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5. Uneasy chapters in the relationship between psychology and epistemology

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In recent years it has become common for some philosophers interested in the problems of ordinary and scientific knowing to identify themselves as “naturalistic” epistemologists. At minimum, this characterization indicates that the bearers consider themselves to be taking science, and particularly the science of psychology, very seriously – to be using empirical observations as an integral part of their attempt to fulfill epistemology’s traditional ambition to explain what knowledge is, how we come by it, and how it can be distinguished from its pretenders. Major difficulties attend any attempt to give a more detailed and general account than this, as to what distinguishes naturalistic epistemologists from their fellow, nonnaturalistic, philosophers of knowledge. Nelson (1984) expresses the problem:

At the heart of naturalization is the idea that the theoretical entities of philosophy be restricted to those explicitly needed in natural science. . . . The restriction to entities ‘needed in science’ is not really a restriction to anything very clear: allowable are sets, numbers, physical bodies, and spatio-temporal coordinate systems; but any general characterization is impossible without begging philosophical questions. . . . Such clarity as the idea of naturalization has will have to remain on an inexact, intuitive basis until we get down to cases. (p. 174)

In begging philosophical questions, a general definition of naturalism would also run roughshod over differences of opinion among naturalistic epistemologists regarding the distinguishing features of their scholarship. Therefore, in this chapter I shall be looking at a number of “cases,” focusing on several naturalistic epistemologists and trying to discover their beliefs on what effect naturalism will have on the working relationship between psychologists and epistemologists.

Whereas many naturalistic epistemologists regard themselves as part of a wholly new enterprise, there are others who stress the continuity between their views and those of, for example, Descartes (Paller & Campbell, 1987), Kant and Locke (Goldman, 1985b), and Hume (MacCormac, 1980; Ruse, 1986). However, in the light of the logical positivists’ recent dominance and their total rejection of all types of “psychologism”¹ (reviewed by Houts in

Chapter 3, this volume), it is understandable that naturalism be construed as a novel and revolutionary force within contemporary epistemology.

What then is naturalistic epistemology expected to revolutionize? From some accounts one would expect it to have this effect on the relations between epistemologists and psychologists. For example, Quine, in his founding paper "Epistemology Naturalized" (1969), states that if his arguments are valid, then "epistemology still goes on, though in a new setting and a clarified status. Epistemology, or something like it, simply falls into place as a chapter of psychology and hence of natural science" (p. 82).² This quotation represents what Kornblith (1985) called "the strong replacement thesis" in his introduction to an anthology on naturalistic epistemology. Having examined several alternative views of the relationship between psychology and epistemology entailed by naturalism, Kornblith concluded that "any epistemologist who rejects skepticism ought to be influenced in his or her philosophical work by descriptive work in psychology" (Kornblith, 1985, p. 12). Evidence of this conviction is the inclusion of two chapters by psychologists in the anthology, which Kornblith also edited.

Encouragement to believe that a major change is about to occur in the relationship between psychologists and epistemologists is also provided by Kitchener (1985), who suggests:

It is not too much of an exaggeration to say that post-Gettier epistemology is largely dominated by various types of 'naturalistic' epistemologies, the characteristic feature of which is the blurring of the distinction between science and epistemology. Virtually all of these theories would allow that psychology is relevant to the very program of epistemology. (p. 4)³

The establishment in 1974 of the Society for Philosophy and Psychology, by philosophers, may yet further reinforce the impression that contemporary epistemologists are turning to psychologists, eagerly asking for their collaboration in answering questions about knowledge, particularly scientific knowledge.

You have before you an uneasy chapter *about* some uneasy chapters: In this chapter, I shall attempt to communicate my uneasiness about the predicted collaboration between epistemologists and psychologists. I do not doubt the potential value or the ultimate feasibility of such an interaction, but I suspect that if epistemology is to become in any sense "a chapter of psychology," it will only be through careful negotiation based on frank acknowledgment of the obstacles to that union. It would be naive, especially for psychologists, to anticipate that it will simply "fall into place"; that we are not facing another troubled chapter in the history of interaction between philosophy and psychology. The source of my doubts is the observation that naturalistic epistemologists, viewed as a fraternity formulating policy for their interaction with psychologists, are far from reaching consensus on many im-

portant issues. The members of this unholy chapter disagree not only with one another but also with themselves. The headlines of their publications express an enthusiasm for collaboration which is belied by the small print of their views on why the collaboration is necessary and how it can be achieved.

If one were to approach the contemporary naturalistic epistemological literature for the first time, having been told only that the authors are seeking to have their discipline replaced by psychology, or to produce a merger between psychology and epistemology, then I think that one would expect that literature to have certain general features. In attempting to communicate my uneasiness, I shall discuss several of these "features" – that is, types of argument or recommendation – in turn, suggesting that philosophers do not favor anything that might accurately be described as "replacement" of one extant discipline by another, and that the future collaboration between epistemology and psychology that even the most naturalistic of naturalistic epistemologists envisage is much more limited than psychologists might judge to be optimal. In so doing, I shall sample the work of some philosophers whose views on the proper domain of psychology of science should influence, but not determine, the preparation of an agenda for this new discipline. As contemporary representatives of a discipline that has monitored and attempted to understand science since its inception, we can expect to find among their assumptions, dilemmas, and healthy quarrels, useful insights relating to the topic we psychologists are only beginning to examine.⁴ I hope to encourage exploration of this literature, even if my interpretation of it meets with the reader's resistance.

