

Notes and References

- 1 'Cognitive state' is Stich's term for belief-like information-storing mental states, while 'cognitive processes' is his "cover term whose extension includes our own reasoning processes, the updating of our beliefs as a result of perception, and the more or less similar processes that occur in other organisms." See p. 571 of his "Reflective Equilibrium, Analytic Epistemology, and the Problem of Cognitive Diversity," *Synthese* 74 (1988): 391–413; the references here are to its reprinting in E. Sosa and J. Kim (eds.), *Epistemology: An Anthology* (Blackwell, 2000), pp. 571–83. Parenthetical references in the main text will be to this publication. My own preference is to stretch the terms 'belief' and 'reasoning' to cover these extensions. So the beliefs and reasonings to be discussed in what follows count as such in correspondingly broad senses. *Reasoning*, for example, is a "cognitive process" that bases a *belief* or "cognitive state" on *reasons*, i.e., on other mental states to which the subject gives some weight, pro or con, in forming or sustaining that belief. 'Basing', finally, is quasi-technical. Ordinarily we would speak more naturally of acting *for* a reason, or of being angry or in some other emotional state *for* a reason. Decisions and beliefs are naturally said to be based on reasons, however, and I am here extending the use of that terminology to cover all cases in which one is in a mental state *for* a reason, or for some reasons. (And it is better yet to speak of one's being in that mental state for a reason *that then* "motivates" one to be in that state. This is to distinguish the case of interest from that in which one is in the state in question "for a reason" only in the sense that "there is" a reason why one is in that state, though it is not a reason that one *has*, much less one that *motivates* one to be in that state.)
- 2 It is a great pleasure for me to contribute to this well-deserved tribute to Steve Stich, long-time colleague and true friend, and iconoclast of analytic epistemology. Here I engage only one of the challenges in his stimulating and influential work.
- 3 In *The Philosophy of Alvin Goldman*, in a special issue of *Philosophical Topics*, ed. Christopher S. Hill, Hilary Kornblith, and Tom Senor, Vol. 29, Nos. 1 and 2; pp. 429–61. (Parenthetical page references in the main text will now be to this article.)
- 4 "Meta-skepticism: Meditations in Ethno-epistemology," in S. Luper (ed.), *The Sceptics* (Ashgate, 2003), with a different order of authors, now listed as Nichols, Stich, and Weinberg.
- 5 *Ibid.*, p. 245.
- 6 This leaves open the possibility of a broader concern with the *kind* of knowledge we should seek in a good life. Wisdom might be one such, something closely connected with how to live well, individually and collectively. Another such might be a world view that provides deep and broad understanding of major departments of proper human curiosity, which of course cries out for an account of what makes curiosity proper.

Reflections on Cognitive and Epistemic Diversity: Can a Stich in Time Save Quine?

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In "Epistemology Naturalized," Quine famously suggests that epistemology, properly understood, "simply falls into place as a chapter of psychology and hence of natural science" (1969: 82). Since the appearance of Quine's seminal article, virtually every epistemologist, including the later Quine (1986: 664), has repudiated the idea that a normative discipline like epistemology could be reduced to a purely descriptive discipline like psychology. Working epistemologists no longer take Quine's vision in "Epistemology Naturalized" seriously. In this chapter, I will explain why I think this is a mistake.

In the 1980s and early 1990s, Stephen Stich published a number of works that criticized analytic epistemology and defended a pragmatic view of cognitive assessment (1985, 1987, 1990, 1993). In the past five years, Stich, Jonathan Weinberg, and Shaun Nichols (henceforth, WNS) have put forward a number of empirically based arguments criticizing epistemology in the analytic tradition (Weinberg, Nichols and Stich 2001; Nichols, Stich and Weinberg 2003). My thesis is that the most powerful features of Stich's epistemological views vindicate Quine's now moribund naturalism. I expect this thesis to be met with incredulity – not least from Stich, who has explicitly argued that the reductionist view standardly attributed to Quine is a non-starter (1993: 3–5).

The chapter will proceed as follows. In section 1, I interpret Stich's epistemology in a way that provides a *prima facie* vindication of Quine's naturalism. In section 2, I take a slight detour to consider and reply to a family of arguments that aim to show that giving up the analytic project in the way Stich suggests is ultimately self-defeating. In section 3, I propose a reliabilist approach to epistemic evaluation that I argue is superior on pragmatic grounds to Stich's view. In the final section, I bring the various threads together and suggest that the sturdiest elements of Stich's epistemology drive us to the vision Quine advocated in "Epistemology Naturalized."

1 Stich's Epistemology

At its heart, Stich's view consists of two empirical hypotheses.

- 1 *Cognitive diversity*: There are significant and systematic differences in how different people reason about the world.
- 2 *Epistemic diversity*: There are significant and systematic differences in the epistemic concepts, judgments, and practices different people employ in evaluating cognition.

Cognitive diversity (the subject of 1.1) raises what is, for Stich, the fundamental problem of epistemology: How are we to evaluate the various ways different people can and do reason? And epistemic diversity (the subject of 1.2) undermines the most popular method philosophers have used to solve that problem: by developing theories that capture our commonsense epistemic judgments (i.e., our epistemic intuitions). But if conceptual analysis, the analysis of our epistemological concepts, cannot deliver a solution to the fundamental problem of epistemology, what's left? Stich's epistemological writings suggest two alternative approaches (the subject of 1.3). One approach is explicitly argued for in Stich's earlier writings, and the other is implicitly suggested in the later, co-authored pieces. While I will argue that there may be some tension in these approaches, they both vindicate Quine.

1.1 *The challenge of cognitive diversity*

Cognitive diversity holds that there are significant and systematic differences in how different people reason about the world. In the earlier pieces, Stich does not offer much empirical evidence for thinking that cognitive diversity is true. In *Fragmentation*, for example, Stich tries "to make the world safe for irrationality" by criticizing two arguments against cognitive diversity (1990: 17). I will not pursue these defensive maneuvers because in the 15 years since *Fragmentation*, psychologists have amassed a good deal of evidence in support of cognitive diversity. Richard Nisbett and his colleagues have identified some significant differences in the thought patterns of East Asians (Chinese, Japanese and Koreans) and non-Asian Westerners (from the US and Europe) (Nisbett, Peng, Choi and Norenzayan 2001; Nisbett 2003). The reasoning of Westerners tends to be more analytic, "involving detachment of the object from its context, a tendency to focus on attributes of the object to assign it to categories, and a preference for using rules about the categories to explain and predict the object's behavior. Inferences rest in part on the practice of decontextualizing structure from content, the use of formal logic, and avoidance of contradiction." The reasoning of East Asians tends to be more holistic,

involving an orientation to the context or field as a whole, including attention to relationships between a focal object and the field, and a preference for explaining and predicting events on the basis of such relationships. Holistic approaches rely on experience-based knowledge rather than on abstract logic and are dialectical, meaning that there is an emphasis on

change, a recognition of contradiction and of the need for multiple perspectives, and a search for the "Middle Way" between opposing propositions. (Nisbett et al. 2001: 293)

A few examples will help make this distinction concrete.

In the "Michigan Fish" study, Japanese and American subjects viewed animated underwater scenes and then reported what they had seen (Masuda and Nisbett 2001). The first statement by Americans usually referred to the fish, while the first statement by Japanese usually referred to background elements, e.g., "There was a lake or a pond." The Japanese made about 70 percent more statements than Americans about background aspects of the environment, and 100 percent more statements about relationships with inanimate aspects of the environment, e.g., "A big fish swam past some gray seaweed" (Nisbett et al. 2001: 297). In this study, the Westerners subjects focused on objects detached from their background, while the Japanese subjects focused on the context and the relationships between objects in the field. Referring to this study, Nisbett has joked that for Westerners, if it doesn't move, it doesn't exist.

