Don't Believe the Hype: Why Should Philosophical Theories Yield to Intuitions?

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Abstract: In this paper, I argue that, contrary to common opinion, a counterexample against a philosophical theory does not amount to conclusive evidence against that theory. Instead, the method of counterexamples allows for the derivation of a disjunction, i.e., 'either the theory is false or an auxiliary assumption is false', not a negation of the target theory. This is so because, whenever the method of counterexamples is used in an attempt to refute a philosophical theory, there is a crucial auxiliary assumption that needs to be taken into account. The auxiliary assumption is that making intuitive judgments in response to hypothetical cases about the subject matter in question (e.g., knowledge or proper names) is a good method for finding out truths about that subject matter. Without good reasons to think that this assumption is warranted, the negation of a philosophical theory whose content is alleged to be in conflict with the content of an intuition cannot be justifiably derived using an argument that employs the method of counterexamples.

Keywords: confirmation holism; counterexample; intuition; method of counterexamples; refutation

1. Introduction

A common opinion among philosophers seems to be that the method of counterexamples allows them to conclusively refute philosophical theories. For instance, according to Martinich:

> Some counterexamples simply *refute a theory*. If the theory is important, then the counterexample may be derivatively important. This is especially so when the counterexample attacks some central aspect of the theory, *as Gettier did*. […] Counterexamples are a very important method in philosophical argumentation (emphasis added) [Martinich (2005), p. 118].

Likewise, Cornman, Lehrer, and Pappas write:

> Finding a counterexample to a purportedly valid argument is a matter of constructing a possible world in which the premises of the argument come out true and the conclusion comes out false. [...] *You can refute invalid arguments by the use of your imagination* (emphasis added) [Cornman et al (1992), p. 14].

Even Lewis, who says that "Philosophical theories are never refuted conclusively," qualifies that by adding "Or hardly ever, Gödel and Gettier *may* have done it" (emphasis added) [Lewis (1983), p. x; quoted in Livengood et al (2012), p. 39]. Here are two alleged examples of the method of counterexamples at work:

First example. It is often said that "the JTB analysis was *refuted* by Edmund Gettier" (emphasis added) [Williamson (2011b), p. 209]. In his seminal paper, "Is Justified True Belief Knowledge?," Gettier (1963) presents counterexamples to the Justified True Belief (JTB) analysis of knowledge, according to which *S* knows that *p* if and only if *p* is true, *S* believes that *p*, and *S* is justified in believing that *p*. Gettier's argument against JTB can be summed up as follows:

1. If knowledge is JTB, then *S* knows that *p* in Gettier cases.
2. It is not the case that *S* knows that *p* in Gettier cases.
3. Therefore, it is not the case that knowledge is JTB.

One gets premise (2) in this argument against JTB by considering Gettier cases and intuitively judging that *S* does not know that *p* in those cases.[1] Then, the content of this intuitive judgment, namely, <*S* does not know that *p* in Gettier cases>, is used as a premise in an argument that is taken to amount to a conclusive refutation of JTB.[2]

Second example. It is often said that "Kripke *refuted* specific proposals for descriptive analyses according to which ordinary proper names have their referents semantically fixed by descriptions commonly associated with them" (emphasis added) [Soames (2007), p. 302]. In his seminal book, *Naming and Necessity*, Kripke (1980)

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[1] By "intuition" I mean "intellectual seeming." According to Brogaard (forthcoming), intellectual seemings ('it intellectually seems that *p*') are "seemings that result from implicit or explicit armchair reasoning, where armchair reasoning is reasoning that involves both a priori principles and past experience." See also Boghossian (2009), Huemer (2007), Pryor (2005), and Sosa (2009). For arguments against appealing to intuitions as a method of fixing philosophical belief, see my (2012) and (2013a).

