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Reflections on a Normative Psychology

In 'Epistemology Naturalized' Quine famously recommends the Replacement Thesis — the idea that epistemology, properly understood, "simply falls into place as a chapter of psychology and hence of natural science" (Quine 1969, p. 82). Since the appearance of Quine's seminal article, virtually every epistemologist, including the later Quine (1986, p. 664), has repudiated the Replacement Thesis. Conventional Wisdom holds that psychology is a purely descriptive discipline. It tells us how we *do* reason and how we *do* come to our beliefs. But epistemology is a normative enterprise. It tells us how we *ought* to reason and what we *ought* to believe. We cannot replace epistemology with psychology — and so the Replacement Thesis is false — because it is impossible to extract an epistemological 'ought' from a psychological 'is.'

Conventional Wisdom is wrong about the Replacement Thesis. Quine's vision of epistemology as a chapter of psychology is not dead. The problem with Conventional Wisdom is that it assumes that psychology is a purely descriptive discipline. It is not. Some areas of psychology are robustly and overtly *normative*. By this I mean that psychologists explicitly evaluate our beliefs and our reasoning and they offer explicit prescriptions about what we ought to believe and how we ought to reason. Once we recognize that parts of psychology are normative, the is-ought distinction is no longer a legitimate reason to reject the Replacement Thesis. Of course, there might be other reasons for rejecting it. But the reason offered by Conventional Wisdom for rejecting the Replacement Thesis is flawed.

In this paper, I will explore some normative pockets of psychology and I will draw out some of their potential implications. In section 1, I describe a particularly interesting line of psychological research that is straightforwardly normative — it offers explicit epistemological evaluations as well as explicit epistemological prescriptions. In section 2, I describe — and try to clarify — part of a fascinating epistemological debate that erupted about 10 years ago in the psychological literature. In section 3, I give a brief sketch of the normative framework that grounds the evaluations and prescriptions of these normative parts of psychology, and I respond to some worries one might have about this framework.

1 Epistemological Evaluations and Prescriptions in Psychology

In response to the allegation that psychology is normative, one might be inclined to argue that this does not make psychology special. All branches of science are normative in two senses. First, they have 'good practice' standards. These standards tell us, for example, how to correctly study some aspect of the world or what counts as good evidence for a hypothesis. And second, scientific theories tell us how we ought to think about certain specific aspects of the world. Physics tells us how we ought to reason about physical matters, chemistry tells us how we ought to reason about chemical matters, and psychology tells us how we ought to reason about psychological matters. I will not try to argue that these features of science make science in any substantive sense normative, although I think they do. My contention is that there is a feature of psychology that makes it normative in the following special and perhaps unique sense: Psychology makes explicit normative, epistemological judgments about matters that have nothing to do with the subject matter of psychology. It makes explicit evaluative judgments about what ways of reasoning are epistemically better and worse than others, and it makes explicit prescriptive judgments about how we ought to reason. And these normative judgments concern our reasoning about matters far removed from psychology. In this section, I want to report on a fascinating line of research in which psychologists make explicitly evaluative and prescriptive epistemological judgments.

Gigerenzer, Hoffrage and Ebert (1998) report on a dramatic episode:

> Former Senator Lawton Chiles of Florida reported at an AIDS conference in 1987 that of 22 blood donors in Florida who were notified that they tested HIV-positive with the ELISA test, seven committed suicide. In the same medical text that reported this tragedy, the reader is informed that 'even if the results of both AIDS tests, the ELISA and WB (Western blot), are positive, the chances are only 50-50 that the individual is infected' (Stine 1996, pp. 333 & 338). (Gigerenzer, Hoffrage and Ebert 1998, p. 197)

At the heart of this tragedy is a failure to appreciate diagnostic reasoning. It is possible both that the AIDS tests are highly reliable (in that they make mistakes in a tiny percentage of cases) and that a person who tests positive has only a 50-50 chance of being HIV-positive.