In another fascinating study (Peng and Nisbett 1999), American and Chinese subjects were shown conflicting statements, such as:

- A A survey found that older inmates are more likely to be ones who are serving long sentences because they have committed severely violent crimes. The authors concluded that they should be held in prison even in the cases of a prison population crisis.
- B A report on the prison overcrowding issue suggests that older inmates are less likely to commit new crimes. Therefore, if there is a prison population crisis, they should be released first.

Subjects were asked to rate the plausibility of one or both of these claims. For each pair of statements studied, American and Chinese subjects agreed about which was more plausible (in this case, A). Who gives higher plausibility ratings to A, subjects who rate A by itself, or subjects who rate both A and B together? The answer is different for American and Chinese subjects. American subjects gave higher plausibility ratings to A when they rated both A and B, while Chinese subjects gave higher plausibility ratings to A when they rated it by itself. In the face of conflicting claims, American subjects become more polarized while Chinese subjects become less polarized (Nisbett et al. 2001: 302). East Asians tend to avail themselves of a "Middle Way" whereas Westerners tend to insist on "My Way."

After reviewing the evidence for cognitive diversity, Nisbett and his colleagues conclude that "literally different cognitive processes are often invoked by East Asians and Westerners dealing with the same problem" (Nisbett et al. 2001: 305). Nisbett argues that these cognitive differences are explained by some deep and long-standing cultural differences between East Asian and Western societies (Nisbett 2003: chs. 1-3). In support of this hypothesis, Nisbett and his colleagues note that "Asians move radically in an American direction after a generation or less in the United States" (Nisbett et al. 2001: 307). A fair reading of the literature suggests that Stich's defense of cognitive diversity was prescient.

Why does cognitive diversity matter to epistemology? "It is the prospect of cognitive diversity among normal folk that lends a genuine, almost existential, urgency to the project of cognitive evaluation" (1990: 74). Cognitive diversity motivates a project Stich sees as fundamental to epistemology: How to evaluate the various ways different people can and do reason. Most contemporary epistemology focuses on defending theories and theses about epistemological categories that apply to individual belief tokens (i.e., knowledge or justification). But a theory that assesses how different people reason will not focus on the evaluation of belief tokens. Instead, it

focuses on the evaluation of *methods* of inquiry. It tries to say which *ways* of going about the quest for knowledge – which *ways* of building and rebuilding one's doxastic house – are the good ones, which the bad ones, and why. Since reasoning is central to the quest for knowledge, the evaluation of various *strategies of reasoning* often plays a major role in the assessment of inquiry. (1990: 1, emphases added)

As Stich notes, this project has been pursued by Bacon, Descartes, Mill, Carnap and Popper, among others. But it differs substantially from the projects pursued by almost all contemporary epistemologists. Rather than provide a theory for the evaluation of belief tokens, Stich aims to provide a way to evaluate *methods* of inquiry, *ways* of acquiring and revising beliefs, reasoning *strategies*.^{1,2}

1.2 Epistemic diversity

Epistemic diversity holds that there are significant and systematic differences in the epistemic concepts, judgments, and practices different people employ in evaluating cognition. The early arguments do not offer much in the way of evidence for epistemic diversity. In *Fragmentation*, for example, Stich raises the possibility of epistemic diversity in order to criticize the analytic approach to epistemology. The main aim of the later arguments is to proffer empirical evidence for epistemic diversity. In a typical study, WNS gave the following Gettier-style example to a group of Western subjects and non-Western subjects:

Bob has a friend, Jill, who has driven a Buick for many years. Bob therefore thinks that Jill drives an American car. He is not aware, however, that her Buick has recently been stolen, and he is also not aware that Jill has replaced it with a Pontiac, which is a different kind of American car. Does Bob really know that Jill drives an American car, or does he only believe it?

REALLY KNOWS ONLY BELIEVES

A large majority of Western subjects gave the answer sanctioned by analytic epistemologists ("only believes") but a majority of East Asians and a majority of subjects from India gave the opposite answer ("really knows") (2001: 443). WNS also found cases in which there were significant differences between the epistemic judgments of people of high socioeconomic status (SES) and of low SES (2001: 447–8).

WNS emphasize that they did not merely find random variation in people's epistemic judgments across cultures. The cognitive differences that Nisbett and his colleagues found between East Asians and Westerners are reflected in WNS's epistemic diversity findings. This concision suggests some pretty deep differences in how people in different cultures evaluate reasoning. WNS argue that

the differences between Ws [Westerners] and EAs [East Asians] look to be both systematic and explainable. EAs and Ws appear to be sensitive to different features of the situation, different *epistemic vectors*, as we call them. EAs are much more sensitive to communitarian factors, while Ws respond to more individualistic ones. Moreover, Nisbett and his colleagues have given us good reason to think that these kinds of differences can be traced to deep and important differences in EA and W cognition . . . What our studies point to, then, is more than just divergent epistemic intuitions across groups; the studies point to divergent epistemic concerns – concerns which appear to differ along a variety of dimensions. (Weinberg, Nichols and Stich 2001: 451)

In *Fragmentation*, Stich laid a bet that the epistemic diversity thesis is true. This increasingly coherent body of evidence gives us some reason to think that Stich may well win that bet. Of course, one might reasonably raise concerns about these studies, and defenders of the analytic tradition in epistemology have already done so (e.g., Sosa, this volume). In my view, the best way to rebut WNS's empirical case for epistemic diversity is with more empirical findings. Absent contrary findings, I propose to provisionally accept that people in different cultures evaluate cognition in at least somewhat different ways.

1.3 Two arguments against analytic epistemology

For an epistemological theory to be genuinely prescriptive, for it to earn its normative keep, it must explain why some ways of thinking are better than others. Theories of analytic epistemology seek "criteria of cognitive evaluation in the analysis or explication of our ordinary concepts of epistemic evaluation" (1990: 19). According to WNS, contemporary epistemology assumes that knowledge of the correct epistemic norms is implanted in us and we can discover them by an appropriate process of self-exploration. The analytic philosopher's method for testing and developing epistemological theories, which they dub "Intuition Driven Romanticism," involves three features: (1) It takes epistemic intuitions (spontaneous judgments about the epistemic properties of cases) as inputs; (2) it produces epistemic claims or principles as output; and (3) the output is a function of the input – in the sense that significantly different inputs would yield significantly different outputs (Weinberg, Nichols and Stich 2001: 432).

How does epistemic diversity undermine the method of analytic epistemology? The main early argument is framed in terms of what we *intrinsically value*, what we value for its own sake.

other languages and other cultures certainly could and probably do invoke concepts of cognitive evaluation that are significantly different from our own . . . For many people –

certainly for me – the fact that a cognitive process is sanctioned by the venerable standards embedded in our language of epistemic evaluation, or that it is sanctioned by the equally venerable standards embedded in some quite different language, is no more reason to value it than the fact that it is sanctioned by the standards of a religious tradition or an ancient text, unless, of course, it can be shown that those standards correlate with something more generally valued . . . (Stich 1990: 94)

Stich's contention is that when we "view the matter clearly, most people will not find it intrinsically valuable to have cognitive states or to invoke cognitive processes that are sanctioned by the evaluative notions embedded in ordinary language" (1990: 93). Unless we are inclined toward epistemic chauvinism or xenophobia – preferring our epistemic standards merely because they are our own – we are unlikely to value cognitive states or processes merely because they accord with the contingent and idiosyncratic standards implicit in our epistemic concepts.³ Of course, if that's true, then most of us will not intrinsically value beliefs and belief-forming processes because they are endorsed by *somebody else's* intuitions or practices of epistemic evaluation, either. In *Fragmentation*, the objection to analytic epistemology is that it is incapable of solving the fundamental problem of epistemology – how to evaluate the various ways different people reason. Its basic mistake is to evaluate such matters on the basis of our idiosyncratic, culturally specific epistemic concepts, judgments, intuitions, and practices.⁴

