Reasoning, Normativity, and Experimental Philosophy

***Susana Nuccetelli**, St. Cloud State University, Minnesota*
***Gary Seay**, City University of New York*

I

The development of modern science, as everybody knows, has come largely through naturalizing domains of inquiry that were historically parts of philosophy. Theories based on mere speculation about matters empirical, such as Aristotle's view about teleology in nature, were replaced with law-based, predictive explanatory theories that invoked empirical data as supporting evidence. Although philosophers have, by and large, applauded such developments, inquiry into normative domains presents a different set of problems, and there is no consensus about whether such an inquiry can be naturalized. Since the early twentieth century, attempts at naturalizing ethics have been at the center of heated debates, and later attempts at naturalizing epistemology triggered similarly contentious disputes. In ethics, of course, it was not only the Humean and Moorean arguments of early nonnaturalists and noncognitivists that raised doubts about naturalism, but also the arguments of nihilists, who later joined the chorus of criticism, rejecting any ontology that would countenance moral properties and facts. In epistemology, Quinean eliminativism brought reactions that questioned the consistency of naturalism and asked whether it could accommodate the normativity of fundamental epistemic notions such as belief and knowledge.

There have so far been no similarly substantial reactions to attempts at naturalizing inquiry into another normative domain, that of *reasoning*. We hope to remedy that by offering

here a response to some recent efforts by experimental philosophers. Experimentalism about reasoning is a radical naturalistic program that rejects reflective-equilibrium accounts of the epistemic grounds for the rules of inference. If we ignore other attempts at justifying those rules *a priori* by, for instance, appeal to self-evidence, then those invoking Goodmanian reflective equilibrium can be considered the standard analytic accounts (hereafter, 'SAA'). Of concern here is an objection to SAA, the cognitive-diversity argument,[1] that is part of a broader experimentalist critique of analytic epistemology. The cognitive-diversity argument runs

1. If SAA is plausible, then an inference rule is justified if and only if it accords with the reflective inferential practices of a community of thinkers.
2. It is actual, or at least possible, that different inference rules accord with the reflective inferential practices of different communities of thinkers.

Therefore,

3. SAA entails that different inference rules could be equally justified.
4. If (3), then SAA is committed to either cognitive ethnocentrism or thorough-going cognitive relativism.

Therefore,

5. SAA is committed to either cognitive ethnocentrism or thorough-going relativism.
6. Cognitive ethnocentrism is unacceptable.

Therefore,

7. SAA entails thorough-going relativism.

[1]See Stich (1988: 572-74; 1990: 9 ff.); Weinberg, Nichols and Stich (WNS 2001: 8 ff.); Nichols, Stich, and Weinberg (NSW 2003: 231-32); and Bishop and Trout (BT 2005: 107-109).

Therefore,

8. SAA is implausible.

II

As originally stated, the cognitive-diversity argument aims at undermining SAA by showing that if cognitive diversity is either actual or at least possible, then in cases were "the Goodman account entails that a system of inferential rules is justified and intuition decrees that it is not, this is a symptom that the analysis is in serious trouble" (Stich 1988: 574). Surely, epistemic ethnocentrism, the bias of counting as justified only those inference rules that accord with *our own* reflective inferential practices, is not an option. Thus it appears that SAA has no choice but to embrace an anything-goes cognitive relativism, false on anyone's view, holding that different inference rules can be equally good (Stich 1988: 572). Although the cognitive-diversity argument does not attempt to *refute* SAA, it would, if compelling, plainly render it implausible, thereby saddling it with the burden of proof.

But is it compelling? Properly reconstructed, the argument is valid and has some unobjectionable premises. Among these are premise 6 (see reasons above) and premise 1, whose consequent spells out the constitutive thesis of SAA. Given that thesis, any inference rule would be epistemically justified just in case the rule accords with the reflective practices of a community of thinkers. Rules and instances come into accord as a result of a process of mutual adjustment whereby the instances of a certain rule that thinkers are inclined to accept upon

reflection support that rule, while those they are inclined to reject undermine it.[2] Now, premise 1, together with a suitable casting of premise 2 (that "cognitive diversity is either actual or possible"), entail skeptical conclusion 3. Whether or not 3 implies either cognitive ethnocentrism or extreme relativism, as claimed by premise 4, is a matter well beyond our concern here, so we'll concede 4. Together with 3, 4 would have the challenging consequences for SAA stated in 5, which boil down to the relativism noted in 7. But the argument would fail if previous claims were unsupported – as we think is the case with premise 2, the cognitive-diversity claim. So let's look at that next.