To appreciate the mistake many people make, suppose we know that a pregnancy test is 99.9% accurate. So if Pat is pregnant, the test will give a positive result 99.9% of the time. And if Pat is not pregnant, the test will give a negative result 99.9% of the time. Now, suppose Pat tests positive. What is the probability that Pat really is pregnant? The right answer is: We do not know because we do not have enough information. In order to make a reasonable judgment, we need to know the prior probability that Pat is pregnant (or the 'base rate'—the pregnancy rate of people relevantly like Pat). Suppose that Pat is a man. The probability that Pat is pregnant, even given the positive result, is zero. Now suppose Pat is a 49-year-old woman. Given the low prior probability that a woman of Pat's age will be pregnant, the chances that she is pregnant—even given the positive test—will be significantly less than 99.9%. Of course, if Pat is a sexually active 22-year-old woman who does not use birth control, the probability that she is pregnant, given the positive test, will be very high indeed. The point of these cases is that a positive test for pregnancy or drug use or disease, by itself, does not tell us how likely it is that a person is pregnant, has used cocaine, or has cancer. We also need to know the prior probability that the person is pregnant, has used cocaine, or has cancer.

There is considerable evidence that people are not very good at this sort of diagnostic reasoning. Casscells, Schoenberger and Grayboys (1978) presented the following problem to sixty students and staff at Harvard Medical School.

> If a test to detect a disease whose prevalence is 1/1,000 has a false positive rate of 5%, what is the chance that a person found to have a positive result actually has the disease, assuming you know nothing about the person's symptoms or signs?

This problem is not described fully enough for someone to properly solve it without making some important assumptions. In particular, one must assume something about the test's sensitivity (i.e., the probability that a person would test positive given that she had the disease). Casscells *et al.* assumed that the test's sensitivity is about 100%. Given this assumption, the standard way to solve this sort of problem is with Bayes' Rule, where 'D' is the person has the disease and '+' is the person tests positive:

$$P(D/+) = P(+/D) \times P(D) \;/\; \{[P(+/D) \times P(D)] + [P(+/\text{-}D) \times P(\text{-}D)]\}.$$

Bayes' theorem yields the result that the probability that the subject has the disease given the positive result is about 2%. Among the faculty and staff at Harvard, almost half judged the probability to be 95%; the mean answer was 56%; and only 18% of subjects responded with the answer given by Bayes' Theorem. (Despite problems with the Casscells *et al.* study, their finding has been replicated in studies that include all the necessary information [Gigerenzer and Hoffrage 1995].)

Gigerenzer and Hoffrage (1995) have shown how to dramatically improve people's reasoning on diagnosis problems without a lot of complicated statistical training. It turns out that people do much better on these sorts of problems when they are framed in terms of frequencies rather than probabilities. To see this, consider two mathematically equivalent formulations of a diagnosis problem.

> *Probability format.* The probability of breast cancer is 1% for women at age forty who participate in routine screening. If a woman has breast cancer, the probability is 80% that she will get a positive mammography. If a woman does not have breast cancer, the probability is 9.6% that she will also get a positive mammography. A woman in this age group had a positive mammography in a routine screening. What is the probability that she actually has breast cancer? ____%.

> *Frequency format.* 10 out of every 1,000 women at age forty who participate in routine screening have breast cancer. 8 of every 10 women with breast cancer will get a positive mammography. 95 out of every 990 women without breast cancer will also get a positive mammography. Here is a new representative sample of women at age forty who got a positive mammography in routine screening. How many of these women do you expect to actually have breast cancer? ____ out of ____.

People with no training in statistics tended to do much better on problems in the latter, frequency formats. Gigerenzer and Hoffrage report that 16% of subjects faced with probability formats got the Bayesian answer, while 46% of subjects faced with frequency formats got the Bayesian answer (Gigerenzer and Hoffrage 1995, pp. 693).