There is a second argument against analytic epistemology that is implicit in the later writings. Analytic epistemology seeks theories that aim to capture our intuitive judgments about whether a belief is justified or whether S knows that *p*. Jaegwon Kim gives clear expression to this desideratum: "It is expected to turn out that according to the criteria of justified belief we come to accept, we know, or are justified in believing, pretty much what we reflectively think we know or are entitled to believe" (1988: 382). Of course, in order to clarify and regiment our epistemological judgments, a successful theory might require some minor revision to some of our reflective epistemic judgments. But other things being equal, a theory that captures our reflective epistemic judgments is better than one that does not. J. D. Trout and I (2005a, 2005b) have called this *the stasis requirement*: To be correct, theories of analytic epistemology must "leave our epistemic situation largely unchanged" (Kim 1988: 382).⁵ WNS pointedly suggest that when we couple epistemic diversity with the stasis requirement, the resulting image of analytic epistemology is not of a universal normative theory, but of ethnography:

Our data indicate that when epistemologists advert to "our" intuitions when attempting to characterize epistemic concepts or draw normative conclusions, they are engaged in a culturally local endeavor – what we might think of as *ethno-epistemology* . . . [I]t is difficult to see why a process [Intuition Driven Romanticism] that relies heavily on epistemic intuitions that are local to one's own cultural and socioeconomic group would lead to genuinely normative conclusions. Pending a detailed response to this problem, we think that the best reaction to the high-SES [socioeconomic status] Western philosophy professor who tries to draw normative conclusions from the facts about "our" intuitions is to ask: What do you mean by "we"? (Weinberg, Nichols and Stich 2001: 454–5)

To identify the core of analytic epistemology with ethnography is to suggest that it is empirical. Of course, analytic epistemology is not entirely empirical. Analytic philosophers extract lots of normative, epistemological claims from their descriptive, ethnographic theories. If this is right, then the method of contemporary analytic epistemology has been broadly Quinean all along. It's just that rather than starting with psychology, analytic epistemology starts with anthropology.

This interpretation of analytic epistemology immediately raises alarms about whether it is deriving 'ought's from 'is's. This is rather awkward, since this is precisely the charge analytic epistemologists have repeatedly made against naturalistic approaches to epistemology (e.g., BonJour 2002; Feldman 2003; Plantinga 1993; Williams 2001). It raises the possibility that analytic epistemologists have been throwing stones at naturalized epistemology from glass houses. But WNS don't raise this worry. (Bishop and Trout 2005b, however, do.) Instead, the critique implicit in the above passage is that, as an empirical endeavor, analytic epistemology is bad science. Analytic epistemology relies heavily on the introspective judgments of a relatively small group of idiosyncratic folks (i.e., professional philosophers) to deliver empirical results about what "we" think about certain issues. As such, it ignores well-understood and widely accepted scientific methods that allow us to effectively investigate and discover what some group thinks about a subject.

1.4 Pragmatism and experimental philosophy

We have considered two arguments against analytic epistemology – the early argument from intrinsic value and the later argument (co-authored with Nichols and Weinberg) from bad science. The first argument says that as an attempt to solve the fundamental problem of epistemology, analytic epistemologists are investigating the wrong parts of the world. The second argument says that analytic epistemology uses empirically dubious methods. In a nutshell, the problem with analytic epistemology is that it searches for answers in the wrong place and in the wrong way. To fix these problems, Stich must tell us where to look for answers (1.4.1) and how to look for answers (1.4.2). I will argue (1.4.3) that there might be some tension in the fixes Stich offers.

1.4.1 Where to look for answers: pragmatism Analytic epistemology attempts to solve the fundamental epistemological problem raised by cognitive diversity – how to properly evaluate the various ways different people can and do reason – in terms of considerations most of us do not, upon reflection, value. Stich proposes the obvious fix: "One system of cognitive mechanisms is preferable to another if, in using it, we are more likely to achieve those things that we intrinsically value" (1990: 24).

In evaluating systems of cognitive processes, the system to be preferred is the one that would be most likely to achieve those things that are intrinsically valued by the person whose interests are relevant to the purposes of the evaluation. In most cases, the relevant person will be the one who is or might be using the system. So, for example, if the issue at hand is the evaluation of Smith's system of cognitive processes in comparison with some

actual or hypothetical alternative, the system that comes out higher on the pragmatist account of cognitive evaluation is the one that is most likely to lead to the things that Smith finds intrinsically valuable. (1990: 131–2)

As opposed to the evaluations that come out of analytic epistemology, Stich notes that “there is no mystery about why [we] should care about the outcome of [the pragmatic] evaluation” (1990: 132).

For Stich, pragmatism leads directly to relativism – the idea that the cognitive evaluation of a set of reasoning strategies will be highly sensitive to facts about the person (or group) using those strategies. The pragmatic view of cognitive evaluation is relativistic for two reasons. Most obviously, it is sensitive to what reasoners intrinsically value. If there are significant differences in what different people intrinsically value, then we should expect a pragmatic view to recommend quite different cognitive systems to different people (Stich 1990: 136). The second reason a pragmatic account is relativistic is that it will be sensitive to a reasoner’s environment. A set of reasoning strategies that yields the best expected consequences in one environment might not do so in a different environment (Stich 1990: 136–40).

There is, in the pragmatist tradition, a certain tendency to down play or even deny the epistemic relativism to which pragmatism leads. But on my view this failure of nerve is a great mistake. Relativism in the evaluation of reasoning strategies is no more worrisome than relativism in the evaluation of diets or investment strategies or exercise programmes. The fact that different strategies of reasoning may be good for different people is a fact of life that pragmatists should accept with equanimity. (1993: 9)

Relativism is not the bogeyman it’s cracked up to be.

A natural criticism of Stich’s view would be that he has unnecessarily run together two quite different normative domains – the epistemic and the pragmatic. The complaint might best be put by insisting, “But Stich hasn’t provided an account of *epistemic* evaluation” (table-pounding for emphasis is optional). In reply to this objection, Stich embraces “the very Jamesian contention that *there are no intrinsic epistemic virtues*” (1990: 24). “For pragmatists, there are *no* special cognitive or epistemological values. There are just *values*” (1993: 9). So although one might call Stich an “epistemic pragmatist” or an “epistemic relativist,” these locutions can lead to misunderstanding. Everything one intrinsically values is relevant to one’s evaluative judgments, whether those judgments are (intuitively) moral, esthetic, epistemic, etc. Stich is a pluralist about a great many things, but when it comes to normative, evaluative matters, he is a methodological monist. Regardless of the item one is evaluating, the evaluative considerations that arise are the same: What is most likely to bring about those things one intrinsically values?

1.4.2 How to look for answers: experimental philosophy WNS allege that a central element of analytic epistemology is empirical. As such, it is bad science. A natural way to fix this problem is to replace this bad science with good science. This is a plausible way

to understand the motivation behind *experimental philosophy*. Experimental philosophy involves “using experimental methods to figure out what people really think about particular hypothetical cases” (Knobe forthcoming). It is easy to see how the method of experimental philosophy might be extended. By getting clear about the patterns of people’s judgments across different cultures and socioeconomic classes, experimental philosophers can offer well-grounded hypotheses about the nature of the psychological mechanisms that subservise these judgments (see, e.g., Nichols 2004).

While I am in no position to limn the historical details, Stich seems to be a reasonably central figure in the development of this movement. *Fragmentation* provides a powerful intellectual justification for the empirically respectable study of what people actually think about philosophical matters. Further, as far as I can tell, WNS (2001) authored some of the earliest examples of experimental philosophy (some of which we reviewed in section 1.2). The experimental method in philosophy is growing in popularity. It is being used in epistemology, as we have seen, but also in debates about ethics (Nichols 2004), free will (Nahmias, Nadelhoffer, Morris and Turner 2005), action theory (Knobe 2003), and philosophy of language (Machery, Mallon, Nichols and Stich 2004).