Positive arguments

Approaching the naturalistic epistemological literature for the first time, one would almost certainly expect it to contain positive arguments in favor of the relevance of psychology to the resolution of epistemological issues. However, as Haack (1975) points out, this expectation would be likely to meet with disappointment. Most philosophers arrive at the conclusion that psychology is relevant to epistemology via arguments purporting to show that other arguments, *denying* psychology's relevance, are inconclusive. These objections to psychologism focus on two closely related issues: the supposed circularity involved in using science to explain science (see Barker, Chapter 4, this volume), and the view that discovery and justification are distinct processes in that the validity of a theory or belief cannot be assessed on the basis of its origin. Wood, like many philosophers, past and present, is vehement in his commitment to this view. He insists:

The epistemologist must, however, guard against a particularly insidious form of genetic fallacy: viz. the supposition that the psychological origin of an item of knowl-

edge prejudices either favorably or unfavorably its cognitive validity – a fallacy which is psychologism at its worst. (Wood, 1981, p. 94)

The naturalistic epistemologists' strategy has been to attempt repeatedly to circumvent these objections by pointing out the elusive or illusory character of the nonempirical methods of explaining science and establishing truth conditions which they presuppose. Use of this strategy creates the impression that the naturalists are sadly resorting to psychology, retreating from their original aims and accepting a poor second best. As Quine puts it:

Why not settle for psychology? Such a surrender of the epistemological burden to psychology is a move that was disallowed in earlier times as circular reasoning. If the epistemologist's goal is validation of the grounds of empirical science, he defeats his purpose by using psychology or other empirical science in the validation. However, such scruples against circularity have little point once we have stopped dreaming of deducing science from observations. (Quine, 1969, pp. 75–76)

Similarly Giere, otherwise an enthusiastic naturalist, notes:

The discovery of *a priori* justifiable methods for evaluating evidence was a central goal of the major figures in the philosophy of science when Kuhn's book [*The Structure of Scientific Revolutions*, 1970] first came out. Carnap, Popper and Reichenbach all sought to discover an *a priori* justifiable method for science. The justification of particular scientific claims would then be secured by reference to these methods. If any such foundationalist program were viable, the use of the circle argument against attempts to naturalize the philosophy of science would be vindicated. (Giere, 1984, p. 7, emphasis in the original)

Any reasonable suggestion that these passages are indicative of a lack of commitment to collaboration with psychology must have many qualifications. First, the arguments against psychologism, provided by Frege (1884/1950; see Sober, 1978), but also by many others – for example, Popper (1970) and Reichenbach (1938) – are deeply entrenched within the epistemological tradition, and they need to be met if naturalism is to be more than the preoccupation of a few eccentric figures in the philosophical community. Furthermore, as Barker's chapter in this volume indicates, at least some philosophers do not regard naturalism as a retreat from traditional epistemological goals, and do look forward to its development with enthusiasm. As a final and important qualification, it should be noted that the lack of positive arguments favoring psychology's relevance may be only apparent, an illusion resulting from our implicit "taste" in arguments (influenced by the analytic tradition) having lagged behind our explicit opinions on what constitutes a good argument. Crudely, arguments against naturalism tend to assert their own necessary truth, whereas arguments in favor of naturalism tend to be empirical, contingent, hypothetical, reflecting the epistemological position that they represent. As a result, in the competition for our attention they tend to get lost because they now have for us a less crisp, convincing quality.

However, Sober (1981) suggests that any *a priori* arguments for psychologism, particularly those based on evolutionary considerations, should be viewed with suspicion.⁵

After these lengthy qualifications, I want to make a short and simple point. While naturalistic epistemologists are attempting to answer traditional arguments against psychology's relevance, they are, for the most part, seeking to communicate with their fellow philosophers, and not devoting a major part of their energies toward persuading psychologists to take part in a joint venture.

Claims for related methods and subject matter

If naturalistic epistemologists are predicting a thoroughgoing synthesis between epistemology and psychology, then one might expect them to argue that the proper methods of each field are either similar or closely related, such that their products will be complementary or mutually informative. There is certainly a debate, which is central to the naturalist's program, which some authors describe as being fundamentally a debate about method (e.g., Kitchener, 1985). Its focus is the denial that there is a valid distinction between analytic and synthetic truth; the denial that there is any proposition the truth of which can be assessed without reference to observations or experience.

The sense in which this is at the heart of a discussion about *method* is not entirely clear to me. The framework for the discussion seems to be as follows: Analytic truth tends to be associated with a certain "method" of attaining it – "conceptual analysis" or analysis of the meaning of terms – which is regarded as the intellectual skill possessed by philosophers. Similarly, synthetic truth is associated with the empirical method, which involves observation and experiment, and is regarded as the province of scientists, specifically for our purposes, psychologists. Naturalistic epistemologists almost unanimously deny, with Quine, the validity of the analytic/synthetic distinction – that the truth of a given *statement* can be dissected into a linguistic and a factual component – but this does not seem to lead them to reject the view that a given *individual*, by virtue of his or her training, can be skilled in *either* conceptual or empirical analysis, and that the philosopher is the first type of person whereas the psychologist is the second.

In my confusion, I am reassured to find that Alvin Goldman is also puzzled by the fact that philosophers who deny the possibility of purely analytic truths continue to use a method that apparently depends for its validity upon the existence of such truths. He notes that

although many philosophers preach the abandonment of analyticity, their practice sometimes belies their preaching. People do things very much *like* conceptual analysis even if they officially reject it. It is hard to do much in epistemology (or other branches

of philosophy) without feeling constrained to do something like conceptual analysis. (Goldman, 1986, p. 38; emphasis in the original)

One might infer from this passage that naturalistic philosophers continue to perform conceptual analysis but only with a guilty sense that it is an illegitimate and inferior method of inquiry as compared with empirical techniques. That does not seem to be the case. Goldman himself insists that the "logico-philosophical" method is indispensable both as a means of establishing the criteria appropriate for evaluating cognitive operations, and, more surprisingly, as a way of calculating which combination of cognitive operations will best fulfill these goals (Goldman, 1985b).

Dennett is unusual among naturalistic philosophers in expressing the view that the methods used by scientists and philosophers are different while stressing the *informality* of the philosopher's approach, in a manner that *deemphasizes* the nobility of philosophical argument. In his (1984) book *Elbow Room*, Dennett suggests that it is largely the use of "intuition pumps" by philosophers that distinguishes them from scientists. Intuition pumps are

thought experiments [which] are *not* supposed to clothe strict arguments that prove conclusions from premises. Rather, their point is to entrain a family of imaginative reflections in the reader that ultimately yields not a formal conclusion but a dictate of "intuition." Intuition pumps are cunningly designed to focus the reader's attention on "the important" features, and to deflect the reader from bogging down in hard to follow details. (p. 12; emphasis in the original)

A list of "great" intuition pumps is also provided, which irreverently includes Descartes's evil demon, Plato's cave, and Putnam's twin-earth.