[2] On the hype surrounding Gettier's counterexamples against JTB, see Shope (1983) and Pollock (1986). Note that even those who dispute the lessons of Gettier cases accept that, if Gettier cases were genuine counterexamples to JTB, then JTB would be refuted. I argue that the method of counterexamples simply does not allow us to refute philosophical theories, even if the counterexamples are genuine. Cf. Bonevac, Dever, and Sosa (2012).

presents several counterexamples to the Description Theory of Names, according to which a name has the semantic value of a definite description, e.g., 'Kripke' = 'the author of *Naming and Necessity*'. Kripke's argument against the Description Theory of Names can be summed up as follows:

1. If names are definite descriptions, then 'Gödel' = 'the person who proved the incompleteness of arithmetic' in the Gödel-Schmidt case.
2. It is not the case that 'Gödel' = 'the person who proved the incompleteness of arithmetic' in the Gödel-Schmidt case.
3. Therefore, it is not the case that names are definite descriptions.

One gets premise (2) in this argument against the Description Theory of Names by considering the Gödel-Schmidt case and intuitively judging that 'Gödel' still refers to 'Gödel' even though 'Gödel' does not refer to the person who proved the incompleteness of arithmetic. Then, the content of this intuitive judgment, namely, <Gödel ≠ the person who proved the incompleteness of arithmetic>, is used as a premise in an argument that is taken to amount to a conclusive refutation of the Description Theory of Names.[3]

Accordingly, the Method of Counterexamples (MCE) consists of the following two steps:

Step 1. Considering a hypothetical case C is supposed to elicit an intuitive judgment J whose content is incompatible with a philosophical theory T.

Step 2. Using *modus tollens* to argue as follows:

$T \longrightarrow J$
$\neg J$
$\therefore \neg T$

In this paper, I argue that arguments of this form that employ (MCE) do not amount to conclusive refutations of philosophical theories. In other words, contrary to common opinion, I argue that (MCE) cannot be used to conclusively refute a philosophical theory, since there is a crucial auxiliary assumption that must be taken into consideration. The auxiliary assumption is that making intuitive judgments in response to hypothetical cases

[3] On the hype surrounding Kripke's counterexamples against the Description Theory of Names, see Linsky (2011), pp. 17-48. Note that even those who dispute the lessons of Gödel-Schmidt cases accept that, if those cases were genuine counterexamples to the Description Theory of Names, then that theory would be refuted. I argue that the method of counterexamples simply does not allow us to refute philosophical theories, even if the counterexamples are genuine.

about the subject matter in question (e.g., knowledge or proper names) is a good method for finding out truths about that subject matter. My overall argument, then, runs as follows:

1. An argument that employs (MCE) amounts to a refutation of a philosophical theory T only if the *modus tollens* argument used to refute T implies $\neg T$.
2. It is not the case that the *modus tollens* argument used to refute T implies $\neg T$. (Instead, it implies $\neg T \vee \neg A$, where A is an auxiliary assumption to the effect that making intuitive judgments in response to hypothetical cases is a good method of finding out philosophical truths.)
3. Therefore, it is not the case that an argument that employs (MCE) amounts to a refutation of a philosophical theory T.

This argument is deductively valid. In what follows, then, I flesh out the premises of this overall argument by drawing on the notion of confirmation holism.

2. Confirmation Holism

In philosophy of science, confirmation holism is the idea that, since the empirical content of a theory cannot be clearly separated from the other components of the theory, when the theory makes a prediction that is not borne out by experimentation and/or observation, logic alone does not tell us which component of the theory should be rejected. As Okasha writes:

> According to the doctrine of confirmation holism, also known as the 'Quine-Duhem' thesis, the empirical content of a scientific theory cannot be parcelled out individually among the constituent components of the theory. Thus when a theory makes an empirical prediction which turns out to be false, it will not be automatically obvious where to lay the blame, i.e., which component of the theory to reject. Logic tells us there is an error somewhere in the set of statements which implies the false prediction, but does not tell us where. So there will be various ways of modifying our theory to inactivate the false implication [Okasha (2002), p. 306].

