Cognition and Epistemic Reliability: Comments on Goldman[1]

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Goldman has offered a novel conception of epistemology, or, to use his terminology, epistemics. Although the project is complex and terminologically intricate, the central point that sets it apart from traditional epistemology is his conception of the objects of epistemic evaluation. Goldman claims that the objects of epistemic evaluation include not merely the beliefs a person holds and the relation of these beliefs to the evidence, but the processes and methods that are used in arriving at the beliefs. Processes and methods are epistemically commendable, on Goldman's view, to the extent that they reliably produce true beliefs, where "truth" is defined independently of epistemic norms. It is the role of the cognitive and social sciences to identify the processes and methods that are to be evaluated; the cognitive and social sciences thus have an essential role to play in epistemology, though they can by no means replace it.

The idea that the processes of belief fixation are proper objects of epistemic evaluation runs counter to traditional epistemology. Many epistemologists and philosophers of science have argued that the causal origin of a belief is irrelevant to the question of its justification. Nonetheless, I think that Goldman's case for treating causal origins as epistemically relevant is *prima facie* plausible. If it can be shown that belief fixation is the result of a well-defined process or set of processes, then certainly it would be of epistemic interest to know whether these belief-fixation processes were generally reliable. The strategy is similar to that of the scientific investigator who checks the reliability of her instruments, except that in the present case the instrument to be evaluated is the belief-fixation process of the knower herself. It must be admitted that this strategy is not sufficient to provide a final evaluation of a given knower, since the discovery that the basic processes of belief-fixation are unreliable need not doom the cognitive agent to a life of ignorance. Goldman allows the possibility of a regulative epistemics, which could recommend remedial action.

Although I find nothing basically wrong-headed about the strategy of evaluating the instruments of knowledge, I think that it runs into problems in application. Goldman's reliabilism depends upon cognitive science to determine the basic processes of belief fixation.[2] This assumes that belief-fixation is a well-defined cognitive process. Goldman is aware of this requirement, and he allows that our doxastic categories may need refinement in light of the findings of cognitive science (1986, pp. 15-17, ch. 8).

PSA 1986, Volume 2, pp. 312-318

But if cognitive science is to have implications for epistemology as envisioned by Goldman, then it must minimally be the case that belief-fixation (or its correlate in a refined doxastic taxonomy) is determined by a constellation of basic psychological processes--processes that constitute part of the "architecture of the cognitive system". This basic requirement is in conformity with the assumption that many cognitive scientists make, to the effect that there is a "universal cognitive machine" shared by all human beings. Goldman, however, needs to accept more than this assumption; for his program of primary epistemics to go through, there must not only be belief-fixation mechanisms, but they must affect the truth-ratio of the belief-fixation process in a regular manner--e.g., by determining a fixed ratio, or by affecting the direction of the ratio in a systematic manner. But, I claim, this is dubious. Some of the grounds for doubt may be practical, e.g., the relatively underdeveloped state of theories of higher cognitive processes such as problem solving. But some of the doubts are more fundamental, because they suggest that the project of testing the basic architecture of the human cognizer for reliability is conceptually unsound. Several arguments, each more general in scope than its successor, may be presented in support of these doubts.

First, the examples that Goldman gives of "basic processes" that may be evaluated veritistically are not plausibly regarded as basic processes--as processes that are constrained primarily or fundamentally by innate cognitive architecture. The chief example given in the paper concerns the work of Ross and his colleagues on belief perseverance--the case of misplaced confidence in one's ability to evaluate suicide notes. The alleged cognitive fault in this case is "a disinclination to correct errors," a fault that on the face of it would seem likely to promote unreliability. This "disinclination" is then explained by "basic processes" such as a "spontaneous search of memory" for information that coheres with a newly acquired belief, and the tendency to search for causal explanations. Now the spontaneous search of memory for coherence might indeed be a basic process, if memory itself is partly constituted by the integration of new beliefs into old. But is it a process that conceivably could be assigned a reliability value? The truth-ratio produced by such a process will clearly depend upon facts that can be independent of the process itself--specifically, upon the beliefs that are already stored. If stored beliefs are mainly false, the attempt at coherence will promote unreliability. If they are true, it will promote reliability. But coherence itself, independent of more primary reliability tendencies, does not seem likely to yield a reliability value, or to have a fixed effect on the direction of reliability. Moreover, it seems likely that the type of coherence achieved will be highly subject to the conceptual structure of the subject. A person who views the world in terms of the actions of supernatural forces will fit individual events into one structure, someone who believes that everything must be reduced to atoms in the void into another. So the reliability of a coherence strategy in the assimilation of new beliefs may be highly sensitive to the belief structure of one's culture, and so can be evaluated only in conjunction with *social* epistemics. In this case, primary epistemics in conjunction with cognitive science has no evaluation to report, because cognitive science is unable to provide a complete description of the basic process that is to be evaluated.