1.4.3 A normatively modest approach to experimental philosophy? Whatever the merits of experimental philosophy, and I think there are many, Stich (or at least, the Stich of *Fragmentation*) is committed to the view that experimental philosophy is incapable of solving the fundamental epistemological problem raised by cognitive diversity. Experimental philosophy is victimized by the same objection that defeated analytic epistemology. Recall that Stich argues that the problem with analytic epistemology is that it tries to evaluate different ways of reasoning by appealing to our parochial, idiosyncratic epistemic concepts, judgments and intuitions. Since we don’t intrinsically value cognitive states or processes that are sanctioned by our – or *anybody else’s* – hidebound epistemic practices, a description of those practices can’t serve as the basis for a legitimate evaluation of different ways of reasoning. The fact that analytic epistemology is also bad science merely adds insult to injury. Experimental philosophy avoids the insult, but not the injury. Instead of focusing on the wrong things in the wrong way, experimental philosophy focuses on the wrong things in the right way. So as an attempt to solve the fundamental epistemological problem, the Stich of *Fragmentation* would have to conclude that experimental philosophy is the wrong tool for the job.

The experimental philosopher has lots of potential responses to this argument. One possibility is to adopt a normatively modest view of experimental philosophy, according to which experimental philosophy is entirely descriptive. So when it comes to normative philosophical disciplines, experimental philosophy is ancillary. It can confirm or disconfirm the empirical assumptions or empirical implications of genuinely normative philosophical theories. But that’s it. Of course, there is plenty of positive descriptive knowledge experimental philosophy has to contribute. But a brief survey of some experimental philosophy gives some support to the normatively modest view. WNS’s epistemological writings fit the typical disconfirmatory mold. They argue that, to be fruitful, the method of analytic epistemology requires the truth of an empirical assumption: there is

considerable similarity in different people's epistemic intuitions. WNS then attempt to undermine the legitimacy of this method by showing that the empirical assumption on which it relies is false. Recent examples of experimental philosophy applied to morality seem to further confirm its essentially ancillary normative functions. Shaun Nichols (2004) offers an account of the psychological mechanisms that subserve our moral judgments. Nichols uses this account to criticize a major argument in favor of moral realism – namely, that the best explanation of the historical trend “towards increasingly inclusive and nonviolent norms” is that people are coming to better and deeper understanding of real moral properties or values (149). Nichols argues that his psychological account of how we make “core” moral judgments does a better job of explaining this trend than moral realism (2004: ch. 7). In a pair of recent papers, Doris and Stich (2005) and Machery, Kelly and Stich (2005) have suggested that moral realism is challenged by the apparent existence of fundamental moral disagreements. Experimental philosophy has much to teach us about the nature and structure of the human mind. It promises to put some psychology into moral psychology. And this is all to the good. But *prima facie*, it seems incapable of offering up positive normative theories or principles.

There is not much textual evidence for the normatively modest view of experimental philosophy in WNS's epistemological writings. In fact, there are hints that there might be a legitimate role for epistemic intuitions in our epistemological theorizing.

For polemical purposes we have been emphasizing the diversity of epistemic intuitions in different ethnic and SES groups . . . But we certainly do not mean to suggest that epistemic intuitions are completely malleable or that there are no constraints on the sorts of epistemic intuitions that might be found in different social groups. Indeed, the fact that subjects from all the groups we studied agreed in not classifying beliefs based on “special feelings” as knowledge suggests that there may well be a universal core to “folk epistemology.” Whether or not this conjecture is true and, if it is, how this common core is best characterized, are questions that will require a great deal more research. (WNS 2001: 450)

Perhaps there is some “universal core” of our epistemic practices that can be unearthed by experimental philosophy and that should be taken seriously in our epistemological theorizing (see also Nichols, Stich and Weinberg 2003). This core might involve some set of epistemic intuitions, judgments, or (perhaps more plausibly) it might involve some set of general psychological mechanisms that subserve our epistemic judgments and intuitions. Of course, it is an entirely empirical issue whether such a core exists. But if there is a robust psychological foundation for our epistemic practices, then there may be good reasons – including *good pragmatic reasons* – to take this core seriously in our epistemological theorizing. After all, the costs involved in significantly revising our epistemic practices might overwhelm whatever benefits might accrue from such a revision. Prohibitive start-up costs for new alternatives can be a powerful consideration in favor of the status quo (Sklar 1975; Harman 1986; Bishop and Trout 2005a). There is much more to say about what can be legitimately extracted from experimental philosophy. But for now, I will simply point out the *prima facie* tension between Stich's views in *Fragmentation* and an experimental approach to philosophy that has positive normative aspirations.

2 A Brief Digression: The Role of Intuitions in Epistemology

It is perhaps appropriate to linger a bit over the role of intuitions in epistemology. Epistemic intuitions are usually taken to be our non-discursive, though perhaps considered, judgments about the epistemic properties of some cognitive item (Cohen 1981; Bealer 1987; Bonjour 1998; Pust 2000). The paradigm example of an epistemic intuition is our judgment that subjects in Gettier cases do not have knowledge. For many analytic epistemologists, epistemic intuitions are taken to be basic sources of evidence.

An adequate reconstruction of philosophical methodology here requires a two-step evidential route. In the first step, the occurrence of an intuition that *p*, either an intuition of one's own or that of an informant, is taken as (*prima facie*) evidence for the truth of *p* (or the truth of a closely related proposition). In the second step, the truth of *p* is used as positive or negative evidence for the truth of a general theory. (Goldman and Pust 1998: 182)

So intuitions are a basic evidential source if and only if the intuition that *p* (when formed in favorable circumstances) provides *prima facie* evidence that *p* is true (Goldman and Pust 1998: 182–3; see also Bealer 1998: 204–7). To say that intuitions are a *basic* source of evidence is to say that any (properly formed) intuition that *p* is *by the very fact of its being a (properly formed) intuition* *prima facie* evidence of the truth that *p*.

What happens if we deny that intuitions are a basic evidential source? Some philosophers have argued that a deep skepticism about epistemic intuitions is not viable (Bealer 1993, 1998; Jackson 1998; Kaplan 1994; Siegel 1984) and that it leads to “intellectual suicide” (Bonjour 1998: 99). The basic idea behind these arguments is that we cannot accept beliefs on the basis of good reasons without relying, at some point, on our epistemic intuitions. How could we start believing anything if we never accepted a belief that *p* on the basis of evidence *e* simply because it is intuitively obvious that evidence *e* licenses the belief that *p*? There is certainly room to criticize these arguments but, from my perspective, the best reply is strategic capitulation. Let's distinguish two sorts of epistemological theories.

- 1 An ethno-epistemological theory that aims to capture our epistemic intuitions (with perhaps some light revisions in the service of power or clarity).
- 2 A genuinely prescriptive theory with normative force that will enable us to legitimately evaluate the various ways different people can and do reason.

The first sort of theory might also be a theory of the second sort; but as Stich has stressed, this will require some argument. Now, are intuitions basic sources of evidence? This is a poorly framed question. We need to ask: Evidence for *what*? Our olfactory beliefs might be a basic evidential source, but they're not legitimate evidence for theories of logic. They might play a legitimate, though minor, evidential role in the natural sciences; for example, the belief that there is an acrid burning smell might be a legitimate part of our evidence for thinking that an experiment has gone awry (or not gone awry!). Now suppose we want an epistemological theory of the first sort, one that satisfies the

stasis requirement by (in Kim's words) leaving "our epistemic situation largely unchanged." It is hard to see how our epistemic intuitions could *not* be a legitimate source of evidence. They are, after all, what such theories are trying to capture. But what if we want a theory of the second sort, a theory with genuine normative bite? It's not so obvious that our epistemic intuitions should be a significant source of evidence. This does not imply that our epistemic intuitions never have any normative force. Properly understood, the defenders of intuition may be right that we have no choice but to trust our epistemic intuitions (maybe often). But plenty of philosophers who have reservations about the role of intuitions in our philosophical theorizing have granted this point (e.g., Kornblith 2002: 5; Devitt 1994: 564). I am certainly prepared to grant that our epistemological intuitions have directed our reasoning well enough to have a reasonable claim to some normative legitimacy. So I don't believe there is a case to make that our intuitions are *in principle* irrelevant evidence for or against a genuinely prescriptive epistemological theory. (And as far as I know, no one, including Stich, has made such a case.)

