

Surveys, Intuitions, Knowledge Attributions

Comments on Keith DeRose's "Contextualism, Contrastivism, and X-Phi Surveys"
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Keith DeRose has not been shy about placing a lot of weight on certain intuitions in arguing for epistemic contextualism (EC). Most notably, he advertises the apparent (intuitive) truth of certain surface-incompatible claims involving 'knows'¹ as a central part of "[his] main positive argument for contextualism" (2009, 69). His well-known Bank Case, or pair of Cases, gets introduced on p. 1 of his recent book, *The Case for Contextualism*. There, he writes:

The contextualist argument based on such pairs of cases ultimately rests on the key premisses that the positive attribution of knowledge^[2] in LOW is true, and that the denial of knowledge in HIGH is true. Why think that both of these claims are true?

Well, first, and most directly, where the contextualist's cases are well-chosen those are the fairly strong intuitions about the cases, at least where each case is considered individually. Here we appeal to how we, competent speakers, intuitively evaluate the truth-values of particular claims that are made (or are imagined to have been made) in particular situations [...]. Our intuitions about such matters can be wrong, of course, but still, they are among our best guides when evaluating semantic theories, especially when we are careful to avoid the types of situations where we are likely to be misled.... (*ibid.*, 49-50)

In "Contextualism, Contrastivism, and X-Phi Surveys", DeRose responds to Jonathan Shaffer and Joshua Knobe's (S&K's) claim that a series of recent results in 'experimental philosophy' (X-Phi) casts doubt on whether the intuitions in question are anywhere as widespread and natural as DeRose believes, and so on whether EC and the debate surrounding it really is well-founded. Much of DeRose's response to S&K consists in

¹As opposed to claims *about* knowledge, or who knows what: epistemic contextualism (EC) is a semantic thesis about the truth conditional contents of the propositions expressed by certain sentences – viz., that they are 'context'-dependent and –variable: in itself, EC is silent about *knowledge*. For discussion of this and other points, arguments and issues surrounding EC, see Rysiew 2007a.

² This is a misleading way of putting it since, according to EC, there is no one *knowledge* relation – at least, no unrelativized one – that all occurrences of 'know(s)' pick out (cf. previous note; Bach 2005, Section 1). Fortunately, this doesn't affect the current discussion.

simply pointing out that, in the studies in question, the experimenters (and so, the subjects) for the most part haven't focused on the right kinds of cases after all, and that it's not clear that subjects aren't being misled in one or another way. For instance, some experimenters (e.g., May *et al.*, forthcoming) ask subjects directly whether *S* knows, as opposed to whether some claim involving 'knows' within a certain scenario is true; some (e.g., Buckwalter, forthcoming) ask subjects to evaluate, in HIGH, a positive claim to know, as opposed to a knowledge denial; some (*ibid.*) deliberately try to pull apart the factors which might influence subjects' judgments, whereas DeRose thinks that a fair test will pile on as many possibly relevant features as possible; and so on.³

In fact, among the numerous studies that DeRose discusses, only two, he thinks, pose even a *prima facie* problem for his brand (see n. 5 below) of EC. One, Feltz and Zarpentine's (F&Z's; forthcoming) initial study, because the intuitions it generates *in just the right kind of cases diverge* from the ones DeRose relies on in arguing for contextualism: F&Z's subjects offer a near-neutral response in both HIGH and LOW, as opposed to a firm intuition of the truth of the 'knows' attribution in LOW and the 'knows' denial in HIGH. The second seemingly problematic study is such because it appears to produce results that strongly favor contrastivism over (DeRose's brand of) EC – this is Shaffer and Knobe's (S&K's) third study, their 'context sensitivity' survey, aimed at demonstrating a contrast effect.⁴ But even here, DeRose thinks, there is good reason to think that the studies might not be much a threat after all.

For my part, I'm largely in agreement with what DeRose says about the effectiveness of the X-Phi work he discusses in discrediting his (and others') reliance upon a certain pattern of intuitions about various 'knowledge' attributions. At the same time, I intend to follow DeRose's lead in leaving the design of further experimental probes to those who possess the necessary methodological wherewithal. Instead, I shall focus for the most part on what DeRose says in response to the *prima facie* more problematic survey results just

³ DeRose's is most explicit about what makes for "the best cases" at 2009, 53ff., 155ff; in the present paper, see p. 3.