This line of research makes straightforwardly epistemological judgments. Gigerenzer and Hoffrage are telling us that people reason *better* when they employ frequency formats. They are making explicit evaluative (good/bad) judgments. Indeed, the title of their article explicitly states their normative aspirations: 'How to *Improve* Bayesian Reasoning Without Instruction' (emphasis added). What is more, this line of research offers clear straightforward prescriptions: When faced with a diagnosis problem, people should learn to represent and solve the problem in a frequency format. By doing so, we repackage the problem-task so that it will (for many people) naturally trigger a cognitive mechanism that will quickly and reliably get the Bayesian answer. By framing diagnosis problems in terms of frequencies rather than probabilities, people can reason about significant problems more reliably.

2 Normative Epistemological Debates in Psychology

So far I have argued that psychology is normative because it makes explicitly normative judgments about how we ought to reason about particular sorts of issues. Frequency formats are one example, but there are many others (see Bishop 2000; Bishop and Trout 2005). Another sign that parts of psychology are normative is that theoretical epistemological debates occasionally erupt in psychology. I want to focus on part of one such debate here — the debate between the main proponent of the fast and frugal heuristics program (Gerd Gigerenzer) and the main proponents of the heuristics and biases program (Daniel Kahneman and Amos Tversky) (see Kahneman and Tversky 1996; Gigerenzer 1991, 1996).

Proponents of the fast and frugal heuristics program (FFH) and of the heuristics and biases program (HB) take heuristics to be important to understanding how people reason. A heuristic is a rule of thumb. In psychology, heuristics are relatively simple rules for making judgments. The heuristics and biases program holds that people are disposed to use simple reasoning rules or heuristics. And while these heuristics are reliable in many contexts, there are some contexts in which they lead to systematic error (Kahneman, Slovic and Tversky 1982). A major characteristic of the heuristics and biases program (HB) is its focus on people's systematic reasoning errors (or biases). But what makes something a bias or a systematic reasoning error? According to Kahneman and Tversky, "[t]he presence of an error of judgment is demonstrated by comparing people's responses either with an established fact ... or with an accepted rule of arithmetic, logic, or statistics" (Kahneman and Tversky 1982, p. 493). So consider again that Gigerenzer and Hoffrage report that only 16% of subjects faced with a probability format diagnosis problem got the Bayesian answer (Gigerenzer and Hoffrage 1995, p. 693). So for proponents of HB, 84% of subjects made an error because their response violated an accepted rule of statistics, namely, Bayes theorem.

Why do proponents of the HB program focus on people's systematic reasoning errors? Kahneman and Tversky offer three reasons:

> First, they expose some of our intellectual limitations and suggest ways of improving the quality of our thinking. Second, errors and biases often reveal the psychological processes and the heuristic procedures that govern judgment and inference. Third, mistakes and fallacies help the mapping of human intuitions by indicating which principles of statistics or logic are non-intuitive or counter-intuitive. (Kahneman and Tversky 1982, p. 494)

While Kahneman and Tversky are reasonably cautious in their overall assessment of human reasoning, others have drawn fairly harsh pessimistic conclusions. In an oft-quoted passage, Nisbett and

Borgida (1975, p. 935) claim that the reasoning errors uncovered by the HB program have "bleak implications" for human rationality (see also Piattelli-Palmarini 1994). These pessimistic conclusions have been repeatedly challenged, and have led to considerable debate about whether these biases really are 'errors' and whether, or to what extent, people are rational (e.g., Nisbett and Borgida 1975; Cohen 1981; Stich 1985, 1990; Lopes 1991; Gigerenzer 1991, 1996; Kornblith 1992; Piattelli-Palmarini 1994; Stein 1996; Sosa and Galloway 1999).

Gigerenzer repeatedly levels two objections against the HB program (Gigerenzer 1991). Both objections focus on examples like the Bayesian problem above, in which subjects are asked to judge the likelihood of an individual event.