Accordingly, as far as scientific theories and their predictions are concerned, there is no straightforward derivation like the one outlined above in *Step 2* of (MCE) because, by itself, T does not imply any predictions. There are always background assumptions or auxiliary hypotheses [Okasha (2011), p. 224].

Hence, when T's prediction is not borne out by experimentation and/or observation, all that we can justifiably infer from that is that either T is false or that one of the auxiliary assumptions is false. That is:

$(T \wedge A) \longrightarrow P$
$\neg P$
$\therefore \neg T \vee \neg A$

For example, when Copernicus first proposed his heliocentric model, his opponents objected that the heliocentric model is inconsistent with observations. More specifically, they argued roughly as follows:

1. If Copernicus' heliocentric model were accurate, then we would observe stellar parallax.
2. We do not observe stellar parallax.
3. Therefore, Copernicus' heliocentric model is inaccurate.

As we now know, stellar parallax is detectable only with the aid of sophisticated telescopes. So, this argument against Copernicus' heliocentric model fails to amount to a conclusive refutation of the heliocentric model because Copernicus' opponents were making a crucial assumption. That is, they assumed that the fixed stars are relatively close to Earth, such that stellar parallax would be observable with the naked eye.

So, to take this auxiliary assumption into account, their argument against Copernicus' heliocentric model should be revised as follows:

1. If Copernicus' heliocentric model were accurate, and if the fixed stars are relatively close to Earth, such that stellar parallax would be observable with the naked eye, then we would observe stellar parallax.
2. We do not observe stellar parallax.
3. Therefore, either Copernicus' heliocentric model is inaccurate or the fixed stars are not relatively close to Earth, such that stellar parallax would be observable with the naked eye.

As we now know, Copernicus' opponents were mistaken in assuming that the fixed stars are relatively close to Earth, such that stellar parallax would be observable with the naked eye. Very sophisticated instruments are needed in order to detect stellar parallax.

3. Disconfirmation and the Method of Counterexamples

Now, applying the lessons of confirmation holism to (MCE), the *modus tollens* argument in *Step 2* needs to be revised as follows:

$(T \land A) \longrightarrow J$
$\neg J$
$\therefore \neg T \lor \neg A$

After all, the claim that we would have a certain intuitive response to a hypothetical case C is an empirical claim: we either have that intuitive response or we don't. And the only way to find out is to actually consider C and see how we intuitively respond. If this is correct, then the following auxiliary assumption needs to be taken into account whenever (MCE) is used in an attempt to refute a philosophical theory T:

(AA) Making intuitive judgments in response to hypothetical cases is a good method for finding out truths about the subject matter of those cases.

For example, as far as the Gettier counterexample against JTB is concerned, we need to add the following auxiliary assumption to the argument:

(A1) Making intuitive judgments in response to hypothetical cases about knowledge is a good method for finding out truths about the analysis of knowledge.

Accordingly, Gettier's argument against JTB needs to be revised as follows:

1. If (K = JTB) *and* (A1), then S knows that p in Gettier cases.
2. It is not the case that S knows that p in Gettier cases.
3. Therefore, either ¬(K = JTB) or ¬(A1).

As in the case of scientific theories, logic alone tells us that there is an error somewhere but it does not tell us where. That is, it may be the case that (K = JTB) is false but it may also be the case that (A1) is false. To justifiably derive the negation of (K = JTB), then, we need good reasons to prefer the first disjunct, i.e., ¬(K = JTB), and reject the second disjunct, i.e., ¬(A1). If we do not have good reasons to reject ¬(A1), then we are not justified in deriving the negation of (K = JTB).

