**Quine, Goldman, and Two Ways of Naturalizing Epistemology**

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Throughout much of its post-Reformation history, at least in Western Europe, prominent contributions to philosophy were made almost exclusively by people who also made prominent contributions to the natural sciences, and their contributions to each informed their contributions to the other. But why is this? Is it because the information that we get from the natural sciences is highly relevant to answering philosophical questions? Or is it because the findings of philosophers, such as they are, are highly relevant to answering the empirical questions raised by the natural sciences? In the century-long tradition that began with Gottlob Frege’s seminal writings in the 1870’s, and ran through much of the philosophy written in English in the 1950’s and 1960’s – the tradition that has come to be known as “analytic philosophy” – the prevailing answer to both of these last two questions was: no. According to this analytic tradition, the findings of the natural sciences were of virtually no relevance to philosophy, and vice-versa. Natural science was engaged in the enterprise of constructing a rational, coherent, and true understanding of how the world works, using the materials furnished by sensory experience. Philosophy, in contrast, was engaged in the enterprise of trying to delineate the rules by virtue of which any particular state or event would count as *rational*, *coherent*, *true*, or *understanding*, at all, and it did so independently of the materials furnished by sensory experience, and relying only on reasoning. Philosophy and natural science were not simply distinct, but neither could supply much useful information to the other.

For instance, in his *Logisch-Philosophische Abhandlung*, published in 1921, the great Austrian philosopher Ludwig Wittgenstein wrote:

"4.111  Philosophy is not one of the natural sciences.

....

4.1121  Psychology is no more closely related to philosophy than any other natural science....

4.1122  Darwin's theory has no more to do with philosophy than any other hypothesis in natural science."

The idea present in these passages was one shared by Gottlob Frege, Rudolf Carnap, A.J. Ayer, P.F. Strawson, and other prominent analytic philosophers in the century roughly spanning 1870 to 1970.  These analytic philosophers thought of the philosophical enterprise as one of discovery – by means of reflection alone, and without any essential dependence upon empirical information – of those rules by virtue of which something was good or bad, right or wrong, valid or invalid, true or false, rational or irrational.

How might this sort of philosophical enterprise proceed? Gottlob Frege’s work in the foundations of arithmetic provided the paradigm. By the 1870’s, a great deal was known about arithmetic, and Frege, a professor of mathematics at the University of Jena, was quite familiar with this vast body of knowledge. But Frege was interested not so much in extending the body of arithmetical knowledge as he was in discovering what it was, fundamentally, that made all of these known arithmetical facts true, and what it was, fundamentally, that made the proof of an arithmetical claim a valid proof. In order to discover these two things, Frege set about (in his *die Grundlagen der Arithmetik*, published in 1884) trying to find the smallest and simplest possible set of axioms, and the smallest and simplest set of rules of derivation, such that those rules, when applied to those axioms, would result in proofs of all known arithmetical truths and no known arithmetical falsehoods.

Around 1910, Bertrand Russell and Alfred North Whitehead extended Frege’s efforts, and attempted to axiomatize all of mathematics (in their three-volume *Principia Mathematica*, the volumes of which were published in 1910, 1912, and 1913). Soon after, Russell attempted something even more ambitious (in his *Our Knowledge of the External World* from 1914): to find the smallest and simplest set of axioms, and the smallest and simplest set of rules of derivation, such that those rules, when applied to those axioms, would result in proofs of all known truths concerning physical objects. This latter program was most fully executed by Rudolf Carnap in his work *Der logische Aufbau der Welt*, published in 1928. In this program, natural science was relevant at only one point, and that was in determining which statements concerning physical objects were true, and so needed to be derived within the axiom system that Carnap was attempting to formulate. But once the truths concerning physical objects are fixed, Carnap thought that the information provided by the natural sciences could have no further relevant to philosophy.

One way of understanding the late twentieth-century movement to "naturalize epistemology" is as a reaction against the idea that the natural sciences have such limited relevance to epistemology.  Epistemological naturalists take the findings of the natural sciences to be relevant to the epistemological enterprise of discovering those rules by virtue of which a cognitive state is rational or not, knowledge or not, or, more generally, correct or not. Many such epistemologists take the findings of psychology to be so relevant, and some of them also take Darwin's theory to be relevant.  But there are many different ways in which one can take the findings of the natural sciences to be relevant to epistemology, and consequently, many different forms of naturalism in epistemology.  In this essay, I discuss two very different versions of naturalized epistemology:  one version due to W.V. Quine, and the other due to Alvin Goldman. I then mention, very briefly, a few other forms of naturalism in epistemology.