The Disappearance Argument. The errors subjects make on the diagnosis problem can be made to 'disappear' (or be significantly reduced) by framing the problem differently. As we have seen, Gigerenzer and Hoffrage found that when given a frequency format, 46% of subjects with no training in statistics got the Bayesian answer on a diagnosis problem (Gigerenzer and Hoffrage 1995, p. 693).

The Frequentist Argument. A frequency interpretation of probability states that the probability of an attribute A is the relative frequency with which A occurs in an unlimited sequence of events. According to a frequentist, assigning a probability to a single event is meaningless. So any answer to the meaningless question posed by the above problem ('What is the probability that she has breast cancer?') cannot be a violation of probability theory. As we have already seen, Kahneman and Tversky contend that "[t]he presence of an error of judgment is demonstrated by comparing people's responses either with an established fact … or with an accepted rule of arithmetic, logic, or statistics" (Kahneman and Tversky 1982, p. 493). Since a frequentist would not take subjects' answers to violate any 'rule of arithmetic, logic, or statistics,' those answers are not errors.

These are very puzzling arguments. In fact, there are at least four serious objections to them.

1. The frequentist argument contends that on a frequentist construal of probability, subjects' answers to the diagnosis problem (and other single-event probability problems) are not errors. But Gigerenzer never suggests that subjects understand these single-event probability problems in a frequentist way. A number of authors have noted that it is overwhelmingly plausible to suppose that

subjects understand the diagnosis problem and do not take it to be meaningless (e.g., Kahneman and Tversky 1996, pp. 585–586). Here is an emphatic statement of this point:

> [W]hen a man tests positive for prostate cancer, he wants to know whether he has prostate cancer. In order to make decisions, he might want to ask: Given the positive result, what are the chances that I actually have prostate cancer? In such a situation, it is hard to imagine anyone seriously pointing out that frequentists would deem this a meaningless question — or worse yet, explaining to the patient that he is a frequentist and so his own question can have no meaning for him. If a doctor tells his patient that he has a 99% chance of having cancer, the patient is surely going to have some sort of understanding of what is being said. (It is unlikely, for example, that he will react with glee.) (Bishop and Trout 2005, p. 127)

This point gains even more traction when one realizes that Gigerenzer raises precisely this objection against the HB program. He argues that the HB program does not pay close enough attention to how subjects actually understand these problems (Gigerenzer 1996, p. 593).

2. The two arguments are inconsistent with each other. The frequentist argument aims to show that people are not making (and in fact, cannot make) errors on single event probability problems. But the disappearance argument shows how people can *improve* their reasoning on those exact problems (i.e., by reframing them in terms of frequencies). The second argument contends there is no error, while the first argument shows how to correct the error. Samuels, Stich and Bishop (2000, p. 250) argue that Gigerenzer cannot have it both ways: “If it ain’t broken, you *can’t* fix it.”

3. Gigerenzer insists that the point of the frequentist argument “is precisely *not* to champion” a frequentist interpretation of probability (Gigerenzer 1994, p. 141). But if he does not accept a frequentist interpretation of probability, then what are we to make of the frequentist argument? That argument shows that any answer to a single-event probability problem is not an error *given a frequentist interpretation of probability.*

4. The frequentist argument assumes the HB view about what is

involved in a reasoning error — violating a law of probability. But Gigerenzer is unequivocal in his rejection of this view. He clearly embraces a broadly reliabilist approach to epistemic evaluation: "We do not compare human judgment with the laws of logic or probability, but rather examine how it fares in real-world environments. The function of heuristics is not to be coherent. Rather, their function is to make reasonable, adaptive inferences about the real social and physical world given limited time and knowledge" (Gigerenzer and Todd 1999, p. 22).

