

EPISTEMOLOGICAL BEHAVIORISM

Richard F. Kitchener
Colorado State University

Abstract

Traditionally, philosophical epistemologists have been committed to a 1st-person internalist, Cartesian point of view. But numerous individuals have criticized such an assumption, suggesting instead the adoption of a 3rd-person, externalist behaviorist point of view, a view some have called *epistemological behaviorism*. But what such an epistemology would be has not been adequately discussed, including its underlying epistemology, and its connection to a behaviorist psychology. To contribute to such a clarification, I present a brief survey of the main philosophical representatives of such an account: Richard Rorty, Gilbert Ryle, John Dewey, Bertrand Russell, Willard Quine, and Wilfrid Sellars. The main area of agreement between these individuals is the following: Epistemological behaviorism is the theory that knowledge should be seen in a behavioristic way, not characterized as something inside the mind of the individual—not the Cartesian 1st-person perspective, but something external—the 2nd- and 3rd-person perspectives. Knowledge is fundamentally behavioral in nature, whether actual current public behavior or contained in behavioral dispositions. Such epistemic behavior can be individual or interindividual (social). Hence, private epistemic behavior, if it exists, is of a secondary nature, deriving from the original public domain and dependent on it. In the future development of this view, several problems remain to be solved.

What is the connection between behaviorism and epistemology? At first glance, there does not seem to be any. Traditionally, epistemology was and is conceived by most individuals to be a study undertaken by philosophers, and philosophers have been loath to take a behavioristic turn (or any psychological turn for that matter). Professionally they tend to be rationalists, not empiricists; they ply their trade by dealing with concepts linked together by thought processes characterized as logical or conceptual. By contrast, behaviorists—at least certain influential schools of behaviorism—have been more attracted to empiricism and naturalism.

In addition, epistemologists in the philosophical tradition typically claim to be doing something that is normative. According to this received

view, epistemology is concerned with activities like *evaluating* the adequacy of a belief, determining if a claim is *justified or warranted*, deliberating what one has a *right* to believe, and so forth—concepts not descriptive of empirical fact but involving judgments of a completely different kind, not (as Kant claimed) *de facto* claims but ones that are *de jure*. These normative notions (falling on the ought side of the is-ought to divide) are notions that cannot be defined empirically, reduced to naturalistic facts, or even ones to which empirical fact are relevant. But behaviorists have been reluctant to join in these normative endeavors, perhaps because norms, criteria, standards, and values seem completely out of place in a behaviorist framework. In short, the prospect of an epistemology that is behavioristic seems doomed from the start.

According to the philosophical canon, therefore, epistemology is an endeavor belonging to the philosophical realm, one radically separate from the empirical psychological realm. So, a behaviorist epistemology would seem to be an oxymoron, a category mistake. Of course, this all depends on what one means by “behavior” and “behaviorism” (Kitchener, 1977) and others have argued that an epistemological behaviorism is not absurd.

Along with this received view there is been associated a certain conception of the epistemological enterprise, one associated with René Descartes—what we can call Cartesian, 1st-person Epistemology. According to this account,¹ the knower is directly aware of an internal state—often called the *Given*—a direct and immediate intuitive grasp of a presentation (sensory or logical). The Given is grasped with certainty, absolute unquestioned certainty. This is the initial egocentric starting point for any epistemological endeavor. From this Given, the knower or Cartesian ego must infer the existence of the external world with such logical consequences being known indirectly. But such inferences will always be less certain than the direct cognitive grasp of the Given.

Assuming knowledge requires justification, one’s grasp of the Given is justified because it is intuitively known (impossible to doubt) and our inferences are justified because of their logical relationship to this original cognition. The output is a cognitive representation. To know the external world, therefore, one must have a mental representation of it, a copy of it. If this is an accurate representation, it will be true, true because it is a copy of reality. Hence knowledge is a justified true belief, an account taken to be the standard view, going back to Plato’s *Theaetetus*.

¹ This account (or something like it) is to be found in Descartes’ *Meditationes de prima philosophia* (*Meditations on First Philosophy*) (1641). A different account is to be found in other works of Descartes, e.g., *Principia philosophiae* (*Principles of Philosophy*) (1644) and perhaps in *Discours de la méthode* (*Discourse on the Method*) (1637).

Beginning roughly in the 20th century, a different conception of epistemology has been advanced, one that has criticized this internal Cartesian 1st person account and substituting an external account, a 2nd-person and/or a 3rd-person account. Coupled with this criticism, several individuals have championed what has come to be called an epistemological behaviorism (EB). Although the individuals surveyed here are philosophers, their views have important implications for psychology and for those psychologists who have advocated a behaviorist epistemology.²

Rorty's Epistemological Behaviorism

No one has championed EB more explicitly than Richard Rorty, who is perhaps best known for introducing this notion. In his influential and widely read book, *Philosophy and the Mirror of Nature* (1979), Rorty consistently argues against the very possibility of epistemology—at least epistemology conceived in this particular Cartesian way. Such a view was made famous not only by Descartes, but also by John Locke and, with modifications, Immanuel Kant, Edmund Husserl, Bertrand Russell (in one period), Rudolph Carnap (in one period), and many other epistemologists.³

With this view goes a view about the nature of the mind: the knowing mind is a rational entity, a substance, separate from the natural world, residing in a Platonic or noumenal realm transcendent of space-time and causality. If so, then the rational activities of the mind cannot be causal ones but are of a different nature—rational, intensional ones. Hence epistemology, as a theory about this knowledge, is different from the empirical sciences, not reducible to them, and in fact constituting an independent, autonomous discipline.

Rorty consistently rejects this conception of epistemology. Instead he believes in social or cultural *epistemological behaviorism*. This is the view that “knowledge is a matter of what practices of justification are adopted by one’s peers” (1979, p. 99), of the absence of a normal rejoinder in normal conversation to a certain knowledge claim (1979, p. 96); it is what society likes us say (1979, p. 174); justification being a matter of social practice (1979, p. 186).

Individuals make verbal reports about their internal states and traditional Cartesian epistemology maintains there are internal

² One main motive for producing the present essay is to summarize some major advocates of such an EB since no survey of this topic seems to be extant.

³Such a model lies at the basis of Bertrand Russell’s *Our Knowledge of the External World* (1912) and Rudolph Carnap’s *The Logical Structure of the World* (1928)—two monumental works in 20th century epistemology.

psychological states of the individual that are intuitive, infallible, and incorrigible. Rorty points out that several philosophers have raised serious objections to this notion of the Given: Ryle, Quine, Sellars, Wittgenstein (among others). In large part, these criticisms are based on the necessity of public criteria, standards, and practices to adjudicate the epistemic claims made by individuals: “How do our peers know,” Rorty asks, “which of our assertions to take our word for and which to look for further confirmation of?” (1979, p. 173). Introspective reports need to be verified, confirmed, corroborated by others and this would seem to involve public behavior or a physical state of the person. This is one of the lessons of Dewey’s criticism of this *Quest for Certainty* (Dewey, 1980), Wittgenstein’s private language argument (Wittgenstein, 1958), Ryle’s attack on “the Cartesian myth” and the ghost in the machine (Ryle, 1949), the logical positivist’s doctrine of physicalism, Sellars’ attack on the myth of the given, (Sellars, 1963), Quine’s naturalistic epistemology (Quine, 1969), psychology’s methodological behaviorism, and so forth.

I take Rorty to be saying first that there is a need for justification only when someone else (or a group) calls one’s epistemic claim into question. In the absence of this challenge, there is simply no need to justify it. This assumption is one based on almost all varieties of pragmatism. Second, to justify an epistemic claim is to justify it to a social group by producing reasons underlying the claim. Justification is thus a three-term relation: subject S justifies epistemic claim p to a social group G by citing justification j.

If the group accepts these reasons as adequate justifiers, they will no longer question one’s claim and hence the claim will be justified (justified to them). This is an “innocent until proven guilty” concept: *In the absence of a challenge, one’s epistemic claim is justified.*

But what does justification mean here? On the logicist model, a proposition q is justified by a proposition p if q is logically supported by p in the sense of being related to it deductively, inductively, or epistemically, where these logical inferences are presumed to involve a set of deductive, inductive, or epistemic rules. In short, justification is logical inference.

In deductive inference, there is the essential notion of formal validity; in inductive inference the core notion would be something like inductive strength or degree of validity (Kitchener, 1999); and with epistemic inference, one can say something like an argument is evidentially strong if it

is such that, if the premises were true, the conclusion would be epistemically warranted.⁴

Rorty rejects this account of justification. Instead he seems to believe that a statement or action betokening an epistemic claim is justified if (1) there is no social challenge, or (2) there was a challenge and it was answered to the group's satisfaction. "Επιστήμη is the product of normal discourse—the sort of statement which can be agreed to be true by all participants whom the other participants count as "rational"" (p. 320).

The problem with this account seems clear. As Rorty points out: "It is easy to imagine different social practices in regard to the same objects, actions, or events, depending upon the degree of intellectual and spiritual development of the culture in question. . ." (p. 122). There are many crazy groups out there, and these various groups believe bizarre things. On this account, if a subject makes a claim to which his group agrees, then the claim would be justified. This is certainly an objectionable relativism.

