we will briefly address these claims in §2, the point we want to emphasize is that our goal has never been to challenge Kripke's argument against descriptivism. Our project has a different and, we dare say, broader target: our goal is to challenge the way philosophers of language go about determining what the right theory of reference is. Indeed, here is what we say in the abstract of Machery et al. (2004, B1, emphasis added):

These results constitute *prima facie* evidence that semantic intuitions vary from culture to culture, and *the paper argues that this fact raises questions about the nature of the philosophical enterprise of developing a theory of reference.*

Although philosophers have rarely been explicit about what the correct method is for determining the right theory of reference, they typically appeal to the intuitions of competent speakers about the reference of proper names (or other kinds of words) in actual and possible cases. A theory of reference is undermined if it entails that in an actual or a possible case a proper name (or a natural kind term, etc.) refers to what competent speakers judge is not its correct referent, and it is supported if it entails that in an actual or a possible case a proper name (or a natural kind term, etc.) refers to what competent speakers judge is its correct referent. Following Mallon et al. (2009), we will call this method "the method of cases." In practice, philosophers usually appeal only to their own intuitions about reference and those of a few colleagues, perhaps because they take these intuitions to be representative of competent speakers' intuitions or perhaps because they take them to be more reliable.

Our criticism of the method of cases is straightforward. Evidence suggests that *some* intuitions about the reference of proper names vary within and across cultures—viz., the intuitions about the reference of "Gödel" in the Gödel case. This variation inductively suggests that *other* intuitions about reference are also likely to vary.¹ If intuitions really vary, philosophers of language interested in reference would need to accommodate such variation. Consider the case of names.

One option would be to maintain that names refer in the same way in all languages and thus infer that variable intuitions are not reliable guides

¹ For additional empirical work, see Machery et al., 2009; Machery et al., 2010; Machery et al., forthcoming. Recently, Jonathan Livengood, Justin Sytsma, Ryoji Sato, and Oguchi Mineki have replicated Machery et al.'s (2004) findings with Japanese participants (unpublished data): just like Chinese, it appears that Japanese tend to have descriptivist intuitions.

to the semantic properties of names. Philosophers of language who adopt this view would reject the method of cases, and they in turn owe an account of how the correct theory of reference is to be determined.

Alternatively, philosophers of language interested in reference could maintain that intuitions are reliable guides to the semantic properties of names and go on to infer that names refer differently in different cultures. If they endorse this second option, philosophers of language would need to examine the intuitions of ordinary competent speakers empirically, which would lead to a sea-change in their methods and might compel them to devise new theories of reference (Machery & Stich, forthcoming).

A third option would be to insist that some sources of intuitions, but not others, are reliable guides to the semantic properties of names. For example, they could suggest that the intuitions of academic philosophers, or linguistic experts, or Westerners, or members of some other special cultural group, have reliable intuitions while others do not. The burden for this line of argument is justifying the claim that the favored group of people or of intuitions is privileged. Again, arguably, this would require empirical validation of the reliability of these intuitions, and this would require a sharp departure from the blind reliance on philosophers' intuitions.

In any case, what we have been really concerned with is the method that both Kripke and his opponents embrace: the use of intuitions about reference to identify or justify the right theory of reference. As we will see in the sections that follow, this entails that the fine details of Kripke's arguments against descriptivism as well as most of the other arguments advanced by Devitt and Ichikawa and colleagues are largely irrelevant to the central concerns of Machery et al. (2004) and Mallon et al. (2009).

2. The Dialectical Role of the Intuition about "Gödel"

Both Devitt and Ichikawa and colleagues remind us that Kripke attacks several versions of descriptivism, including those that identify the *meaning* of a term with a description associated with it. Furthermore, they correctly insist that the Gödel case is directed only at the weak form of descriptivism, which holds something like:

- (R) The referent of the term is the thing that uniquely or best satisfies the description associated with it.

While this is correct, we were not concerned with "the strong theory-of-meaning construal of descriptivism" (2011, 421), to use Devitt's

terminology, but with this “weaker construal” (what Ichikawa et al., following Soames, call “weak descriptivism” (forthcoming, 6)) on which “the associated description simply identifies the referent” (420). Moreover, as Devitt himself acknowledges (2011, 421), only what he calls *the argument from ignorance and error* is relevant against this weak descriptivism, and the Gödel case *is* used in the service of that argument.