In order to be convinced that cognitive science can contribute to epistemology in the way Goldman suggests, we need to find an example of a basic process that can plausibly be assigned a steady influence on the reliability of belief fixation. The other chief examples from the paper--the use of imagery and of deductive logic--seem to share the problems of the first example. Although the "use of imagery" can be an epistemic virtue, it is difficult to suppose that it has a fixed effect on epistemic reliability; and the use of deductive logic is likely to be heavily permeated by learned factors, and hence more appropriately characterized as a method, not a process. Perception might seem to be the most likely candidate for a basic process that could be evaluated as regards its truth ratio, for it is typically viewed as the belief-fixer *par excellence*, and Goldman views it as such. Perception is simply mentioned as a highly-

reliable but non-inferential process in the paper; it is treated more fully in the book (1986, ch. 9). The chief analysis as regards reliability in the book asks whether the top-down component of perception need yield unreliability; the discussion is linked with "theory ladenness" in the philosophy of science, as an example of a potentially unreliable perceptual process. Goldman argues that top-downism needn't produce unreliable beliefs, and so isn't essentially pernicious; some top-down processes might be unreliable, but others might promote reliability. In all of this there is no evaluation of top-down processing as basically reliable or unreliable, and I would suggest that this is because there cannot be. "Top-down" processes do not constitute a basic psychological process or even type of process; top-downism is a schema for a process. So Goldman is right to insist that it need not lead to unreliability as regards perceptual belief-fixation. But that is because it yields no prognosis at all.[3]

So we are still looking for a basic process that gives a reliability value; and perception still seems the most likely candidate. It is, as it were, the inlet of information, and clearly if it were a faulty channel that would have disastrous implications for our epistemic lives. Now, we don't need cognitive psychology to tell us that perception is not a faulty channel with respect to basic uses such as navigating the spatial layout--our success insures us of that. The question is, can cognitive science give us a precise characterization of perceptual reliability, and inform us of principled limits?

One way cognitive science, or at least psychophysics, could do so is by informing us of acuity limitations, or of our inability to discriminate certain wavelengths of light. But this doesn't give us a stable reliability measure with respect to the associated epistemic tasks; it tells us that they cannot be performed by our visual apparatus unassisted, but of course we have developed other ways in which they can be performed. This is a general limitation on the attempt to assess our perceptual apparatus, since it seems unlikely that psychophysics could discover any principled limitations on our perceptual apparatus that could not be overcome through the use of instruments. But maybe we should be satisfied with local reliability measures that inform regulative epistemology but do not provide a permanent assessment of our epistemic situation. Indeed, if the most forceful argument against reliability measures on perception as a belief-fixation process were that they provide at best a local measure, Goldman's project could proceed by determining local reliabilities and fitting them into a larger picture.

With respect to the reliability of perception as a source of beliefs about the environment in the paradigmatic case--as a source of beliefs about the spatial layout and the physical objects in it--it seems that the belief fixation part of perception cannot be evaluated as a basic process even for the purpose of providing a local reliability measure. It has no reliability *per se*, and sets no limits on reliability for true beliefs about, say, ordinary physical objects. We know that things seen far away are less reliably known than things seen close at hand; this much we can count on, although we don't need cognitive science to inform us of this situation. But let us turn to the core case, the perception of ordinary objects under ordinary viewing conditions. Here, as in the other cases, too, it is not the perceptual apparatus by itself that fixes belief, but the perceptual apparatus plus our conceptual scheme. To detect chairs reliably we need a chair-concept. Now Goldman could respond that chair-concepts and their ilk should be included within the relevant cognitive machinery when one speaks of the basic-process of perceptual belief-fixation. But the concepts we use even in making ordinary object identifications are not basic; they are not a given part of the "architecture of cognition". Short of a radical fixedconcept innatism, which Goldman shows no signs of endorsing, our concepts are learned and therefore are a part of the subject-matter of what Goldman calls secondary epistemology, the epistemology of methods used by individual knowers. But so far as I can see, secondary epistemology is just traditional epistemology itself, with no apparently crucial need for cognitive science.