In reply to this, Rorty queries, "why should not the unit of empirical inquiry be the whole of culture?" (1979, p. 201). But the answer is: there is no such monolithic culture with this type of single ideology, e.g., just consider the contemporary political scene in the United States.

Following Sellars, Rorty distinguishes causes and reasons, with causes being empirical and justification being rational. But not only is this distinction questionable, Rorty cannot consistently adhere to it, if 'justified' just means 'socially accepted' since this latter notion seems to be a causal one. Furthermore, "An account of knowledge can at most be a description of human (epistemic) behavior," Rorty says (1979, p. 182). Is a description not something empirical? Is it non-causal? Why can't there be an explanation of epistemic behavior? These are questions awaiting answers.

Rorty does not see psychology as the discipline to provide such a description (or explanation) but (at most) to the history and sociology of science and the history of ideas (p. 226)—are these not empirical (causal) disciplines? I am not suggesting, Rorty says, that "Quine and Sellars enable us to have a new, better "behavioristic" sort of epistemology. Rather they show us how things look when we give up the desire for confrontation and

⁴ It is not clear whether such a social justification practice requires an underlying set of rules—justifying rules—or not, although Rorty does not see the need for this. One could argue that a practice is a set of actions and interactions—in this case verbal ones—and these need not require an explicit set of rules consciously followed. A similar claim has been made by Thomas Kuhn (1962) concerning the practices of science: there need not exist a set of explicitly formulated rules governing that practice. It constitutes tacit knowledge (Polanyi, 1958), a kind of social, interpersonal skill, and skills do not require explicit rule-following.

constraint” (1979, p. 315). There is nothing, therefore, that will fill the gap left by the demise of epistemology (1979, pp.210, 315). His personal favorite, hermeneutics, is an expression of hope that the cultural space left by the demise of epistemology will not be filled—that our culture should become one in which the demand for constraint and confrontation is no longer felt (p. 315).

I must confess I do not see how Rorty can be pushing for the demise of epistemology and also to be saying other things he quite clearly says. In what sense, for example, is his “epistemological behaviorism” not an epistemology at all? How can he (p. 11) advocate pragmatism as an epistemological conception of knowledge? I think that most individuals would interpret Rorty as arguing against traditional representational epistemology—that it is in some sense not possible—but also that he is advocating an epistemology based on “what society lets you get away with,” a conception of justification essentially social in nature.

Has psychology anything to offer the program of epistemology? Assuming epistemology should be seen as something anti-representational, why isn’t it the case that psychology could be a successor project (as suggested for example by Quine)? As far as I can tell Rorty offers no reasons for refusing this suggestion. Quine is not a traditional representationalism and neither are many psychologists including Skinner, Gibson, Piaget. Individuals such as Sellars, Piaget, Vygotsky and others have a two stage, hybrid account with the first level being that of a theory of external behavior (action, conduit) and a second level of representation based on this first stage. There does not seem to be anything in Rorty’s account to rule out these two-stage accounts.

In conclusion, therefore, even if Rorty has made the case against traditional, representational epistemology, he hasn’t shown that there cannot be successor epistemologies and (perhaps) ones based upon psychology. We should take Rorty’s adoption of EB as an invitation to construct just such an epistemology, one that very well might be parsed as a behavioristic epistemology. What resources are available for the construction of such an EB? Such an epistemology would be one based on behavior as the evidential basis of a justification for a knowledge claim. It would involve a 3rd person epistemological point of view, not a 1st-person.

Given Rorty’s heroes—Dewey, Wittgenstein, Quine, Sellars, Davidson—such an account would be some kind of naturalistic or empirical epistemology (since all of these individuals were arguably naturalists—although of different stripes). This suggests that the appropriate science will have much to contribute to such an account since the basis of such an

epistemology will be empirical or naturalistic in nature. It would be some kind of behaviorism, therefore, but what kind?

II. Early Versions of Epistemological Behaviorisms

Gilbert Ryle's Logical Behaviorism: The Ghost in the Machine

One philosopher well-known as a champion of some kind of behaviorism was Gilbert Ryle. Many commentators ascribe to him a view sometimes called logical behaviorism, criterial behaviorism, or semantic behaviorism (Kitchener, 1998, 2004a), the view (roughly) that mentalistic terms could be analyzed in terms of behavior and behavioral dispositions. "When we describe people as exercising qualities of mind, we are not referring to occult episodes of which their overt acts and utterances are effects; we are referring to those overt acts and utterances themselves (1949, p. 25).

To talk of a person's mind is not to talk of a repository which is permitted to house objects that something called 'the physical world' is forbidden to house; it is to talk of the person's abilities, liabilities, and inclinations to do and undergo certain sorts of things and of the doing and undergoing of these things in the ordinary world (1949, p. 199).

Ryle explicitly took as the objectionable view a purely mentalistic one, "the ghost in the machine" conception he ascribed to René Descartes. According to this view, one categorically rejected by Ryle, the mind was seen as a non-mental, "ghostly"-like entity, not in space or time, lacking material and causal properties. Somehow it resided in a physical body, one subjected to the usual physical laws involving space and time. Because of the hegemony of the physical-mechanical world view of the 17th century, this human body was considered to be something that worked along purely mechanistic lines—a machine or automaton. Hence, according to Ryle, this was the para-mechanical Cartesian picture of the "ghost in the machine."

Cognitive verbs constitute a substantial part of these mentalistic ascriptions we make of others and "know" is a prime example. To ascribe knowledge to a person, animal, or machine—S knows ϕ , S knows that p, S knows-how to ϕ —is to ascribe a certain kind of achievement to S. It is for the speaker to be committed to the claim that S has achieved a certain kind of end-state, one characterized as being "justified," "warranted," "true," etc. 'Belief' does not have this presupposition.

Ryle's theory is widely taken to be an example of behaviorism (where mental qualities are displayed in behavior—in actions, not movements). But the public realm in which epistemic concepts get a purchase includes not

only occurrent, episodic behavior but dispositions to behave in certain ways under certain kinds of situations. Ryle's dispositional account gets complicated and refined, however, by his arguing that such epistemic dispositions are multi-track. Such a concept entails a rich but complicated account of behavior.

Distinguishing "knowing-how" from "knowing-that," Ryle argued that knowing-how is more basic and fundamental than knowing-that and is the primary locus of intelligence: knowing-that, he claimed, presupposes knowing-how, not the other way around as the Cartesian myth would have it. "Knowing-how, then, is a disposition, but not a single-track disposition like a reflex or a habit. Its exercises are observances of rules or canons or the application of criteria (1949, p. 46).

Ryle's account seems to be a candidate for an EB since public behavior constitutes the criteria and evidence for someone to know something. But Ryle denies he is a behaviorist—would he deny he is an epistemological behaviorist? —at least a behaviorist of a certain kind.

Ryle rejects mechanistic accounts of what we do along with the notion that our doings are reducible to movements causally explainable. Because of this identification of behaviorism with such a mechanistic account, Ryle disowns that label (although his theory might be called a theory of action or praxeology).

Although it is unclear which psychological behaviorist he had in mind, the obvious candidate here would be John Watson, who considered behavior—human and animal—to be equivalent to "muscle movements and glandular secretions" (even though Watson's [1913] theory was much more complicated than this) (see Kitchener, 1997).

Ryle rejected such a mechanistic account of (human) behavior, based as it was on a simple mechanistic model. To employ such mechanistic terms about human action was a "category mistake" since human action was not amenable to mechanistic explanations but to reason explanations

Psychology was not held in high esteem by Ryle in part because of its mistaken philosophical assumptions. In fact, Ryle is committed to what some have called *the asymmetry thesis*: psychology cannot (or need not) explain the normal behavior of individuals, why the farmer returned from the market with his piglets unsold. Common-sense explains such behavior—the prices were lower than expected—and we have no need of technical psychological theories about hidden entities. However, the psychologist can explain abnormal or deviant behavior; whereas normal actions have reason-explanations, these have causal ones. "Let the psychologist tell us why we are deceived [in visual illusions]; but we can tell ourselves and him why we are not deceived" (1949, p. 326). Psychology

is not “the sole empirical study of people’s mental powers, properties and performances” (1949, p. 327); ordinary common sense also has a claim to do that. Presumably, therefore, these technical psychological accounts cannot overturn or correct these common-sense explanations of behavior. In the case of knowledge, such common-sense stories would be called folk-epistemologies, so Ryle presumably would argue that such folk-epistemology cannot be eliminated or refuted by technical scientific behaviorist epistemology or a philosophical epistemology at odds with it. This raises the question of how a folk epistemology (common sense) gives as an adequate account of knowledge or whether a more technical scientific epistemology would challenge it.

Epistemology for Ryle can only be one of two things: either what we would now call philosophy of science or a theory of “learning, teaching and examination”—a theory of “getting to know.” This latter endeavor is apparently a kind of philosophy of pedagogy (1949, p. 318) but epistemology does not have its traditional role of charting “the nature, scope, limits and structure of knowledge.” For Rorty, this would be welcomed as a kind of limited or negative EB.