In other words, to justifiably derive the negation of JTB, we need good reasons to think that (A1) is true. Accordingly, those who claim that (MCE) can be used to refute JTB have to show why we should reject a simple theory on account of an intuitive judgment,[4] an intuitive judgment that, *pace* Williamson (forthcoming in *Philosophy*),[5] is probably rather idiosyncratic, since it varies across cultures [Stich (2013)] and is subject to all sorts of effects, such as the epistemic side-effect effect [Buckwalter (2013)] and the Knobe effect [Beebe and Shea (forthcoming)].[6] In his recent review of Alexander (2012), Williamson writes:

> In both everyday and scientific situations, when I say that P, I would say that P, and (if I am sincere) it seems to me that P. *If I am not idiosyncratic*, we would say that P, and it seems to us that P. *If I believe that I am not idiosyncratic*, I believe that what we would say, and how things seem to us, is that P (emphasis added) [Williamson (forthcoming in *Philosophy*)].

Williamson may believe that his intuitions are not idiosyncratic but empirical evidence suggests otherwise. At the very least, now that the empirical evidence is out there, one cannot assume without argument that one's intuitions are not idiosyncratic [cf. Nichols and Ulatowski (2007)]. Even if intuitions are not idiosyncratic, the basic methodological point still stands, namely, ¬(K = JTB) cannot be justifiably derived from premises about intuitive judgments in response to Gettier cases unless we have good reasons to think that (A1) is true.

Similarly, as far as the Kripke-style counterexamples against the Description Theory of Names are concerned, we need to add the following auxiliary assumption to the argument:

> (A2) Making intuitive judgments in response to hypothetical cases about proper names is a good method for finding out truths about the semantics of proper names.

Accordingly, Kripke's argument against the Description Theory of Names needs to be revised as follows:

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[4] Cf. Bonevac, Dever, and Sosa (2012) who argue that Gettier cases are not sufficient to refute the possibility of a conjunctive analysis of knowledge. See also Weatherson (2003) who argues that intuitions about Gettier cases should be explained away rather than respected. For a philosopher who rejects the Gettier intuition outright, see Musgrave (2012).
[5] Available here: http://www.philosophy.ox.ac.uk/__data/assets/pdf_file/0009/26739/Xphireview.pdf.
[6] See also Beebe and Buckwalter (2010), Beebe and Jensen (2012), and Buckwalter (forthcoming).

1. If names are definite descriptions *and* (A2), then 'Gödel' = 'the person who proved the incompleteness of arithmetic' in the Gödel-Schmidt case.
2. It is not the case that 'Gödel' = 'the person who proved the incompleteness of arithmetic' in the Gödel-Schmidt case.
3. Therefore, either names are not definite descriptions or ¬(A2).

Again, as in the case of scientific theories, logic alone tells us that there is an error somewhere but it does not tell us where. That is, it may be the case that the Description Theory of Names is false but it may also be the case that (A2) is false. To justifiably derive the negation of the Description Theory of Names, then, we need good reasons to prefer the first disjunct, i.e., 'names are not definite descriptions', and reject the second disjunct, i.e., ¬(A2). If we do not have good reasons to reject ¬(A2), then we are not justified in deriving the negation of the Description Theory of Names. In other words, those who claim that (MCE) can be used to refute the Description Theory of Names have to show why we should reject a simple theory on account of an intuitive judgment, an intuitive judgment that is probably rather idiosyncratic.[7] Even if intuitions are not idiosyncratic, the basic methodological point still stands, namely, that the negation of the Description Theory of Names cannot be justifiably derived from premises about intuitive judgments in response to Gödel-Schmidt cases unless we have good reasons to think that (A2) is true.

In fact, there might even be a good reason to reject (AA) rather than the target philosophical theory. That is, as far as scientific theories are concerned, we sometimes have good reasons to think that our testing methods are reliable, and so we reject the scientific theories that conflict with evidence obtained by means of these methods. A case in point is the Ptolemaic model and the telescope [Kitcher (2001)]. Once Galileo had established, to the satisfaction of his contemporaries, that the telescope is a trustworthy instrument of celestial observation, the observational evidence obtained by means of the telescope (e.g., Jupiter's moons) was then used as evidence against the Ptolemaic model. That is, the Ptolemaic model made a prediction that turned out to be false:

$(T \wedge A) \longrightarrow P$
$\neg P$
$\therefore \neg T \vee \neg A$

Since Galileo had provided good reasons to think that A (i.e., that the telescope is a trustworthy instrument of celestial observation) is true, the falsity of the Ptolemaic model could be justifiably derived:

[7] On the idiosyncrasy of intuitions in response to Gödel-Schmidt cases, see Machery et al (2012). Note that even if Williamson (forthcoming) is right about intuitions, i.e., that they are not idiosyncratic, it still does not follow that (AA) is true, and hence the basic methodological point of the paper stands, namely, that applications of (MCE) do not amount to conclusive refutations of philosophical theories.

$\neg T \vee \neg A$
A
$\therefore \neg T$

As far as philosophical theories are concerned, however, we do not seem to have good reasons to think that our testing methods are reliable. In fact, as I have argued elsewhere (2014), these methods might even be unreliable. In my (2014) I argue that hypothetical cases, or "intuition pumps," are bad epistemic circumstances, which is why making intuitive judgments in response to such hypothetical cases is not a reliable method of fixing philosophical belief. Now, since (MCE) presupposes that the method of cases (i.e., the method of making intuitive judgments in response to hypothetical cases) is a reliable method of fixing philosophical belief, if the method of cases is unreliable, as I have argued in my (2014), then it follows that (MCE) is unreliable as well. In any case, just as the burden of proof was on Galileo to show that the telescope is a reliable instrument for celestial observation, the burden of proof is on those who wish to use (MCE) to show that it is a reliable method for refuting philosophical theories.

4. Objections and Replies

In this section, I discuss several objections to my overall argument, which is outlined in Section 1. The first objection is motivated by something that Kripke says in *Naming and Necessity*. Kripke writes:

> Of course, 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 (1980), p. 42].

Inspired by Kripke, then, some might object to my overall argument by claiming that (MCE) is either the *only* method or the *best* method we have for testing philosophical theories.

I think it is clear, however, that these are rather weak objections. Even if method M is the only method we have, it does not necessarily follow that M is a good or a reliable method. Similarly, from the fact that method M is the best method we have, it does not necessarily follow that M is a good or a reliable method. Our best method could be the best of a bad lot. In any case, we would still need good reasons to think that *intuitions should count more heavily than theories*, such that if theory and intuition clash, it is always theory that should yield to intuition.

In the case of scientific theories, I submit, it would be rather unreasonable to argue that scientific theory *T* should always be rejected if it clashes with our intuitions. For example, quantum mechanics has several counterintuitive implications. But no scientist would argue that quantum mechanics' counterintuitive implications should count as conclusive evidence against it. So why is it that a philosophical theory's counterintuitive implications should always count as conclusive evidence against it?

Indeed, one could argue that the history of science teaches us that we should not trust our intuitions completely, as far as theories and their predictions are concerned. For example, Aristotelian physics seemed intuitive enough until Galileo came along and showed that, despite its intuitive appeal, it is probably wrong, particularly in its claims about motion. Similarly, Newtonian mechanics seemed intuitive enough until Einstein came along and showed that, despite its intuitive appeal, is it probably wrong, particularly in its claims about space and time. Based on these, and other similar examples, one could argue that the historical record teaches us that the intuitive or counterintuitive content of a theory should not count as conclusive evidence either for or against it.

Some might also object to my argument by invoking the "expertise defense." According to the expertise defense, philosophers are experts, and only the intuitions of experts should count as evidence in philosophical arguments.[8] If this is correct, then (AA), (A1), and (A2) should be amended as follows:

(AA*) *Experts* making intuitive judgments in response to hypothetical cases is a good method for finding out truths about the subject matter of those cases.

(A1*) *Epistemologists* making intuitive judgments in response to hypothetical cases about knowledge is a good method for finding out truths about the analysis of knowledge.