**Section I.  Quine**

In contrast to many contemporary epistemologists, Quine takes epistemology to be a particular branch of the study of a particular kind of animal behavior.  To understand his conception of epistemology, we need to begin by understanding how he conceives of the study of animal behavior in general, and of linguistic behavior in particular.

All animals engage in goal-directed behavior in order to satisfy their needs in a challenging environment.  At any given point in time, we can think of an animal's behavioral repertoire at that time as a function from sensory stimuli to motor response.  Animals can change their behavioral repertoire over time as a result of conditioning, but different animals are susceptible to different sorts of conditioning.  For instance, dogs can be conditioned to salivate in response to noises that human beings cannot be conditioned to respond to at all.  And, to take another example, human beings can be conditioned to respond to certain complicated patterns of noises, gestures, or inscriptions by producing other complicated patterns of noises, gestures, or inscriptions; but other, non-human animals cannot be conditioned to respond in this same way to such stimuli.  In other words, human beings, unlike other animals, can be conditioned to respond to human language by producing human language themselves.

An animal's susceptibility to conditioning can itself be adaptively beneficial to the animal, or its species.  For instance, when a dog is conditioned to salivate in response to a particular noise that has, in the past, been perceptually associated with feeding, the dog's salivation makes it easier for the dog to chew and digest the food that it may be about to receive.  Again, when a dog is conditioned to respond to the sound "sit!" by sitting, that response makes it easier for the dog to acquire the desirable reward that has been associated with this response in the past.  And when a human being is conditioned to respond to the perception of human language by the production of human language, then the human being acquires a whole new mechanism for altering its behavioral repertoire:  communication.  The person who hides in fear when she sees the Aurora Borealis can learn to stay and calmly gaze at the sight once she learns through communication that there is no basis for her fear.  The leader who speaks in a frightened tone of voice to her followers can learn to speak in a calm and reassuring tone of voice once she learns through communication that this is more likely to produce the desired submissiveness in her followers.  And finally, the scientist who describes a fire as the shedding of phlogiston can learn to describe it as the consumption of oxygen once she learns through communication what the mechanisms of combustion are.

So, because humans can be conditioned to respond to human language by producing human language, they can be conditioned to communicate, and communication provides them with a new mechanism for altering their behavioral repertoire, including their linguistic behavioral repertoire.  Now, so far, I have described communication simply as a process of responding to certain complicated patterns of noises, gestures, or inscriptions by producing other complicated patterns of noises, gestures, or inscriptions.  But because human beings can be conditioned to respond differentially to patterns of enormous complexity, the patterns of which we speak here can be extremely complex.  And, as it happens, over the course of human linguistic history, these patterns have actually become extremely complex, and are now multi-layered.  We can describe one layer of pattens by segmenting the gestures, noises, and inscriptions into a certain set of units that we call "words"; we can describe another layer of patterns by segmenting the gestures, noises, and inscriptions into a set of larger and more inclusive units that we call "sentences"; and we can describe yet another layer of patterns by segmenting the gestures, noises, and inscriptions into a set of still larger and still more inclusive units that we call "theories".

We said above that human beings can alter their linguistic behavioral repertoire either as a result of conditioning or as a result of communication.  But when they alter their linguistic behavioral repertoire, they can alter any or all of these layers of patterns in the noises, gestures, and inscriptions they make.  Sometimes, the only part of their behavioral repertoire that they alter is their tone of voice (as in the case of the leader who learns to speak to her followers in a calm and reassuring tone of voice).  Sometimes, the only part of their behavioral repertoire that they alter is their words (as when someone learns to call someone else by a particular name).  But sometimes, they alter very large scale units in their gestures, noises, and inscriptions -- they alter their theories.  This process, whether it is brought about by conditioning or by communication, is what we will call "theory change".  And epistemology, for Quine, is a branch of the study of theory change.

Of course, the study of theory change includes many different branches.  Which of these branches is the one that Quine identifies as epistemology?  Here is how Quine himself puts the point:

"From impacts on our sensory surfaces, we in our collective and cumulative creativity down the generations have projected our systematic theory of the external world.  Our system is proving successful in predicting subsequent sensory input.  How have we done it?

"Neurology is opening strange new vistas into what goes on between stimulation and perception.  Psychology and more particularly psycholinguistics may be looked to for something to say about the passage from perception to expectation, generalization, and systematization.  Evolutionary genetics throws further light on the latter matters, accounting for the standards of similarity that underlie our generalizations and hence our expectations.  The heuristic of scientific creativity is illuminated also, anecdotally, by the history of science.