Ludwig Wittgenstein’s Criterial Account

Any discussion of EB would have to mention the views of Ludwig Wittgenstein, whose argument against the possibility of a private language has important implications for epistemology in general and EB in particular. A *private language* is a language whose words have a meaning given to them by the individual and not known by any other person. A solitary individual creates a language by himself and assigns his own meaning to these words and sentences. It is a “language which describes my inner experiences and which only I myself can understand” (Wittgenstein, 1958, §256, p. 91e).

Such a language, Wittgenstein argued, would be logically or conceptually impossible because there would be no way to determine if this individual was consistently using these words in the way he had privately defined them. There would be no criterion of correctness or incorrectness: “. . . whatever is going to seem right to me is right: And that only means that here we can’t talk about ‘right’” (1958, §258, p. 92e). A definition of a word must be used by the individual correctly over time. But there would be no way for the individual (or us) to determine if this occurred or not. How could she decide that she used a word incorrectly—if she thought that she did? But was this thought true? What would be the relevant evidence here?

It could only be what seemed to be the case, but seeming in no criterion. In short, there would be no way to verify it. (1958, §353, p. 112e).

By contrast, a language must be a public language, one in which individuals are taught the meaning of words by means of public procedures involving public criteria. Criteria “give our words their common meanings” (1960, p. 57). In order to be taught the meaning of words, you need public criteria for their correct use. Meaning is something public, not private; it is not an internal mental thing. “What would it be like if human beings showed no outward signs of pain (did not groan, grimace, etc.)? Then it would be impossible to teach a child the use of the word ‘toothache’” (1958, §257, p. 92e). Pain-behavior is the natural expression of pain and presumably certain kinds of behavior is the natural expression of knowing.

Meaning is something linguistic. “It is only in a language that I can mean something by something.” “When I think in language, there aren’t meanings going through my mind in addition to the verbal expressions: the language itself is the vehicle of thought” (1958, §329, p. 107^e). “You learned the *concept* ‘pain’ when you learned language” (1958, §384, p.118^e).

We are taught the meaning of words (by others) involving a variety of particular examples in a variety of contexts. If we are puzzled about the meaning (use) of a word, “always ask yourself: How did we learn the meaning of this word. . .? From what sort of examples? In what language-games? (1958, §77, p.36e). The meaning of a word will have a variety of uses—to do a variety of different linguistic things in a variety of situations: to make commands, to express feelings, to state beliefs, etc. (1960, p. 68). Hence, “the meaning of a word is its use in the language” and between these various uses there is no common definition; there is only something like a family resemblance.

How did we originally learn the meaning of a word? As children we were taught this. How did this happen? Wittgenstein’s answer is that we were trained to do so, we learned it as a drill or habit.” I am using the word “trained” in a way analogous,” he says, “to that in which we talk of an animal being trained to do certain things. It is done by means of example, reward, punishment, and suchlike (1960, p. 77). Hence, explanation involving appeal to rational inference ultimately rests on a-rational training.

Wittgenstein’s argument against the possibility of a private language and his alternative account involving the need for verification by public criteria provides the basis for his account of EB, a “theory” of knowledge involving epistemic terms—know, belief, doubt, certainty, justification, etc.

Wittgenstein’s most explicit epistemological comments occur in his *On Certainty* (1972), which was a response to G. E. Moore’s famous “Defense

of Common Sense” in which Moore claimed he knew various things and hence that idealism was false. Wittgenstein argued that Moore was wrong in his use of phrase “I know.”

Following his account of games involving the notion of family resemblance, Wittgenstein argued that there is no single, common meaning of “I know” (and ‘knowledge’). Instead that utterance occurs in a variety of settings with multiple uses. There are many meanings of ‘know’ I do not doubt; I know how to do a certain thing; I am certain; I have grounds supporting what I say; etc. The meaning of ‘know’ and other epistemic terms are its varied uses. “To whom does one anyone say that he knows something? To himself, or to someone else? If he says it to himself, is it distinguished from the assertion that he is sure that things are like that? There is no subjective sureness that I know something. The certainty is subjective, but not the knowledge” (1972, §245, p. 32e). I say “I know” to other people in a particular social context; I don’t say it to myself. The same applies to the crucial notion of the justification of a belief. I do not need to justify my belief to myself; I need to justify it to others. and this involves appeal to common justificatory practices.

Knowing, doubting, justifying, etc., are notions acquired as social practices involving behavioral criteria. Hence, Wittgenstein insists, there is the behavior of doubting, knowing, justifying, etc. “If someone is looking for something and perhaps roots around in a certain place, he shows that he believes that what he is looking for is there” (1972, §286, p. 37^e). Likewise, I show that I know calculus or French by my being able to solve certain kind of mathematic problems, answer questions about them, speak in a Gallic certain sort of way, etc. (given my past learning history). It is not enough for someone to assure us that he knows something, he has to show it—in his behavior (1972, §431, p. 56^e). Once again knowledge, etc., is not an internal “mental state” (1972, §286, p. 37^e): “Try not to think of [knowledge] as a ‘mental process’ at all—For that is the expression which confuses you. But ask yourself: in what sort of case, in what kind of circumstances, do we say, ‘Now I know how to go on’” (1958, §154, p. 61^e).

Wittgenstein’s account can be seen as an example of folk epistemology (Kitchener, 2002). Folk epistemology is the theory of knowledge held by the common folk—the man on the street, the housewife, the fishmonger. If it is correct to say that the use of a word F represents (or is) the concept F, then Wittgenstein and those engaged in ordinary language philosophy can be seen to be doing folk epistemology. The linguistic usage of ‘know’ embodies the concept of knowledge as we understand it as a result of our learning it in our ordinary socio-linguistic practices. Our ordinary

epistemological language thus contains our ordinary theory of knowledge. It is a natural extension of such an account to argue that since our folk epistemology was acquired as we originally learned language, we need to look at how children learn these epistemic terms and how these change over time as we mature into adults. (See Kitchener, 2002).

Wittgenstein frequently makes empirical claims in his writings. “When philosophers use a word— “knowledge,” “being,” “object,” “I,” “proposition,” “name”—and try to grasp the essence of the thing, one must always ask oneself: is the word every actually used in this way in the language-game which is its original home? (1958, §116, p. 48e). “When we first begin to believe anything, what we believe is not a single proposition, it is a whole system of propositions” (1972, § 141, p. 21e). These statements are empirical claims. Are they actually true? Does he have sufficient evidence for them? Is philosophy in the business of making such empirical claims?

Wittgenstein is not shy about telling us how we actually use a word, even what the correct use of a word is. He seems to assume he has access to such an account, setting forth what we ordinarily mean by a particular word. Now clearly such claims are empirical in nature, what he calls description, not explanation, and he reserves their determination for philosophy, not for science. But the determination of the common meaning of a word seems to be a sociological question, not one for private adjudication. As he pointed out about games, we shouldn’t say things have to be a certain way, we should look and see. But the ‘we’ is a sociological “we.”

John Dewey’s Pragmatism

One of the first individuals to advance something like an EB was John Dewey. Although Dewey was initially schooled in the philosophy of Hegel, he soon came under the influence of C. S. Peirce, William James, and Charles Darwin. This led him to propose something like a biological (evolutionary) epistemology and with it came a stress on contextualism (situationism), continuity, functionalism, and developmental process, a kind of biological Hegelianism. This can be seen in his *The Influence of Darwin on Philosophy* (Dewey, 1910), in which he argues that Darwin’s theory of evolution (really the Darwinian Revolution and Darwinian World View) had important implications for the philosophical enterprise.

The basic unit of analysis for the Darwinian is an organism acting in an environment. The organism has basic needs and external material objects are capable of satisfying these needs (food, a sexual partner); the organism

has various resources for obtaining these objects and hence satisfying these needs. Since there often are obstacles, impediments, barriers, etc., standing in the way of satisfying these needs, the organism has a basic task of problem-solving, with the proficiency of this ability being a measure of the organism's ability to adapt to this environment. Such is the basic paradigm or model, Dewey argued, that philosophy must adopt if it is to be in tune with reality and hence valuable; in short philosophy must be thoroughly *naturalistic* in its perspective.

Being pragmatic, therefore, means being able to solve problems, to attain one's goals, to satisfy one's needs, to do so involves a process of inquiry, and rational inquiry involves employing the experimental method of the sciences. Epistemology will thus become an experimental empiricism whose fundamental epistemic criterion will be the production of effects (consequences) that successfully terminates inquiry and solve the problem.