However, as we have seen in §1, Devitt and Ichikawa and colleagues go on to suggest that we have exaggerated the importance of the Gödel case in Kripke’s argument from ignorance and error against weak descriptivism. They hold that other, actual cases play a far more important role in this argument and that the epistemic status of the intuitions elicited by these cases is much stronger than the intuitions elicited by the Gödel case.

The argument from ignorance is fairly straightforward.² Consider first the example of “Cicero”:

1. Many competent speakers do not associate any individuating description with “Cicero.”
2. “Cicero” refers to Cicero for these competent speakers.
3. If the weak form of descriptivism is true, then the referent of “Cicero” is the individual that satisfies the individuating description competent speakers associate with “Cicero.”
4. Hence, the weak form of descriptivism is false, at least for “Cicero.”

The argument is then generalized:

5. *Mutatis mutandis*, (1) and (2) are true of many actual proper names.
6. Hence, the weak form of descriptivism is false of many actual proper names.

Premise (1) is supported by the fact that many competent speakers cannot produce any individuating description to go with “Cicero.” What about premise (2) and its generalization to other names? In support of this premise, Devitt writes that:

² For the sake of space, we will not discuss the argument from error.

inspired [by Kripke's humdrum cases] it is *very easy* to come up with countless cases, each just as humdrum and each yielding the intuition that a speaker's use of a name designates an object despite the speaker's ignorance or error about the object. These intuitions are very powerful because *to reject them is to rule that many names out of just about every mouth fail to designate what they should.* (2011, 421)

Thus, the bulk of argumentative weight relies on the fact that premise (2) cannot be denied, on pain of maintaining something very ridiculous indeed. Ichikawa et al. concur with Devitt:

It can't be that the experiments about the Gödel/Schmidt example show that intuitive judgments about reference are systematically mistaken. Most of our intuitions in this field are surely correct. For instance, our intuitions that 'Kripke' refers to Kripke and not Obama, and that 'Obama' refers to Obama and not Kripke, are correct. (And experiments like the ones MMNS ran don't give us any reason at all to doubt that.) And we could produce many more examples like that. (forthcoming, 9).

They go on to argue that, with regard to the real examples Kripke uses,

It seems to us that these cases are much more like the cases where we know people have accurate intuitions (e.g. 'Obama' refers to Obama), than they are like cases where there is some dispute about their accuracy (e.g. 'Gödel' would refer to Gödel even if Schmidt had proved the incompleteness of arithmetic) (forthcoming, 10).

Now that Devitt's and Ichikawa and colleagues' response is clarified, it should be clear that it fails to address our argument. To see this, let's suppose for an instant that, because of the actual cases highlighted by Devitt and Ichikawa and colleagues, the argument from ignorance really shows that the correct theory of reference for proper names is not among the versions of descriptivism discussed in *Naming and Necessity*. The problem is that refuting these versions of descriptivism falls far short of determining how proper names refer. Even if these versions of descriptivism are false, we still need to determine what the correct theory for the reference of proper names *is* and we still need to provide evidence supporting this theory. And—here is the rub—intuitions about possible cases are likely to be needed to determine what the correct theory of reference is, and our findings about the variation in the intuitions elicited by the Gödel case as well as others findings (see footnote 1) suggest that the intuitions elicited by the possible cases needed to support any theory of reference are themselves going to vary.

Here is another way to make the same point. Let's suppose that Kripke has shown that the versions of descriptivism considered in *Naming and Necessity* are incorrect. Let's also suppose, though only for argument's sake, that the causal-historical approach to reference is by and large correct. The problem is that Kripke himself does not offer a fully worked-out causal-historical account of reference. Rather, he is quite clear that he was not

going to present an alternative theory. [...] [M]y characterization has been far less specific than a real set of necessary and sufficient conditions for reference would be. [...] I don't know that I'm going to do this because, first, I'm sort of too lazy at the moment; secondly, rather than giving a set of necessary and sufficient conditions which will work for a term like reference, I want to present just a *better picture* than the picture presented by the received views. (Kripke, 1972/1980), 93)

And, as we have argued at length in an earlier paper (2009, 349ff), it takes numerous auxiliary assumptions to turn such a picture into a full-fledged theory of reference that offers determinate answers to the kind of questions philosophers apply them to. Once we see that lots of assumptions are needed to turn the "picture" into a full-fledged theory, we realize, first, that there are lots of nearby theories of reference, and, second, that jiggling one or another fine point about baptism or transmission yields an alternative theory from which different philosophically significant conclusions follow.