In a later paper, Dennett (1986) explores in more general terms what philosophers mean by "conceptual analysis." He distinguishes two types. The first is traditional, although it continues to be practiced by many contemporary philosophers. This form of analysis presupposes that concepts such as "justice," "mind," or "knowledge" each have a true meaning which can be formalized in the same way as can mathematical concepts. The second type is practiced by naturalistic philosophers and is based on an acknowledgment that concepts are typically "embedded in practices, theories, institutions, preconceptions and projects" (Dennett, 1986, p. 18). In Dennett's opinion, the latter sacrifices "rigor of method for relevance of result" by treating conceptual analysis as

akin to anthropology or literary criticism, involving imaginative and critical interpretation of observed manners of speaking, presuppositions, and connotations, for instance, and as irreducibly informal and non-algorithmic in its methods. Here philosophy appears as a meta-discipline, parasitic on the investigations conducted by others with other agendas. (Dennett, 1985, p. 3)

I am tempted to conclude from this that the naturalist's variety of conceptual analysis is not a distinctive method at all, but a shared method applied to

distinctive subject matter, that is, concepts – people's beliefs about such things as knowledge, truth, etc. Surely "the scientific method" also involves imagination and critical interpretation; it is certainly nonalgorithmic, and post-positivist philosophy of science suggests that it is informal in the sense of being very difficult to specify. Perhaps the only difference lies in the extent to which scientists and philosophers are precise and systematic in making their observations.

These remarks are conventional enough, but if, as they suggest, the methods used by scientists and philosophers differ only in degree – a little more deduction and imagination here, a little less precision measurement there – then it is difficult to understand why naturalistic philosophers continue to have the use of conceptual analysis (construed as a method) as an important part of their self-image. If conceptual analysis involves little more than a certain sensitivity to language users' pragmatic intent, then one can sympathize with Goldman when he finds it difficult to do epistemology without doing conceptual analysis. Thus deprived, one would be hard put to have a chat about the price of eggs.

As a term denoting the subject matter, rather than the method, of philosophy, "conceptual analysis" conveniently introduces the question of what naturalistic epistemologists regard as an appropriate division of labor within the predicted "psychological epistemology." As far as I can tell, they are in agreement that philosophers and psychologists will have very different tasks, and address very different kinds of questions, owing to their respective preoccupations with "concepts" and "facts."

Under positivist rule, the boundary between epistemological and psychological concerns was defined by Reichenbach's distinction between discovery and justification (Reichenbach, 1938). The psychologist was supposed to be responsible for tracing the origin and development of beliefs (including scientific theories), whereas the epistemologist's duty was to assess their status as knowledge. These tasks were regarded as quite distinct in that the genesis of a belief was viewed as irrelevant with respect to its validity. Although the boundary has not been eliminated, its location has certainly changed. Goldman, for example, goes to great lengths to demonstrate that psychological data are necessary to address questions of justification, that psychology's relevance is *not* confined to descriptive, rather than normative epistemology. However, he continues to anticipate a clear division of labor. He writes:

Given this [Goldman's own] conception of epistemic rules, a paramount question is: What cognitive states and operations are available to human beings? What combinations of such operations could human beings instantiate or realize? These are the questions that should prompt epistemology to seek help from psychology. They are the questions that make psychology relevant. Will epistemology become, on our view, a branch of the psychology of cognition? Not at all. . . . Even a full (and accurate) set of answers to the above questions would not determine the correct set of epistemic

rules. Psychology cannot do this on its own. For one thing, a choice of right-making characteristic must be made and this falls outside the domain of psychology. . . . Second, if this characteristic features truth and falsity . . . some nonpsychological inquiries will be needed to help decide *which* of the available mental operations best promote these ends. (Goldman, 1985b, p. 55, emphasis in the original)

In sum, according to Goldman, the epistemologists' tasks include (1) instructing psychologists in appropriate methods of identifying belief-forming processes, (2) deciding how, in general, to evaluate these processes once they have been identified, and (3) performing the evaluation. Goldman misinterprets Campbell, a psychologist, as assenting to the view that these are exclusively philosophers' tasks, citing his characterization of evolutionary epistemology as a "descriptive epistemology" (Campbell, 1974). In fact, in referring to evolutionary epistemology as descriptive, Campbell (personal communication, 1986) was stressing that its methods are essentially empirical, that it does not seek general epistemological principles primarily through the analysis of various knowledge-related terms. Because it is also Campbell's view that methods of information acquisition are subject to empirical evaluation, he may be said to regard "analytic," rather than "normative" epistemology as the salient alternative to "descriptive" epistemology.

Goldman explicitly rejects the view that epistemology will become a branch of psychology, but even Quine, the leading proponent of "the strong replacement thesis" (Kornblith, 1985), anticipates a strict division of labor, at least while the new epistemology is becoming established. He suggests that his speculations concerning the relationship between language acquisition in childhood and scientific change

would gain, certainly, from experimental investigation of the child's actual learning of language. . . . But a speculative approach of the present sort seems required to begin with, in order to isolate just the factual questions that bear on our purposes. For our objective here is still philosophical – a better understanding of the relations between evidence and scientific theory. Moreover, the way to this objective requires consideration of linguistics and logic along with psychology. This is why the speculative phase has to precede, for the most part, the formulation of relevant questions to be posed to the experimental psychologist. (Quine, 1975, p. 78)

If it is part of the epistemologist's task to determine the questions to be addressed by experimental psychology, then it seems unlikely that their duties can be discharged in one shot before final extinction. Macnamara (1984), a psychologist with a deep admiration for philosophical approaches to the investigation of reference and meaning, nevertheless argues convincingly that Quine would have departed yet further from sense-data empiricism if he had been more familiar with psychologists' research on children's language. Thus he suggests that experimental work in this area has important philosophical consequences; it is not merely in need of philosophical grounding.