We need our intuitions to get on in the world; and perhaps having an epistemic intuition that reasoning strategy *R* is the epistemically best way to reason may be *prima facie* evidence for thinking that, in fact, reasoning strategy *R* is the epistemically best way to reason. But it doesn't follow that our epistemic intuitions are (or must be) the primary source of evidence for our epistemological theorizing. We can draw an illuminating analogy with our physical intuitions (our immediate judgments about the behavior of physical objects).⁶ We would not long survive without trusting our physical intuitions. If someone genuinely stopped trusting his physical intuitions, we would worry that he was trying to commit actual suicide rather than just intellectual suicide. Besides their pragmatic importance, our physical intuitions are almost surely essential to our theorizing about the nature of the physical world. If physicists could never rely on their physical intuitions (about, e.g., whether some pointer has come to rest at zero or how, roughly, an object will fall), it's doubtful they would get far in their theorizing. So our physical intuitions are "basic evidence" in the sense given above: having a physical intuition that *p* is *prima facie* evidence that *p* is true. In fact, we know that our physical intuitions are often true, but we also know quite a bit about their systematic failures (Clement 1982; Halloun and Hestenes 1985). So even though a deep skepticism about our physical intuitions would be catastrophic on practical and theoretical grounds, it would be grotesque to suggest that we must therefore trust our physical intuitions to play a significant evidential role in the testing and development of our physical theories. As far as I can tell, the analogous point is all Stich needs. Those of us who are skeptical about our epistemic intuitions are denying that they ought to play a significant role in epistemological theorizing, an activity engaged in by a tiny fraction of the world's population and even then only occasionally. This highly limited pessimism about our intuitions is perfectly coherent and, in fact, is consistent with the claim that intuitions are a basic source of evidence (as characterized above). Giving up epistemic intuitions in our epistemological theorizing does not lead to intellectual catastrophe.

The case for a circumscribed skepticism about our intuitions is incomplete. I have granted that our intuitions have *some* *prima facie* normative force. So whether it is reasonable to ignore them in our epistemological theorizing depends on whether we have

an evidential source that is better than our intuitions. The physics analogy is apt. If we don't have some source of evidence that is better than our physical intuitions and if our physical intuitions are reasonably reliable, then the best we can do is appeal to our physical intuitions in constructing our physical theories. Our resulting theories might be disappointing. But cooks are only as good as their ingredients. So what alternatives do we have to evaluating epistemological theories in terms of how well they capture our epistemic intuitions? Well, at least a few. Stich has offered a pragmatic alternative in *Fragmentation*: Epistemological theories, principles and judgments are to be evaluated in terms of how well they promote what we intrinsically value. Some experimental philosophers might offer a different alternative: Epistemological theories, principles and judgments are to be evaluated in terms of how well they capture a universal "core" of people's epistemic practices (as described by a scientifically respectable account of those practices). J. D. Trout and I (2005a, 2005b) have offered yet a third alternative: Epistemological theories, principles and judgments are to be evaluated in terms of how well they capture the normative judgments of "Ameliorative Psychology" – those areas of psychology that for more than 50 years have been extremely successful in giving us useful advice about how to reason about matters of great practical significance (e.g., whether a convict is likely to be violent if paroled, whether a heart-attack victim is at risk of dying within the next month, whether a newborn is at risk for Sudden Infant Death Syndrome, whether a Bordeaux wine will be any good when it matures).⁷ These are all genuine options for epistemology. And by firmly grounding epistemology in science, they are all animated by the spirit of Quine's naturalism.

3 A Critique: The Pragmatic Virtues of Reliabilism

What if we evaluated different ways of reasoning in terms of their tendency to deliver true beliefs? Against this suggestion, Stich has argued that "once we have a clear view of the matter, most of us will not find any value, either intrinsic or instrumental, in having true beliefs" (1990: 101). In 3.1, I will spell out this argument, surely one of the more shocking positions in the Stich oeuvre. In 3.2, I will argue that, despite the power of Stich's case against truth, we are naturally drawn to true belief even when it is against our interests to do so. In 3.3, I use our default attraction to true belief to try to justify on pragmatic grounds a reliabilist theory of cognitive evaluation.

3.1 *Against truth*

The heavy work in Stich's argument against the value of truth involves spelling out in considerable detail what is involved in having true beliefs (1990: 101–18). The crux of the story involves an account of the interpretation function, which maps belief tokens onto entities that can be true or false (e.g., propositions, truth conditions, sets of possible worlds). So the interpretation function maps a particular brain state of mine onto the proposition <Lincoln was a great president>. According to that interpretation function,

that mental state (we'll suppose it's a belief) has the content: Lincoln was a great president. The belief is true just in case the proposition <Lincoln was a great president> is true.

Since the interpretation function is simply a mapping, there are "endlessly many" different possible interpretation functions – different ways of mapping belief tokens onto propositions (1990: 119). What makes one of these functions the right one, the one that accurately specifies the content of our beliefs? Stich contends (with good reason) that when philosophers are articulating various aspects of the interpretation function, they are trying to accurately capture

the judgments of the man or woman in the street about what content sentences or truth conditions get paired with the ordinary beliefs of ordinary folk. Thus any theory of interpretation that assigns to all of my beliefs truth conditions pertaining to events in the Crimean War, or to events in my own brain, would be immediately ruled out of court . . . (1990: 105)

The interpretation function favored by our intuitive judgments will be idiosyncratic. The best way to see this is to consider evidence suggesting that there are systematic, cross-cultural differences in people's intuitive semantic judgments (Machery, Mallon, Nichols and Stich 2004). If true, this means that the interpretation function favored by our culture will be different from the interpretation function favored by some other cultures. Stich argues that once we reflect upon what's involved in having true beliefs, we will find that we don't intrinsically value them: "Those who find intrinsic value in holding true beliefs . . . are accepting unreflectively the interpretation function that our culture . . . has bequeathed to us and letting that function determine their basic epistemic value. In so doing, they are making a profoundly conservative choice; they are letting tradition determine their cognitive values without any attempt at critical evaluation of that tradition" (1990: 120). Stich readily admits he has no useful reply to the person who, when fully informed, intrinsically values true belief. But he maintains that most people, when they are clear about what it involves, will come to see that they don't intrinsically value true belief.

What about the instrumental value of true belief? The issue here is not whether true belief is sometimes instrumentally valuable (surely it is) but whether true belief is more instrumentally valuable than potential alternatives. To get clear about this, let's consider an example. Shelley Taylor and her colleagues (Taylor 1989; Armor and Taylor 2002) have argued that having views that are somewhat unrealistically optimistic can promote mental and physical health. "Unrealistic optimism about the future is generally adaptive in that it promotes . . . feelings of self-worth, the ability to care for and about others, persistence and creativity in the pursuit of goals, and the ability to cope effectively with stress" (Taylor et al. 1992: 460). Before the advent of reasonably effective antiretroviral drugs, a study found that men with AIDS who were unrealistically optimistic lived longer than those who had more realistic views about their prospects (Reed et al. 1994). More generally, research suggests that mildly unrealistic optimists cope better with health issues than realists, they tend to recover more quickly than realists, and they have

higher recovery rates than realists (Scheier et al. 1989; Fitzgerald et al. 1993). So consider the possibility of having true* beliefs. Most true* beliefs are also true, but true* beliefs are more optimistic than true beliefs in certain cases having to do with health prospects.⁸ "True beliefs are not always optimal in the pursuit of happiness or pleasure or desire satisfaction, nor are they always the best beliefs to have if what we want is peace or power or love, or some weighted mix of all of these" (1990: 123).