The relativism problem facing SAA rests crucially on that premise. But as stated, 2 fails to raise an interesting cognitive-diversity problem, since *different* inference rules that are (or can be) in accord with the reflective inferential practices of different communities could be equally good without this amounting to extreme relativism. Compare the so-called rules of deductive logic: surely many *different* rules of this sort can accord with the reflective deductive practices of different groups of thinkers. But the Goodmanian could accept that they are all equally good (i.e., correct) without fearing a relativism problem. What is required to raise the problem experimentalists have in mind is instead a cognitive-diversity claim involving, not merely different, but *rival* rules of inference. Consider, for example, an actual or possible scenario featuring two communities of thinkers, one with reflective inferential practices leading to the acceptance of certain inference rules, the other with similar practices leading to their rejection. More perspicuously construed, premise 2 reads

[2]For experimentalists (see note 1), reflective equilibrium amounts to the standard analytic attempt at justifying inference rules. First proposed by Goodman (1965: 66-67) for the

2.* It is actual, or at least possible, that some *rival* rules of inference accord with the reflective inferential practices of different communities of thinkers.

Now premise 2* has the potential of raising relativism troubles for SAA, but we think that the premise can be challenged. Putting aside possible *a priori* grounds that do not concern us here, we believe 2* is open to doubts on strictly empirical grounds of the very sort adduced by Stich *et al.* Here is why.

Let's consider first the factual cognitive-diversity thesis (FCD) that

There are rival rules of inference that accord with the reflective inferential practices of different communities of thinkers.

To support FCD, experimentalists invoke results from cross-cultural psychology, notably some surveys conducted by Richard Nisbett and collaborators to test alleged differences in East Asian and Western habits of reasoning.[3] Needless to say, those surveys would fail to ground FCD empirically if questions arise about their design or interpretation of data. But in fact both types of question do arise here. Problems of design notably include questions about the researchers' individuation criteria for tested groups. We learn from their reports that a certain hypothesis is tested on a single group (for instance, "East Asians") who later turn out to comprise more than one group, such as Chinese from China and Chinese Americans. Clearly, there would likely be levels of acculturation to be found among the latter, though not among the former. Parallel

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justification of deductive and inductive principles, the method is not without critics. See e.g. Siegel 1992.

[3] See Stich 1988: 574 and WNS 2001: 8-10. Empirical studies relevant to the experimentalist claims include, e.g., Nisbett 2003; Nisbett and Masuda (NM) 2003; Nisbett, Peng, Choi, and Norenzayan (NPCN) 2001; Norenzayan, Nisbett, Smith and Kim (NNSK) 1999; and Peng and Nisbett (PN) 1999.

questions arise for Westerners, first lumped together as a single group under that name, later identified as including only Americans of European descent. About the experimental subjects themselves we are told very little, beyond that they are "Koreans," "East Asians," and "European Americans," or "Chinese." All seem to be university students, some undergraduates, others graduates mostly at the universities of Michigan and Beijing. We're rarely told *how many* students were surveyed, and when we learn about this, the numbers are disappointingly small (an average of 30 subjects in each tested group).[4] We are rarely told how the subjects were selected, or whether the surveys were designed to isolate certain variables, such as socio-economic class and exposure to outside influences, that might have skewed the experimental results. Without more information, we must simply trust Nisbett and his collaborators to tell us what to make of it all.

Among questions of interpretation, some concern data which might suggest that different communities of thinkers use rival rules of inference. Experimental subjects from the two communities were presented with arguments of the same logical form but differing in the familiarity of the content of premises and conclusions, others offering statements in apparent opposition or featuring varying degrees of abstraction. Results seemed to show that Westerners were more inclined to reject one side of a contradiction and reason in ways that abstracted argument form from content and context, while East Asians were more likely, in their reasoning, to try to find the middle way by "transcending" contradictions (whatever that means) or somehow resolving them, and took more account of contextual factors and content. Both groups appeared