A basic assumption of the pragmatists—captured in the motto “if it ain't broke, don't fix it”—is that inquiry only begins, intellectual reflection only occurs, when a problem is encountered. But when a question is asked, a barrier preventing the continuation of action arises, a problem suddenly emerges, the interruption of this normal behavior is necessary, and *reflection* is demanded. This critical reflection is what we call problem solving, but no need to solve them in the absence of such problems. Hence a problem to be a problem must be a real problem; this includes traditional philosophical problems. “...inquiry begins in an *indeterminate* situation, and not only begins in it but is controlled by its specific qualitative nature. Inquiry... [is] the set of operations by which the situation is resolved (settled, or rendered determinate) . . . (1941/77, p. 276). Knowledge is the outcome of “competent and controlled inquiry” (1938, p. 8).

Dewey spent a great deal of time working out a theory of the scientific method, the experimental method (contained in his *Logic: The Theory of Inquiry* [1938]). Several features of the experimental method stand out: the problem or question to be solved or explained, the step of proposing a conjecture or hypothesis, the evaluation of the adequacy of these hypotheses consisting in making predictions—claims that these proposed solutions will remove the puzzle—making an observation that will decide if the claim is correct or not. This we call falsifying the hypothesis or confirming it, which means the proposed solution did or did not successfully remove or explain the original puzzle. “Anything that may be called knowledge, or a known object, marks a question answered, a difficulty disposed of a confusion cleared up, an inconsistency reduced to coherence, a perplexity mastered” (1929/80, pp. 226-227). We know “whenever our inquiry leads to conclusions which settle the problem out of which it grew” (ibid, p. 198).

For Dewey, therefore, rationality is problem solving and this is impossible if one does not check out the actual consequences of an idea. This is the heart of the experimental method and hence the employment of the experimental method is essential to rationality, to what we call the theory of knowledge. Knowledge (knowing) is essentially related to action, to doing; performing various experimental operations lies at the heart of the knowledge enterprise.

On this account, therefore, epistemology must be conceived differently than it has been. Epistemology must be a descriptive, empirical, naturalistic theory of experimental inference. Hence epistemology must abandon its age-old quest for a “transcendental grounding” and become experimental, a “synonym for a descriptive logic; for a theory that takes knowledge as it finds it and attempts to give the same kind of an account of it that would be given of any other natural function or occurrence” (1910, pp.96-97). The epistemologist here is a scientist, an experimental psychologist, who begins with certain concrete experiences, acts such as perception or memory or inference, and then determines the conditions under which they arise and “what effects they produce” (p.251), adding that he analyzes the states of consciousness involved. But the stress is always on action: to remember is to do something. In short, the issue is to investigate acts and the conditions of their occurrence. It is not much of a stretch to conclude that for Dewey, the basic epistemic unit is: condition-action-effect. Such a unit of analysis could easily be seen as an EB with an obvious similarity to B. F. Skinner’s operant model.

III. Recent Analytic Versions of Epistemological Behaviorism

Bertrand Russell’s Causal Theory of Knowledge

Bertrand Russell is widely recognized as one of the most important and revolutionary philosophers of the 20th century. Along with G. E. Moore, he initiated the “revolution in philosophy” (Ryle, 1956). Russell’s innovation was to introduce the method of logical analysis into philosophy, making philosophy scientific. Russell applied this new method to many areas, especially epistemology, thereby creating a naturalistic epistemology (NE), in fact a behaviorist epistemology (Kitchener, 2004b, 2007). Because Russell had a well-developed EB (unlike others) I would like to discuss it in some detail.

Throughout Russell's many writings on epistemology, two different conceptions of epistemology can be found (Russell, 1912/1984; 1927/1995; 1940/1955; 1948/1992; 1994), an internalist 1st person Cartesian conception

and an externalist 3rd person conception. On the traditional 1st person Cartesian account, the primary task of the epistemologist is to answer the sceptic and to show that knowledge is possible by showing that we have certain, indubitable knowledge. From these secure foundations, one then shows how it is possible to derive the rest of one's knowledge. Until one secures these certain foundations, however, no other kind of knowledge can be assumed to exist.

Alongside this traditional concern of epistemology, however, is another, quite different conception of epistemology—a 3rd person Naturalistic Epistemology (NE). This concept of epistemology began as a kind of traditional Kantian epistemological project—asking how it is possible for us to have the knowledge we do have (Russell, 1899). This original kind of transcendental question was gradually turned into a different kind of epistemological question, however (see Moore, 1899), for the question concerning what mental operations and faculties were necessary in order to have our knowledge was turned into an empirical psychological question the result was thus a kind of descriptive or naturalistic epistemology founded upon the sciences of physics, physiology, and especially psychology.

Russell's initial foray into epistemology occurred in several works (1912/1984; 1992). Returning to epistemology in 1918, he said: “I found my thoughts turning to theory of knowledge and to those parts of psychology and of linguistics which seemed relevant to that subject. This was a more or less permanent change in my philosophical interests” (Russell, 1959, p. 128).

Russell thus turned to psychology—in particular behaviorism. As he immersed himself in psychology, he struck out on a naturalistic path. At first, this NE was tacit, but he soon entertained the possibility of behavioristic analysis of 'belief.' ascribing such a view to James and Dewey and later (1919/1950) explicitly introducing the views of John Watson. It is here that Russell first suggested the possibility of a naturalistic (psychologistic) theory of knowledge (1919/1950, p. 307). Such an approach then became explicit (1921, 1927/95), where his infatuation or flirtation with behaviorism is most evident (see Kitchener, 2004), still present later (1940/1955, 1948/1992). Hence, until the very end of his life, Russell entertained the possibility of a NE (although not a purely behavioristic one).

The basic tenet of Russell's NE is that knowing is a natural state in the world, basically a relation between an organism and its environment. “The world, as presented by science, contains a phenomenon called ‘knowing,’ and theory of knowledge ... has to consider what sort of phenomenon this

is” (1940/1955, pp. 12-13). Knowledge, considered to be a naturalistic phenomenon, observable by the senses, and studied like any other natural phenomenon by science, in this case, psychology, would involve taking up a third-person point of view, that of the epistemologist, and observing the epistemic subject as it engages in various kinds of epistemic activities resulting from various kinds of epistemic states. “Knowledge, traditionally, has been viewed from within, as something which we observe in ourselves rather than as something which we see others displaying” (1927/1955, p. 14). Instead, he suggested, knowledge could also be viewed from an external point of view—“man from without.”

For a behaviorist, the activity of knowing must consist of observable behavior of the organism in an environment. When we do observe such behavior, we seem to be committed to a belief in a causal sequence of events running from the remote environment to the proximal environment to hidden processes in the nervous system to proximal responses and finally to distal responses. It is this complex causal sequence that must be the locus of knowledge; hence Russell was firmly committed to a causal theory of knowledge and then a causal theory of belief, justification, truth, and meaning.

Russell gave several behavioristic accounts of knowledge, but all of them included the notion that knowledge is a certain kind of response (e.g., a verbal answer) to a stimulus (e.g., a question). We can observe such behavior in other humans and in animals, the best example of which involves knowing-how to do a certain kind of task and hence the ability to reach a certain kind of goal. Russell calls this animal knowledge and distinguishes it from machine knowledge.⁵ Although Russell is willing to concede that there is such machine knowledge possessed by instruments, he does not believe such examples are adequate as an account of other, higher forms of knowledge, such knowledge possessed by animals. Animals, unlike machines, manifest behavioral properties thermometers don't, namely, they learn. Hence, in such animal knowledge, organisms demonstrate improvement in behavior over time as they acquire and perfect a skill or ability. A behaviorist account of knowledge, therefore, must secure a place for such improvement in response over time as a result of environmental input, and this will necessitate a stress on the plasticity and adaptiveness of behavior. To incorporate these aspects into a behavioristic epistemology, one would also have to carve out a behavioristic account of desire and purpose.

⁵Armstrong (1973) advocates a thermometer model of knowledge and Sosa (1991) calls this servo-mechanic knowledge.

For something to be a case of animal knowledge, it must possess the characteristics some individuals call “persistence until” a goal is reached, where such persistence is manifested in behavioral variability (plasticity) correlated with goal attainment. Such behavior is goal-directed and shows purpose.⁶

Animal knowledge and servo-mechanic knowledge must be accounted for in a comprehensive epistemology, but of course the kind of knowledge most epistemologists have been concerned with is propositional knowledge, knowing-that something is the case. Such knowledge seems to involve beliefs—propositional attitudes—and such beliefs must not only be true, they must fulfill an evidence criterion.

In his new theory of belief, the old self-act-content-objective schema was abandoned, inspired by neutral monism. Now he analyzed a belief state into his neutral monistic elements—sensations and images. A belief state consisted of images plus feelings (sensations), with images taking over the role propositions once played, and feelings accounting for the attitudinal part of propositional attitudes, where the feeling is that of an expectation (as opposed, say, to desire). Hence, when S believes that the cat is on the mat, this is to be analyzed as (1) S has an image of a cat on mat, and (2) S has a feeling of expectation, of expecting the cat to be on the mat. The image has one kind of representational function and the expectation has a different one, a more behavioristic one.

In adopting the older empiricist account of images as the vehicle of meaning, where images are copies of impressions, Russell took over much of their intellectual framework. An external situation A can cause its image B in which case B is said to be the prototype of A, the meaning of B. Thus, B is an image of A if and only if B is the prototype of A, and B is the prototype of A if and only if: (1) There is a resemblance (of structure) between B and A, (2) A causes B, (3) A and B have common effects. The key component in all three clauses is that of causality.