⁴ The other two such studies are, for reasons DeRose gives (pp. 18-25), not after all clearly relevant to arguing for contrastivism.

alluded to. – Not merely because *they can* seem to challenge EC. (But not, again, because I think DeRose’s response to these studies is way off-base – as just indicated, I don’t.) Just as importantly, what DeRose says in responding to these surveys in particular interacts in some interesting ways with some of his own positive views and arguments and raises some general, and crucial, questions and issues about philosophical methodology – not least, concerning the use of intuitions in developing and defending philosophical theories. These are questions and issues that merit attention all on their own, and that considering, even criticizing, empirical findings such as those X-Phi trades in make all the more pressing.

First, then, taking the relevant cases in reverse order (and by way of disclosing some of my own theoretical commitments), consider contrastivism, the main theoretical alternative to DeRose’s brand of contextualism that gets attention in his paper. This is the view that S&K are concerned to promote, and for the ‘intuitiveness’ of which they think they’ve uncovered experimental evidence. Briefly, while EC holds that ‘knows’ expresses different two-term relations (between subjects and propositions) in different contexts, according to contrastivism, ‘knows’ denotes a three-place relation, with a contrast variable being included among the *relata*. The latter may be ‘shifty’ when it is not explicitly provided, which allows for the essentially contextualistic result that “one ascriber could truly say ‘*s* knows that *p*,’ while a second ascriber in a second context (with a different range of relevant alternatives) could truly deny ‘*s* knows that *p*.’ This is because the first ascriber could truly express the proposition that *s* knows that *p* rather than *q1*, while the second ascriber could truly deny *s* knows that *p* rather than *q2*” (S&K, p. 14).⁵

DeRose casts contrastivism as a specific, extreme form of what he calls *epistemic aspectism*, where an ‘epistemic aspectist’ is someone who holds that, when accompanied

⁵ Whether this suffices to make contrastivism a form of EC is most likely a terminological issue. (For Schaffer on the differences between contrastivism and canonical versions of EC, see his 2004.) Here, purely for the sake of ease of reference in differentiating between contrastivism and more standard forms of EC – such as have been endorsed by DeRose, Stewart Cohen, and others -- I’ll tend to use ‘contextualism’ to refer to the latter.

by certain devices (stress, word order, etc.) serving to focus attention on some part of what's (supposedly) known, what might *appear* to be equivalent knowledge sentences can in fact diverge in truth value (pp. 25-26). “Contrastivists [such as Schaffer, say],” DeRose writes, “tend to hold what we might call ‘full-blown contrastivism,’ according to which ‘Mary knows that Peter stole the rubies’ can have different truth values in different contexts, even when describing the same situation *and even in different contexts which are focused on the same aspect* of ‘Peter stole the rubies’” (p. 27).

DeRose reports that a “main stumbling block [for him] to accepting these versions of aspectism has been the thought that the intuitions that drive it invariably seem to be in conflict with other intuitions that seem about as strong, or often stronger, than they are” (p. 28). For myself, and for what it's worth, aspectism *per se* isn't immediately attractive to begin with. It seems clear that *of course* selective focusing can affect whether you *take* a given proposition -- whether or not it involves an occurrence of ‘know(s)’ -- to be true: attentional shifts can beget cognitive differences. This is not to minimize the significance of the phenomenon in question. The point is merely that, from the reality of contrastive focusing and its effects *per se*, nothing *immediately* follows about the truth conditional contents of the relevant sentences.

Now, this might sound like an attempt to write off the relevant ‘appearances’ -- i.e., any ‘intuitive’ difference among the truth values of members of DeRose's 1a-c, 2a-c, etc. (pp. 25-26) -- as the result of some combination of focal bias and shallow processing. And S&K are quick to respond to *that* suggestion by noting that while shallow processing effects are certainly real, “[they] think that there are *both* shallow processing and contrast effects, and suspect that it would be a mistake to try to reduce either effect to the other” (S&K, p. 28). This, they say, is what's suggested by, e.g., their studies of salience effects (*ibid.*, p. 29, and Section 5).

