The Epistemological Status of a Naturalized Epistemology

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Philosophically inclined psychologists and psychologically inclined philosophers often hold that the substantive discoveries of psychology can provide an empirical foundation for epistemology. In this paper it is argued that the ambition to found epistemology empirically faces certain unnoticed difficulties. Empirical theories concerned with knowledge-gaining abilities have been historically associated with specific epistemological views such that the epistemology gives preferential support to the substantive theory, while the theory empirically supports the epistemology. Theories attribute to the subject just those epistemic abilities which associated epistemologies attribute to the scientist. The concept of epistemological significance is introduced as the significance a psychological theory can have for modifying the epistemological suppositions with which the theory was originally associated. Substantive psychological theories are strongly constrained by the epistemologies used in their development; the endorsement an epistemology receives from its associated theory should carry no weight. The alliance between psychology and epistemology is not progressive to the development of either field. Alternative sources of progress in epistemology and psychology are suggested.

I

Philosophical epistemology deals with a broad range of problems. Many of these are shared with the 'cognitive sciences' of psychology,¹ and certain branches of anthropology and sociology. Both epistemologists and psychologists are interested in the conditions under which and the processes by which beliefs are formed, and the susceptibility of humans to perceptual illusion and conceptual confusion. There are, to be sure, epistemological topics relatively remote from psychology – the nature of epistemic justification, rationality, and the concept of knowledge. The two sets of problems are not entirely separate. A psychologist's account of a perceptual illusion presupposes his knowledge that the subject's belief is false; some concepts (or other) of knowledge and falsity are necessary to his work.² These are not, however, the topic of his investigation, unlike the psychological processes resulting in the illusory perception. I will refer to the areas shared by epistemologists and psychologists as descriptive epistemology, in contrast with the (mostly) normative issues which are philosophy's alone. The existence of the shared problems is illustrated in any
standard text in the history of psychology. Typically about a third of the volume is devoted to pre-scientific (i.e. pre-Wundt) psychology, and most of this refers to classical philosophical epistemologists.

Contemporary epistemology shows strong interest in relating epistemology to the substantive discoveries of psychology. This interest results from several influences. One important influence is many epistemologists' loss of the Cartesian ambition to provide an epistemological foundation for non-philosophical (including scientific) knowledge. As long as our task was epistemically to support the sciences, we would be begging questions were we to use the results of scientific inquiry in constructing their own foundation. Such circularity is not encountered in the task simply of describing human epistemic processes. This kind of description was itself a part, but only a part, of classical epistemology. The other part was to show that these processes are rational, and that they do lead to knowledge. Earlier epistemologists tried to do two tasks with one theory – both to explain human belief causally and to justify human beliefs as knowledge. If we divide the question, we may address the descriptive problem free of the burden of the normative and foundationalist issues. This is much of the point of Quine's influential 'Epistemology Naturalized'. We may now use the otherwise inaccessible data provided by empirical psychology in our quest.

A second influence on the empirical basing of epistemology was Gettier's stunning refutation of the traditional justified-true-belief account of knowledge. This stimulated accounts according to which the knowing character of a belief was separated from its normative, justificatory background. Justified belief was often replaced by appropriately caused belief, or belief which is counterfactually (and thus nomically) related to the situation described in its content. This brought even knowledge closer to empirical study. Psychologists may have no account of epistemic justification, but they do deal in the etiology of belief. A third influence is the ascendancy of cognitivist psychology. Any dynamic scientific movement draws philosophical attention. But the cognitivists promise answers to questions which epistemologists were just about to ask. Cognitivist psychologists today have journals and professional organizations in common with epistemologists. Nary a behaviorist or logical positivist is to be seen.

There is a sense of security in all this. At last descriptive epistemology has found a way to escape the armchair and base its conclusions on cold fact. I will here argue that this security is false. Recent history of science has shown that substantive scientific theories are typically associated with specific methodological, metaphysical, and epistemological views, and that when the theory changes, the epistemology shares its fate. Some have inferred (mistakenly, I believe) a kind of relativism regarding the truth of
the substantive theories from these observations. I will argue a more modest position. I will suggest that empirically based epistemologies are quite common in the history of the cognitive sciences. I will argue, however, that this congruence of epistemology and substantive theory (1) undermines the rational support of any 'naturalized' descriptive epistemology, and (2) is to some extent counterproductive to progress in the cognitive sciences.