In order for S to know that p, it is necessary that S have adequate evidence for p, S’s belief must that p must be rational, verified or confirmed, etc. Opinions differ over the precise way of formulating this requirement of justification. Russell’s describes, including that of verification. How, then, can one naturalize this condition? The answer is by means of a causal theory of evidence or verification.

Rejecting the popular coherence theories of verification, Russell suggested there must be a causal tie between a protocol sentence (or better

⁶ Such an account shows a strong family resemblance to the views of the neo-realists Edwin Holt (1915) and Ralph Perry (1917), whose early works Russell read.

the acceptance of a protocol sentence) and the experience in question. Russell offered a causal interpretation of verification itself, a psychological interpretation of verification rooted in the feeling of expectedness, a crucial part of his account of belief (1927/95, p. 207; 1940/1955, p. 80).

Verification of a statement about the future, according to what we have said, occurs in two stages. First, the statement (if believed) produces a certain bodily and mental condition which I call expectation; then a certain occurrence or set of occurrences transforms the expectation into surprise or into the feeling expressed by 'ah yes!' In the latter case the statement is verified, in the former it is refuted. (1921, p. 18)

The verification of a belief thus involves a causal account in which, first, a statement or assertion causes an expectation, a psychological 'set,' and, second, facts cause certain emotions in the subject. This account is "emotional rather than intellectual (1921, p. 269; 1927/1955, p. 207; 1940/1955, p. 142). Even when it came to the notion of truth, Russell did not shy away from proposing what may be called *a causal theory of truth*. A belief or cognition is analyzed as an image plus feeling with the image component constituting the semiotic or representational part of belief. An image represents the original situation (or impression) causing it, which is the prototype of the corresponding image. Hence, one can say that a fact F is the prototype of the belief image B just in case there is a resemblance (of structure) between F and B, F causes B, and F and B have common effects. In such a case, Russell says, we can say B is true. "A sentence of the form 'this is A' is called 'true,'" Russell says, "when it is caused by what 'A' means" (1948/1992, p. 134) or in less sociological terms, "'this is red' is 'true' if it is caused by something red" (1948/1992, p. 141) (see also (1940/1955, p. 214; 1948/1992, pp. 133-4). In another passage, Russell says:

a form of words is true if a person who knows the language is led to that form of words when he finds himself in an environment which contains features that are the meanings of those words, and these features produced reactions in him sufficiently strong for him to use words which mean them ... The environment causes words, and words directly caused by the environment (if they are statements) are 'true'. (1927/1995, p. 210).

It thus appears that Russell found no incompatibility in believing both a causal theory of truth and a correspondence theory of truth no doubt because he believed that, in some way, the correspondence relation could be

explicated in naturalistic (causal) terms. How this was possible he never says.

It is to causality, therefore, that Russell looked to provide a naturalistic account of knowledge; indeed, as we have seen, he uses it to explain or analyze servo-mechanistic knowledge and animal knowledge. Russell had initially thought that behaviorism could account for such animal knowledge, but he soon gave up this assumption largely because Watson had denied the existence of mental images and Russell crucially depended upon them in his later theory of belief. A purely behavioristic theory of knowledge was ultimately incomplete, requiring supplementation by an account of our inner life—our introspective awareness.

Russell believed that an externalist account of knowledge would have to be supplemented with an internalist account. A naturalistic account of such an externalist epistemological perspective, although encountering well-known philosophical objections, would seem to be a viable option. But can it provide a naturalistic account of an internalist epistemology? Russell did not address this question, an answer to which is difficult to ascertain largely because of the unclear status of such an internalist perspective (at least within Russell's program). Nevertheless, there is no evidence that Russell believed such an internalist account was incompatible with naturalism (even if it might be incompatible with materialism). In fact, there is considerable evidence of an indirect kind indicating that that he did believe that introspection (first-person awareness of things like images) was something that could be handled in a purely naturalistic way. For if one adopts, as Russell did, a version of central state theory of mind, images are to be equated with neural states and hence located in the brain. So, even if behaviorism was not a complete account of knowledge, this did not mean that Russell gave up (or had to give up) a naturalistic, psychological account of knowledge.

Russell had an inchoate version of an EB. True, it was sketchy, fragmentary, and undeveloped, but it was nevertheless present alongside his other, more Cartesian account of epistemology, with Russell's emphasis switching from one to the other. Russell seems to have thought that any adequate theory of knowledge had to take into account both types of knowledge. He also seems to have thought that knowledge might be completely accounted for in naturalistic terms.

One way of viewing the relation between these two types of knowledge (and the corresponding two types of epistemology) is to imagine that what we can call characteristically human knowledge occurs at the highest epistemic level but is based upon lower levels of animal knowledge, and animal knowledge, in turn, has its roots in what Sosa (1941) calls servo-

mechanism knowledge. As we have seen Russell allowed for these three different kinds of knowledge and so does Sosa (1991).

Such a three-tiered account matches (roughly) what we know about the evolution of knowledge in the species and in the development of knowledge in the person. Most epistemologists have been concerned with characteristically human knowledge—the type of knowledge possessed by mature, normal, language-using adults who can introspect and who have certain reflective abilities. Adult humans can introspect and reflect upon their epistemic states, subjecting the grounds and reasons for their beliefs to critical scrutiny. Such an ability can be called a meta-epistemic or meta-cognitive ability (Lehrer, 1990). This is the 1st person, internalist perspective of Descartes normally distinguished from the third person, externalist perspective of naturalistic epistemology. But why should internalism be viewed as a version of non-naturalism?

At least four reasons might be given: (1) introspection is a non-naturalistic procedure, (2) indubitability is a non-naturalistic state of certainty, (3) internalism is wedded to a rationalistic account of *a priori* knowledge, meta-epistemology is an *a priori* endeavor, and rationalism is non-naturalistic, and (4) the normativity of epistemology is incompatible with naturalism. I believe all four of these claims are mistaken and based upon conceptual misunderstandings. If so, then there is no reason for supposing that even the traditional Cartesian conception of epistemology should be construed in a non-naturalistic way. Russell may have done this, but there was no reason for him to do so. Both conceptions of epistemology—Cartesian internalism and behavioristic externalism—can be interpreted in a naturalistic way. If we adopt something like this three-level approach to knowledge, knowledge can be said to develop from servo-mechanism knowledge to animal knowledge to human knowledge in a stage-like manner. Cognitive developmental psychologists and developmental epistemologists have studied and analyzed this developmental process (although they have used somewhat different terms). But it does seem to be clear, for example, that meta-cognition, this meta-epistemic faculty underlying human knowledge, develops from childhood into adulthood. But it is also clear that it is based upon earlier forms of epistemic cognition and that these forms may be like animal knowledge.

If there is such a developmental process, presumably it would require a developmental explanation (Kitchener, 1983). But such a developmental explanation is (arguably) a naturalistic one—an explanation involving natural developmental processes and, at the same time, a normative one. The normative dimension is one of progress and improvement in knowledge by virtue of the fact that the subsequent stage of knowledge is epistemically

better than the preceding one. But, at the same time, any epistemic norms involved must be supervenient on naturalistic facts. Russell was notoriously skeptical about the normative dimension of life but, if I am right, he should not have been. Russell's NE, suitably expanded by current scientific knowledge, can be seen to be a plausible research program, one to be analyzed, subjected to scrutiny, and then suitably modified. This is precisely how Russell saw the task of the epistemologist—to propose tentative hypotheses and then to critically evaluate them in keeping with the best scientific evidence. This is what Russell himself did and it is something that other epistemologists should be doing. This would be perfectly in keeping with the kind of NE Russell was advocating.

Willard Quine's Naturalistic Epistemology

The most well-known attempt to make epistemology empirical is, without doubt, Quine's proposal for a new and radical program—naturalistic epistemology (NE). Quine explicitly takes Carnap's *Aufbau* program (Carnap, 1928/1967) and thus indirectly Russell's (1912/1984) *Our Knowledge of the External World* program, as his point of departure.⁷ Russell and Carnap were concerned with showing how, beginning with the private and subjective experience of the individual, it was possible to wind up with objective, scientific knowledge. They were thus concerned with the epistemic transition mechanisms mapping the initial data of sense into the final data of objective scientific knowledge. Although Russell and Carnap's particular answers differed in important ways, both were concerned to show how the public space-time of physics could emerge from the private spaces and times of the individual. Indeed, both relied heavily upon the philosophy of structuralism and the use of definite descriptions. Both were attempting to defend empiricism in one form or another.

Although the status of the empirical (or the natural) in their respective accounts may be unclear, in neither case did they claim that the hypothetical epistemic transition operations were to be completely established by *a priori* philosophical methods in isolation from the results of the empirical sciences; on the contrary, both Russell and Carnap admitted that their epistemological accounts were, in some sense, to be evaluated by the same empirical standards as those used in science. Hence, both epistemological projects were subject to empirical check and this meant the empirical

⁷ I discuss the historical development of the program from Russell to Carnap to Quine in my forthcoming *Developmental Epistemology* (in preparation).

evaluation of sciences such as psychology. There is, therefore, an important sense in which both Russell and Carnap were engaged in a tacit NE.