And how are we to know which assumption to make, or which way to jiggle? It seems that in practice the way to know which one of these options is the right one is to rely on intuitions, and not only on intuitions of the "*Obama*" refers to *Obama* sort, but also of the Gödel-case sort. Moreover, in light of our findings, it seems quite likely that these intuitions will vary within and across cultures.

Devitt's earlier work is instructive here. His first book, *Designation*, provides an admirable attempt to say, in much more detail than Kripke, exactly how a causal theory of reference might work. But two things are noteworthy about it. First, intuitions play a crucial role in the attempt, serving both as evidence and as the explananda that a theory must organize and illuminate (see, especially, Ch. 5, e.g. 145ff). And second, even this book length treatment stops short of being "comprehensive" (130, 138). The message we get from Devitt's *Designation* is that using intuitions about many elaborate cases is the only method to get to a full-fledged theory of reference.

So far, we have been conceding that intuitions about actual cases may be sufficient to show that the versions of descriptivism considered

in *Naming and Necessity* are incorrect. But this concession was mostly strategic since it seems to us that Devitt and Ichikawa and colleagues exaggerate the role that *actual* cases play in the dialectic against descriptivism.

First, while some intuitions elicited by actual cases (e.g., “Peano” and “Dedekind”; “Cicero”) are apparently supportive of the argument from ignorance and error and thus appear to undermine the versions of descriptivism that Kripke considers, other intuitions (e.g., “Madagascar”, “King Arthur”, as noted by Evans, 1973) are *prima facie* supportive of descriptivism.

Second, resourceful descriptivists can accommodate the intuitions about actual cases that form the basis of the argument from error and ignorance (just as resourceful non-descriptivists can accommodate the intuitions about “Madagascar” and “King Arthur”; see, e.g., Kripke, 1972/1980, 163; Devitt and Sterelny, 1999). For instance, intuitions about actual cases fail to undercut the cluster of theories according to which the reference of a proper name is determined by the description conventionally associated with this proper name in the language this name belongs to. Similarly, intuitions about actual cases do not undermine the view that the reference of a proper name is determined by the description that experts in the relevant linguistic community associate with the name. The reason the actual cases do not undermine these forms of descriptivism is that, even if a speaker does not associate an individuating description with a given name (a case of ignorance), such a description might be conventionally associated, or associated by experts, with this name in the language she speaks. The Gödel case (including the version used in Machery et al., 2004) allows us to elicit intuitions that bear on these more complex forms of descriptivism since one can stipulate that everybody is mistaken.³

To conclude this section, let us repeat that our aim was never to experimentally refute Kripke’s attack on descriptivism. Rather, we wanted to raise much more general concerns about philosophers’ implicit endorsement of “the view that the semantic task simply is the systematization of our ordinary intuitions about meaning, reference, and the like” (Devitt, 2011, 424). Devitt concedes that, on this common understanding, our findings do indeed “raise questions about the philosophical enterprise of developing a theory of reference” striking “at the very subject matter of semantics” (Devitt, 2011, 424). *And this is exactly what we wanted to show.* Our findings (if borne out by further

³ Ichikawa and colleagues concede that the argument based on the Gödel case is the only argument that refutes “even the weak version of weak descriptivism according to which in the special case in which subjects do possess individuating descriptions, those descriptions determine reference” (forthcoming, 8).

work) suggest that the standard view of semantics is in deep trouble. So Devitt *agrees* with the most important conclusion we wanted to establish. Ichikawa and colleagues, on the other hand, have taken on a quite different burden. For they endorse the method of cases, and they also seem to endorse some causal-historical theory of reference. But defending the truth of some specific version of the causal-historical theory requires quite a bit more than showing that Kripke's attacks against descriptivism do not depend on the intuition about the Gödel case. It requires showing that you can get to a full-fledged causal-historical theory of reference using intuitions that don't vary in the ways we have suggested. Ichikawa and colleagues have offered no reason at all to think that this is true.