II

It is widely recognized that the cognitive scientist is himself using the cognitive systems which are the object of his study. This dual aspect of the scientist, as investigator and as exemplar of cognition, opens the possibility that, as his science advances, he will increasingly come to understand how science ought to be done. This is true of natural sciences as well, but in a different way. A natural scientist learns how best to do science in a world such as this one. A cognitive scientist learns how best to do science with a mind such as ours. In just this sense the behaviorist S. S. Stevens spoke of his brand of psychology, together with a certain kind of logical analysis, as 'the propaedeutic science' and 'the science of science'.

Consider the position of a philosopher striving to base her descriptive epistemological views on the substantive claims of psychologists. She will note immediately that there are competing theories within the cognitive sciences, especially if she wishes to take seriously views which are not, at present, viable contenders (such as cognitivism in 1950, or behaviorism in 1980). Second, she will note that each theory was generated, confirmed, and defended in association with specific methodological and epistemological commitments. (We may as well call such a set of methodology/epistemology/theory a 'paradigm'.) So the epistemologist cannot take the epistemological message of a substantive theory at face value. For how can she be sure that she is not simply gazing on the reflected face of the cognitive theory's ancestor epistemology? Perhaps the epistemologically relevant features of the theory were conditioned by the descriptive features of the epistemology with which the theory was associated. The problem is supposed to be solved by a bootstrap procedure within the paradigm – the epistemology has been gradually adjusted as the empirical results demand.

Although [psychology] must begin with a methodology (whether explicit or not), as the science develops, it generates its own methodology that in turn promotes the further growth of the science. Distilled in the process is an empirical epistemology.

Whatever the paradigm's archetypal epistemology might have been, it has been refined or replaced in accordance with the paradigm's empirically
discovered facts about cognition. The epistemologist can trust the paradigm because, it is supposed, if it weren’t based on a good epistemology, it would have produced results which would have shown the epistemology to be in error. Perhaps this is enough. Perhaps not.

Consider scientific theory $T$ and the epistemology $E$ with which $T$ was developed. The following is a General Significance Criterion:

A theory $T$ based on epistemology $E$ is significant to the extent that it is possible for $E$ together with some observations to lead a theory $T'$ in conflict with $T$.

This trivial criterion requires only that a theory must have more empirical content than its ancestor epistemology. Any theory, cognitive or not, which fails this criterion ought not to be considered a substantive theory, but a treatise on epistemology, with illustrations. Our present interests are more specialized. We are considering how a cognitive theory might have consequences for epistemology. The kind of significance required in $T$ is significance for a possible revision of $E$. If a cognitive theory can provide no challenge to the epistemological assumptions of the theory-builders, it surely cannot be taken as empirical support of those assumptions. Let us formulate, as a special case of the Significance Criterion, an Epistemological Significance Criterion:

A cognitive theory $C$ based on an epistemology $E$ is *epistemologically significant* to the extent that it is possible for $E$ together with some observations to lead to a theory $C'$ in conflict with $E$ (not merely with $C$).

Now, epistemological significance is not the only significance. A cognitive theory may lead to the discovery of many fine facts without being epistemologically significant. But it will not, by definition, lead to the discovery of facts which can be used to question its own epistemology. Since $E$ is never in jeopardy from $C$ (or from any of $C$’s possible alternatives) $E$ should not be regarded as having been validated by the fact that $C$’s conclusions assert $E$’s validity. This yields a surprising implication – $C$ does not provide rational empirical support for $E$ even though $C$ is an empirical theory which supports $E$’s validity.

Major cases in the history of the cognitive sciences fail to exhibit epistemological significance, as thus defined. This is not to say that the theories necessarily lacked such significance – perhaps an especially enlightened investigator could have made discoveries which supervened upon the ancestor epistemology. The historical fact is that this tends not to happen. The rejection of an epistemology in cognitive science is rarely, if ever, a result of empirical discoveries, inconsistent with the epistemology, which were
generated from within the theory’s cognitive paradigm. The epistemology and the substantive theory seem to come in a package, and the epistemology’s rejection occurs only through a ‘paradigm change’ which rejects the theory along with the epistemology. (Cases to be noted below.) The epistemologies/methodologies of the cognitive sciences seem to have exerted powerful constraints on the epistemological specifics of their associated theories – constraints which protect the epistemology against empirical attack. Again, the problem is not that no empirical discoveries are made, that nothing surprises the scientists working within the paradigm. The case is only that there are no epistemological surprises. The descriptive epistemology which comes out of a cognitive paradigm is very, very similar to the one which went in. Ideally, one would expect to find an occasional psychologist discovering that the facts of cognition show his epistemological assumptions to have been too liberal, or too restrictive, or otherwise mistaken. This seems not to happen. Why?