Goldman has a response to this, which is that the basic processes by which we learn concepts, or, more generally, epistemic methods, might be evaluated for their reliability. Here again, this seems unlikely. What architectural constraints are there on what belief-structures can be learned? So far, none. Learning theories are *general* theories, that place no limits on the content and the truth value of what is learned. As Goldman makes clear in some of his remarks on social epistemics, it is possible to conceive a social context in which the population is instilled with a set of concepts and beliefs that leave them in the dark. We might expect that animals in the wild will, on the basis of their cognitive capacities, learn the vicissitudes of their environment with reasonable success; we expect that on the whole such animals will be reliable learners, although it is unclear that they have beliefs. But with humans no such biological arguments are likely to work; as Goldman points out, our true beliefs about the atom could well lead to our destruction. As regards learning, we might be compared to a general purpose machine--it can run any of a wide range of programs. The reliability and the success of the machine depends upon its programming. And here again, a person's cultural context will be the central determiner of the reliability of what one learns, since it will provide the source of the programming.

There are two ways in which the line of discussion that I have been pursuing might be developed into a principled argument against the very idea of belief fixation as a psychologically primitive process. The first way would be to apply Jerry Fodor's distinction between "input systems" and "central systems" (1983). Perception, presumably, is an input system and might be reasonably ascribed a set of basic psychological processes--e.g., those underlying shape constancy and motion perception. The basic "veridicality" of these systems might be a necessary condition for a perceptual system being a locally reliable system in Goldman's sense, and this is perhaps a condition that we can grant. But this talk of "veridicality" and "reliability" does not in any way suggest that the basic truthfulness of perception has been assessed for the purposes of epistemology, for "veridicality" in this context means something like "accurate imagistic representation of spatial and chromatic properties;" to say that perception is veridical is to say that circles appear circular. But epistemology is about belief, and our beliefs about the world are a function of input systems plus central system processes. It is one thing to have a spatial representation of a circle; it is another to believe that one sees a circular object, or a dinner plate (or whatever). Now Fodor's position about the beliefs formed by the perceiver, as opposed to the quasipropositional states of the input-module's hypothesis formation routines, is that these beliefs are highly sensitive to the whole belief system, and hence can't be readily evaluated independently of the belief system as a whole. Note that this is not to suggest that perception itself is primarily "top-down" or "cognitively penetrable." Our belief system does not always or even often determine how things look spatially and chromatically--but one's basic conceptual repertoire and background beliefs *do* determine what one *takes* the things one sees to be. Goldman, although he makes scattered remarks in the book about modularity, at no place squarely faces the challenge of Fodor's argument about central systems.

The second argument stands back from the assumptions of contemporary cognitive science and questions the very meaningfulness of talking about a basic cognitive architecture. Goldman's program of primary epistemics is based on a picture of the human being as having a fixed cognitive architecture that is ready to be called into play. Expose the individual possessed of this architecture to experience, and some beliefs will be formed. On this view, variations in the social or cultural environment may lead to the formation of different sets of beliefs, but in each case the same belief-forming mechanism is in operation, reliably or not. To judge its reliability, we see how well it does at tending toward true beliefs in the widest range of circumstances. Or, on the assumption that some belief-fixation processes are really methods, then we need to test both the belieffixer and the method-fixer, by seeing the latter in action across a wide

range of circumstances. If, on the whole, the beliefs fixed and the belief-fixing methods learned are reliable, we can assess our epistemic situation favorably. If not, not.