3. Experts' vs. Lay People's Intuitions

As we have seen, our argument rests crucially on the variation of the intuitions that are taken to provide evidence for theories of reference. But perhaps lay people's intuitions are the wrong kind of intuitions to look at. We noted above the possibility that one might try to restrict which intuitions are relevant. Linguists and philosophers of language have developed an expertise about natural languages. They have acquired a large amount of knowledge about natural languages; they have developed methods for testing their theories; and so on. So perhaps their intuitions are more reliable. If so, and if the intuitions of experts (linguists and philosophers of language) that are elicited by the Gödel case and other relevant cases varied much less than ordinary people's intuitions, then our critique of the methods used by philosophers of language for determining the correct theory of reference would fail.

In fact, Devitt argues that, if one is to rely on intuitions, then experts' intuitions should be used because they are more reliable. According to Devitt, intuitions are "empirical theory-laden central-processor responses to phenomena, differing from many other such responses only in being fairly immediate and unreflective, based on little if any conscious reasoning" (2011, 425). Their reliability is a function of the epistemic worth of the underlying empirical theories, and experts just have better theories than laymen.

The first point we want to make about Devitt's defense of experts' intuitions is that his account of what intuitions *are* is more than a bit controversial. Perhaps it is true that intuitions in some domains are the product of people's more or less inchoate empirical theories, but it is far from clear that this is true of *linguistic* intuitions. As Devitt himself acknowledges, linguists in the Chomskian tradition have long defended an alternative account on which, "the speaker's linguistic intuitions are

good evidence for linguistic theories because she derives the intuitions from those representations by a causal and rational process like a deduction” (Devitt, 2006b, 483). On this view, these intuitive judgments are “the voice of competence” (Devitt, 2006b, 483–484). Though Devitt has railed against this “voice of competence” view in a long list of publications (Devitt, 2006a, 2006b, 2010a, 2010b), to the best of our knowledge, he has had no success at all in convincing the experts whose intuitions he favors. As far as we know, there is not a single well known linguist who has endorsed Devitt’s critique of the “voice of competence” account in print or embraced Devitt’s alternative account. Moreover, if the widely accepted “voice of competence” view is correct, there is no reason to expect the linguistic intuitions of philosophers of language to be significantly more reliable than the intuitions of the ordinary folk since in both cases the intuitions reflect the representation of the rules of the language in the intuiter’s language faculty, and it makes no sense to suppose that rules in a philosopher of language’s language faculty are better or more reliable than the rules in a lay person’s language faculty.

Even if Devitt is right about linguistic intuitions being empirical theory-laden central-processor responses, however, it does not follow that the intuitions of philosophers of language are more reliable. As Weinberg et al. (2010, 334) note in their valuable exploration of the idea that philosophers are expert intuiters, there is a rich empirical literature on the development of expertise. What this literature shows is that “not just any experience or training will result in expertise, no matter how prolonged or effortful.” Rather, the evidence indicates that in some areas, like meteorology, experience and training does indeed lead to expertise, while in other areas, like personnel selection and polygraph testing they do not. Moreover, “‘one of the most enduring findings in the study of expertise’ is that there is ‘little transfer from high-level proficiency in one domain to proficiency in other domains—even when the domains seem, intuitively, very similar’.[...] So philosophers’ possession of such demonstrable skills as, say, the close analysis of texts, or the critical assessment of arguments, or the deployment of the tools of formal logic, does little to nothing to raise the probability that they possess any correspondingly improved level of performance at conducting thought-experiments.” (Weinberg et al., 2010, 335; the embedded quote is from Feltovich et al., 2006, 47) Thus even if it is the case that philosophers of language have a great deal of expertise about many aspects of natural language, it does not follow that their intuitions about thought-experiments concerned with reference are more reliable than those of other speakers. Perhaps they are. But Devitt offers no evidence at all that this is the case.