III

I have suggested that the epistemology/methodology associated with a cognitive theory places important constraints on certain features of the theory. Constraints placed on theory production are not in themselves distressing. Any modern methodology constrains against the production of theory by the use of, say, ouija boards. Constraints within the cognitive paradigm are more perplexing than most. This is because (1) the epistemologies embody claims about the dual aspect ‘knower’, and (2) the theories produced are to be used to modify or give empirical support to the paradigm’s epistemology. But if the production of a cognitive theory is constrained by the substantive claims appearing in its epistemology, the theory’s epistemological significance suffers with respect to that epistemology. The hopes for an empirically based epistemology are pinned on the ability of a theory selectively to endorse the epistemology which is most consistent with the discovered facts of cognition. But we must not overlook the possibility that a previous selection has occurred – a selective endorsement of that theory which is most consistent with the ancestor epistemology. This would vitiate any support the epistemology can reasonably receive from the theory.

As an illustration of this problem let us consider one of B. F. Skinner’s arguments against mentalist/cognitivist explanation. Skinner insists that he is not opposed to mental entities just because they are mental, but because they fail to explain. The criticism is epistemological, not ontological. Cognitivists cite as causes of observed behavior such unobserved entities
as motives, desires, anxiety, and so on. But: 'A disturbance in behavior is not explained by relating it to felt anxiety until that anxiety has in turn been explained.' Skinner surely does not intend this principle to be universally applied – all explanations would await the explanation of the Big Bang. The reason anxiety cannot serve as an explanans without itself being an explanandum is that anxiety is not an observed feature of the world. On Skinner’s view, an explanation of behavior must involve the deducibility of a behavior (or behavior-probability) from prior observable conditions. This is an epistemological principle, guiding Skinner’s scientific endeavors.

Here is a question for empirical psychology: Do internal (‘mental’) representations of unobserved events play a part in human cognitive activity? To this question behaviorists answer ‘No’ and cognitivists ‘Yes’. Here is a question for epistemology: Are explanations by reference to unobserved causes meaningful? To this question positivists answer ‘No (unless the unobserved causes can be inferred from prior observation)’ and others (call them ‘causalists’) answer ‘Yes’. A behaviorist has empirical reason to reject a causalist view of explanation. Unobserved causes are neither stimuli nor responses, and so (according to the behaviorist) play no role in human behavior. This is an implication of substantive behaviorist theory. If Skinner’s behaviorism is true, then whatever the significance of those verbal events which causalists call ‘explanations’, they do not create internal representations of perceptually remote states of affairs in the minds of the hearers of the verbal events. Since internal representations do not exist, any attempt at explanation which relies on creating such an object is doomed from the start. The only purpose an explanation can have is to adjust an organism’s responses to possible stimuli. Unobserved events are not stimuli, so causalist explanation cannot succeed.

Skinner is aware of the dual aspect of the cognitive scientist. He is conscious of developing an empirically validated epistemology. Moreover, he has succeeded. The epistemological account of explanation as deduction from observed events is supported by substantive behaviorist theory. Skinner’s anti-causalist views on explanation are not mere armchair prejudice; they have empirical support. Causalist explanation would require cognitive capacities which (behaviorist) psychology has discovered scientists not to possess.

Something is amiss. 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. This tastes like a free lunch. One is tempted to ask a chicken-or-the-egg question here – did the epistemology determine the theory or the theory the epistemology? What can be said is that, given this egg, this chicken was no surprise.
IV

I would urge on these grounds that the dual aspect of the cognitive scientist has reactionary, not progressive, effects. The effects are similar to those which methodological commitment has been claimed to have in other sciences. They are, if anything, more insidious in the cognitive sciences. Among the deepest epistemological debates are those between realist, positivist, and conventionalist interpretations of science. A commitment to a version of one of these epistemologies constrains the interpretation one might put on a given theory in the physical sciences, but it does not dictate theory choice itself. In contrast, an epistemology seems strongly to constrain even theory choice in cognitive science. Machian positivism did not legislate between nineteenth-century physical atomism and energeticism, so long as each was phenomenally interpreted. But it did side with Wundt’s content-psychology against Brentano’s act-psychology. If Mach was right about scientific method, then the introspective structuralists were right about psychology.12

The above examples of Skinner and the structuralists refer to quite Machian psychological paradigms. I will briefly note several paradigms associated with other epistemologies. In each case the substantive claims of the cognitive theory can be seen to give preferential ‘empirical’ support to the associated epistemology, while the epistemology can be used to defend the theory against extra-paradigmatic challenge.