Wimsatt (1984) seems to have a less divisive conception of the distinctive

role of epistemologists, and specifically of philosophers of science, than either Quine or Goldman. He construes them as scientists with a certain specialty – that is, the practice of other scientists. This is not just rhetoric. He insists that philosophers of science must know as much about at least some parts of the subject matter and techniques of the scientific discipline that they study as the scientists themselves. But the philosophers must *also* be adept in conceptual analysis so that they are able, with the help of the psychological literature on problem solving, to identify the heuristics used by scientists and their potential biases. Thus, in an attempt to bridge the gap between factual and normative concerns, which Goldman finds so impassable, Wimsatt describes the primary role of the philosopher of science as that of a "therapist with respect to scientific strategy" (Wimsatt, 1984, p. 478).

Before concluding this section, it may be worth drawing attention to the fact that there is at least one philosopher (Shapere, 1988) who regards himself as adopting a naturalistic approach to the theory of knowledge, but who can see no role whatever for psychology in that enterprise. Following Quine, Shapere tends to equate psychology with the study of "human sensory receptors and their 'triggering,'" and denies that these processes can inform us about the use of evidence by scientists, since the latter is crucially dependent upon "well-founded background ideas." Perhaps Shapere does not realize that specialists in the study of sensation and perception no longer adopt an exclusively "bottom-up" approach. However, he *is* aware of the existence of cognitive and social psychology, and he attempts to block any immediate contribution they might make to science studies by alleging that they are parts of a discipline that is in a profound state of disorder.

It is certainly very possible that someday we may find that certain aspects of human psychology are relevant, in specific, well-grounded, and well-understood ways, to the epistemology of the knowledge-seeking and knowledge-acquiring enterprise and its assessment. But as matters stand today, there is no such relevance. And considering the pervasive disagreements among psychologists about fundamental psychological theory and methodology (they do not even agree about the proper vocabulary for talking about their subject-matter), and the quite primitive state of many areas of psychological investigation, perhaps this is just as well. If it were relevant, the best advice we could follow would be, "Wait; save your epistemological questions until (much) later." (Shapere, 1988, p. 102)

In this section I have pointed out that even those contemporary naturalistic epistemologists who do seek to collaborate with psychologists, do *not* regard the methods and subject matter of psychology and epistemology as the same, as a naive interpretation of the "replacement thesis" might lead one to expect. On the contrary, they seem to remain firm in the belief that conceptual and empirical analysis represent distinct intellectual skills (even if they now deny that they correspond to distinctive types of truth), and this conviction leads them to assign very different roles to philosophers and psychologists in the

epistemological enterprise. Wimsatt is unusual in stressing that heuristics are important investigative devices, the use of which is common to both groups; but he joins the others in assigning so many weighty tasks to epistemologists that one wonders how they will cope, and, if they can, whether their relationship with psychologists might not be one of intense supervision rather than collaboration.

Nickles (1986) has suggested that insistence upon the autonomy of the philosophy of science, in the form of attempts to demonstrate that its methods and subject matter are unique, is a consequence of the field's recent professionalization. This process, the transformation of philosophy of science from a back-burner or late-career interest of a few scientists, into a legitimate discipline with its own university departments, graduate training programs, etc., required practitioners to demonstrate the independence and integrity of their interests. Now that professional recognition has been achieved, claims to absolute distinctiveness are, according to Nickles, difficult to drop, even as they become insupportable and/or dysfunctional.

I certainly would not wish to deny the plausibility and potential usefulness of distinguishing types of intellectual skills – even those types that can be loosely labeled “conceptual analysis” and “empirical analysis.” What *does* make me pause is the implicit assumption that the necessary amalgam of these skills can be achieved either by collaboration among people each of whom represents one *or* the other, or by bringing the skills together in the heads and the careers of *epistemologists*. Failure to consider sharing the burden of combining intellectual skills is a theme that I shall pursue in the next section.

Proposals for organizing collaboration

It is not only the nature of epistemologists' assumptions about division of labor which perplexes me, but also their very implicitness. For if, as Wimsatt suggests, naturalistic epistemologists are at once scientists and therapists of scientific strategy, would not one expect them to discuss explicitly the relative merits of various practical ways of promoting collaboration between epistemology and psychology; to make recommendations concerning the adaptation of academic training programs, editorial policy, and institutional recruitment plans? If, ironically, they regard this not as their task but as the job of psychologists or sociologists of science, one might at least expect them, if they are eager for intensive collaboration, to be pressing these issues upon the attention of the relevant communities.

Instead, they seem content to accept the current arrangement which is roughly as follows: It is regarded as desirable for philosophers to have some training in the field of science that they intend to study, but *not* necessary for psychologists to comprehend the epistemological issues to which their work

relates. Philosophers hold appointments in philosophy departments, psychologists in psychology departments. The more adventurous philosophers spend time observing laboratory procedures, interviewing scientists, attending research group seminars, presenting papers at scientific meetings, arguing with scientists about their results and theories, or just reading the appropriate scientific literature, but it is not considered necessary to have scientists (and, of particular interest to us, psychologists) similarly taking part in the day-to-day life of philosophy departments. At its most stark, it appears that the “collaborative” role assigned to psychologists by philosophers consists in their being expected to (1) accept the prescriptions for good scientific practice made by philosophers of science, and (2) allow epistemologists of any stripe to plunder their literature, to take whatever they, the philosophers, find interesting and attribute whatever significance to it they please. Epistemologists and psychologists seem to be expected to collaborate in the sense of swapping the results of their endeavors, not by working together on the same problems. As Goldman writes in his book on *Epistemology and Cognition*:

I naturally select themes taken seriously by at least part of the cognitive science community. And I pay *some* attention to the evidence supporting various hypothesized processes, although I do not do this systematically or rigorously. Weighing evidence is the task of cognitive science itself (using currently accepted scientific methods). The strict role of primary epistemology is to borrow the results of cognitive science and assess the epistemic repercussions of these results. (Goldman, 1986, p. 182; emphasis in the original)