3.2 *True belief as prodigal son*

These are powerful arguments. And properly understood, I don't want to challenge them. Upon reflection, we have no good reason to value true beliefs (intrinsically or instrumentally) as opposed to true* beliefs. I want to suggest, however, that we distinguish between what we find valuable (worthy of value) upon reflection and what we actually value. We value the truth, despite the results of our Stich-inspired deliberations on its idiosyncrasies and practical failings. The truth is like the prodigal son. We might realize that he does not deserve our love, our care, our energy; we might realize that we would be much better off committing those feelings and resources to a more deserving child. But despite what our heads say, we can't help but embrace him.

To make the case for truth, let's go back to the benefits of unrealistic optimism. Suppose Hobart is suffering from a serious disease for which there is some hope for recovery. Hobart is unrealistically optimistic about his prospects, as are his friends and loved ones. This sort of example seems to give succor to the pragmatist and headaches to the supporter of true belief. It suggests that we ought to reason in ways that are unreliable. But now suppose that Hobart gets a visit from Lance, who is a decent enough fellow but who can sometimes be stunningly thoughtless. Lance brings Hobart a large notebook full of evidence that supports in gruesome detail the hypothesis that Hobart's prospects for recovery are rather poor. What's more, Lance presents this evidence to Hobart. He raises every consideration that might lead Hobart to be overly optimistic about his prospects for recovery, and he utterly demolishes each consideration in meticulous detail with true and well-grounded evidence. Lance concludes by allowing that there is a small probability that Hobart will recover, but the probability is considerably lower than the average patient with Hobart's condition. Let's suppose that Lance's performance is not the result of any cruelty or malevolence – he just has a stunningly tin ear when it comes to interpersonal relationships. And let's further suppose Lance delivers this information in a genuinely friendly manner.

Despite Lance's intentions and affability, we recoil at the prospect of his forcing Hobart to hear the truth about his situation. But why? Suppose that Pam had thoughtlessly gone on about the genuinely pathetic prospects of the Cubs, Hobart's favorite baseball team. Assuming his attachment to the Cubs is not unreasonably fanatical (not always a safe assumption), Pam's behavior would be rude and stupid, but not nearly as cruel as Lance's. Unlike telling Hobart the truth about the Cubs, telling him the truth about his prospects for recovery threatens to destroy Hobart's hope and weaken his will. In this way, it threatens to rob Hobart of the resources he needs to survive his disease.

And here's the point: Lance's performance is appalling precisely because Hobart values true beliefs. If he didn't value true beliefs but instead valued true* beliefs, he could laugh off Lance's evidence and stick to his rosy beliefs about his prospects. But when we become convinced that we hold a false belief, even when that belief is highly useful, we are often drawn to the true belief like a moth to the flame. And that is why forcing the truth about Hobart's prospects on him undermines those prospects. When we are clearly convinced that *p* is true, we are naturally and powerfully drawn to the belief that *p*. We are drawn to the truth (or rather, what we clearly and fully understand to be the truth) in matters of belief in a way that we are not drawn to (what we clearly and fully understand to be) the good, the beautiful, or the useful.

Luckily, we have powerful psychological systems that enable us to elude a clear vision of the truth, and hence to escape its attraction. We are deceived and self-deceived in many ways (e.g., Nisbett and Wilson 1977; Wilson and Stone 1985). And most of us have a surprisingly robust capacity for adopting and protecting from disconfirmation overly rosy beliefs about ourselves (Taylor 1989: ch. 4).

We tend to believe that we possess a host of socially desirable characteristics, and that we are free of most of those that are socially undesirable. For example, a large majority of the general public thinks that they are more intelligent, more fair-minded, less prejudiced, and more skilled behind the wheel of an automobile than the average person. This phenomenon is so reliable and ubiquitous that it has come to be known as the "Lake Wobegon effect," after Garrison Keillor's fictional community where "the women are strong, the men are good-looking, and all the children are above average." A survey of one million high-school seniors found that 70% thought they were above average in leadership ability, and only 2% thought they were below average. In terms of ability to get along with others, all students thought they were above average, 60% thought they were in the top 10%, and 25% thought they were in the top 1%! Lest one think that such inflated self-assessments occur only in the minds of callow high-school students, it should be pointed out that a survey of university professors found that 94% thought they were better at their jobs than their average colleague. (Gilovich 1991: 77)

While we are adept at protecting these rosy beliefs from negative evidence, it is surely a mistake to suppose that they are totally immune from disconfirmation. It might be hard to convince us that our rosy beliefs are false. But when we do finally become convinced, we find it quite difficult (or perhaps even impossible) to continue believing them – even when doing so would be in our interests. We might agree with Stich that upon reflection, true beliefs are not intrinsically valuable and not as instrumentally valuable as true* beliefs. The problem is the hard empirical fact of our mindless, unthinking, default attraction to true belief.

3.3 *The pragmatic benefits of reliabilism*

The cool-eyed pragmatist will be the first to insist that a theory of cognitive evaluation should not make demands on us that we can't meet. Our judgment and decision-making

capacities are deeply imperfect, and the limits on our memory, computing power, time, energy, patience, and will are legion (Stich 1990: 149–58). I'm suggesting the pragmatist add one more imperfection to the list: We tend to value truth, even when, from a pragmatic perspective, we shouldn't. Once we take this fact about ourselves to heart, the pragmatist is faced with a familiar challenge: What sorts of normative principles, theories, or recommendations can we offer that will effectively guide our reasoning but that will clearly recognize *and compensate for* our built-in limitations and imperfections? Perhaps our regrettable attraction to true belief gives us pragmatic grounds for placing truth at the center of our epistemological theory. Not because truth is more valuable to us than truth* (or truth** or truth***) . . . but just because we're stuck valuing true belief.

To compensate for our unfortunate attraction to true belief, two different strategies suggest themselves. A direct strategy, which Stich adopts in *Fragmentation*, places pragmatic virtues center stage. Normative claims about cognitive matters – generalizations about good reasoning as well as evaluations of particular cognitive states and processes – are framed directly in terms of what we intrinsically value. An indirect strategy would place truth (or some other non-pragmatic category) center stage but would find some way to license the adoption of false (or true*) beliefs when they serve our interest.^{9, 10} What might such a theory look like? I suggest it might look something like Strategic Reliabilism (Bishop and Trout 2005a). While Strategic Reliabilism has not been defended on pragmatic grounds, I want to argue here that (with some minor modifications) it can be.

The indirect pragmatic theory of cognitive evaluation I will sketch here consists of two parts. The first holds that an excellent reasoner will have at her disposal a set of reasoning methods or strategies that are robustly reliable – they yield a high percentage of true beliefs across a wide variety of situations. The second part of the theory focuses on the problems these strategies will apply to, and how the excellent reasoner will use them. Given the infinitely many reasoning problems one might tackle at any moment and our limited resources, the excellent reasoner will do more than reason reliably about useless matters. It is here that the pragmatic element of the indirect theory plays a vital role. Given the indefinitely many reasoning problems we face, some are more worthy of attention than others in a straightforwardly pragmatic sense:

- 1 Constructive problems: Reasoning in a robustly reliable way about some problems will help us achieve those things we intrinsically value.
- 2 Neutral problems: Reasoning in a robustly reliable way about most problems we face will bring us nothing that we intrinsically value – they are just a waste of time and resources.
- 3 Undermining problems: Reasoning in a robustly reliable way about some problems will lead to results that undermine those things we intrinsically value.¹¹