"Within this baffling tangle of relations between our sensory stimulation and our scientific theory of the world, there is a segment that we can gratefully separate out and clarify without pursuing neurology, psychology, psycholinguistics, genetics, or history.  It is the part where theory is tested by prediction.  It is the relation of evidential support, and its essentials can be schematized by little more than logical analysis."  (*The Pursuit of Truth*, 1-2)

The segment of which Quine speaks in the preceding paragraph -- the segment that can be clarified without pursuing neurology, psychology, psycholinguistics, genetics, or history; the segment that involves the relation of evidential support -- that is that segment the study of which constitutes epistemology.  In short, epistemology, for Quine, is the study of evidential support.

This characterization of epistemology is completely unremarkable for an analytic philosopher:  Carnap, Ayer, or Russell could easily have offered just the same characterization.  So what is distinctively naturalistic about Quine's conception of epsitemology then?  And in what sense does epistemology, for Quine, become a "chapter of psychology", as Quine repeatedly says?

The answer to these questions is strongly suggested by the context in which Quine situates naturalistic epistemology throughout his writings.  For Quine, the effort to naturalize epistemology is a response to the failure of a particular program that Carnap undertook.  Recall that Carnap initially attempted to construct a so-called “phenomenalistic” language into which (along with the terms of logic and set theory) all our talk of material things and events could be translated.  (A “phenomenalistic” language is one in which there are no expressions that refer to material things and events, and all reference is directed towards features of our subjective experience.) This program failed, even by Carnap's own lights: Carnap was never able to specify a language that was both phenomenalistic and also adequate for translating all our talk of material things and events. Carnap concluded, as did most other philosophers, that our talk of material objects and events is not simply a means for stating facts that could be more elaborately stated using phenomenalistic language.  But Carnap still thought that he could at least use his phenomenalistic language as a device for stating *the evidential basis* for all our talk of material things and events.  In attempting to naturalize epistemology, Quine was simply calling into question the central presupposition of this latter program, viz., that there is a phenomenalistic language in which the evidential basis for all our talk of material things and events could be stated.  If our talk of material things and events is not itself phenomenalistically statable (which it isn't, given the failure of the earlier program), then why suppose that the evidential basis for our talk of material things and events is phenomenalistically statable?  To naturalize epistemologize is to give up the presupposition that our evidential basis is phenomenalistically statable, and to treat the question of what our evidential basis is as itself an empirical question -- one to be answered by looking and seeing what our evidential basis is.  That is what is distinctively naturalistic about Quine's epistemology.

This interpretation of Quine's naturalistic program can help us to understand the otherwise puzzling paragraph of "Epistemology Naturalized" in which Quine first explicitly introduces the idea of a naturalized epistemology.  The paragraph that I have in mind here is the third paragraph of the following famous passage:

"...Two cardinal tenets of empiricism remained unassailable... and so remain to this day.  One is that whatever evidence there is for science is sensory evidence.  The other, to which I shall recur, is that inculcation of meanings of words must rest ultimately on sensory evidence.  Hence the continuing attractiveness of the idea of a *logischer Aufbau* in which the sensory content of discourse would stand forth explicitly.

"If Carnap had successfully carried such a construction through, how could he have told whether it was the right one?  The question would have no point.  He was seeking what he called a *rational reconstruction*.  Any construction of physicalistic discourse in terms of sense experience, logic, and set theory would have been seen as satisfactory if it made the physicalistic discourse come out right.  If there is one way there are many, but any would be a great achievement.

"But why all this creative reconstruction, all this make-believe?  The stimulation of his sensory receptors is all the evidence anybody has had to go on, ultimately, in arriving at his picture of the world.  Why not just see how this construction really proceeds?  Why not settle for psychology?  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.  If we are out simply to understand the link between observation and science, we are well advised to use any available information, including that provided by the very science whose link with observation we are seeking to understand."  ("Epistemology Naturalized", 75 - 76)

Many philosophers have wondered what Quine could have in mind here when he invites epistemologists to "see how this construction [of one's theory of the world] really proceeds", to "settle for psychology", and to "understand the link between observation and science".  These claims seem to suggest that Quine wants epistemology to cease studying how we *ought* to think, and confine itself to studying how we actually *do* think – and this is just how some philosophers have interpreted Quine’s program (e.g., see Jaegwon Kim’s widely influential interpretation of Quine in “What is Naturalized Epistemology?”). But this interpretation of Quine has two problems. First, it makes Quine’s view look very unattractive: clearly, there is some worthwhile project of studying how we ought to think, and if epistemology isn’t the study of how we ought to think, then what is? And second, this interpretation of Quine is very hard to square with the passage quoted above from *The Pursuit of Truth*, in which Quine says that we can clarify relations of evidential support "without pursuing psychology".  Isn't this latter claim simply inconsistent with Quine's claim that epistemology -- the study of evidential support -- can become a chapter of psychology?