When psychologists such as Nisbett and Ross (two of the three chosen for representation in Kornblith's collection on naturalistic epistemology), and Tversky and Kahneman step out of line and take it upon themselves not only to study belief-forming processes empirically, but also to evaluate them epistemologically, they receive a rap on the knuckles from Goldman. In commenting on the former partnership's research on belief perseverance, Goldman (1986, pp. 214–219) regards himself (legitimately in my opinion) as sufficiently competent in the analysis and interpretation of empirical data to fault Nisbett and Ross (1980) on their tendency to overlook individual differences in response profiles. However, when it comes to arguing against their normative appraisal of the belief perseverance phenomena, Goldman does not approach Nisbett and Ross on equal terms. His principal objection is that these psychologists have not initiated their analysis by specifying general criteria of epistemic evaluation; they have not approached the problem as Goldman, a philosopher, would have done.

Goldman finds Tversky and Kahneman's (1974) research on the representativeness heuristic yet more suspect because what constitutes a “correct” response in many of these experiments depends on complex issues in probability theory, regarded by Goldman as one of philosophy's inviolable do-

mains. It is while discussing this research that Goldman states most clearly his view that psychologists have no business speculating about rationality. It does not seem to occur to him that if Nisbett and Ross or Tversky and Kahneman had not indulged in such speculation then they probably would not have made the observations that he finds so interesting.

[T]here is the question of the normative status of the subjects' responses. . . . This question, it seems to me, falls outside the domain of psychology, narrowly construed. It is not the job of empirical science to make normative judgments (whether ethical, aesthetic, or epistemological). When it comes to epistemic normative judgments, this is the task of epistemology. (Goldman, 1986, p. 306)

The bone of contention between these authors and Goldman lies in the former's willingness to regard some human cognitive processes as fundamentally irrational. Many other philosophers – for example, Dennett (1978, 1981) and Cohen (1981) – would also oppose the psychologists' interpretation. Even Giere and Wimsatt, who have much more sympathy with Nisbett and Ross, and with Tversky and Kahneman's analyses, than does Goldman, generally oppose what they perceive as cognitive psychology's tendency to, in Wimsatt's words, "revel in human irrationality."⁶ To a large extent, psychologists are allied with sociologists of science in the eyes of many contemporary naturalistic epistemologists, the sociologists being regarded as arch opponents of scientific realism.⁷

Extensive use of the psychological literature

Perhaps, biased by Goldman's view, I have presented a caricature of the kind of collaboration that epistemologists anticipate. But even if it were thoroughly representative, we as psychologists might still conclude that our data are much needed for the epistemological enterprise, even if we are not invited to attend to epistemological issues ourselves. Even this interpretation would have to be qualified.

First, it is not only psychological data that naturalistic epistemologists use. In a recent colloquium at the University of Chicago, Giere said quite explicitly that epistemology needs "cognitive science," but he defined cognitive science as anything from anthropology to neurophysiology, and suggested that the naturalistic philosopher of science should just lift concepts and techniques from these disciplines wherever and whenever he thinks they may be useful. I am not objecting to this, but merely pointing out that we should not be too flattered by epistemologists' attention; despite what Quine says, it is not exclusive. Only if it were exclusive might we be able to sit back and rely upon epistemologists actively to solicit our contribution to science studies.

Second, naturalistic epistemologists certainly do not embrace as useful all psychological research. They are far more interested in contemporary cog-

nitive psychology than anything else, and it appears that this is largely due to its convergence with philosophical preconceptions about the way in which thought should be thought about. As Sober (1978) puts it: "Psychologists are now more than ever talking about cognition in terms of information processing, an attitude which pictures thinking as an inferential process of the sort that logicians have long been investigating" (p. 165). Similarly, Haugeland's enthusiasm for research on artificial intelligence (AI), which he regards as almost indistinguishable from cognitive psychology, under the joint heading of "cognitive science," is rooted in its perceived continuity with philosophical tradition:

According to a central tradition in Western philosophy, thinking (intellection) essentially *is* rational manipulation of mental symbols (viz., ideas). Clocks and switchboards, however, don't do anything at all like rational symbol manipulation. Computers, on the other hand, can manipulate arbitrary 'tokens' in any specifiable manner whatever; so apparently we need only arrange for those tokens to be symbols, and the manipulations to be specified as rational, to get a machine that *thinks*. In other words, AI is new and different because computers actually do something very like what minds are supposed [by philosophers] to do. Indeed, if that traditional theory is correct, then our imagined computer ought to have "a mind of its own": a (genuine) *artificial mind*. (Haugeland, 1985, p. 4; emphasis in the original)

Indeed, the strength of epistemologists' commitment to cognitive psychology is such that in his essay on "True Believers" (1987), Dennett thought it necessary to remind them that the fundamental thesis of cognitive psychology, that there is a "language of thought," is an empirical hypothesis, not a necessary truth.

The third qualification is the fact that the use made of even the cognitive psychological literature is less than extensive. When naturalistic epistemologists adopt the role of philosophers of science, many of them refer almost exclusively to the work led by Newell and Simon (e.g., 1972) on problem-solving heuristics. Goldman (1985a, 1986) is exceptional in his attention to a broad range of cognitive psychological data. Some two hundred pages of his book are devoted to agile discussion of grass roots research, not just that which can be gleaned from textbooks, review papers, and other philosophers. In some chapters, such as that dealing with the literature on constraints on representation (how we "chunk" information for storage and manipulation) and their bearing on originality as a criterion of normative epistemic appraisal, Goldman manages to provide both a lucid discussion of the epistemological issues and an up-to-date survey of empirical research. In other sections, such as that concerning the top-down/bottom-up controversy in the study of perception and its implications for coherence and foundationalist conceptions of justification, the psychological sources are of a distinctly older vintage than the epistemological ones.