Now, DeRose has doubts about S&K's interpretation of the latter results as clearly demonstrating the importance of (really, very) salient error possibilities, as opposed to the importance of stakes, even if the height of the latter is largely only implied (see pp. 17-

18). Here too, the doubts seem perfectly reasonable. But even setting such doubts aside, as I've argued elsewhere (2001, 2005, 2007b), we have good reason to suspect that making various possibilities salient can affect people's willingness to attribute/deny knowledge because of what's presupposed, communicated, and/or inferable from a given knowledge sentence's use, *even given* a non-contrastivist (non-contextualistic, etc.), invariantist semantics. (This, by the way, is very much in the spirit of some of the most prominent recent urgings for greater methodological care in designing and interpreting surveys: for instance,⁶ Schwarz (1995), and following him Cullen (forthcoming), stress how everyday conversational pragmatics are very much a factor in how subjects respond to survey questions – for such subjects, as for us (most of the time), “[i]t is not literal meaning which matters...it's speaker meaning, and this crucially depends on context” (Cullen, Section 2), which includes such things as which possibilities have been made salient.)

Now, my take on the at-most-pragmatic role of salience is, admittedly, controversial. But nothing even that ‘fancy’⁷ is needed to think that S&K's attempt to elicit a contrast effect by relying on conversational context is problematic – and for pretty much the reason that DeRose and others⁸ have suggested: given the way the *thief contrast* question is posed – “Everyone is now asking the big question: Who stole the rubies?” – it is perfectly natural,

⁶ For other examples see Sudman *et al.* (1996) and several of the contributions to Schwarz and Sudman eds. (1992).

⁷ S&K (Section 6.3) anticipate an attempt to explain the results in the ‘context’ case in terms of conversational pragmatics, one that's styled after Rysiew 2001 and Brown 2006. While they think the imagined view represents “an interesting and plausible strategy”, they register two objections to it: it cannot explain why we would *deny* certain claims, they think, and it fails the cancelability test. Brown (2006) and Rysiew (2001; 2005, Section 3; 2007b, Section 4) have responded to just these worries about their views. However, as discussed, it is doubtful that any very sophisticated theory is required for handling the ‘context sensitivity’ case.

⁸ This and other recent studies have been the subject of a lot of web discussion among philosophers. That the subjects in the *thief contrast* case may well be picking up on a presupposition about what was stolen and reading it into the story itself is among the ideas floated at *Certain Doubts*, by Aaron Zimmerman and Jennifer Nagel in particular: http://el-prod.baylor.edu/certain_doubts/?p=908.

even *expected*, that subjects will take it as obvious (hence, known) to all, including Mary, that it's rubies that were stolen. Whereas, there's nothing to invite that assumption about Mary's (or the general) evidentiary situation in *jewel contrast*. So it's not surprising that subjects considering *thief contrast* will tend to agree with 'Mary knows who stole the rubies' while those considering *jewel contrast* will not. – This seems like a clear example of how, relying on general and familiar conversational mechanisms, respondents can “learn from questionnaires” (Schwarz 1995). Notice, though, that this concern is, at least as just presented, completely independent of something else that DeRose builds into his relaying of the complaint – namely, that the original *jewel thief vignette* is, strictly speaking, incredible on a certain crucial point. He writes, registering his own, *non-neutral* “WTF?” reaction (cf. p. 15):

Though the last sentence above [i.e., in the *vignette*] stipulates that Mary “has no further information,” that statement will be hard for readers to interpret, in large part because they cannot take it in full seriousness. Can or will readers safely assume that beyond having been told that a theft has taken place and having the evidence described above Mary literally has *no* further information? Can they safely assume that she wasn't told where the theft took place? If so, how did she end up, as she seems to have done, at the right place to conduct her investigation? Or will readers assume that that was pure luck? And if she was told where the theft took place, is it safe to assume that she was not told roughly when it took place – that whoever told her a theft took place didn't tell her, for instance, that it took place “earlier today”? Or, to get to the issue crucial to our discussion, is it safe to assume that she wasn't told what was stolen? So, as I would interpret the above vignette, I wouldn't take it that Mary has literally *no* information beyond what I'm being told, but rather that she was told *some* basic information (including where the theft took place), and I'd interpret that last sentence as saying she has no further information beyond such quick basics that she would have been told by whoever sent her over to investigate. It would be unclear whether this basic information would include that rubies had been stolen, as opposed to the vaguer information that some jewels or other had been stolen, or, I suppose (though this would appear far less likely) that nothing at all was said about what was stolen. (p. 31; cf. p. 21)

Whereupon, DeRose presents his own version of the worry about presupposition, and a consequent asymmetry in how the two versions of the case are being understood, that I've just rehearsed.