The interpretation that I have offered dissolves the apparent inconsistency, and also helps us to understand why Quine is not rejecting the epistemologist’s effort to understand how we ought to think.  I’ll take up each of these points in turn.

When Quine says that we can clarify relations of evidential support without pursuing psychology, what he has in mind is this:  relations of evidential support are, broadly speaking, logical or statistical, and we can understand what those relations are by doing logic or statistics, not by doing psychology.  But when Quine says that epistemology can become a chapter of psychology, what he has in mind is this:  in order to understand what evidence supports our theories, and to what extent, what we need to do is to locate our actual evidential bases, and determine to what extent those evidential bases do stand in the relevant logical or statistical relations to our theories.  Of course, in figuring out what our actual evidential bases are, or even in figuring out what our theories are, we may apply a principle of charity, and allow our determination of those bases or those theories to be partly guided by consideration of which evidential bases would successfully support which theories.  This is a way in which logic and statistics can themselves help to guide the empirical study of psychology.  But still, the attempt to figure out what our actual evidential bases are, and what our theories are, is a thoroughly empirical enterprise, and this is the sense in which epistemology -- the study of evidential support relations -- is a chapter of psychology.

But, to say that epistemology is a chapter of psychology is *not* to deny that epistemology has a normative dimension, i.e., that it concerns how we *ought* to think.  As Quine writes:

"To emphasize my dissociation from the Cartesian dream, I have written of neural receptors and their stimulation rather than of sense or sensibilia.  I call the pursuit naturalized epistemology, but I have no quarrel with traditionalists who protest my retention of the latter word.  I agree with them that repudiation of the Cartesian dream is no minor deviation.

"But they are wrong in protesting that the normative element, so characteristic of epistemology, goes by the board.  Insofar as theoretical epistemology gets naturalized into a chapter of theoretical science, so normative epistemology gets naturalized into a chapter of engineering:  the technology of anticipating sensory stimulation."  (*The Pursuit of Truth*, 19)

So there is theoretical epistemology, which is the study of what evidence we actually have for whatever theories we actually hold.  And there is normative epistemology, which is the study of how to adjust our theories in order most effectively to anticipate sensory stimulation.  And both of these pursuits can be guided by logic and statistics, though both of them are heavily empirical.  In this respect, the different branches of epistemology are like the different branches of the study of human action:  we can study the reasons for which people do what they do, and we can also study what sort of action proves most successful in achieving its goals.  But both of these branches of empirical study are guided by our theory of what it is to act rationally (or what is typically called “rational choice theory”).

For Quine, then, epistemology is the study of evidential support, and this study has three branches.  First, there is the empirical, psychological study of what evidence we actually have, and what theories we actually hold.  Second, there is the empirical, normative study of what strategies of theory change we should employ, in order to optimize or at least improve our ability to anticipate future sensory stimulation.  And third, there is the logical and statistical study of evidential support relations, i.e., of the logical or statistical relations that obtain between the sentences our assent to which is most directly and universally keyed to particular sensory stimulations ("observation sentences", as Quine calls them) and our theories.  The first two of these branches are obviously empirical, and the second of those two empirical branches is obviously normative.  In describing only the first two of these branches as obviously empirical, I do not mean to suggest that the third branch is non-empirical.  But empiricality is a matter of degree for Quine.  Our beliefs about logic and statistics are less immediately or directly impacted by our sensory stimulations than our beliefs about psychology are.  Or, to put the same point using Quine's famous metaphor, logic and statistics are farther from the sensory periphery of our web of belief than psychology is. (An accessible discussion of this metaphor is in Quine and Joseph Ullian’s co-authored book *The Web of Belief*, published in 1970.)

Quine himself devotes most of his efforts in epistemology to the second of these three enterprises.  Specifically, he outlines a number of principles that should (and, he thinks, for the most part do) guide our theory change so that it can successfully anticipate future sensory stimulation.  There is, for instance, the principle of simplicity, according to which we should hold to the simplest theory that predicts and explains all our evidence.  And there is the principle of minimum mutilation, according to which we should change our theories as slightly as possible in order to predict and explain all our evidence.  (Again, see *The Web of Belief* for discussion of these principles of theory revision, as well as others.) These principles can conflict with each other, and sometimes conflict with other principles that guide our efforts to change our theories so as most successfully to anticipate future sensory stimulation.  In case of such conflict, we must simply strive to do the best we can, and there is no recipe for that.  All we have are principles to guide our theory choice, and good judgment to guide us in cases in which the principles conflict.