This tacit NE became more articulate and self-conscious in the work of Quine, beginning with his “Two Dogmas” paper (Quine, 1961). Later, his conception becomes more explicit in *Word and Object* (Quine, 1960), and received canonical formulation in his “Epistemology Naturalized” (Quine, 1969).

Quine’s (1961) development of his NE is the direct outcome of his criticism of Carnap’s distinction between analytic and synthetic propositions. and, correlatively, of the distinction between *a priori* and *a posteriori* proposition. Standard attempts to characterize (or explicate) ‘analytic,’ Quine famously argued, involved other terms of the same semantic family: ‘meaning,’ ‘synonymy,’ ‘definition,’ ‘necessary,’ ‘semantic rule,’ etc., concepts which, in turn, also required explication because they were also unclear, and any such explication would involve terms in this same semantic family. These distinctions were crucial for the program of Logical Positivism and indeed these distinctions have played a fundamental role in virtual all epistemologies. How can an EB handle these distinctions?

One possible way of breaking out of this semantic circle, Quine suggested, might involve the verification theory of meaning—the meaning of a term is its method of verification—which would provide a way of translating the synonymy of statements into experiential means of verification. Here we seem to have a way of breaking out of the semantic circle, indeed we would have a kind of naturalistic semantics, already present in Carnap’s earlier work. The question was whether this would work, and whether it would salvage more traditional semantic and epistemological properties.

Carnap took up such a challenge in his paper on “Meaning and Synonymy in Natural Language” (Carnap, 1956), and proposed a behavioristic solution involving a thought experiment with a linguist studying the behavior of a speaker to determine the environmental conditions under which the speaker would apply a word to an object. Carnap argued we could determine the intension of a word in this way and hence synonymy and analyticity would be acceptable. One could then construct a manual for determining intensions, with such manuals being the same across scientific observers. Hence, the introduction of intensions for

Carnap was a perfectly acceptable scientific procedure—one based upon a behavioristic methodology involving physical properties.⁸

Quine set out in *Word and Object* (1960) to criticize the above Carnapian behavioristic account. Quine begins with the same project that Carnap and Russell did but from a 3rd—person epistemological perspective: to show how, beginning with certain sensory input (sensory stimuli) to the individual S, how is it possible for S to arrive at his final, public, scientific knowledge of the world? I was,” he says, “posing a Kantian sort of question: how is reification possible? (1990a, p. 291).

An answer to this how-possibly question would involve an account of the transition mechanisms taking the knower from the initial surface irritations to final language competence, and the best subject for such an investigation would be the child: How does the child manage to construct his final theory of the world? How does the child learn language? As Quine puts it (1981): “Accordingly the development from sensory stimulation to objective reference is to be seen as beginning with the flat conditioning of simple occasion sentences—to stimulatory events.

For Quine the failure of traditional justification shows why we need to abandon all traditional epistemological attempts and, instead, to plot the actual developmental trajectory of our beliefs. Why all this creative construction, Quine famously asks, why 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?” (1969, p. 75). Once we relax the demand for definitional reduction and traditional justification, we no longer seek a rational reconstruction as something superior to psychology. “If all we hope for is a reconstruction that links science to experience in explicit ways short of translation, then it would seem more sensible to settle for psychology. Better to discover how science is in fact developed and learned than to fabricate a fictitious structure of a similar effect” (1969, p. 78).

Once we abandon the hope for such a traditional epistemological justification, we abandon traditional epistemology. “Epistemology, or something like it, simply falls into place as a chapter of psychology and

⁸ Carnap suggested that the assignment of a property to a term, i.e., its intension (e.g., “hund” = dog), thus “is an empirical hypothesis which, like any other hypothesis in linguistics, can be tested by observations of language behavior” (1956, p. 237), e.g., by presenting actual and possible cases to the native speaker and seeing whether he ascribed the term to them or not. On the other hand, Carnap goes on to say, an extensionalist such as Quine will deny there is any question of fact here but merely one of choice based on simplicity, and hence no empirical procedure will ever be successful.

hence of natural science. It studies a natural phenomenon, viz., a physical human subject” (1969, p. 82). It presents sensory input to the subject and observes the final output. The study of the relation between “the meager input” and “the torrential output” is a historical and developmental relation, one that requires the postulation of various kinds of transitional mechanisms.

It is frankly not clear if Quine’s account of how our final knowledge develops from our initial knowledge should be seen as a straightforward empirical account or not. Call Quine’s theory of the intervening stages of epistemic transitions from initial stimuli to final output Q. Is Quine advancing Q as a regular scientific hypothesis, one to be confirmed or disconfirmed by actual empirical evidence? It is clear that Q is not the result of Quine first consulting the empirical evidence from developmental psychology and then making an inductive inference; instead, it is a hypothesis initially proposed to describe and to explain the stage transitions. Presumably, therefore, such a hypothesis should be subject to subsequent empirical check by psychologists. Quine, therefore, seems to be doing some kind of armchair theorizing.

In at least one place (Quine, 1969, p. 83; see also 1974, pp. 3, 101,105; 1976, pp. 230-1), Quine suggests this is what he was up to in earlier works (1960, 1974): he was engaged in armchair psychology. On the other hand, Quine sometimes seems to reject the above interpretation, returning to a more traditional interpretation of what he is doing as “rational reconstruction” or “explication.” For example, in his reply to a criticism that his account (1974) is unrealistic, he said: “I was not interested in how people learn our actual quantifiers. . . . My own interest was merely in how the child might progress to an adequate apparatus of reference. I suggested steps whereby he might plausibly arrive at the construction (x) (Gx \supset Fx).” (1979, pp. 429-430). In *Roots of Reference*, he says:

. . . I am not bent even upon a factual account of the learning of English, welcome though it would be. My concern with the essential psychogenesis of reference would be fulfilled in fair measure with a plausible account of how one might proceed from infancy step by step to a logically regimented language of science (1974, p. 92).

Such passages seem to betoken not just a lack of knowledge about the empirical facts but some kind of respect of them as if his project was not subject to empirical appraisal at all. One can speculate about the neurophysiological basis of our construction, but another direction is “more analogous to Carnap’s Aufbau” and this is Quine’s own naturalism. “It is rational reconstruction of the individual’s and/or the race’s actual acquisition of a responsible theory of the external world. It would address

the question how we, physical denizens of the physical world, can have projected our scientific theory of that whole world from our meager contacts with it. . .” (1974, p. 16).

It is frankly unclear, therefore, what the nature of Quine’s theory is: a hypothesis to be tested by empirical research or a rational reconstruction of the actual epistemic development of the child. But the problem with a rational reconstruction has always been: how is it related to the empirical evidence (if at all)?

His program of NE proposes the replacement of a traditional epistemology by a NE, which is an account of the actual development of the individual. Remember his question: “why all this creative reconstruction, all this make-believe? . . . Why not just see how this construction really proceeded? Why not settle for psychology?” (1969, p. 75)? Better to discover how science is in fact developed and learned than to fabricate a fictitious structure of a similar effect” (1969, p.78). In short, is Quine’s NE reducible to psychology (as many suppose) or not?

It is widely asserted that Quine is a behaviorist. For Quine, behavior consists of dispositions—largely verbal dispositions, i.e., tendencies to utter statements under certain kinds of environmental conditions. Ryle’s dispositional account comes to mind and so does Quine’s recognition of the Wittgenstein’s argument against a private language. The input on the stimulus side is less clear since it seems to be on the one hand, “stimulation of nerve endings” but on the other “distal objects and social contexts.” Quine often talks of ‘conditioned response,’ but whether he means classical conditioning or not is unclear: the influence of Watson (1913) is clearly seen given Quine’s early reading of him in college. Quine’s conception of NE reinforces this S-R model. On the other hand, the influence of Skinner also seems undeniable to many, assigning (1960, pp. 80-82) considerable importance to Skinner’s account of operant behavior, citing Skinner’s works (1953, p. 107; 1957, pp. 20, 203).

Several individuals have speculated on the historical connection between these two behaviorists assuming some kind of influence since they were at Harvard together and had extensive discussions. It seems natural to assume they influenced each other in the development of their respective behaviorisms but how much and in what ways is unclear. But recently Verhaegh (forthcoming) studied their historical interactions in considerable detail, concluding that Skinner had little influence on the genesis of Quine’s early behaviorism, which already had made its mark on Quine by 1930. This was reinforced perhaps by Quine’s study (1932-33) with Carnap and Logical Positivism. He concludes that Quine and Skinner did not influence

each other in developing their behaviorisms and in fact had diametrically opposed versions of them in several important ways.