So the excellent reasoner will have at her disposal robustly reliable reasoning strategies for handling constructive problems. In her daily life, she will mete out attention and resources to these problems in a prudent, cost-effective manner. She will ignore neutral

problems. And she will be naturally disposed to avoid spending cognitive resources on undermining problems. The idea here is not that the excellent reasoner will consciously work through these various cost-benefit calculations every time she faces a reasoning problem. That would lead to an infinite regress. (I'm faced with problem P. Before solving it, I must solve problem P': Is P a constructive, neutral or undermining problem? But before solving P', I have to solve problem P'': Is P' a constructive, neutral or undermining problem? . . .) So a lot of these "decisions" occur at a non-conscious level. That's *not* to say that we have no control over what reasoning strategies we employ ("I need to insist on controls when thinking about causal claims") or over where to put our cognitive resources ("I should spend less time and energy thinking about the mind-body problem"). It is to say that, most of the time, we run our cognitive lives without any high-level, conscious decision-making about what reasoning problems to tackle. And this is part of (and perhaps a large part of) why our overly rosy beliefs can be so effective. Imagine if Hobart had to consciously decide to not spend a great deal of energy scrutinizing the issue of his prospects for recovery on the grounds that doing so might kill him. That just wouldn't work. Psychological research (e.g., Wegner 1994) as well as schoolyard wisdom ("Don't think about pink elephants!") supports the idea that we aren't very good at suppressing our thoughts. So Hobart's rosy belief survives because we are naturally inclined to not bring our critical faculties to bear on undermining issues; and absent critical scrutiny, our rosy beliefs are protected by the powerful psychological mechanisms that account for the sturdiness of the Lake Wobegon effect (Gilovich 1991; Taylor 1989: ch. 4).

On what grounds are we to choose between a direct and an indirect pragmatic theory of cognitive evaluation? Insofar as Stich is committed to a direct theory, he will surely not argue that his direct theory is to be preferred because it is a true account of the nature of cognitive evaluation. Any choice between a direct or indirect theory must ultimately be made on pragmatic grounds. For Stich, whichever theory works better in straightforwardly pragmatic terms (i.e., it helps people better achieve those things they intrinsically value), that's the one we should believe. So the choice between these theories is, for Stich, an empirical matter. Since I don't have the empirical goods to settle the issue, plausibility considerations will have to suffice.

The primary mission of epistemology is to evaluate the various ways different people can and do reason and (hopefully) offer useful suggestions about how we can reason better. A direct theory will frame these normative judgments in terms of useful belief, while an indirect (reliabilist) theory will frame these normative judgments in terms of true belief. Given our default attraction to true beliefs, the indirect theory starts off with all the pragmatic advantages of incumbency. We are already used to thinking about our epistemic responsibilities in terms of true belief. And so given any claim about the relative quality of one of our reasoning strategies, we'll have an easier time understanding, accepting, and reflecting upon the claim made by the indirect theory. Further, we are likely to find it easier and more natural to be guided by the normative recommendations made by the indirect theory. This is a significant, built-in advantage for the indirect (truth-based) pragmatic theory of cognitive evaluation.¹²

Can the direct theory overcome its built-in deficit? Perhaps. Consider that we sometimes accept beliefs we know are useful but not true, as, for example, when we use Newtonian theory to predict and explain the motion of medium-to-large objects. But in that case, we

give up negligible amounts of accuracy for a great deal of utility. Further, I think we are naturally inclined to explain the utility of such beliefs in terms of their proximity to the truth. So this example doesn't give us any reason to suppose that revising our epistemic habits so we are no longer mindlessly attracted to true belief will be easy. In fact, I speculate that it would take considerable work to refashion our habitual epistemic practices in such a dramatic way. Of course, such a change might be psychologically impossible, or so difficult as to be practically impossible. If that's so, then the pragmatist will have to settle for an indirect theory.

Let's suppose we can alter our epistemic habits so that pronouncements about how to reason and believe that are framed in terms of useful belief are as easy for us to grasp, ponder, accept and act on as pronouncements that are framed in terms of true belief. Should we go to the trouble of revising our epistemic practices? It depends on the pragmatic benefits of doing so. *These benefits will come from our increased ability to reason unreliably (i.e., reliably*) to useful beliefs in those cases the indirect theory cannot plausibly handle.* If the direct and indirect theories were to make exactly the same recommendations, then the direct theory would be doomed. It couldn't overcome the indirect theory's incumbency advantages. But I think Stich will argue (rightly) that we have no good reason to suppose the direct and indirect theories will yield exactly the same cognitive evaluations. To see how the direct and indirect theories might make different recommendations, let's go back to the Hobart example. The indirect theory could license Hobart's useful, false but true* (rosy) belief because of two contingent psychological facts:

- (a) Hobart's reasoning strategies naturally deliver rosy beliefs.
- (b) Hobart is inclined to avoid critical scrutiny of his rosy beliefs.

Without these psychological mechanisms, the indirect theory has no epistemic resources to license Hobart's rosy belief. In fact, the indirect theory will tend to undermine (a), Hobart's natural inclination to reason to the rosy belief rather than the true belief. That's because the indirect theory directs us to adopt *robustly* reliable reasoning strategies. Doing so could readily undermine our natural tendency to adopt rosy beliefs. Once a reasoner comes to a true belief as a result of robustly reliable reasoning, the indirect theory has no resources to license the true* belief or the reliable* reasoning strategy. Now consider those cases in which (b) is false. Suppose Hobart is naturally disposed to critically scrutinize his rosy belief. The indirect theory might pronounce that, to be an excellent reasoner, Hobart ought to avoid scrutiny of such beliefs. But as we have already seen, this is not likely to be a particularly effective recommendation ("Don't think about pink elephants!"). So if there are undermining problems that the indirect theory can't handle (i.e., it can't effectively recommend the rosy belief), then the direct theory has a chance to overcome the indirect theory's incumbency advantages.

The case for an indirect pragmatic theory is by no means a slam dunk. But the direct theory faces two pressing problems. First, the direct theory fails if we cannot alter our default epistemic practices. And second, the direct theory must show that the costs of altering our default epistemic practices will be more than compensated for by the direct theory's advantages over the indirect theory. The direct theory needs both claims, but

it's not clear it can have either. Without a clear case for the pragmatic advantages of revising our epistemic habits, on pragmatic grounds, we should stick with what seems to be working well enough. Given what we know, the pragmatist should provisionally adopt an indirect theory.

4 Conclusion

In "Epistemology Naturalized," Quine offered a vision of epistemology that has fallen on hard times. "The simulation of his sensory receptors is all the evidence anybody has had to go on, ultimately, in arriving at his picture of the world. Why not just see how this construction really proceeds? Why not settle for psychology?" (1969: 75)

Epistemology, or something like it, simply falls into place as a chapter of psychology and hence of natural science. It studies a natural phenomenon, viz., a physical human subject. This human subject is accorded a certain experimentally controlled input – certain patterns of irradiation in assorted frequencies, for instance – and in the fullness of time the subject delivers as output a description of the three-dimensional external world and its history. The relation between the meager input and the torrential output is a relation that we are prompted to study for somewhat the same reasons that always prompted epistemology; namely, in order to see how evidence relates to theory, and in what ways one's theory of nature transcends any available evidence. (1969: 82–3)

For decades, analytic epistemologists have been united in their opposition to the idea that epistemology could be reduced to psychology. Tradition holds that such a reduction is subject to a devastating objection: It empties epistemology of its normative character. On this point, Stich agrees with tradition.

The Quinean naturalized epistemologist can explore in detail the various ways in which different people construct their "picture of the world" on the basis of the evidence available to them. But he has no way of ranking these quite different strategies for building world descriptions; he has no way of determining which are better and which are worse. And since the Quinean naturalized epistemologist can provide no normative advice whatever, it is more than a little implausible to claim that his questions and projects can replace those of traditional epistemology. We can't "settle for psychology" because psychology tells us how people *do* reason; it does not (indeed cannot) tell us how they *should*. (Stich 1993: 4–5)

But what is the lesson? That we can't put empirical theories at the heart of our epistemology? But then what survives? Not Quinean naturalism. Not analytic epistemology. If we can't replace epistemology with psychology, then we can't replace it with ethnography, either. Not Stich's pragmatism, which is built on empirical claims about how people reason, about how people evaluate cognition, and about what we intrinsically and instrumentally value. And not experimental philosophy, either.