I have so far ignored a complication in Quine's account of evidence and of evidential support.  For Quine, the logical or statistical relations involved in evidential support must be relations among sentences; some sentences imply or probabilify other sentences.  But when Quine describes our evidence, he frequently speaks of sensory stimulations.  How do the sensory stimulations that Quine takes to constitute our evidence relate to the sentences that Quine takes to stand in relations of evidential support (e.g., implication or probabilification) with our theories?  For Quine, this relation is causal.  The sentences that Quine takes to stand in relations of evidential support with our theories are what Quine calls "observation sentences".  Observation sentences are those sentences which, for each person in a given community, have the following property:  no matter what else that person believes, when she receives a sensory stimulation of a particular kind, then she will assent to that sentence.  Assent to observation sentences is causally keyed to the occurrence of a particular kind of sensory stimulation within a given community.  This makes observationality relative to a community of speakers, but it also insures that the same evidence can be had by multiple speakers within that community.  In short, it insures the publicity of evidence, at the cost of making it community-relative.

Two things remained constant throughout Quine's writings in epistemology.  The first is that he held steadfastly to the conception of epistemology that I have outlined above:  I see no sign of his deviating from this conception -- at least at the level of abstraction described above -- at any point in his career.  Quine changed his mind about a number of things in philosophy, but he did not change his mind about the points mentioned above.  The second is that, at no point in his career did Quine have anything to say in response to some of the questions that form the focus of so much epistemological attention since the 1963 publication of Edmund Gettier’s agenda-setting paper “Is Knowledge Justified True Belief?”, e.g., what is knowledge?  What is it for a belief to be justified?  What is it for someone to be justified in believing a proposition?  Does knowledge require justification?  Does knowledge require some especially strong relation between the believer and the world?  It is these questions to which Alvin Goldman has devoted most of his attention in epistemology over the past four decades, and it is to the history of his various views that I now turn.

**Section II.  Goldman**
Unlike Quine, Goldman did not begin his career in epistemology by attempting to develop a comprehensive account of how epistemology fits into the study of human behavior. Rather Goldman began by addressing himself to a very local epistemological issue to which Quine never addressed himself. Specifically, Goldman began work in epistemology by trying to solve the Gettier problem, i.e., by trying to explain what must be added to an account of empirical knowledge as justified, true belief in order to offer non-circular conditions for knowledge that are both necessary and sufficient.  The Gettier problem can be briefly illustrated by means of the following example (but for more detailed discussion of the problem and the vast variety of proposed solutions, see the chapter from Richard Feldman in this volume):

Jones believes, on the basis of very compelling evidence that his coworker Smith owns a Ford.  And so Jones has a justified belief that Smith owns a Ford.  But this belief is false:  despite all the evidence to the contrary, Smith does not in fact own a Ford.  Nonetheless, Jones's coworker Brown owns a Ford.  And so, when Jones infers from his belief that Smith owns a Ford to the conclusion that one of his coworkers owns a Ford, Jones comes to believe something true.  Furthermore, this latter true belief is justified, since it is deduced (by means of an obviously valid deduction) from something else that Jones justifiably believes.  So Jones has a justified and true belief that one of his coworkers owns a Ford.  But Jones does not know that one of his coworkers owns a Ford.  So justified true belief is not sufficient for knowledge.

It is possible to multiply such examples *ad infinitum* for cases of empirical belief, but it is not clear that such cases can be constructed for non-empirical belief.  In fact, Goldman took the traditional analysis of knowledge as justified, true belief to be correct for non-empirical knowledge, but he wanted to find an adequate analysis of empirical knowledge.

Roughly, on Goldman's earliest published view (in his 1967 essay "A Causal Theory of Knowing"), S has empirical knowledge that p if and only if S's true belief that p is causally connected in an appropriate way with the fact that p, where what counts as an appropriate causal connection is to be specified simply by appeal to an open list of examples (e.g., perception, memory, testimony from someone who perceives or remembers, and good inference from something perceived or remembered).  Gettier cases fail to qualify as knowledge because they do not involve the appropriate sort of causal connection between the fact believed and the believer's belief.