Patricia Churchland's (1986) preliminary synthesis of issues in the philos-

ophy of mind and in neuroscience represents a project comparable to Goldman's. Although not directly relevant to the emergence of psychology of science, Churchland's book exemplifies the kind of exciting, crossbred analysis that can be achieved when one adopts the view that

where one discipline ends and the other begins no longer matters, for it is in the nature of the case that the boundaries are ill-defined. This book is thus the result of what I came to regard as *neurophilosophical inquiries*. (Churchland, 1986, p. x, emphasis in the original)

In general, however, the proportion of the psychological literature that naturalistic philosophers of science actually use is very limited, in contrast with, for example, Singer's (1972) suggestions concerning what they *could* use. In his paper "Towards a Psychology of Science," he related techniques and findings from traditional learning theory, personality psychology, psycholinguistics, and the study of subliminal perception to epistemological issues. Most of these suggestions have no representation in the subsequent literature.

Perhaps the reason is that psychologists are still not collecting just the right kind of data. If so, one might expect suggestions from philosophers, inviting us to investigate particular questions in particular ways. Perhaps I am insensitive, but I can identify very few such requests. Indeed, the absence of an invitation to contribute is sometimes quite startling. For example, a group of authors led by Larry Laudan have recently stressed the need for philosophers to test empirically their theories of scientific change. Laudan et al. (1986) attempted to enlist the support of historians of science in this venture, but despite recognition of the need to examine both past and *present* examples of scientific change, they did not mention the possibility that psychologists may be able to help. This is surprising, given that many of the hypotheses that they would like to see tested, concern the motives and reasoning processes of individual scientists – the kind of information that psychologists are skilled in collecting.⁸

Summary and conclusions

To summarize the points made so far, I have suggested that although contemporary naturalistic epistemologists are firm in their conviction that epistemology and psychology are extensively and legitimately related endeavors, they continue to regard certain questions as distinctively epistemological and others as distinctively psychological, and anticipate that, on the whole, these questions will be tackled by different sets of individuals who, by virtue of their training, possess skills in *either* conceptual *or* empirical analysis. Where a combination of these skills is required, it is epistemologists who will attempt to bridge the gap.

This arrangement hardly represents the radical change in the relationship

between epistemology and psychology that the "replacement thesis" suggests. As Goldman (1986) puts it, within a naturalistic framework "philosophy is still the chief conductor or orchestrator of epistemology [although] many other disciplines . . . are important parts of the ensemble" (p. 1). To extend the metaphor: Contemporary naturalistic epistemologists seem to regard cognitive psychology as the first violin section, and perhaps Herb Simon as the concertmaster. Although social psychology and personality theory provide some interesting percussion, their perceived fellowship with the tympani (those sociologists of science whose rhythm threatens to drown the whole sweet melody) renders them suspect. Behaviorism and introspectionism are like sousaphones and church organs; they made pleasing music in other places and at other times, but they have no place in the contemporary Anglo-American orchestra.

My principal goal throughout this essay has been to provide an accurate description of naturalistic epistemologists' intentions toward psychology, but I could hardly deny that I have persistently portrayed them as, if not dishonorable, then somewhat misguided. Therefore, in concluding, I feel compelled at least to make more explicit the grounds for my discomfort, and my intuitions about the way in which collaboration should proceed. I wish I had a mature social epistemology/social psychology of science to appeal to in the process,⁹ but because preparing for a journey cannot be delayed until the destination is reached, I will lean heavily upon common sense.

I assume that epistemological theories differ from psychological theories in being more general and abstract (Dennett, 1978), and more likely to embody explicit claims concerning which belief-forming processes *ought* to be operative. I further assume that the purpose of naturalizing epistemology is to create an interdisciplinary arena in which it is possible for empirical observations to change epistemological theories. I use the term "change," rather than "affect" or "influence," deliberately. It seems that no sympathetic party to this debate would be content with an arrangement in which psychological data and theories are powerless to do more than endorse or substantiate a particular epistemological position.

Surveying the brief period since psychology and philosophy became distinguishable disciplines, Amundson (1983) has found that close associations between particular epistemological theories and particular psychological theories are not at all uncommon. He discusses several examples of such pairs, including Skinnerian behaviorism and positivism, Tolmanian learning theory and New Realism, and (straying toward psychology's interface with anthropology) Whorf's linguistic relativism and conventionalism. Amundson claims that for each of the pairs he has studied, the relationship between the epistemological theory and the psychological theory has been one of mutual endorsement. That is, empirical observations did not alter the scientific theory

such that it, in turn, changed the epistemological theory. Instead, the epistemological theory embodied a model of scientific explanation which validated the scientific theory, whereas the scientific theory generated data interpreted as empirical support for the epistemological theory. Amundson (1983) gives a succinct expression of an alleged example of this relationship: "Positivist explanation rules out cognitivist theory in favor of behaviorist theory, which returns the favor by ruling out causalist explanation in favor of positivist explanation" (p. 338).

Although Amundson does not deny that psychological data can change epistemological theory in principle, he does not explore in any detail why it may regularly fail to do so in practice. However, he airs the suspicion that this failure is associated with psychologists' eagerness for epistemological warrant, and with epistemologists' freedom to select among available findings those that support their own position.

In the light of Amundson's suggestions, let us consider one of the styles of collaboration that naturalistic epistemologists seem to have in mind; that in which psychologists and epistemologists each have distinct roles. Of the epistemologists whose views I have sampled, Goldman represents this persuasion most clearly, but there may be other philosophers, barely recognizable as having taken the naturalistic turn, who would lend it their support. Essentially, the idea is that philosophers will use conceptual analysis with the successful products of science as their raw material, whereas psychologists will perform empirical analysis with the behavior of epistemic agents, notably scientists, as their raw material. Collaboration will consist in swopping results and requests. Having little capacity to evaluate scientific data themselves, philosophers must trust the workings of the scientific community to ensure that those that reach the better journals and become distilled into textbooks are of the best quality. Similarly, psychologists will not be competent to evaluate competing epistemological positions, so they must take a philosopher's word for it that their studies are peculiarly relevant to his or her work, and pursue their investigation in whatever direction he or she recommends.