As it happens, while Devitt simply assumes that the linguistic intuitions of linguists and philosophers of language will be more reliable than the intuitions of ordinary speakers, methodology-savvy syntacticians have begun to explore the issue empirically. As we read this growing body of literature, there is little in it to support the idea that the intuitions of linguists and philosophers of language are more reliable than those of ordinary speakers, and there is some reason to think that they may in fact be *less* reliable. Though space does not permit a detailed review of this literature, we will mention three key findings (for further discussion, see Schütze, 1996; Fanselow, 2007; Machery and Stich, forthcoming):

- (1) Experimental work on linguists' and ordinary competent speakers' intuitions has not shown that the former are more reliable than the latter, provided that ordinary speakers understand the nature of the task (Schütze, 1996; Culbertson & Gross, 2009; Gross & Culbertson, 2011).
- (2) Theoretical commitments, which differ in different groups of linguists, may sometimes influence their intuitions, undermining the evidential role of these intuitions. For example, it has been found that linguists in different fields respond differently to the Gödel case in ways that are a priori predictable. Sociolinguists, historical linguists, and anthropological linguists, who are likely to be sensitive to the descriptions associated with words, are more likely to have descriptivist intuitions than philosophers of language and semanticists, who are likely to be familiar with Kripke's *Naming and Necessity* (see Machery, forthcoming for discussion; for a different sort of example, see the discussion of the debate about the ambiguity of sentences such as "Why do you think that he left?" in Machery and Stich, forthcoming).
- (3) Linguists' intuitions might be entirely unrepresentative of the language they speak (see, e.g., the discussion of the "wanna" construction in English in Machery and Stich, forthcoming).

For these reasons, linguists are increasingly replacing the traditional informal reliance on their own and their colleagues' intuitions with systematic experimental surveys of ordinary speakers' intuitions.

4. Doing without Intuitions?

Though Devitt thinks that the intuitions of philosophers of language are more reliable than the intuitions of ordinary speakers, he also

suggests that one could develop a theory of reference without appealing to intuitions. He appeals to an analogy with scientific research (one could rely on the intuition of a biologist and one could also do tests), and he then sketches how this non-intuition based study would go.

The first thing to be said in response is that Devitt is granting our main point. Remember (again!) that we are claiming that, in light of the growing body of evidence about the variation in intuitions about reference, the standard, intuition-based method for determining the correct theory of reference should be revised. In proposing that philosophers of language working on reference might do without intuitions altogether, Devitt seems to be endorsing a view that is fully compatible with our most important conclusion.

The second thing to note is that Devitt's positive proposal is part of an elaborate theory that has been defended at great length in a long list of articles and books (e.g., Devitt, 1994, 1996, 2003, 2009). This is not the place to undertake a general response. However, one of us (Stich, 1996, 2009) has offered a critique of the methodology Devitt proposes for producing a naturalistic account of reference, and in that critique the empirical evidence for variation in intuitions about reference plays a central role.

Finally, we submit that Devitt's account of how semantics can do without intuitions is really a bait-and-switch. We are promised that one can provide evidence about the nature of reference without appeal to intuitions, but, when Devitt describes his method in detail, it turns out that it does appeal to intuitions after all, just not intuitions *that are explicitly about the reference of terms*. Rather, the evidential basis for deciding between theories of reference is supposed to be mental state ascriptions—intuitive judgments about other people's mental states. Furthermore, it is unclear that this alternative involves no appeal to intuitions about reference since ascribing beliefs or thoughts to others involves determining what they are thinking about. On many accounts, this amounts to determining what they are referring to.

5. A Litany of Objections

In their paper, Ichikawa and colleagues endorse a number of other objections to our work. With a single exception, all of these objections have been addressed in detail elsewhere. Rather than repeating those replies, we simply offer the appropriate references.

- (1) *Objection 1*: The question in Machery et al. (2004) Gödel case ("When John uses the name "Gödel," is he talking about the person who really discovered the incompleteness of arithmetic

or the person who got hold of the manuscript and claimed credit for the work?") is ambiguous between speaker's and semantic reference. For responses see Machery, 2011; Machery and Stich, forthcoming; Machery et al., forthcoming.