While Goldman gave up this causal account of knowledge nine years later, he never gave up its central contention that, whether or not S knows that p (in the particular sense of "know" that interests epistemologists, Goldman now adds) depends upon the *causal origination* or *causal sustenance* of S's true belief that p.  In fact, it was Goldman’s insistence upon this point, and his way of developing the point, that put his epistemology in contact with empirical science, and thereby made him a kind of “naturalist” in epistemology. For Goldman, knowing is a matter of having the right kind of causal history. Epistemology is supposed to tell us what very general sort of causal history is the “right” kind for knowing, but cognitive science is supposed to spell out the details, and tell us specifically which of our cognitive faculties or methods of belief-formation provide this right kind of causal history, and under what conditions. Thus, epistemology and cognitive science work together, on Goldman’s view, to provide us with a detailed account of what it is to know something, which is just an account of the causal history that constitutes knowledge.

In his 1976 essay "Discrimination and Perceptual Knowledge", Goldman presented what is generally regarded as a counterexample to his 1967 causal theory of knowledge, and attempted to accommodate the example by appeal to a view that he would eventually call "reliabilism", which is itself a version of the general idea that whether or not S knows that p depends upon the causal formation or causal sustenance of S’s true belief that p.  Here is the example that Goldman presented (and which he credits to Carl Ginet):  Henry is driving through the countryside looking at a barn.  His vision is normal, and he sees the barn clearly, and thereby forms the belief "there's a barn there".  Unbeknownst to Henry, however, this is fake barn country, and almost all of the apparent barns in this region happen to be mere barn facades; Henry just happened to be looking at one of the only real barns in the area.  While Henry has a justified, true belief that there's a barn there, he does not know that there's a barn there.  But notice that, in a normal situation in which Henry is driving through the countryside and sees a barn, he can come thereby to know that there's a barn there, even though the causal relation between his belief and the fact that there's a barn there is no different in the barn facade case than it is in the normal case.  So Henry's failure to know, in the barn facade case, that there's a barn there, cannot be due to any lack of an appropriate causal connection between his belief that there's a barn there and the fact that there's a barn there.  His failure to know must be due to something else.  But what could it be due to?  Goldman claims that it is due to the fact that Henry cannot reliably discriminate real barns from barn facades (at least not by looking at them from the distance and angle that he actually occupies), and that the exercise of this discriminatory ability is required for knowledge that there's a barn there when Henry is in barn facade country, but it is not required for such knowledge when Henry is normally situated.  In short, knowing that p requires an ability reliably to discriminate between its being the case that p and its being the case that some relevant alternative to p obtains, where which alternatives count as "relevant" varies from context to context.

Goldman leaves it open whether the context that fixes the range of alternatives that are relevant to whether or not someone knows something is the context of the putative knower, or is rather the context of someone who is thinking about the putative knower. But, no matter how this issue is resolved, Goldman’s reliabilism involves the claim that knowing involves the exercise of a discriminative ability. And this is just one way of spelling out the very general claim that, for S to know that p, S’s true belief that p must have the right kind of causal history.

The 1976 view that I just described is a view about the requirements for perceptual knowledge, and it is a view according to which, as Goldman originally presented it, perceptual knowledge does not require that one's perceptual belief be justified.  But by the time of his 1979 paper "What is Justified Belief?" Goldman reversed himself on this last point, and claimed that perceptual knowledge, like non-empirical knowledge, does require something that epistemologists call “doxastic justification”: in other words, it requires that the belief that constitutes knowledge is itself a justified belief.  (For someone to have a justified belief is different from her having a justification to believe something: you can have a justification to believe something whether or not you actually believe it.) But doxastic justification, on Goldman’s view, itself requires a certain kind of causal reliability.  On Goldman's view, for a belief to be justified involves its being formed by a *reliable process*.  A reliable process is one that tends to produce a high ratio of true beliefs.  It is empirically obvious that such processes include at least normal forms of perception, memory, inference, and testimony:  this is why these processes are sources of doxastic justification, and so of knowledge.

Goldman elaborates the view just mentioned in much greater detail in his 1986 book *Epistemology and Cognition*.  That book contains a detailed account of knowledge, as well as of doxastic justification.  And the book also elaborates the relationship that Goldman takes epistemology to have to the empirical sciences of cognition.  I'll start by explaining Goldman's account of knowledge, then proceed to his account of justification, and finally say something about how he conceives of the relation between epistemology and cognitive science.

Goldman's 1986 account of knowledge and of justification, like his earlier accounts, is a causal reliabilist account.  According to Goldman’s 1986 account:

"S's believing p at t is justified if and only if
(a) S's believing p at t is permitted by a right system of J-rules, and
(b) this permission is not undermined by S's cognitive state at t."  (Goldman 1986, 63)

This requires a bit of gloss: J-rules are rules according to which some beliefs are permitted and others are not.  A system of J-rules is "right" just in case its dictates concerning which beliefs are permitted and which are not are correct (and such correctness may, for all we say here, be independent of whether or not anyone takes such dictates to be correct).  Finally, there are a couple of ways in which a permission may be "undermined" by the believer's cognitive state.  One way in which such undermining can occur is for the believer to have reason to believe that her belief that p is not justified.  Another way in which such undermining can occur is for the believer simply to believe that her belief that p is not justified.  In either case, S's belief that p is not justified.