This proposal seems to me to be based on an exceedingly optimistic assessment of scholars' capacities to communicate research findings and project enthusiasm in an unbiased way when their audience has little understanding of the issues to which they relate. Without encouragement to take a broad interest in the scientific literature, philosophers are likely to be prey to their own confirmation biases. Philosophers and psychologists generally agree that this is a category of cognitive biases that are pervasive. They were numbered among Francis Bacon's "idols of the tribe" in his *Novum Organum* of 1620 (Bacon, 1853), and are the focus of a recent article by Greenwald, Pratkanis, Leippe, and Baumgardner (1986). Campbell (1959b) gave a succinct characterization of several confirmation biases in his discussion of systematic error

in communication systems. In Campbell's terms, a philosopher monitoring the psychological literature is faced with a "reductive coding" task. The confirmation biases to which he is consequently susceptible include "overdependence upon single input sources" (selective attention), and assimilation of the psychologists' messages that he *does* receive, to his "own attitudes" (epistemological theory), "prior output" (what he has written in support of that theory), "prior input" (psychological literature which he has read previously), and "the expected message." If the philosopher was untutored in psychology until he or she adopted a particular epistemological position, then there is little to prevent these biases from accumulating and converging upon his or her dogmatic persistence in holding that position. A tendency toward "distortion to please the receiver" on the part of psychologists may exacerbate the perseverative effect of philosophers' own biases. If psychologists are largely ignorant of epistemological issues, then it is unlikely that more than a few of them will tailor their research to address these issues in the first place. However, those who do may be motivated by a desire for epistemological warrant (sustained by ignorance of the details of philosophical quarreling), to avoid presenting data that threaten the epistemological theory with which they have made contact, and to acquiesce if the supporters of that theory interpret their data in a doubtful manner. The result of this scenario would be just the kind of mutual congratulation that Amundson claims has characterized collaborative efforts to date.

Apparently sensitive to these communication problems at a certain level, some epistemologists would attempt a solution by placing a tremendous burden of responsibility upon themselves. For example, Wimsatt's epistemologist-of-the-future must be an expert in so many fields that it seems he or she would be able to relax and let psychologists take over only when the apparatus is being built and data are being collected. Wanting this style of collaboration is not necessarily a sign of arrogance on the part of philosophers. It could result from a *lack* of self-confidence, doubts that they will be able to interest psychologists in the questions they find most interesting. Or, as Haack (1975) has suggested, it could be due to fear that if epistemologists do not advertise their many talents, they will soon be made altogether redundant, hoist by their own petard. However, I think that it is most likely to be the simple outcome of having been trained in one area of study and later discovering that another area is also valuable and exciting. Having developed respect for the methods and subject matter of science, these epistemologists would like to get involved without abandoning their original territory altogether.

Whether motivated by paranoia or by enthusiasm, a collaborative strategy in which epistemologists (or psychologists, or both) try to play the part of polymaths could still lead to an unsatisfactory psychological epistemology. Writing at the time when psychology and philosophy were suing for the divorce

from which they now seek reunion, Oswald Külpe underscored the fairly obvious reasons why this is the case. (For contemporary relevance, interchange "psychology" and "philosophy.")

The connection of the independent science of psychology with philosophy completely exceeds the working capacity, talent and inclination of a scholar. We older ones grew up into this situation and can still cope with it, if need be. But for the newly rising breed it is becoming practically impossible to serve both masters, to do one thing and not let the other lapse, if they do not wish to sink into dilettantism and superficial busy-work. It is therefore not surprising, when the psychological specialists take the upper hand and enter into a merely external relation with philosophy. Thus it becomes understandable that philosophy begins to defend itself against the invasion of such specialists and give vent to its displeasure about it more or less tastefully. (Külpe, 1912, *Zeitschrift für Pathopsychologie*, 1; translation from Ash, 1980, p. 403)

If it was impossible to be both a philosopher and a psychologist in 1913, then it could hardly be easier now. Külpe feared that attempts to combine identities would result in "superficial busy-work," but, prompted by Amundson, I wonder if the danger may not lie in "vertical" rather than "horizontal" task reduction. That is, instead of developing a cursory acquaintance with many epistemological positions and scientific literatures, individuals may focus on one epistemological viewpoint and the empirical research to which it is apparently related. This "overdependence on single input sources" (Campbell, 1959b, p. 35) would invite many of the problems encountered by a strict division of labor strategy of collaboration.

In case we need it, Külpe reminds us that, among other things, psychology and epistemology are social programs (Shadish, Chapter 15, this volume), and therefore we can expect attempts to alter their relationship to result in intergroup jealousy and obstreperousness. This tendency toward territorial squabbling might promote a strong psychological epistemology if it could be harnessed by common purpose and mutual comprehension. If a critical mass of both epistemologists and psychologists developed a broad and basic familiarity with the methods and preoccupations of the other group, then we could hope that they would be both motivated and able to criticize each other's work. I would expect this arrangement of partial overlap to increase outgroup iconoclasm by reducing the perceived value of easy epistemological warrant or empirical validation (familiarity with the muddle from which pockets of order occasionally appear in each discipline would surely breed healthy contempt), and by enhancing across-the-border detection of fudged data, feeble argument, and biased interpretation.

How, in practice, might these changes be effected? An obvious way to promote broad familiarity with issues in an allied discipline would be to make them part of undergraduate and graduate teaching programs. A psychologist with a basic education in epistemology would be more likely to choose to relate their subsequent research to ongoing epistemological debate, and much

better equipped to do so, than a psychologist who has discovered only in mid or late career that epistemology and psychology are enmeshed. Those psychologists who develop this interest as students, might pursue it later on by establishing dialogue with epistemologists at their home institution, reading epistemological books and journals, participating in epistemology discussion groups and conferences, etc.¹⁰ In short, they could profitably follow the example of those many philosophers of science who are currently finding ways to interact with the bench scientists whose work they study. As a result of these moves, a modest subset of psychologists might become competent both to conduct experiments capable of endorsing and *challenging* epistemological theories, and to communicate effectively the significance of psychological experiments to the epistemological community. This group might be regarded as a caste of interpreters, facilitating communication between psychologists and epistemologists, whereas psychologists at large would continue to be responsible primarily for the empirical component of the epistemological enterprise. If this were not the case, then epistemologists and psychologists would become indistinguishable, Külpe's worst fears would be realized, and the well-known advantages of a division of labor society would be lost. With an efficient executive and communication system, provided by a caste of interpreters on *both* sides of the disciplinary divide, those advantages might be optimized.