- (2) *Objection 2*: The vignettes cannot be understood by descriptivists. For a response see Machery et al., 2010.
- (3) *Objection 3*: The experiment asks participants to make a theoretical, metalinguistic judgment about the semantic properties of this proper name rather than to use a proper name. For a response see Machery et al., 2009.
- (4) *Objection 4*: "Kripke's argument [against descriptivism] relies on the fact that "Gödel" refers to Gödel, not to [sic] the universality or otherwise of intuitions about what it refers to" (Ichikawa et al. ms., p. 2; see also Deutsch, 2009). This is the objection to which we have not previously replied, but it strikes us as clearly mistaken. The evidence that "Gödel" refers to Gödel rather than Schmidt in the counterfactual situation that Kripke describes (or that our vignette characterizes) is an intuition (or "spontaneous judgment" if you prefer) that it does. If this intuition varies, then our reason for believing the alleged fact is undermined. Kripke was well aware of his evidential use of intuitions, as his well-known claim about intuitions illustrates.⁴

It is also worth noting that it is far from obvious that Ichikawa and colleagues' objections are all consistent, either with one another, or with Ichikawa and colleagues' argument. For example, if intuitions do not matter to the selection of a correct theory of reference (as suggested in Objection 4), then it's hard to see the point of taking care to find the right sort of intuitions, as Ichikawa and colleagues seem to urge in their first point and in their endorsement of Martí (2009). While we do appreciate the attention lavished on our paper by Ichikawa and colleagues and by the authors they cite, we think our critics routinely underestimate the extent to which they disagree with one another. They ought to fight with one another more.

⁴ "Some philosophers think that something's having intuitive content is very inconclusive evidence in favor of it. I think it is very heavy evidence in favor of anything, myself. I really don't know, in a way, what more conclusive evidence one can have about anything, ultimately speaking." (Kripke, 1972/1980, 42)

6. The Discussion of “Against Arguments from Reference”

In the last section of their paper, Ichikawa and colleagues engage with the argument set out in Mallon et al. (2009). We will respond briefly to their three main arguments.

First, they remark that metalinguistic variation *need* not lead to communicative failure since, even if ways of fixing reference differ among dialects, the different ways may agree upon the same referent. We agree, but we think that this point is mostly irrelevant. While we do note in passing that our argument extends to ordinary discourse (Mallon et al., 2009, 348), the bulk of our discussion consists in arguing that relativizing reference to different intuition groups would cause a breakdown of *philosophical* argument as we know it. Since philosophical arguments often center precisely on cases where different full-fledged theories of reference allow one to draw different philosophical conclusions (e.g., “What does ‘good’ refer to?”, “Are there races?”, “Do beliefs exist?” etc.), there is little reason to think Ichikawa and colleagues’ remark offers much comfort.

Second, Ichikawa and colleagues ingeniously attempt to show the argument developed in Mallon et al. (2009) is analogous to earlier arguments against descriptivism alone, which would result in the following dilemma: Either this argument is good and then (by the earlier arguments) descriptivism is false (and this can be assumed), or the argument is not good, and it is okay for philosophers to appeal to causal-historical theories (at least for their dialect) because they are unthreatened by this argument. From this, Ichikawa and colleagues want to conclude “there’s no problem with philosophers making arguments from the falsity of descriptivism” (forthcoming, 11).

There are several problems with this line of argument. First, Ichikawa and colleagues ignore the distinction between strong descriptivism (the strong theory-of-meaning construal of descriptivism in Devitt’s terminology) and weak descriptivism (the weaker construal in Devitt’s terminology). The argument presented by Ichikawa and colleagues is meant to show that strong descriptivism is untenable: one cannot identify the meaning of a term with the description a speaker associates with this term on pain of compromising the possibility of communication. But this argument does not undermine weak descriptivism because weak descriptivism says nothing about meaning. Because the arguments from reference we discuss in “Against Arguments from Reference” appeal to weak descriptivism⁵ (descriptivism as a theory of reference),

⁵ This is particularly clear in the argument for the elimination of propositional attitudes. Eliminativists and anti-eliminativists disagree about how propositional attitudes terms refer, not about what descriptions are associated with them.

Ichikawa and colleagues are mistaken to conclude that “there’s no problem with philosophers making arguments from the falsity of descriptivism.”