At this point, though, three things deserve noting.

First, one might worry that DeRose is just being too literal-minded here – that he’s ‘over-thinking it’ and not, as we might say, ‘playing along’. But that’s not clear: after all he, no less than the subjects in S&K’s probe, has a right to know just what situation he’s being asked to consider. However, the worrisome sentence (“[Mary] has no further information”) could likely just be left out. What immediately precedes it, after all, is:

So far [Mary] has the following evidence. She has been told that there was a theft, she has found and identified Peter’s fingerprints on the safe, and she has seen and recognized Peter on the security video, filmed in the act of forcing open the safe. (S&K, p. 16)

Surely S&K could simply leave it at that, and count on respondents to make the natural inference that all the relevant evidence available to Mary is what’s just been described. (Pragmatic factors and inferences aren’t just a useful, though dispensable, short-hand: it is *not possible* to make explicit everything that is/isn’t meant, such that all possible misinterpretations are forestalled.)

But – second – this won’t much help because, as we’ve noted just above, the worry that the subjects faced with the two contrast cases are being invited to construe the situation in importantly different ways would remain: the *thief contrast* question (“Everyone is now asking the big question: Who stole the rubies?”) generates this result all on its own.

Third, however, the type of concern that DeRose is raising here – that subtle, if unintended, cues in the question itself might causing subjects to understand the two cases in importantly different ways -- is of course a quite general one: it can arise, not just in formal surveys, but in informal ones, including when philosophers ask us, as they commonly do, to consider some hypothetical example. And I can easily imagine someone wondering whether DeRose’s Bank Case(s), one of his own “best cases”, isn’t vulnerable to the same sort of worry. (The Bank Case is so familiar by now that I won’t rehearse it.) What I have in mind in particular is the stipulation, in DeRose’s Case B, that the speaker says, “Well, no, I don’t know. I’d better go in and make sure” -- while, we’re told, he “*remain[s] as confident as [he] was before*” (2009, 2; emphasis added).

For better or worse, I’ve spent a lot of time with this and similar examples. However – and I’m a bit embarrassed to admit it -- I must have been engaged in some pretty

“shallow processing”, because it took me a long while before I really ‘noticed’ this important qualification. (It is an important qualification because, among other things, diminished confidence – which it might be otherwise natural to assume, given the scenario described – might be sufficient to undermine knowledge all on its own, in which case there would be no need to go outside of ‘classical invariantism’ to explain what’s going on.) And once I did really notice this qualification, I began exhibiting symptoms of the sort of reaction to it that DeRose has to the *Mary has no further information* stipulation in S&K’s ‘context’ survey: Is it really plausible that the speaker’s confidence is *completely* undiminished? Is he acknowledging the possibility of changed hours as a good enough reason to go in the bank today, but *not* treating it as good reason to modulate his confidence that it’s open tomorrow? Is he just placating his wife, then? And if so, does he really mean what he’s saying? – And so on.⁹

Now, DeRose anticipates just this worry about the stipulation of equal confidence in Bank Case B. He proposes distinguishing between construals of confidence in ‘stable’ and ‘unstable’ ways – of thinking of confidence, that is, in terms of ‘local’ dispositions, or in terms of “different dispositions that tend to remain steady as a subject moves back and forth between high- and low-stakes situations” (2009, 192-3). The ‘non-local’ construal of confidence would provide a way of making the stipulation of equal confidence in Case B unobjectionable. However, that such a construal exists -- even supposing it is, as DeRose believes, the one that’s “appropriate to issues of knowledge” (*ibid.*, 193) -- won’t really help with the worry at hand unless this ‘non-local’ notion of confidence is the one that will naturally occur to subjects; and it’s just not clear, to me anyway, that that’s so. Perhaps confidence is regularly and naturally “(mis)construed in the unstable way” (*ibid.*).