Goldman fleshes out this schematic account of doxastic justification by imposing conditions on the rightness of a system of J-rules.  A right system of J-rules must make the permissibility of a belief depend upon the process by which the belief is formed or sustained.  More specifically, a right system of J-rules must make the permissibility of a belief depend upon the extent to which the processes by which the belief is formed or sustained tend to result in valuable consequences.  And more specifically still, a right system of J-rules must make the permissibility of a belief depend upon the extent to which the processes by which the belief is formed or sustained tend to result in a high ratio of true to false beliefs.  While a number of different accounts of J-rule rightness satisfy these conditions, Goldman favors the following schematic account:

"A J-rule system R is right if and only if
R permits certain (basic) psychological processes, and the instantiation of these processes would result in a truth ratio of beliefs that meets some specified high threshold (greater than .50)."  (*Epistemology and Cognition*, 106)

When Goldman speaks of a "basic" psychological process, he means a process that is not acquired as a result of learning:  it is either unacquired, or else acquired simply through maturation.  And when Goldman speaks of the truth ratio that would result from the instantiation of such a process, he means the truth ratio that would result in normal worlds, i.e., in worlds that work in roughly the way that we take the actual world to work (whether or not the actual world does, in fact, work that way).  Finally, the normal worlds in question, whether or not they include the actual world, will include a wide range of counterfactual worlds.

This, then, is Goldman's 1986 account of doxastic justification.  What is his account of knowledge?  For Goldman, knowledge involves true belief that is doxastically justified, and is formed by a process that is *locally* reliable, i.e., reliable specifically in the kind of setting in which the belief is actually formed, whether that kind of setting obtains in a world in which the belief is true, or in a world in which some relevant alternative to the belief is true.

Since knowledge and doxastic justification both involve reliability of one kind or another, and questions concerning the identity and reliability of our psychological processes are empirical questions, Goldman sees a close relationship between epistemology, on the one hand, and empirical questions concerning the identity and reliability of our psychological processes, on the other. What we know and what we’re justified in believing depends upon how we form our beliefs and how reliable those belief-forming processes are.

While Goldman has continued to refine a few details of his views concerning knowledge and justification, much of his work over the past quarter century has involved applications of his reliabilist account of knowledge and of justification to questions concerning how to organize social institutions so as to improve the production and dissemination of knowledge. Thus, in his 1999 book *Knowledge in a Social World*, Goldman discusses how the institutions of science, law, politics, education, and communication can be organized so as to maximize our acquisition of true belief, and minimize our acquisition of false belief. This book was seminal in creating the now very professionally active field of social epistemology.

Clearly, Quine and Goldman conceive of epistemology, and its connection to the empirical findings of psychology and cognitive science, very differently. Goldman devotes much of his epistemological work to addressing questions such as “what is knowledge?” or “what is it for a belief to be justified?”; these are questions that he takes to be answerable simply by *a priori* reflection. He also devotes much of his epistemological work to addressing questions such as “what are our sources of knowledge?” or “which sorts of beliefs are justified?”; these are questions that he takes to be answerable empirically, largely by means of cognitive science. Quine, in contrast, has nothing to say about any of these questions. For Quine, epistemology is simply the study of evidential support: what evidence do we have? How does evidence support theory? What principles guide proper theory choice given a body of evidence? These are the epistemological questions to which Quine devotes all of his attention. Goldman has very little to say about any of these questions. In fact, perhaps the only place in his corpus in which Goldman discusses any of these questions about evidence is in his 2010 paper “Williamson on Knowledge and Evidence”, in which he proposes that a person’s evidence set at a given time t consists of all and only those propositions that the person is non-inferentially propositionally justified in believing at t. But this proposal is offered not on the basis of any empirical considerations, but solely on the basis of its plausibility to reflection. In short, the epistemological question that Quine took to be an empirical question is one that Goldman barely treats at all, and when he does treat it, he treats it as an *a priori* question. And the epistemological questions that Goldman took to be empirical questions are questions that Quine simply never addressed.

**Section III. Other forms of naturalism in contemporary analytic epistemology**

Although Quine and Goldman are the two most influential naturalistic epistemologists, the past two decades have witnessed a number of other important efforts to naturalize epistemology. In this concluding section, I’ll mention three of these, and then conclude with a brief remark about how the term “naturalism” has come to be extended in recent epistemological theorizing.