[4]See e.g., Nisbett 2003:169, 172, 173, 181. The same problem is evident in Nisbett and Masuda (NM). And although Peng and Nisbett (NP) 1999 provide more information, most of their studies

to have committed a similar number of reasoning errors (Nisbett 2003: 168-171).[5] The Asians also sometimes appeared to allow their feelings about the content of arguments to influence their judgment about the right conclusions to be drawn, while Westerners appeared to show more concern for logical consistency in their reasoning and were less apt to be distracted by content (Nisbett 2003:171-173). On the subject of contradiction, the thesis that Asians are more tolerant than Westerners was tested by, for example, asking experimental subjects to choose from among sets of proverbs: Asians seemed more comfortable with proverbs that appeared to say contradictory things, Westerners with proverbs that did not. In some cases, researchers admitted that results were possibly influenced by mere familiarity, so they administered more surveys in order to eliminate the "typicality effect."[6] The experimenters speculate that the supposed Eastern tolerance of contradiction has deep philosophical roots traceable to the ancient Chinese conception of "dialectical" reasoning – "...meaning that it focuses on contradictions and how to resolve them or transcend them or find the truth in both" (Nisbett 2003: 173-174).[7]

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involve an average of 32 people in each group, all students at a university, sometimes living together in the same housing facility.

[5]In another of their studies the same year, Westerners are said to have committed more errors (NM 2003: 11163).

[6] For example, Study 2 in PN's 1999, which attempts to avoid the typicality effect by presenting Asians and Westerners with Yiddish proverbs. According to the researchers, Asians were also more inclined to selected proverbs that involved, if not logical contradiction, some sort of tension between opposite sides.

[7] Nisbett is also given to making what are sometimes plainly overblown assertions about cultural divergence. Noting that Westerners tend to stick to the law of identity, while East Asians follow a "principle of holism," he makes the incredible claim that, as result of this, for the Chinese "[a] man is literally a different person in the family than in his role as a businessman..." (2003: 177). This claim is repeated in Nisbett and Masuda 2003.

In any case, the results from these surveys fall short of supporting a cognitive-diversity thesis such as FCD, since they have no bearing on whether *rival* inference rules in fact accord with the reflective inferential practices of the tested groups. Even Nisbett is, for the most part, guarded in his conclusion about this. He writes,

> "I have presented a large amount of evidence to the effect that Easterners and Westerners differ ... in the *inclination* to use rules, including the rules of formal logic" (emphasis ours, 2003: 189-190; but *cf.* NM 2003: 11163).

If sound, the evidence from Nisbett's work *might* show varying degrees of difference between Asians and Westerners in their *attitudes* toward certain rules. But it can hardly do more than that, since it is simply not clear from the data that the Asians' and Westerners' attitudes toward, for instance, contradiction, are really different. The experimenters themselves concede that, on the evidence, Easterners appear inclined to see a contradiction as something *to be gotten beyond* or *resolved* in some way, much as Westerners do. That is, both cultures seem equally to sense in contradiction (and in other sorts of conceptual conflict or incompatibility) something inherently unstable. We submit that the results from such field studies bring no substantive support to the claim that there is cognitive diversity of the sort needed to raise a relativism problem for SAA.[8]

Let's consider now the modal cognitive-diversity thesis (MCD) that,

> It is possible that rival rules of inference accord with the reflective inferential practices of different communities of thinkers.

[8]For other objections to Nisbett's interpretation of the data on reasoning, see e.g. Chan and Yan 2007, and Huss 2004.

Do the surveys by Nisbett and his collaborators support MCD? We believe that Goodmanians could remain agnostic about this, for the thesis alludes to the possibility of rival rules of inference's *objectively* being in accord with the reflective inferential practices of different communities of thinkers. It's one thing for an inference rule to be *believed* to be in accord with the reflective inferential practices of a community of thinkers, and quite another for it actually to *be* in accord with those practices. Call these "subjective" and "objective" reflective equilibrium (RE) about a set of inference rules. Clearly, objective RE and subjective RE need not coincide. Furthermore subjective RE does not entail objective RE. After all, a community of thinkers could *mistakenly* believe that some rules of inference accord with their reflective inferential practices when they do not. Thinkers might, unawares, neglect to consider instances of certain rules that they would find unacceptable upon consideration. Or they might be distracted by content, mistakenly failing to accept that some instances of inference are in accord with a rule they do accept upon reflection.