The standard interpretation of Quine is that he is eliminating the normative in favor of the empirical. However, this view must surely be reexamined given Quine's more recent remarks denying this. For example, in his reply to Morton White, Quine (1986) denied that his NE was an abandonment of epistemology. Epistemology is not abandoned but naturalized, with the normative dimension not being jettisoned. As he put it later (1990b): "The normative is naturalized, not dropped" (p. 229). What does he mean by this?

It means, first of all, that NE, the successor to traditional epistemology, is the abandonment of first philosophy, the attempt to construct a philosophical account of knowledge based upon infallible, incorrigible, *a priori* principles concerning the nature of knowledge and how we attain it, employing distinctive philosophical methods and resulting in analytic propositions involving the meaning of terms. Instead, naturalism is seen as an inquiry into reality "fallible and corrigible but not answerable to any supra-scientific tribunal and not in need of any justification beyond observation and the hypothetico-deductive method (1981, p. 72). In short, first philosophy is to be replaced by fallible scientific research.

Secondly, this *a posteriori* method of science is compatible with establishing normative claims, in particular, normative claims about what kinds of processes (means) results in what kinds of states (ends). NE "still has a normative component," Quine says. "It is a kind of technology or engineering" (1992, p.19), indicating what kinds of thing to employ in order to reach what kinds of goals. Obviously, the kind of rationality or normativity meant here is an instrumental rationality involving hypothetical imperatives: if you want G, then do A (because A is an efficient way to get to G). According to Quine, these means-end connections are to be discovered by science (including mathematics, logic, and statistics), e.g., the gambler's fallacy, margin of error, confirmation bias, etc. "There is no question here," Quine says, "of ultimate value, as in morals; it is a matter of efficacy for an ulterior end, truth or prediction. The normative here, as elsewhere in engineering, becomes descriptive when the terminal parameter is expressed." (1986, p. 665). Presumably, therefore, Quine has a naturalistic account of what ultimate values are, perhaps one similar to Dewey's.

As a corollary of this instrumental rationality, Quine recommends certain kinds of norms in the process of hypothesis construction (Quine & Ullian, 1970), e.g., conservatism, simplicity, generality, refutability, modesty, etc. These norms or values are not, Quine insists, criteria for the

selection of which hypothesis to believe subsequent to empirical test, but rather heuristic aids in the initial construction of hypotheses. Presumably, these norms are good norms because, in the past, they have led to the successful creation of scientific hypotheses. Hence, although Quine does not actually defend them (empirically); it would not take much effort to see what kind of defense Quine might mount. Furthermore, it would seem to follow—for the same reason—that such norms should be used in regard to theory acceptance. And yet (puzzling as it is) Quine does not take this step because, for him, the only epistemic criterion is an empiricist one and these norms appears to be non-empirical.

Thirdly, there are normative implications that come out of the results of science. For example, “nothing is in the mind that was not first in the senses,” Quine (1992) believes is a discovery of science. However, it has tremendous normative implications for all of those methods and approaches that violate this empiricist stricture, e.g., telepathy, soothsaying, radical nativism. Hence, empiricism is a norm in Quine’s NE because it has been established on empirical grounds, i.e., in the past we have learned “what leads to what,” in particular, we have learned that using empiricism by testing our theories leads to better results than using non-empirical methods. Once again, this would seem to be an instance of a hypothetical imperative: if you want the best results, then use empiricist methods.

It is clear, therefore, that Quine defends (or would defend) the above set of norms by citing the relevant empirical evidence and in so doing he would be rationally defending his norms. If this is correct, then Quine’s NE is an attempt to “naturalize the normative” by showing that normative means can be established by empirical evidence. If this is so, then in providing a rational reconstruction, such a reconstruction would be rational because it was in accordance with a norm about instrumentality, i.e., a reconstruction of a sequence Q from the initial state I to the final state G is rational if Q actually results in G . Of course, Q can result in G and be illogical, inelegant, ugly, or time-consuming, etc.; in fact, this may be the way the knowing subject actually constructs his world view, e.g., it is now widely accepted that rational agents are not perfectly rational in their market deliberations but that they are “satisficers” (satisfy + suffice) in Simon’s (1956) famous phrase. Perhaps it would be more rational to be infallible in one’s deliberations, but actual agents operate differently. Nevertheless, according to Simon, satisficing is a rational strategy to employ to obtain a certain means. In short, it remains unclear whether a Quinean rational reconstruction is anything over and above an empirical account of the actual empirical transitions or not.

Wilfrid Sellars' Myth of Jones

Many individuals (e.g., Rorty, 1979) ascribe the notion of an EB to Wilfrid Sellars, whose article, "Empiricism and the Philosophy of Mind" (Sellars, 1963) is widely taken to be one of the most important philosophical articles of the 20th century. Sellars had a twofold aim in that article: first, a negative one of characterizing the notion of the "given" as a myth; and secondly offering a possible replacement—the myth of Jones—his EB.

Sellars was concerned to show that the notion of the Given, what epistemologists have traditionally taken as the absolute foundation of knowledge, variously called basic statements, sense-data, protocol sentences, intuition, and so forth is a myth. The "given" was what our senses directly and immediately presented to one's mind, e.g. a red expanse of color, a pain, an awareness of oneself, or even a logically obvious thought. One might doubt whether the cognitive appearance really was that of a red tomato, but one could hardly doubt that something reddish and bulgy was directly present to one's consciousness. This "given" was absolutely certain, indubitable, incorrigible, etc., and hence was the foundation upon which all the rest of one's (empirical) knowledge was built—as logical derivations. Such a notion, employed by so many epistemologists, was central to the Cartesian, 1st person perspective adopted by many epistemologists and psychologists of an older generation.

Sellars spent a considerable amount of time and resource showing that such a notion was a myth, not an intelligible and legitimate epistemological concept (see Brandon, 1997; de Vries & Triplett, 2000 for useful guides through Sellars dense argument). For many individuals, Sellars has made a case that the "given" is a myth and that there is no such thing. So, I will simply assume, for the sake of argument, that he is correct.

If there is no given, then there are a number of phenomena that need to be explained. This is where the "myth of Jones" comes in. Briefly put, Sellars constructs a thought experiment involving, first, a hypothetical culture of individuals—our "Rylean (behaviorist) ancestors"—but who do not yet speak and do not know about their innermost private thoughts. They are behaviorists pure and simple. Although this is debated, they appear to be "logical (analytic, philosophical) behaviorists," concerned to "analyze" their common-sense mentalistic concepts via public behavior (just as Ryle is purported to have wanted to do and perhaps also Wittgenstein).

This public language contains basic logical rules of inference and semantics, and allows individuals to think behavioristic ally. Sellars believes that thoughts are linguistic. According to his "verbal behaviorism,"

“thought episodes are, in the first instance, candid linguistic utterances” (Sellars, 1980). The intentionality and meaning of thought come from the intentionality and meaning of language; not the reverse (for a discussion of current theories about this issue see Kitchener, 1994). Thus, they can be said to “think out loud” via their overt verbal behavior. Then “along came Jones.”

Jones is a genius and comes up with a theory—a folk psychology and/or folk epistemology of a certain sort: “overt verbal behavior is the culmination of a process which begins with ‘inner speech’ (1963, p. 186), where this “inner speech” of individuals is not observable to the participants but (causally) results in the public verbal behavior. Jones is not a logical behaviorist but a *scientific behaviorist*, a *methodological behaviorist* according to Sellars. He is engaged in theory construction, a task that involves the issue of how concepts should be introduced into the language of psychology.

As Sellars points out there are two approaches to the introduction of concepts in science: one can introduce concepts by explicitly defining them in terms of behavior—strict operational definitions or intervening variables—or by postulating their existence as internal states—hypothetical constructs. The second approach is the better one, Sellars argues, and so Jones postulates that what is true of public verbal behavior is also true, in an analogous way, of inner verbal behavior (internal thoughts) with the semantic categories of the former—language is essentially inter-subjective—also applying to this inferred private realm.

Now for the final step:

. . . Jones, has developed the theory that overt verbal behavior is the expression of thoughts and taught his compatriots to make use of this theory in interpreting each other’s behaviour. Thus, when Tom, watching Dick, has behavioural evidence which warrants the use of the sentence (in the language of the theory, ‘I am thinking “p”’ (or ‘I am thinking that p’). And it now turns out—need it have?—that Dick can be trained to give reasonably reliable self-descriptions, using the language of the theory, without having to observe his overt behaviour. Jones brings this about, roughly, by applauding utterances by Dick of ‘I am thinking that p’ when the behavioural evidence strongly supports the theoretical statement ‘Dick is thinking that p’; and by frowning on utterances of ‘I am thinking that p,’ when the evidence does not support this theoretical statement. Our ancestors begin to speak of the privileged access each of us has to his own thoughts. *What began as a language with a purely theoretical use has gained a reporting role’* (p. 189).

In short, “overt behaviour *is* evidence for these episodes” (p. 189). Sellars then extends this account to the internal impressions of immediate experience, thus abandoning the notion of the Given but explaining more adequately what it was supposed to explain.