(A2*) *Philosophers of language* making intuitive judgments in response to hypothetical cases about proper names is a good method for finding out truths about the semantics of proper names.

Note, however, that invoking the expertise defense does nothing to change the basic *methodological* point of this paper, namely, that all that can be justifiably derived from an argument that employs (MCE) is a disjunction, i.e., $\neg T \vee \neg A$, rather than a refutation, i.e., $\neg T$. To see why, consider the argument against JTB again. Even if we supplement it with (A1*) instead of (A1) as follows:

1. If (K = JTB) and (A1*), then *S* knows that *p* in Gettier cases.
2. It is not the case that *S* knows that *p* in Gettier cases.

[8] On the expertise defense, see Williamson (2007) and (2011a). Cf. Weinberg et al (2010) and Ryberg (2013).

3. Therefore, either ¬(K = JTB) or ¬(A1*).

All that can be justifiably inferred from the premises of this argument is a disjunction, namely, 'either ¬(K = JTB) or ¬(A1*)', not a conclusive refutation of JTB, i.e., ¬(K = JTB). We can justifiably derive ¬(K = JTB) only if we have good reasons to think that (A1*) is true. One is not entitled to assume without argument that JTB must yield to the Gettier intuition expressed in the second premise of this argument against JTB.

Likewise, even if we supplement the argument against the Description Theory of Names with (A2*) instead of (A2) as follows:

1. If names are definite descriptions and (A2*), then 'Gödel' = 'the person who proved the incompleteness of arithmetic' in the Gödel-Schmidt case.
2. It is not the case that 'Gödel' = 'the person who proved the incompleteness of arithmetic' in the Gödel-Schmidt case.
3. Therefore, either names are not definite descriptions or ¬(A2*).

All that can be justifiably inferred from the premises of this argument is a disjunction, namely, 'either names are not definite descriptions or ¬(A2*)', not a conclusive refutation of the Description Theory of Names. We can justifiably derive the negation of the Description Theory of Names only if we have good reasons to think that (A2*) is true.[9] One is not entitled to assume without argument that the Description Theory of Names must yield to the Kripke intuition expressed in the second premise of this argument against the Description Theory of Names.

Are there good arguments for rejecting a philosophical theory T (e.g., JTB or the Description Theory of Names) in favor of (AA*) [e.g., (A1*) or (A2*)]? To the best of my knowledge, there aren't any in the extant literature on philosophical methodology. In the extant literature on philosophical methodology, appeals to intuitive appearances or intellectual seemings are usually supported by analogy with sensory appearances or perceptual seemings. For instance, according to Hales, "if we regard sense perception as a mental faculty that (in general) delivers justified beliefs, then we should treat intuition in the same manner" [Hales (2012), p. 180.][10] Even if the perception-intuition analogy holds, however, it merely provides a reason to believe that, if perceptual beliefs are justified, then intuitive beliefs are justified as well, not that intuitions should count more heavily than theories, evidentially speaking. For recall that the question is *why should theory always yield to intuition*? In other words, as far as applications of (MCE) are concerned, why prefer $\neg T$ over $\neg A$? Even if sense perception and intuition are analogous in epistemically relevant

[9] For experimental evidence against (AA*), see Schultz et al (2011) and Machery (2012). It is also worth noting that there are professional philosophers who reject the Gettier intuition, e.g., Musgrave (2012).
[10] Others who endorse the perception-intuition analogy include Bealer (1998), Bonjour (1998), Chudnoff (2013), and Sosa (1996). Cf. my (2014).

respects, it does not follow that intuitions evidentially outweigh philosophical theories such that theories should always be rejected when they are at odds with intuitions. Even in science, observations do not always count more heavily than theories, as the case of the discovery of Neptune demonstrates [Rosenberg (2000), pp. 139-140].