If Amundson's historical analysis is even partially right, then we need a collaborative relationship between psychology and epistemology that substitutes mutual monitoring for mutual congratulation. My proposal for achieving this is simplistic, it might even be quite wrong, but I would like to have persuaded the reader that plans of some kind are necessary.¹¹ Even though they are inviting psychologists to join the epistemological enterprise, epistemologists do not have a satisfactory niche prepared for us. It is our responsibility to assess our potential contribution to solving the problems of knowledge, and not only to expect but also to welcome informed resistance from epistemologists as we try to act on our conclusions.

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Notes

1. "Psychologism," is a term said by Sober (1978, p. 165-166) to denote "a family of views, all tending to downplay or deny distinctions between epistemology and logic on the one

- hand and psychology on the other." Kornblith (1985, p. 8) gives a more specific interpretation: "Psychologism is the view that the processes by which we ought to arrive at our beliefs are the processes by which we do arrive at our beliefs." The term has been used pejoratively since the nineteenth century when Gottlob Frege (1884/1950) was the most prominent opponent of psychologism.
2. Regarding the prior rejection of such efforts, it is noteworthy that in the only work by a psychologist that Quine cites in "Epistemology Naturalized" (i.e., Campbell, 1959a), the author is chary enough to devote three pages to making it clear that he is doing *psychology*, not philosophy, by quoting philosophers making the distinction and thereby apparently demonstrating that he understands and accepts their view. For example, "I should like to make this point without blurring the distinction between the philosopher's task and an empirical science of induction: indeed, I am dependent upon the distinction for a freedom to participate in the area without a feeling of intruding upon the private domain of a jealous speciality" (p. 155).
 3. In 1963, Edmund Gettier caused considerable unrest among ordinary language philosophers by demonstrating a fundamental weakness in their equation of knowledge with justified true belief. He showed that this formula does not specify sufficient conditions for knowledge, by elaborating two hypothetical cases in which a person would not be said to know a certain proposition even though the proposition is true, the person believes it, and he or she is justified in believing it. In each case the proposition was true only by virtue of a series of accidents.
 4. Attention will not be limited to the views of epistemologists who are regarded primarily as philosophers of science. Epistemologists such as Goldman (1986) and Lehrer and Wagner (1977) find the distinction between individual and social epistemology increasingly useful, and stress that both must contribute to an understanding of science. Similarly, psychologists might expect the study of perception, cognition, and personality at the level of the individual, as well as social psychology, to prove relevant in the analysis of science.
 5. Sober suggests one of many specific claims that are constitutive of the general view that psychology is relevant to epistemology; he believes that "the rules of correct reasoning have psychological reality." According to Sober, Quine is asserting the necessary truth of this proposition when he argues that linguistic translation entails attribution of belief in classical logic to the user of the language. This seems to Sober to be fundamentally inconsistent with Quine's appeal to evolutionary theory to explain how it could be that the rules of correct reasoning have psychological reality. He insists that "an evolutionary explanation pictures *alternative traits* enjoying different degrees of reproductive success. One cannot give an evolutionary account of the prevalence of a logically inevitable trait" (Sober, 1978, p. 185; emphasis in the original).
As a whole, Sober makes a persuasive case against the use of a priori arguments in support of psychologism. However, it is not clear that Quine could not answer this particular point by arguing that the a priori truth of a trait entails not that there are no alternatives, but that there are no *true* alternatives. This would amount to claiming that if the trait exists, then it is logically inevitable that selection will act to make it more prevalent.
 6. Stich (1985) is exceptional among philosophers in having not only supported the claims of those psychologists working on human irrationality (see Kahneman, Slovic, & Tversky, 1982, for a collection), but also in having collaborated with one of them (Stich & Nisbett, 1980). Grunbaum (1984) is another philosopher who is "irregular" in this regard. Although he does not identify himself as a naturalistic philosopher, and is not recognized as such, as part of his critique of psychoanalysis Grunbaum cites the work of Nisbett and Ross as an authority on the self-deceptive nature of retrospective introspection.
 7. Hesse (1980) is one of a very few epistemologists who regard contemporary sociology of science as potentially instructive rather than necessarily subversive. Knorr-Cetina and Mulkey (1983), and Collins (1985) are jointly representative of current research in this area.

8. See Houts (Chapter 3, this volume) for a list of areas in which psychology might contribute to science studies – a list that shares many items with Laudan's agenda.
9. Although I do not draw on them directly, Sherif and Sherif (1969), Bechtel (1986), and Chubin et al. (1986) have provided important contributions to the development of a social epistemology of interdisciplinary science.
10. The cognitive science groups at MIT and the University of Sussex provide examples, which may proliferate, of cases in which collaboration across administrative boundaries has led to the establishment of an interdisciplinary department, recruiting a mixture of philosophers, psychologists, linguists, and computer scientists.
11. Regarding the need for psychologists to participate more actively in their interaction with philosophers, it is interesting that, although psychologists now account for 50 percent of the membership of the Society for Philosophy and Psychology (Secretary/Treasurer: Pat Kitcher, Dept. of Philosophy, UC San Diego, La Jolla, CA 92093; annual dues \$15, students \$5), they are underrepresented at annual meetings, and the Society attracts fewer eminent psychologists than it does philosophers of high standing.

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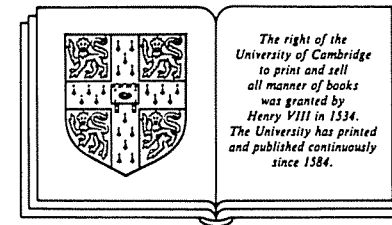
Psychology of science

Contributions to metascience

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