If we take the arguments proposed by Stich and by WNS against analytic epistemology seriously, then a genuinely normative, reason-guiding epistemology cannot be in the

business of a priori reflection on our parochial epistemic concepts, judgments, and intuitions. But then science is all that's left. If this is right, then Quine's mistake was not identifying epistemology with science. His mistake was identifying it with the wrong bit of science. Of course, we're still left with the very serious problem of how to extract normative, epistemological claims from science. But if Stich's and WNS's critique of analytic epistemology is correct, we don't have a choice. At every crucial step, Stich's epistemology drives us to the conclusion that genuinely normative theories find their homes in science. Further, the alternatives to analytic epistemology we have considered all make epistemology a chapter, though perhaps a rather quirky chapter, of psychology. Maybe a Stich in time saves Quine.¹³

Notes

- 1 Has Stich changed the subject so much that he is not engaging, and hence not disagreeing with, analytic epistemologists? Goldman (1978) recognizes that some contemporary epistemologists might be disinclined to take this project to be epistemology. To avoid becoming embroiled in boundary disputes, Goldman calls this project by a different name, "epistemics." Stich has at least two replies to the "change of subject" worry. First, reasoning strategies typically yield belief tokens. So to recommend a reasoning strategy is to recommend belief tokens, at one remove. Thus, a theory that evaluates reasoning strategies and a theory that evaluates belief tokens can recommend incompatible belief tokens. And second, Stich's pragmatic view of cognitive evaluation can be applied directly to belief tokens. So his theory can make recommendations that directly conflict with those of standard theories of justification.
- 2 Debates about the quality of people's default reasoning strategies, and the variation in the quality of different people's default reasoning strategies, provides another motivation for this project (see, e.g., Stanovich 1999).
- 3 Stich also argues that we have no good reason to believe that our epistemic standards are instrumentally valuable – or rather, more instrumentally valuable than any alternative set of epistemic standards. And, in fact, he argues that we have some reason to doubt that they are (1990: 96–127).
- 4 The intrinsic value argument does not appear in WNS's articles, although a weaker form of it does. WNS note that if we were to begin the analytic project with different sets of intuitions (e.g., intuitions of East Asians, or of Westerners, or of low SES subjects or of high SES subjects) we would end up with very different epistemological theories. WNS then ask: "What reason is there to think that the output of one or another of these Intuition-Driven Romantic strategies has real (as opposed to putative) normative force?" (Weinberg, Nichols and Stich 2001: 434). Not all of these theories can be genuinely (as opposed to putatively) normative. While I think this argument is less ambitious than Stich's earlier intrinsic value argument, I needn't press the point here.
- 5 I have been surprised to find philosophers occasionally denying that stasis really is a requirement on contemporary theories of analytic epistemology. Here is a crucial experiment to test this suggestion: Find cases of contemporary analytic epistemologists "outsmarting" themselves (where "outsmart" is a technical term from *The Philosophical Lexicon* (Dennett 1987): "outsmart, v.: To embrace the conclusion of one's opponent's *reductio ad absurdum* argument. 'They thought they had me, but I outsmarted them. I agreed that it *was* sometimes just to hang an innocent man.'") If the stasis requirement is not operative, we should find lots

of cases in the epistemological literature in which people embrace positions with little regard for their intuitive judgments – as occurs in the sciences. But we don't find lots of instances of contemporary analytic epistemologists "outsmarting" each other. In fact, except for the occasional skeptic, I am hard pressed to think of any.

- 6 Bealer (1993, 1998) has argued strenuously that philosophical ("a priori") intuitions are unlike physical intuitions. Perhaps. Still, their evidential relationship to certain theories might be similar.
- 7 A potential advantage of this approach over the others is that it grounds epistemology in a bit of psychology that, prima facie, already has legitimate normative force. The great challenge for the naturalist is how to extract 'ought's from 'is's. That challenge is met if we're extracting epistemological 'ought's from epistemological 'ought's we find in psychology. That still leaves the puzzle of what we're to make of these apparently normative parts of psychology.
- 8 In this case, some true* beliefs are true and others false. But some alternatives to true beliefs won't involve false beliefs. The interpretation function is not only idiosyncratic, it is also partial: it won't assign propositions to many belief-like states. So some true** beliefs might be true while the rest have no truth values at all (Stich 1990: 121–2).
- 9 This indirect strategy is familiar in ethics, particularly among hedonists who argue that, although only happiness is intrinsically valuable, in order to achieve happiness, people ought to intrinsically value lots of things besides happiness (e.g., see Mill's *Utilitarianism*: ch. IV).
- 10 The case for an indirect theory undermines Stich's methodological monism with respect to normative issues in a way that does not require table-pounding (see the discussion in 1.4.1).
- 11 The main difference between this indirect pragmatic view and Strategic Reliabilism has to do with the nature of these values. Trout and I assume (without argument) a realistic conception of value, but this is an optional part of the theory (2005a: 93–103). What I have done here is to modify Strategic Reliabilism by replacing the realistic conception of value with a pragmatic one.
- 12 As I suggested at the end of section 1, the experimental philosopher can adopt a remarkably similar pragmatic defense of her approach.
- 13 The credit for this line and for the subtitle of this paper belongs to the anonymous wag who defined 'stich' in *The Philosophical Lexicon* (Dennett 1987): "stich, n. (cf. *croce*) The art of eliminative embroidery. In the art of stich, one delicately strips the semantics off the rich tapestry of folk psychology revealing the bare warp and wood of pure syntax. 'A stich in time saves Quine.'"

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Simulation Theory and Cognitive Neuroscience

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Is Simulation a Natural Category?

One topic on which Steve Stich and I have occupied opposing positions is the topic of mindreading (also called mentalizing, or folk psychology). In his first position statement on the subject, he and Shaun Nichols framed the debate as one between simulation theory and theory-theory (Stich and Nichols 1992). Responding primarily to Bob Gordon (1986) and me (Goldman 1989) as protagonists of simulation theory, they offered a lucid and spirited defense of theory-theory. As the decade of the 1990s proceeded, Stich and Nichols published a seemingly endless stream of articles on the subject, most of which appeared in *Mind and Language*. A funny thing happened, however, on the way to the century's close. The Stich–Nichols attack on the simulation theory gradually softened. In Stich and Nichols (1995) and Nichols, Stich, Leslie and Klein (1996), they showed grudging appreciation of simulationism's virtues, at least in some of its forms or applications. By the time they published their book on mindreading (Nichols and Stich 2003), their preferred theory was acknowledged to be "very eclectic" (2003: 100). The book rarely refers to their position as theory-theory, and emphatically rejects the theory-theory of self-awareness (2003: ch. 4). One mindreading process (inference prediction) is definitely said to be executed by simulation, and other mindreading processes are allowed to bear some similarities to simulation prototypes (2003: 135).

Not surprisingly, I find this increased appreciation for simulation quite congenial. But philosophers are a hard bunch to please, myself included. For my money, Stich and Nichols still haven't moved far enough in the direction of simulationism. Let us look more closely at their early and late assessments of its prospects. In early writings they viewed simulation theory as an unwelcome intruder into cognitive science. In 1992 they warned that it is fundamentally at odds with "the dominant explanatory strategy in cognitive science" (Stich and Nichols 1992: 36). Again in 1996 they called it "a radical departure from the typical explanations of cognitive capacities" (Nichols et al. 1996: 39). In 1997 their complaint shifted a bit in tone and focus. Surveying what they regarded as