All that said, however, the point under consideration isn’t intended as any kind of “gotcha!” moment. And there are lots of other examples – Stewart Cohen’s (1999) equally well-known Airport Case, say -- that lack the feature on which we’ve been focusing. Nor, for the record, do I myself think that the pattern of ‘intuitions’ that EC

⁹ Here, I’m drawing on Bach’s (2005, 76) articulation of the worry. For related concerns, see Nagel (2008).

predicts, and which the X-Phi'ers whom DeRose is addressing are questioning, is somehow unreal.¹⁰ Rather, I've drawn attention to this example by way of making it vivid that, whatever problems there are in survey methodology – in the design of probes meant to reveal folks' intuitions about various cases and issues – they're often ones that we need to be sensitive to and actively guard against when we're doing 'experiments' in the armchair. Whatever one thinks about recent X-Phi work, such as the specific studies we're considering here, one of the *welcome* things about such a consideration is that it forces a renewed caution and self-consciousness about deploying and understanding good old-fashioned philosophical 'examples' – including, but not only, those upon which DeRose wants to base 'the case for contextualism'.

What about F&Z's initial study? This, recall, is the survey that seems to generate intuitions *in just the right kind of cases* that *diverge* from the ones DeRose relies on in arguing for contextualism – subjects were close to neutral about the truth values of both assertions. DeRose doesn't so much address these results directly as he asks us to consider them in light of what's known about general problems in survey methodology, some of which were mentioned above. In presenting such examples himself, DeRose says, his "audience has been largely professional philosophers, and otherwise students, with the students usually having had enough experience with philosophical examples to have some idea why they are being asked these strange things, and so what to focus on" (p. 13). Whereas, he suggests, it could well be that the subjects in F&Z's study just didn't have a good sense of what the survey was really *about*, and so didn't know what to focus on; they may have been "shallow processing right over subtle differences that are of vital

¹⁰ Though I do think that care should be taken in articulating just what the intuitions are, exactly – whether, e.g., it's that both of the relevant claims are true (flat-out), that they each enjoy some intuitive plausibility, that what the subject "says" in both cases is "correct" or "appropriate", or what. In stating what is uncontroversial in this regard, I myself have put it in deliberately weak, and so (it's hoped) uncontentious, terms -- for instance, by saying that the Bank Case, e.g., "illustrates the fact that there is a manifest flexibility in our willingness to attribute knowledge, whether to ourselves or to others: it is incontrovertible that, in some sense anyway, "what counts as knowing" depends upon 'context'" (Rysiew 2001, 477-478).

importance to the philosophical issues under discussion” (p. 14). Not to mention, of course, the real possibility that they weren’t well-motivated – that they just wanted to get the survey done and go cash in their coupons, or what have you (p. 14).¹¹

Again: all of this seems quite reasonable to me. But, as with DeRose’s reply to S&K’s ‘context sensitivity’ case, his response here gives rise to some more general points, issues and concerns. These go well beyond the real target of DeRose’s paper – but they do so, I hope, in natural and productive ways. I’ll close by briefly relating them.

First, an observation: even if it’s something that’s best explained away, perhaps in the manner DeRose invites, that F&Z’s subjects *did* produce such under-whelming (under-whelmed?) responses might itself be significant. They are, after all, competent speakers, and they’re being presented *with just the right kind of case*. And I suspect – *though I don’t have much empirical backing*, I suppose I should say – that even unmotivated and unphilosophical respondents would produce a far less neutral response to analogous questions involving *uncontroversially* context-sensitive terms (‘here’, ‘tall’, and so on). This is just an instance of the familiar point that if ‘know(s)’ is context-sensitive, the latter can be rather unobvious, even to appropriately-situated subjects considering just the right kind of example. In spite of its familiarity, this point shouldn’t be forgotten, particularly when our grasp of such context-sensitivity is supposed to be what accounts for our using knowledge-sentences as we do in the cases of interest.¹²

Second, insofar as what we’re really interested in are the intuitive responses of those with a good sense of the sometimes-subtle, philosophically relevant differences that are

¹¹ Another possibility here -- one that DeRose doesn’t consider but which complements his discussion -- is that subjects are taking cues from the structure of the questionnaire itself: when they are asked to indicate *how strongly* they agree with the sentence “*When Hanna says, ‘I know [don’t know] that they bank will be open tomorrow, what she says is true*” by selecting a value *on a 7-point Likert scale*, that itself might be taken to suggest that the matter is not straightforward but complex (see Cullen, forthcoming, Section 3.3.1); if so, in the absence of a clear grasp of what such complexities are, much less how to negotiate them, subjects might naturally default towards the ‘safe’, neutral response.