Again, I think that whether the intuitions in question are those of experts or non-experts makes no difference at all. Here is another reason why. Presumably, the reason why we would give more evidential weight to the intuitive judgments of experts, as opposed to the intuitive judgments of non-experts, in philosophical arguments is that we think that the intuitive judgments of experts are significantly more likely to be true. In other words, those who appeal to the expertise defense endorse something like the following principle:

(E) Judgment *J* is significantly more likely to be true when made by an expert than when made by a non-expert.

However, research on expertise shows that (E) is probably false. To cite just one out of many studies as an example, Camerer and Johnson (1991) found that decisions made by experts are often no more accurate than decisions made by non-experts.[11] If this is correct, then, *even if philosophers are expert intuiters*, their intuitive judgments are not significantly more likely to be true than the intuitive judgments of non-experts. Some experimental evidence is already suggesting that that is the case.[12] If that is the case, then there is no reason to give the intuitive judgments of philosophers more weight in philosophical arguments.

If the aforementioned considerations are correct, then the two instances of (MCE) discussed above, namely, the argument against JTB from Gettier cases and the argument against the Description Theory of Names from Gödel-Schmidt cases, and others like them, do not amount to conclusive refutations of the philosophical theories they target. In general, it is not the case that an argument that employs (MCE) amounts to a conclusive refutation of a philosophical theory *T*. For, without good reasons to think that philosophical theories must always yield to intuitions, all that can be justifiably inferred from the premises of the *modus tollens* argument in *Step 2* of (MCE), after taking the fact of confirmation holism into account, is a disjunction, i.e., $\neg T \vee \neg A$, not a conclusive refutation of *T*, i.e., $\neg T$.

To be clear, I am not defending JTB as an analysis of knowledge. Nor am I defending the Description Theory of Names. Rather, I use these two examples to illustrate a

[11] For more studies on expertise, experts' performance and failure, see Tetlock (2005) and Ericsson et al (2006). Additional empirical studies are cited in my (2013b) where I argue that appeals to expert opinion are weak arguments.

[12] See, e.g., Schulz et al (2011), Schwitzgebel and Cushman (2012), Knobe and Samuels (2013), and Tobia et al (2013).

methodological point about philosophical argumentation. The point is that, *pace* the philosophical hype about counterexamples, the method of counterexamples (MCE) does not allow one to conclusively refute a philosophical theory. This is so because, without good reasons to think that philosophical theories must always yield to intuitions, all that can be justifiably derived from an argument that employs (MCE) is a disjunction: either the theory is false or an auxiliary assumption is false. Since we have no reason to think that the auxiliary assumption is true, we are not entitled to conclude that the target theory is false.

I am also not saying that intuitions do not play any evidential role whatsoever in philosophical argumentation [cf. Cappelen (2012)]. Instead, I am saying that intuitions cannot play the role that many philosophers think they can play. That is, many philosophers think that (MCE), which relies on appealing to intuitions, can be used to conclusively refute philosophical theories (see the quotations in Section 1). I argue that it cannot, not because intuitions do not play an evidential role in philosophical argumentation, but rather because intuitions and (MCE) cannot do the work that many philosophers think they can do.

5. Conclusion

In this paper, I have argued that a counterexample against a philosophical theory does not amount to conclusive evidence against that theory. Instead, all that can be justifiably derived from an argument that employs (MCE) is a disjunction, i.e., $\neg T \vee \neg A$, because there is a crucial auxiliary assumption that needs to be taken into account. The auxiliary assumption is that making intuitive judgments in response to hypothetical cases about the subject matter in question (e.g., knowledge or proper names) is a good method for finding out truths about that subject matter. As far as scientific theories are concerned, we sometimes have good reasons to believe that our methods work, and so we reject theories that conflict with evidence obtained by means of these methods. As far as philosophical theories are concerned, however, we do not seem to have good reasons to think that our methods (specifically, the method of counterexamples) work, and so we are not warranted in rejecting theories that conflict with evidence obtained by means of these methods. If this is correct, then those who wish to use (MCE) as a method for conclusive refutation must provide an argument as to why philosophical theories should always yield to intuitions.

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