Given these four objections, we might charge Gigerenzer with confusion or worse (as critics have done; see Kahneman and Tversky 1996; Samuels *et al.* 2000; Bishop and Trout 2005). But I want to suggest that these objections — particularly the last two — point to a different reading of the frequentist argument. I propose an interpretation that smoothly handles the objections, but that Gigerenzer (to my knowledge) has never clearly and explicitly embraced in print. The trick to understanding Gigerenzer's arguments is to take proper account of the fact that the frequentist argument employs assumptions that Gigerenzer does not accept. This does not make sense if we take him to be arguing for a positive conclusion, a conclusion he embraces. But what if we take Gigerenzer's frequentist argument to be a *reductio ad absurdum*? Then, I contend, the objections disappear. Here is the clear version of the frequentist argument I propose to ascribe to Gigerenzer:

> Suppose we adopt the HB standard of what counts as an error — a violation of the rules of logic or probability. Unless proponents of the HB program have an argument against the frequentist interpretation of probability (and they don't), they have no way to block the committed frequentist's argument. And so they cannot legitimately conclude that the subjects are making reasoning errors. But this is absurd — those subjects are making reasoning errors. In fact, there are some nifty ways of getting people to make fewer such errors (see the disappearance argument). Therefore, in order to avoid this absurd conclusion, we must reject either frequentism or the HB standard of what counts as an error. Which disjunct will the proponent of the HB program reject? In perhaps the most stinging criticism of my [Gigerenzer's] research, Kahneman and Tversky insist that the HB program has relied heavi-

ly on studies (on availability, anchoring, and overconfidence) in which probability statements were interpreted as frequency claims (Gigerenzer 1996, pp. 583–584). So do not take it from me. Take it from Kahneman and Tversky: The only disjunct the proponent of the HB program can reject is the second — the HB standard of what counts as an error.

Now let us see how the four objections to Gigerenzer's arguments disappear. The fact that Gigerenzer does not accept all the premises of the argument (objections 3 and 4) is perfectly appropriate if the argument is a *reductio*. The fact that the frequentist argument comes to a conclusion that is inconsistent with what Gigerenzer believes (objection 2) is also to be expected. If the frequentist argument is a *reductio*, Gigerenzer believes its conclusion is false. So it should not be surprising that it is inconsistent with something Gigerenzer believes (i.e., the conclusion of the disappearance argument). And what about the fact that Gigerenzer does not worry about whether subjects understand single-event probability problems in a frequentist way (objection 1)? This is perfectly appropriate. After all, Gigerenzer is not arguing that the subjects who come to non-Bayesian answers are not making errors. In fact, he believes they are making errors (which is why he recommends that subjects use frequency formats to address diagnosis problems). Gigerenzer is arguing that *if* one takes these subjects to be making an error because they are violating laws of probability and *if* one has no principled objection to frequentism, *then* one is forced to the absurd view that the subjects who give non-Bayesian answers to the diagnosis problems are not making errors. Gigerenzer's *reductio* might be seriously flawed. But if it is, it is not because Gigerenzer is under any illusions about how the typical person understands single-event probability statements.

3 Implications of a Normative Psychology

Psychology is chock full of normative epistemological judgments. And what is more, in psychology, the occasional epistemological fracas breaks out. The philosopher's antennae buzz. This means that these studies are presupposing some sort of normative, epistemological framework. Going further out on a limb, perhaps the various areas of psychology that make normative, epistemological claims presuppose a single, general, coherent epistemological framework. A project naturally suggests itself: Articulate the epistemological framework that grounds the normative (evaluative and prescriptive) judgments of these areas of psychology (Bishop and Trout 2005; Bishop 2000). Here, my plan is to give a quick sketch of the normative framework that grounds the evaluations and prescriptions of the psychological research on frequency formats. (For more details, see Bishop and Trout [2005].)