(i) Oswald Külpe and the Würzburg school of introspectivists developed the hypothesis of ‘imageless thought’. This was in opposition to the positivist atomism and sensationalism of the structuralists. Imageless thoughts were propositionally described mental states, not reducible to the structuralist elements of sensations, images, and feelings. Somewhere around this time Külpe became a realist.13 E. B. Titchener’s structuralist/positivist arguments against imageless thought resonate with later behaviorist/positivist methodological criticism of cognitivist theories.14

(ii) E. C. Tolman developed strongly cognitivist accounts of learning, involving propositionally described ‘hypotheses’ and ‘cognitive maps’ internal to the subject. His strongest epistemological influences were the New Realists.15 Although Tolman dubbed his program ‘operational behaviorism’, both psychologists and their rat subjects were described as having internal states representative of the outside world. A close reading of his most ‘operationalist’ writings reveals that his operationalism was only nominal. He was a realist.16

(iii) Neobehaviorism (the school centered around Clark Hull and Kenneth Spence) was associated with the logical positivist view of theory structure and explanation, although they too referred to their view as
'operationalism'. Unlike the positivisms of Skinner and Titchener, this allowed 'logical constructs' which were not directly observed, but inferred from and 'defined' in terms of observable stimuli and responses of the psychological subject. One special feature of neobehaviorism was that it attributed 'mediating responses' to the subject. These were unobserved, inferred responses which themselves served as stimuli for further, ultimately overt responses. Mediating responses obeyed the same laws of conditioning as overt responses. Clearly the logical positivist (and not the Machian) view of theoretical terms was necessary to warrant the use of the 'mediating response' concept. But in addition, consider the application of neobehaviorist theory to the behavior of the scientist. The psychologist's 'logical construct' is defined in terms of his observations (stimuli?) some of them involving his active experimentation. This 'construct' sounds awfully like a behaviorally unobserved response, mediating between the scientist's environmental stimuli and his subsequent behavioral responses, some of them verbal or written. The neobehaviorist account of meaning itself as a mediating response is equally interpretable as a substantive theoretical development and as a rephrasing of the logical positivist account of theoretical meaning. Would not a scientist whose cognitive makeup was truly described by neobehaviorist theory be especially justified in using logical positivist theoretical structures? Would he not be unwise to restrict himself to Machian tools, and foolhardy to attempt realist objective reference? The epistemic powers of the scientist, as implied by his epistemology, are remarkably similar to those of his subject, as implied by his substantive theory.

(iv) Cultural anthropology has a heritage of conventionalism, derived in part from Whorf's linguistic relativism. Conventionalism is commonly endorsed in textbooks, and field results are even cited to support the paradigm's epistemology (unlike most psychology texts).

It is our position that [scientific] concepts are arbitrary selections from the vast range of things and events in the universe. Anthropological literature is full of examples that illustrate the arbitrariness of concepts. I have argued elsewhere that the methodology of 'cognitive anthropology' (the area with the most direct epistemological implications) is systematically biased so that native cognitive systems can only be described in ways consistent with conventionalism. This would surely make suspect the use of field results to justify epistemological principles.

(v) It is a commonplace that today's cognitivist psychology is associated with a realist epistemology. Jerry Fodor's 1968 Psychological Explanation served the same purpose as Rudolf Carnap's 1932 'Psychology in Physical Language'. Each aligned contemporary epistemological trends with the developing psychology of the time. Fodor pointed out that behaviorist
anti-cognitivism, stripped of positivist epistemology, is pitifully vulnerable to realist/cognitivist criticism. Fodor did not note that, if one were to adopt a robust behaviorist account of the behavior of scientists, realism would be a singularly implausible epistemology.

V

My purpose is only to provide a prima facie case for the notion of the self-supporting epistemology/substantive theory paradigm in the cognitive sciences. I take it that the cases I have mentioned show a significant congruence between descriptive aspects of an epistemology and substantive aspects of its associated theory. I have shown that the paradigm could function in the self-supporting mode. The reliability of the congruence in so many paradigms is circumstantial evidence that they do function in this way.