¹² For further discussion, see Rysiew 2007a, Section 4.3.

important to at-times-pretty-esoteric issues, it's not clear that it's the plain old untutored intuitions of 'the folk' – or, for that matter, considerations of 'ordinary language' -- that are of singular or direct significance for constructing and testing philosophical theories. (Which is not to say that our theories should be allowed to simply ignore or ride roughshod over folk judgments and everyday linguistic usage.¹³)

Third, it seems to me that, contrary to what DeRose at least *implies*, even when one does know what the purpose of given test is, it can still be quite unclear "what to focus on". As DeRose says in responding to Buckwalter (and S&K), "one person's 'confound' is another person's wise testing of a combination of factors" (p. 9). So too, one person's 'reading too much' into some 'irrelevant' detail is another person's savvy attending to empirically real and philosophically relevant distinctions. A case in point might be my own experience with the Bank Case, mentioned above: prior to noticing the sentence about the speaker's confidence remaining undiminished, was my 'intuitive reaction' to the case defective; or was I wisely (if unreflectively) ignoring something that I sensed was not to be taken seriously? Or consider DeRose's experience with Ginet's fake barn case, which he relates in his new book (2009, 49-50, n. 2). (This is in the footnote omitted from the passage quoted at the outset above.) Is the fact that Henry hasn't *encountered* any of the fake barns in the area significant? I don't know. But the question was enough to loosen DeRose's judgment (intuition) that Henry clearly lacks knowledge.

Notice that these points aren't intended to motivate skepticism about intuitions: to say that both formal surveys and philosophical examples are 'messy' in the indicated way, and that the judgments they elicit must therefore be treated with a good deal of caution, isn't to say that they can't be legitimately appealed to, or that they are to be dismissed, as some have suggested, as "epistemologically valueless" (Cummins 1998, 125). Nor, finally, am I claiming that all intuitions are 'theory-driven' in any interesting way – at least, in any interesting way that would undermine their evidentiary value. The point is

¹³ Nor is it to say that subjects and their judgments can't be tutored to a large degree, or that carefully crafted probes – where sensitivity to the relevant distinctions is built into the survey itself, as it were -- can't themselves do the work of philosophical savvy on the part of subjects, reliably eliciting their views on the desired issues.

the more modest -- and, I'd hope, agreeable -- one that *especially* where we have divergent views, hotly contested theories, technical distinctions, and so on, we should be very cautious in the use we make of various intuitions. And, in fact, having related his experience with Ginet's barn case DeRose, himself says that he has "since become wary of intuitions used in much epistemology" (2009, 50, n. 2).

Finally, even when there *is* a fairly strong intuition -- as DeRose and others insist¹⁴ there is about the truth/falsity of various knowledge claims -- we need of course to consider any relevant contrary intuitions we might have¹⁵ -- or, more generally, any countervailing considerations (assumptions, arguments, theories, etc.) there might be. (Such as those that lead DeRose to an on-balance rejection of aspectism; see above and n. 17, below. Or those intuitions, arguments and considerations that account for the enduring pull of invariantist thinking.¹⁶) And we need to somehow adjudicate, not just their relative strength, but their relative merits and importance.¹⁷ Like the previous point, this makes it look as though it's not the initial intuitions themselves that are doing much of the *real*

¹⁴ Such an insistence follows immediately after the remark just quoted. DeRose writes: "However, at least with regard to many cases, the intuition about whether a claim involving 'know(s)' made in the examples is true can at least be fairly strong, and when that intuition can be buttressed in the ways we are about to investigate, I think it can become the basis of a strong philosophical argument" (2009, 50, n. 2).

¹⁵ DeRose (1999, 2002; in 2009, see 112-117) has argued against the existence of some crucial conflicting intuitions in the case of EC and its competitors; Brown (2006) and Rysiew (2005) have replied.

¹⁶ For some of which, see Rysiew 2007a. My own reaction to EC is rather like DeRose's position on epistemic aspectism (see above): in spite of whatever intuitive pull the relevant claims enjoy, those intuitions -- especially as concerns the sense that the HIGH denial is "correct" (to use a neutral term) -- conflict with other things that are in my view much more strongly grounded; and they can, moreover, be accommodated otherwise than by taking them as all face-value true.

