
Reply to Nicholas Smith's Comments on *Inconstancy and Inconsistency*

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Smith's comments on "Inconstancy and Inconsistency" are clear and valuable, and there is a great deal in them that I agree with. For maximum entertainment value, of course, I'll focus on the bits I disagree with. First, I'll defend judgment-based experimental methodologies against Smith's worries. Then, I'll argue that the specific arguments I make in the main paper stand despite Smith's objections to them.

1 Reason for hope

Smith begins by pointing out the gulf between our philosophical and logical theories of vagueness on the one hand and the data we have on the other; and there is indeed a dearth of data (for now) about judgments of compound sentences, particularly the seemingly-contradictory compound sentences I've discussed, in borderline cases. For the most part, psychologists have been much more interested in other sorts of judgments, particularly simple judgments. Despite this lack of data, philosophical and logical work has proceeded at a brisk pace; we have a variety of frameworks that make a variety of predictions here. As a result, data collected here should have considerable theoretical payoff. Smith is right: we need more.

Smith seems to claim, though, that there is little research even on simple categorization judgments in borderline cases, and this is just not so. There is, in fact, a great deal of experimental research on just the phenomena Smith points to, mostly reported in the literature on the psychology of categorization. Examples range from the classic [McCloskey and Glucksberg, 1978] to the much more recent [Hampton, 2007]. Nor are the results just noise: there is often surprising and intricate texture to judgments in this middle ground. For an example of particularly interesting data, see [Hampton et al., 2011]; this paper reports experiments that seem to provide evidence against epistemicism precisely due to the texture of judgments in borderline cases.

Moreover, the obstacles Smith envisions to such research do not in fact create a great deal of difficulty; really, they are the very object of the research. There is no a priori reason to expect experiments in which participants are comfortable with the task they are asked to perform to produce more reliable data than experiments in which participants are uncomfortable. Further, if the data being sought is data about the form that discomfort takes, there is every reason in the world to expect the reverse. (To build on Smith's analogy, if we wanted to study how judgements of attractiveness interact with the incest taboo, we might well follow a methodology like the one he dismisses. Indeed, the experiments reported in [Fraleigh and Marks, 2010] are not so far off.)

But this is just our situation: data about clear cases of vague predicates are not theoretically important. Almost all going theories will agree on them. It's data about borderline cases that we need. Since the goal is to learn about borderline cases, diving in and asking participants about borderline cases is an important source of data. It cannot be dismissed as unreliable on the grounds of participants' discomfort; that discomfort is itself part of the phenomenon to be studied. This sort of methodology has resulted in considerable success when it comes to simple categorization judgments, and there is no reason to expect it to be less reliable when it comes to compound judgments.

Of course, other data is available too. We shouldn't restrict ourselves to methodology based on eliciting judgments; Smith is absolutely right that we should explore other options as well.¹ But I think he shouldn't give up on judgment-based methodology so hastily, in view of its history of successful applications.

2 Inconsistent theories and 'neither'

Smith argues that I am too quick to allow that inconsistent theories predict agreement to 'neither' sentences in borderline cases. He points out that we cannot expect participants to carry out logical reasoning quickly or accurately. *If* participants can arrive at the conclusion that something is neither P nor not P only by starting from the premise that it is both P and not P , we should not expect every participant who reaches the starting point to make it to the finish line. They may get lost along the way. As such, we should expect fewer positive responses to 'neither' sentences than to 'both' sentences. We should expect this all the more, he says, if participants are under cognitive load.

I agree with this as far as it goes—but notice the 'if'. I don't agree that an inconsistent-extension theorist is committed to the claim that participants can arrive at the 'neither' conclusion only via reasoning from a 'both' premise. Nor do I see a reason that an inconsistent-extension theorist should accept this principle. Rather, the 'neither' judgment can be reached compositionally, in the same way that a 'both' judgment can be reached. We merely need to suppose that, when asked to check whether something is neither A nor B , participants check whether it is A and whether it is B and then combine those checks in an appropriate way. This way need not involve 'and' at all. In symbols: there is no need to get to $\neg(A \vee \neg A)$ from $A \wedge \neg A$. Both can be reached from judgments of A and $\neg A$, which are then combined in the appropriate ways.

The reasoning from 'both' to 'neither' I make in the initial paper, and which Smith quotes, is reasoning intended to show *us* that the inconsistent theory in fact predicts 'neither' sentences to be true. It's meta-level reasoning, and not to be attributed to experimental participants. The point is quite general: a formal semantic theory ought to make predictions, and we, as theorists, must reason about it to see what predictions it makes. (Less-familiar theories, like inconsistent ones, call for quite careful reasoning here.) Whether those predictions are correct or not does not depend on whether speakers in fact conduct reasoning that parallels ours.

There is something in the area, though, that seems worth mentioning: it might well be easier to combine individual judgments with 'and' than it is to combine them with 'neither...nor'. That seems like a plausible hypothesis on its own, and data in [Geurts

¹Corpuses!

and van der Slik, 2005] is at least friendly to the idea. If this is so, we might expect some differences between 'both' and 'neither' sentences, particularly under cognitive load. It's not clear to me, however, that we should expect *lower* agreement to 'neither' than to 'both'; it depends on the details of the differences between 'and' and 'neither'.

3 Stickiness

Finally, I think things are worse for the stickiness-based inconstancy hypothesis of [Raffman, 1996] than Smith supposes. While I agree with Smith that stickiness on its own involves no commitments to any particular speed, only to some asymmetry, Raffman's particular use of the stickiness hypothesis does involve a commitment to some particular speed. Smith includes only the first half of the quote I used from Raffman's paper, but the very next sentence is crucial: "Analogously, once the competent speaker has shifted from 'red' to 'orange', if asked to retrace his steps down the series he will now call some patches 'orange' that he formerly called 'red' [Raffman, 1996, p. 179]. That is, the stickiness, on Raffman's theory, ought to be sticky enough to influence participants' responses to multiple future stimuli. It's this that causes trouble for a switching-in-the-middle theory of judgments about single stimuli. I agree with Smith that it would be possible to develop a theory on which context shifts are sticky that still succeeds in explaining the observed responses. However, in allowing for context shifts rapid enough to do the job, it would give up the prediction Raffman makes (and for which she provides some evidence in [Raffman, 2012]).

Every one of these issues deserves more discussion than there is space to give it here. Many thanks are due to Smith for raising these issues, and to the editors of this volume for giving us an opportunity to discuss them.

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