In a number of papers, and in his 2002 book *Knowledge and its Place in Nature*, Hilary Kornblith argues that, contrary to what Goldman and most other epistemologists suppose, the question “what is knowledge?” is like the question “what is water?” or “what is soil?” in being an *empirical* question, i.e., a question the answer to which can be discovered only on the basis of sensory evidence. More specifically, on Kornblith’s view, empirical findings from cognitive ethology can furnish us with an answer to the question of what knowledge is. According to these findings, knowledge is reliably formed true belief. Of course, Kornblith concedes, our intuitive judgments about hypothetical cases can also lead us – as they lead Goldman – to the view that knowledge is (at least something like) reliably formed true belief. But, while most philosophers take such intuitive judgments to be a priori evidence for a hypothesis concerning the nature of knowledge, Kornblith takes these intuitive judgments to be formed on the basis of a great deal of background empirical knowledge that we have. For Kornblith, to the extent that our intuitive judgments are a good guide to the nature of knowledge, this is because they embody a great deal of empirical knowledge.

Second, in the past decade, a number of epistemologists (including Jonathan Weinberg, Shaun Nichols, Stephen Stich, Stacy Swain, and Joshua Alexander) have begun to engage in the empirical study of our intuitive judgments about hypothetical cases. Rather than attempting to answer epistemological questions by consulting their own intuitive judgments about hypothetical cases, these epistemologists survey a large sample of people, under a large variety of conditions, in order to study the conditions that influence our intuitive judgments. By understanding what these conditions are and how they operate, these epistemologists hope to be able to factor out the influence of irrelevant factors (e.g., cultural bias, order effects) on our theorizing about epistemological matters. Even if epistemological questions are *a priori*, these epistemologists insist that it is an empirical matter to figure out which *a priori* considerations are worthy of our trust and which ones are not. (Influential papers in this tradition include Weinberg, Stich, and Nichols’s 2001 paper “Normativity and Epistemic Intuitions”, as well as Swain, Alexander, and Weinberg’s 2008 paper “The Instability of Philosophical Intuitions”.)

Third, since the publication of Christopher Cherniak’s 1986 book *Minimal Rationality*, a number of epistemologists, impressed both by the dictum that “ought” implies “can”, as well as by the empirically demonstrable limits and failures of human rationality, have attempted to specify epistemic norms that can be followed by creatures whose rational powers are as feeble as ours are. Such epistemologists (e.g., Michael Bishop and J.D. Trout, in their 2005 book *Epistemology and the Psychology of Human Judgment*) have tended to reject, for instance, the claim that our beliefs ought to be logically consistent (and that our degrees of belief ought to be probabilistically coherent) on the grounds that it is, as a matter of empirically demonstrable fact, impossible for any normal human being to have fully consistent or coherent beliefs. On their view, epistemology has to begin with the empirical findings of cognitive science, for the issue of how we ought to think is constrained by the issue of how it is possible for creatures like us to think, and the latter issue can only be settled by cognitive science.

I have now surveyed the various forms of naturalized epistemology that are influential in the world of contemporary English-speaking philosophy. But I should mention that the term “naturalism” is sometimes used to describe a kind of epistemological theorizing that does not make close contact with the empirical findings of the natural sciences. Consider the following passage from Bernard Williams’s 2002 book *Truth and Truthfulness*:

“Naturalism is a general outlook which, in relation to human beings, is traditionally, if very vaguely, expressed in the idea that they are ‘part of nature’ – in particular, that they are so in respects, such as their ethical life, in which this is not obviously true. …

“Questions about naturalism… are questions not about reduction but about explanation. …The questions concern what we are prepared to regard, at each level, as an explanation. Moreover, we have no reason to think that what is to count as an explanation, from bits of nature describable only in terms of physics to human beings and their cultures, is at each level the same kind of thing. The question for naturalism is always: can we explain, by some appropriate and relevant criteria of explanation, the phenomenon in question in terms of the *rest* of nature?” (*Truth and Truthfulness*, 22 – 23)

To naturalize epistemology, on this conception, is not necessarily to put epistemology into contact with empirical science, but rather to explain epistemic facts by appeal to the natural, non-epistemic facts, whatever exactly those are, and however *a priori* or unscientific may be our knowledge of them. On this broader conception of naturalism, any attempt to explain the epistemic facts without appeal to anything over and above nature (e.g., a Cartesian god, or a Kantian thing-in-itself) and without appeal to other epistemic facts (e.g., brute support relations between mental states and propositions) counts as naturalizing epistemology.

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