It is true that the argument put forward by Ichikawa and colleagues could be reformulated so as to apply to weak descriptivism too: because different speakers are likely to associate different descriptions with, say, a proper name, they are likely to refer to different individuals, which would compromise the possibility of communication. So, is it the case that “there’s no problem with philosophers making arguments from the falsity of descriptivism”? Not at all—for a number of reasons. First, when individuating descriptions are associated with a proper name (or a natural kind term, etc.), most of the descriptions associated with this name are likely to pick out the same individual. For instance, “the loser at the battle of Waterloo” and “the winner at the battle of Austerlitz” refer to the same individual. We expect that Ichikawa and colleagues will agree with this observation since it is similar to one of their own arguments (discussed first in this section). Second, in practice, philosophers can, and often do, associate the same descriptions with the words involved in arguments from reference (“belief”, “race”, “good”, etc.) by explicit specification, or even stipulation. For instance, eliminativists and anti-eliminativists about propositional attitudes agree about the description associated with propositional attitudes terms: it is determined by the role that the concepts expressed by propositional attitudes terms play in our folk theory of mind. Third, resourceful descriptivists won’t be impressed with the reformulations of Ichikawa and colleagues’ objection. For instance, they could hold that, while competent speakers might associate different descriptions with a proper name, the description that fixes the reference of this name could be the single description conventionally associated with this name.

Finally, the conclusion of the argument put forward in “Against Arguments from Reference” is much more ambitious than the argument highlighted by Ichikawa and colleagues. Our argument undermines causal-historical theories as much as descriptivist theories insofar as causal-historical theories are justified by appeal to intuitions of the sort in question. Importantly, vindication for anti-descriptivism (supposing that anti-descriptivism is vindicated) is hardly support for causal-historical theories. As we noted above (and in Mallon et al., 2009), a full-fledged causal historical theory requires a great many assumptions to apply to actual cases, and it is far from clear how these assumptions are to be justified except by appealing to the intuitions elicited by cases similar to the Gödel case.

Ichikawa and colleagues’ third argument is that philosophical uses of theories of reference do not typically rely on the sorts of intuitions

we have called into question, and they illustrate this claim by appeal to the case of Boyd's moral naturalism.⁶ Boyd's naturalism (and by extension, perhaps, other arguments from reference) remains untouched by our arguments since all Boyd needs for his argument is that some names (or perhaps natural kind terms) operate causally-historically. They note that this contributes to a realist account of issues in the philosophy of science, and allows a story about knowledge about unobservable kinds. And "by analogy, he [Boyd] suggests that we should be optimistic that a naturalistically acceptable moral theory exists, and that it is compatible with us having lots of moral knowledge" (forthcoming, 12).

On Ichikawa and colleagues' reconstruction of Boyd's argument, intuitions serve as an evidential basis for the selection of the correct account of reference for a class of terms (names or natural kind terms), which then serves as the basis for an analogy to moral terms. So, as a first pass, it looks like variation in such intuitions is a threat to Boyd and other purveyors of arguments from reference. However, Ichikawa and colleagues are at pains to point out that Boyd doesn't need to rely on Gödel-type intuitions: "Just looking at the arguments Kripke raises before that example gives us more than enough evidence to use in the kind of argument Boyd is making," and a little later "few, if any, of the arguments they attack use the parts of the causal-historical theory that Kripke is defending with the Gödel/Schmidt example" (12–13).

Thus, their defense of arguments from reference depends precisely on their argument for the privileged status of other Kripkean arguments we discussed in §2, and to which our responses in §2 also apply. We especially emphasize that, even if you think that some of Kripke's arguments succeed in refuting a particular version of descriptivism, this falls short of giving any reason to endorse a full-fledged theory of reference (causal-historical or otherwise) that can serve to underwrite arguments from reference.

⁶ Ichikawa and colleagues note in passing that this argument from reference is *prima facie* concerned with natural kind terms rather than names (forthcoming, 12). And of course, we agree that different reference relations might obtain for different classes of terms (Mallon et al., 2009, 339). One worry is, then, that the type of variation we have found for proper names need not be found for natural kind terms. On inductive grounds, this is a risky assumption, for variation in intuitions about proper names suggests that intuitions about natural kind terms may vary too. Empirical evidence emerging from our own investigations (Machery and Olivola, *nd*) suggests that the type of intuitions causal-historical theories of reference about natural kind terms rely on are even more variable than the intuitions about proper names. Nonetheless, this is an empirical question, the answer to which will become more clear as evidence emerges.

Conclusion

We are grateful for Devitt's and Ichikawa and colleagues' thoughtful discussions of our work. But our main goal has been to challenge the methods philosophers of language have embraced in their efforts to develop a satisfying theory of reference. In light of this goal, many of the arguments put forward by Devitt and by Ichikawa and colleagues turn out to be irrelevant.

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