This picture of the belief or the method-fixer as basic components of the cognitive architecture that are constant across social or cultural contexts has been challenged by another empirical discipline besides psychology and sociology, one to which Goldman gives scant attention. I am speaking of anthropology. In a pair of essays from his collection *The Interpretation of Cultures*, Clifford Geertz makes two points that bear on the acceptability of the picture just sketched (1973, chs. 2-3). The first runs as follows: "Whatever else modern anthropology asserts--and it seems to have asserted almost everything at one time or another--it is firm in the conviction that men unmodified by the customs of particular places do not in fact exist, have never existed, and most important, could not in the very nature of the case exist" (1973, p. 35). This is not the point that in fact every human grows up in some cultural context or other that affects that person's beliefs, but rather it is the claim that the very notion of a human being independent of culture, or having cognitive capacities defined independently of culture, is a conceptual confusion. There is no human cognitive architecture independent of culture because humans, unlike lower mammals, are not properly conceived as humans unless it is recognized that independent of acculturation the human organism is not possessed of an articulated set of cognitive capacities ready to be called into play. The second point undergirds the first by putting it into a biological context. Geertz reviews findings on human evolution (1973, chs. 2-3). Earlier views had it that biological evolution was complete before cultural development took off. These views were consistent with the idea that one could peel off the social and the learned to find the basic psychological and biological constitution of the organism, and hence they fit well with Goldman's primary epistemics, which attempts just such a peeling. But according to Geertz, new findings indicate that the development of culture *preceded* the biological development of *homo sapiens*. From this point of view, it has become a part of our biological makeup that we are not only physically immature at birth, but mentally unformed. And our mental formation is not pre-programmed, but depends upon the process of acculturation. The basic architecture must be such that it can receive its cultural programming, but this suggests that a primary fact about this architecture will be its plasticity. Hence, in order to assess one's basic cognitive architecture, the appropriate unit of analysis is the acculturated individual. But then one is assessing the methods programmed into the individual by his culture, not a barebones, innate cognitive structure. So we are once again directed toward secondary and "tertiary" epistemics, or the evaluation of both individual and socially-structured methods.

If the argument so far is correct, then Goldman's project must fall back to social epistemics. Since this portion of the project is just being born, there is less to evaluate. I find myself in basic agreement with Goldman's point that social structures are relevant to epistemological evaluation. He adopts the position, in common with the Edinburgh School of sociologists of knowledge, that social explanations can be given for both true and false beliefs, and that the characteristics of social structures can have a systematic influence on the beliefs that individuals form. Also, his characterization of a variety of epistemic virtues at the social level, including reliability, power, fecundity, speed, and efficiency, is certainly a welcome broadening of the dimensions of epistemic evaluation, just as was his inclusion of power and speed among the individual epistemic virtues. Although I do have reservations regarding his particular characterization of the Edinburgh school, and also regarding the forcefulness of his response to their anti-realism and his defense of realism, time does not permit these to be discussed here.

To summarize, Goldman's project is a serious contribution to the problems of the relationship between epistemology on the one hand and cognitive science and sociology on the other. His proposal to broaden the epistemic virtues, and his suggestion that psychological factors and social structures may condition knowledge are well taken.

But I have argued that the contribution of cognitive science to epistemic evaluation based upon reliabilist standards is problematic. The worries expressed here are not based upon a denial of the viability of cognitive science or of the notion of reliabilism; rather, they are the result of questions about the plausibility of there being a direct link between "basic psychological processes" plausibly construed and "reliable belief-fixation". Belief-fixation is liable to be most heavily determined by learned methods; while these methods might themselves be made the subject of epistemological investigation, one might well do so without assigning a crucial role to cognitive science. Indeed, the investigation of such methods most plausibly will be regulative as well as descriptive. But such evaluative and regulative activity is the mainstay of present epistemology and philosophy of science. By my lights, Goldman has suggested ways of broadening the standards of evaluation; he has not made the case that cognitive science has an essential, as opposed to a suggestive role to play in the work of epistemology.

Notes

[1]The text of the paper is as presented at the October meeting in Pittsburgh, except that time constraints required some abridgment of the last few paragraphs in the spoken version. The notes have been added.

[2]This role for cognitive science is made clear in the sections of the paper (as read) devoted to "the process/method distinction" and to "justification and reliability". According to Goldman, the basic processes identified by cognitive science are evaluated with respect to a system of J-rules (justificational rules) that describe the standards of evaluation (see the section of the paper on "justification and reliability" and Goldman 1986, chs. 4-5). He makes clear that these standards are or may be applied to particular belief-forming processes as part of the task of primary epistemics: "justificational rules are rules that authorize particular belief-forming processes, whether these be perceptual processes, mnemonic processes, reasoning processes, or what have you." See also Goldman 1986, pp. 21, 85-89, 181.

[3]It may be granted that Goldman's arguments do show the relevance of cognitive science for epistemology insofar as they speak against the idea that the existence of topdown factors in perception must show that perception is inherently unreliable owing to problems arising from theory-ladenness. Here, arguments drawing upon cognitive science are used to counter arguments for perceptual unreliability which themselves were based on psychological theories of perception that claimed to show the heavy influence of background knowledge and assumptions on the the very character of perceptual experience. But granting this point does not weaken the objections raised to the general reliabilist program as applied to perception. For in order for background beliefs and knowledge to affect perceptual reliability, they need not affect the very structure of perceptual experience (the way things look); they need only affect the beliefs formed on the basis of that experience (what we take the things that we see to be).

References

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