¹⁷ Compare DeRose, discussing aspectism: "The issue has always seemed to me to be one of finding the best resolution of such conflicts [among intuitions]. . . . Key to the construction of resolutions to these conflicts is not just measuring the degree to which the various claims are intuitively plausible, but also, vitally, accounts of the meanings of and connections between the various relevant forms of knowledge claims, by which the intuitive plausibility of the various claims can be explained -- and sometimes explained away" (p. 29).

‘heavy lifting’, as far as the justification of philosophical theories goes. Whether this is something that X-Phi’ers would find salutary, or whether it exposes a faulty assumption behind certain brands of X-Phi, I’m not sure.¹⁸

References

Bach, Kent (2005): “The Emperor’s New ‘Knows’”, in G. Preyer and G. Peter (eds.), *Contextualism in Philosophy: Knowledge, Meaning, and Truth*. Oxford: Clarendon Press, 51-89.

Brown, Jessica (2006): ‘Contextualism and Warranted Assertibility Manoeuvres’, *Philosophical Studies* 130, 407-435.

Buckwalter, Wesley (*Forthcoming*): “Knowledge Isn’t Closed on Saturdays,” *Review of Philosophy and Psychology*.

Cohen, Stewart (1999): ‘Contextualism, Skepticism, and the Structure of Reasons’, *Philosophical Perspectives* 13: *Epistemology*, 57-89.

Cullen, Simon (*Forthcoming*): ‘Survey-Driven Romanticism’, *European Review of Philosophy*.

Cummins, Robert (1998): ‘Reflections on Reflective Equilibrium’, in M. DePaul and W. Ramsey (eds.), *Rethinking Intuition: The Psychology of Intuition and Its Role in Philosophical Inquiry*. Lanham: Rowman and Littlefield, 113-127.

DeRose, Keith (1999): ‘Contextualism: An Explanation and Defense’, in *The Blackwell Guide to Epistemology*, J. Greco and E. Sosa (eds.), Malden MA: Blackwell, 185-203.

— (2002): ‘Assertion, Knowledge and Context’, *The Philosophical Review* 111.2, 167-203.

— (2009): *The Case for Contextualism*. Oxford: Oxford University Press.

Feltz, Adam and Chris Zarpentine. *Forthcoming*. ‘Do You Know More When it Matters Less?’, *Philosophical Psychology*.

May, Joshua, Walter Sinnott-Armstrong, Jay G. Hull, and Aaron Zimmerman (*Forthcoming*): ‘Practical Interests, Relevant Alternatives, and Knowledge Attributions: An Empirical Study’, *Review of Philosophy and Psychology*.

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Nagel, Jennifer (2008): 'Knowledge Ascriptions and the Psychological Consequences of Changing Stakes', *Australasian Journal of Philosophy*, 86.2, 279-294.

Rysiew, Patrick (2001): 'The Context-Sensitivity of Knowledge Attributions', *Noûs* 35.4, 477-514.

___ (2005): 'Contesting Contextualism', *Grazer Philosophische Studien* 69, 51-70.

___ (2007a): 'Epistemic Contextualism', *The Stanford Encyclopedia of Philosophy*, Edward N. Zalta (ed.), latest archived version, Spring 2009, URL = <http://plato.stanford.edu/archives/spr2009/entries/contextualism-epistemology/>.

___ (2007b): 'Speaking of Knowing,' *Noûs* 41.4, 627-662.

Schwarz, Norbert (1995): 'What Respondents Learn from Questionnaires: The Survey Interview and the Logic of Conversation', *International Statistical Review/Revue Internationale de Statistique* 63.2, 153-168.

___ and Seymour Sudman (eds.) (1992): *Context Effects in Social and Psychological Research*. New York: Springer-Verlag.

Schaffer, Jonathan (2004): 'From Contextualism to Contrastivism', *Philosophical Studies*, 119.1-2, 73-103.

___ and Joshua Knobe (*Forthcoming*): 'Contrastivism Surveyed', *Noûs*.

Sudman, Seymour, Norman M. Bradburn, Norbert Schwarz (1996): *Thinking about Answers*. San Francisco: Jossey-Bass Publishers.