More direct evidence that such self-support occurs must come from detailed historical studies; aside from the few specifics mentioned, these are beyond the scope of the present work. One can easily find methodological/epistemological arguments used by cognitive scientists in criticizing competitor theories. Demonstrating the converse effect – the empirical support of an epistemology – is (except in anthropology) more difficult. It is often endorsed as a goal, but details are hazy. The spelling out of the epistemological implications of substantive theory may be seen as the philosopher’s job. (Few psychologists are as explicit as Stevens.) Once the philosopher has developed her epistemological position, perhaps using substantive theory in its articulation, the epistemology is now available for psychological purposes. Such cooperative efforts may still result in a self-supporting paradigm. In developing his proto-positivistic ‘logical atomism’, Bertrand Russell dismissed the problem of intentionality on the grounds that the then-new behaviorism did not require the ascription of intentional states. Once logical positivism had developed, it was of considerable use to later behaviorists in criticizing their intentionalist cognitivist opponents.

By way of a disclaimer, I am not asserting that some kind of logical impossibility enslaves theoretical progress in the cognitive sciences. I am simply presenting an account which would explain observed historical congruences, and which ought to be bothersome to the epistemological naturalist. Fortunately, theoretical change in psychology does not depend only on epistemological change for its impetus. However, the guaranteed use of a free, sympathetic epistemology cannot but be regressive to an
empirical science. The cognitive scientist’s dual aspect is not a blessing. It is a disguised burden.

One objection to the present view may be that it makes epistemological change in the cognitive sciences impossible, or at least inexplicable. Such changes do occur, and in response to the success or lack of success of the associated paradigm. This is true, but consistent with the present point. Such support (or stimulus for change) is not simply the assertion (or denial) of the epistemological arm’s validity by the substantive arm of a paradigm. Such an endorsement is a matter of course, and should carry no weight. (As for the possibility of a challenge, I know of no such cases.) But what is not a matter of course is the substantive arm’s success in the cruel world outside the paradigm. The epistemology may insulate the substantive arm to some extent, but it cannot fully isolate it from contact with other sciences and the contingencies of experimentation. The epistemology can inherit genuine support from its paradigm’s long-term success. But its in-house endorsement is automatic, and so insignificant. Like long-term success (or lack of it) other sources of epistemological change are extra-paradigmatic. Novel forms of explanation develop in allied fields (e.g. Pavlov’s digestive experiments, and the information processing model). Epistemological change in the natural sciences, and philosophical criticism both make modest contributions. The cognitive paradigm, left to its own devices, shows no great ambition to test its epistemology.

I urge that we reinterpret, but not abandon, the attempts to base descriptive epistemology on substantive cognitive theory. Philosophers who are today involved in showing the implications for realist epistemology of current cognitivist psychology should be seen as articulating a paradigm. They are not performing the historically unique task of empirically justifying an epistemology. Such empirically supported epistemologies are common in the history of the cognitive sciences. Perhaps today’s cognitivist/realists are articulating the right paradigm. Nothing I have said implies a relativist view of epistemology or of substantive cognitive theory. I believe that there is a true descriptive epistemology, and a true theory of cognition, and that the two are mutually endorsing. Spotting the right pair is simply more difficult than we may have hoped.

We epistemologists may desire something other than just one more in a series of empirical epistemologies. The goal of a rationally (and not just empirically) justified epistemology is not in principle beyond our reach. To achieve it, however, we must be able to assess the epistemological significance (as defined above) of cognitive paradigms. Perhaps realism is more subject to refutation by cognitivism than is positivism by behaviorism. If so, then the continued support of realism by cognitivism has objective value. But we cannot assess the epistemological significance of differing
paradigms until we begin to grant the same respect to the history of psychology (and other cognitive sciences) which we now grant to the history of the physical sciences. Until this happens the most reasonable support for a given epistemological position will come not from its direct psychological endorsement, but from its ability to cope with relatively uncontroversial epistemic achievements – for example those which occur within the physical sciences.  

NOTES

1 I will use the term ‘cognitive science’ in a very broad sense, to include any empirical science addressing as its topic the acquisition of knowledge. These will include ‘non-cognitivist’ behaviorist learning theories. ‘Cognitivism’ will refer to the non-behaviorist psychological schools. I apologize for this terminological puzzle.

2 I will arbitrarily refer to unnamed psychologists as ‘he’ and philosophers as ‘she’.


7 There is an important gap between descriptive epistemology and prescriptive scientific methodology. For present purposes it will be ignored. This ‘fact/value’ leap seems less difficult in scientific practice than in philosophical theory.


11 Cf. Zuriff, op. cit.


14 E. B. Titchener, Experimental Psychology of the Thought Processes, Macmillan, Syracuse 1909, Chs. II and III.


of epistemology in the debates of the period are the topic of ‘Place versus response: the epistemology of a “pseudoproblem”’, in preparation.


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