The first notable feature of the normative framework used to recommend frequency formats is that the items it evaluates are reasoning strategies. In tackling diagnosis problems, we are advised to employ frequency formats rather than probability formats. This distinguishes this framework from the normative theories of contemporary epistemology. The theories of contemporary epistemology aim to evaluate belief tokens. Theories of justification tell us when a belief token is justified, and theories of knowledge tell us when a belief token is an instance of knowledge. Psychologists tend to have a very different orientation. They are primarily interested in evaluating and recommending various methods or strategies for reasoning about a certain class of problem.

A second notable feature of the normative framework that recommends frequency formats is that reasoning strategies are evaluated in terms of their robustness, their reliability and their tractability. Let us consider each of these items. Frequency formats are recommended in large part because of their reliability — their tendency to produce true beliefs or judgments. So other things being equal, a more reliable reasoning strategy is superior to a less reliable one. The robustness of a reasoning strategy is the extent to which it applies to a wide range of contexts. The frequency format is quite robust, as it operates on any well-framed diagnosis problem. So other things being equal, a reasoning strategy that has a wider range of application is superior to one that has a narrower range of application. Another way in which reasoning strategies are evaluated in psychology is in terms of their tractability — how easy they are to use. A significant virtue of frequency formats is that they are relatively easy to employ — or at least easier to employ than the competition (i.e., employing Bayes' theorem).

Putting all this together, the normative framework we have been considering takes good reasoning to be efficient and robustly reliable. The notion of efficiency suggests that this framework involves an ineliminable cost-benefit component. It is important to be clear about the precise role of this cost-benefit element in our epistemological theorizing. Reasoners do not have an inexhaustible supply of resources to expend on problems; and any normative theory that pretends to have any reason-guiding force must recognize our limitations. Rationality is bounded (Simon 1982; Gigerenzer and Todd 1999). In order to adequately recognize our limitations, our epistemological theorizing must appeal to cost-benefit considerations. But this does not mean that we require that excellent reasoners always (or even typically) engage in cost-benefit calculations when thinking about the world. Cost-benefit considerations are always important to the epistemologist who wants to make appropriate evaluations and offer useful prescriptions. But cost-benefit considerations are not always (and probably seldom) important to the reasoner who wants to think effectively about the world.

Let us consider one final worry about the normative framework sketched here. One might wonder how it is going to generate useful prescriptions. In many situations, there will be trade-off considerations that are fiercely difficult to manage. How much reliability will the excellent reasoner exchange for lower-cost reasoning? How many resources will the excellent reasoner give up for a reasoning strategy that is more robust? How much reliability will the excellent reasoner give up in order to tackle problems of greater practical significance? These are all legitimate worries. And our inability to address these worries in a principled manner might drive us to despair about the possibility of a useful epistemology. But there are two points to keep in mind. First, these worries are not specific to any particular epistemological theory. They are real worries for *any* epistemological theory that aspires to offer useful advice. What is more, they are worries for any thoughtful person. Life is complex. In our role as children, spouses, parents or friends, we are constantly acting in ways that have consequences for others, and we are constantly trading-off a wide array of costs and benefits against each other. And we manage, and sometimes we manage quite well. So while the worry raised is a legitimate one, there is no reason for despair. The second point to keep in mind is that not all cases will be hard cases. In at least some cases, one reasoning strategy will so dominate the alternatives that no serious trade-off considerations will arise. Consider again the 22 blood donors who were notified that they had tested HIV-positive, seven of whom committed suicide. What reasoning strategy is the epistemically best one for the blood donors and the medical professionals to use in thinking about this particular diagnosis problem? Frequency formats. For a minor expenditure of effort, we employ a reasoning strategy with unbeaten reliability on these problems. What is more, using this reasoning strategy would have led the donors to more accurate and realistic views about their health prospects — and almost surely to far less tragic results. Sometimes the epistemically best way of reasoning is also best from any other reasonable evaluative standpoint (e.g., moral and pragmatic). So trade-off worries will not arise. I venture to guess we are lucky enough to live in a world in which this is the rule rather than the exception.

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