In fact, we can appeal to the same empirical evidence brought to our attention by experimentalists to contend that such subjective RE errors may be far from uncommon. Consider the typicality effect: Nisbett and his collaborators seem to have shown a subtle bias in reasoning that supposedly marks a difference between East Asians and Westerners (both groups appear to have it, though in different percentages). To test this effect, experimental subjects were presented with valid arguments about birds, some about eagles, others about penguins. Although all arguments were of exactly the same logical form, subjects were generally more inclined to accept entailment in typical cases (where the birds were eagles) than in non-typical ones (where they were penguins). Of course, training and reflection might reduce error in such cases – but even

then, there's no reason to think that a bias can be eliminated altogether. There are also studies by psychologists of the "heuristics and biases" tradition that point to biases such as overconfidence, which leads people overrate their own cognitive abilities.[9]

Of course, there is also abundant anecdotal evidence that it is possible to be mistaken in one's own reflective judgements about whether certain rules and their instances are in accord. Compare the parallel case of logicians struggling with complicated proofs: don't they sometimes reflectively believe themselves to have "deduced" conclusions from premises that in fact do not entail them? Similarly, chess players sometimes reflectively conclude that they have successfully checkmated their opponent, only to realize that are actually defeated themselves. And don't pianists sometimes reflectively "recollect" that a certain chord occurs at a certain bar in a composer's score when in fact it is not that chord, but a slightly different one?

In light of results from both empirical studies and anecdotal evidence, then, SAA can safely conclude that subjective RE may fail to coincide with objective RE. Therefore experimentalists have failed to support MCD: there is logical space for remaining agnostic about the possibility of rival inference rules' being in accord with the reflective inferential practices of different communities of thinkers. Thus, premise 2* of the cognitive-diversity argument fails. It follows that SAA is in the clear: there is no relativism problem for it.

III

We have argued that the experimentalists' objection to SAA from cognitive diversity cannot succeed. But what about their own positive account of reasoning, as developed, for

[9]See Stich 1990: 4-9; and Samuels and Stich 2004.

example, by Bishop and Trout (2005: 104 ff.)? How does it compare with standard analytic accounts? A central experimentalist contention is that traditional disciplines devoted to the study of reasoning, such as critical thinking, fail to attend to empirical evidence from cognitive science, thereby failing to be normative in any pragmatically interesting sense. In view of this, critical thinking needs to be replaced with a new, empirically-based discipline, which, for Bishop and Trout, would be 'applied epistemology,' a branch of philosophy of science. The chief function of this discipline, it appears, will be that of making recommendations for improving one's reasoning, all which must be based on evidence provided by an exclusively descriptive branch, 'ameliorative psychology.' On our view, it's plausible that informal logic might develop in new directions that would include substantial discussion of its normative aspects concerning the rules of inference – such as whether or not we should be consequentialist about them. These developments might turn out to be parallel to discussions in normative ethics that have produced a significant body of theories about consequentialism and its competitors.

But the study of reasoning that we envision will be a *normative*, not an *applied*, branch of epistemology. That is, normative epistemology (assuming it will take root and flourish), might be related to meta-epistemology much as normative ethics is related to meta-ethics. After all, in both cases there are first-order questions to be dealt with by a normative branch, but also meta-level questions involving related issues of ontology, semantics, and epistemology. The resulting picture seems more plausible to us than Bishop and Trout's applied epistemology, because we are skeptical about the possibility of a *single discipline* devoted to issuing prescriptions of the spectacularly diverse and varied sort they have in mind. Intended to be pragmatically useful in reasoning tasks, such prescriptions involve matters as different as advising on a tentative

diagnosis of a psychiatric patient (p. 16), estimating a person's ability to repay a loan, or deciding whether a prisoner up for parole is a threat to society ("on a stronger basis that flipping a coin," p. 17). But it is hard to see how there could be a single discipline capable of sanctioning all the various types of norms needed, given the infinite variety of different subjects addressed by human reasoning.[10]

Be that as it may, we do not dispute the experimentalists' complaint that critical thinking has not (for the most part) been conceived as related to epistemology (BT 2005: 7). But the proposed amendment is so drastic that it would eliminate whole topic-areas within informal logic and render others but a chapter of the cognitive sciences. Support for such amendment should be contingent upon experimentalism's success in their objection to standard analytic accounts – which in turn rests on the argument from cognitive diversity. If that argument is not compelling, then the burden of proof remains on the experimentalist challenger. Here we have shown that the argument is not compelling.

[10]What would the generalizations of applied epistemology be? And is there any reason to think that if that discipline develops, it would replace or eliminate meta-epistemology? Here is Sosa's (2006: 10) negative answer, "Such casuistry would encompass all the manuals for all the various instruments and how to read all the various gauges, for one thing. And it would also include the variegated practical lore on how to tell what's what and on what basis: the lore of navigation, jungle guidance, farming tips, and so on and so forth. That is all of course extremely useful, but it is no part of the traditional problematic of epistemology. Nor is there any reason to replace either of epistemic casuistry or traditional epistemology with the other. Each has its own time and place."

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