Sellars’ model appears to involve a secondary, conditioned reinforcement of overt verbal responses: Suppose there is an internal verbal utterance paired with the overt verbal statement in the presence of the external (evidential) behavior followed by Tom’s reinforcing stimulus. A secondary reinforcer is thus established. Then suppose the external behavior is no longer present; Dick now utters, “I am thinking” in the absence of the external (evidential) behavior but on the assumption of the presence of the internal conditioned secondary response. From Tom’s point of view, all that is visible is the verbal report “I am thinking that p.” Dick has learned to report an internal thought on the basis of behavioral evidence.

Clearly something like a Skinnerian model is appropriate here and Sellars says as much. Behaviorism had an early appeal to Sellars and he was familiar with both the S-R model of Hull and Spence (when he was at Iowa 1938-1946) and the Skinnerian approach (when he was at Minnesota 1947-1958). While at Minnesota he surely was cognizant of the classic MacCorquodale and Meehl (1948) distinction between intervening variable and hypothetical constructs. Skinner was also at Minnesota for a while but left before Sellars got there, but MacCorquodale carried on the Skinnerian tradition. Although it is common to assert that Sellars was not a behaviorist, this is true only of logical behaviorism.

What this thought experiment means is that originally knowledge is encapsulated in public behavior but later applied to postulated inner behavior. Originally, we know what others are thinking (and what they know) by virtue of their public behavior (EB), not because of our supposed access to the mythical given. Then we are trained to report these internal states and ascribe epistemic adjectives to them on the basis of their original public meaning, saying things like, “I know I’m in pain,” “I can’t doubt. . .” etc. Public epistemic behavior is the foundation, therefore, of this subject private epistemic behavior.⁹

Sellars EB is a philosophical type, a typical hypothetical thought-experiment of the philosopher, but Sellars was a naturalist and hence presumably scientific evidence could be brought to bear on it. The relevant science here is psychology and one naturally think of behavioristic psychology, but there is another area of psychology that is relevant here—the developmental psychology of Lev Vygotsky.

⁹ The relevance of all of this to Skinner’s account of privacy is well worth pursuing.

Vygotsky's essential assumption, like Sellars, is that the process involved in the course of this epistemological development is not from inside-out (as in most theories in this area) but outside-in, that is to say, the temporal and logical sequence here is from the external social realm (the inter-personal) realm to the individualistic realm (the intra-personal). Vygotsky's law of cultural development reads "all higher mental functions appear first on the inter-psychological plane and then on the intra-psychological plane: ". . . the true direction of the development of thinking is not from the individual to the social, but from the social to the individual (1934/80, p. 36).

Any function in the child's cultural development appears twice or on two planes. First, it appears on the social plane, and then on the psychological plane. First it appears between people as a neuropsychological category, and then within the child as a neuropsychological category. This is equally true with regard to voluntary attention, logical memory, the formation of concepts, and the development of volition. We may consider this position as a law in the full sense of the word, but it goes without saying that internalization transforms the process itself and changes its structure and function. Social relations or relations between people genetically underlie all higher functions and their relationships (1981b, p. 163; see also p. 57).

One example he gives of this process is an especially interesting one for epistemologists, namely those who, like the empiricists, based their foundational premise on the act of ostension, of using the pointing gesture to refer to the basic qualities in experience that serve as their initial starting points, e.g., red color, shape, location, pain, etc. For the empiricist this is the meaning of its basic primitive epistemic terms and the epistemic justification of their subsequent conclusions. In the experience of these qualities (or qualia as they are called) is to be found what is sometimes called "the given," those ultimate qualities or experiences that present themselves to the conscious mind, those things that are just there, the data. Sellars ground-breaking article was directed at undercutting any belief in "the given" by means of his thought-experiment. Humans are capable of such ostensive gestures, indicating what they mean by a word, but animals do not seem to be able to do so. Some psychologists have claimed that pointing gestures do not occur in animals but that some gestures of apes are transitional between grasping and pointing.

In the development of the pointing gesture, "this gesture is nothing more than an unsuccessful attempt to grasp something, a movement aimed at a certain object, which designates forthcoming activity. The child attempts to

grasp an object placed beyond his reach; his hands, stretched toward that object, remain poised in the air. His fingers make grasping movements. At this initial stage pointing is represented by the child's movement, which seem to be pointing to an object—that and nothing more" (1980, p. 56).

The first stage, therefore, is frustrated grasping.

The next stage in this process involves the mother. When she "comes to the child's aid and realizes his movements indicate something, the situation changes fundamentally. Pointing becomes a gesture for others. The child's unsuccessful attempt engenders a reaction not from the object he seeks but from another person. Consequently, the primary meaning of that unsuccessful grasping movement is established by others." (1980, p. 56). An action on the part of the child results in a response on the part of the mother: action-reaction, e.g., the mother fetches the object for the child, carries the child to the desired object, etc. At this point, the meaning of the gesture is something only the other understands. The child wants the object (indicated by his behavior) as interpreted by the mother. But the gesture has no meaning for the child yet.

Later, "when the child can link his unsuccessful grasping movement to the objective situation as a whole," the child begins

to understand this movement as pointing. At this juncture there occurs a change in that movement's function: from an object-oriented movement it becomes a movement aimed at another person, a means of establishing relations. The grasping movement changes to the act of pointing. As a result of this change, the movement itself is then physically simplified, and what results is the form of pointing that we may call a true gesture. It becomes a true gesture only after it objectively manifests all the functions of pointing for others and is understood by others as such a gesture (1980, p. 56).

Vygotsky might be empirically in error about all of this but his account of the acquisition of the pointing gesture is an interesting example of how psychology can empirically support the EB of a philosopher like Sellars involving his "myth of Jones."

IV. Behaviorist Epistemology

I've discussed several varieties of philosophical EB. They share the view that epistemic evidence is behavioral: it is the behavior of people that is the evidence for deciding if S knows that p, S knows how to ϕ , etc. Taking a 3rd person point of view, a claim by the speaker S that another individual or organism O knows something can only be defended by means of the behavior of O and this behavior would appear to be public behavior.

S must be prepared to defend himself, therefore, since S is making a claim to know that O knows something. S observes the behavior of a dog and exclaims “Fido knows where its bone is buried.” Fido’s knowledge is shown by the behavior of Fido, it is (Wittgenstein would say) the *criterion* that Fido knows something, not a symptom. In saying “Fido knows where its bone is buried,” S is prepared to defend that claim against the retort, “how do you know Fido knows that?” Answer: “Look at what Fido is doing.” When S makes that statement, S is making a claim to know something himself, a meta-level epistemic claim about the epistemic state of O. It is S’s perception that is the occasioning event for S’s epistemic claim. It could be that S stated: “Fido is aware that” (sees where) instead, but these are all cognate verbs, ones that differ from saying “Fido thinks that or believes that.” These statements don’t carry the commitment that saying Fido knows, sees or is aware. S must be in a position to offer evidence in support of these claims (just as S must when S ascribes a belief to O, but the latter is a much weaker claim. It makes a truth-claim to say: Fido knows where his bone is buried, namely that Fido is correct. Fido cannot know its bone is buried under the rose bush if it isn’t.

Wittgenstein’s private language argument, Ryle’s dispensationalism, Dewey’s empirical pragmatism, Russell’s naturalistic epistemology, Quine’s naturalistic epistemology, Sellars “myth of Jones,” and Rorty’s sociological version of EB are united in forging an attack on the notion of the Given, the notion that, in the knowing enterprise, there is something present to our consciousness that is certain because it involves no interpretation, judgment, theorizing, no “going beyond the information given.”

The notion of the Given presupposes a 1st person perspective—one committed to the model of the solitary knower privately engaged on the task of constructing the foundation of knowledge by using introspection, one’s immediate intuition as the rock-bottom source of knowledge.

If one criticizes and rejects such a perspective, the way is open to adopt a 2nd person or 3rd person perspective, one that views knowledge as the result of an individual’s interaction with and in an environment—one that is physical and social. An individual S (“you” or “he”) knows something if S knows how to do certain kinds of things, S has knowledge that something is the case because of the presence of certain kinds of features in this interaction process—behavioral features open to public view. This is the heart of EB: an individual S has knowledge only if S displays (or would display) certain kinds of “epistemically relevant behavior” in certain kinds of actual and possible environments. An epistemic claim made by S or by someone else (“S knows that p” or “S knows how to F”) needs justification

under certain conditions—when someone else challenges S—and such justification must be within the public sphere—at least originally. There must be evidence supporting such a claim and such evidence must be inter-subjective. How else can we know that someone else has knowledge?

EB is the theory that knowledge should be seen in a behavioristic way. Knowledge is not to be characterized as something inside the mind of the individual—the Cartesian 1st person perspective—but understood as something external—the 2nd and 3rd person perspectives. Knowledge is fundamentally behavioral in nature, whether actual current public behavior or encapsulated in behavioral dispositions. Such epistemic behavior can be individual or inter—individual (social). Private epistemic behavior, if it exists, is of a secondary nature, deriving from the original public domain and dependent on it

Such an EB is naturally drawn towards a naturalistic epistemology (NE), especially empirical psychology. Arguably, all versions of EB I have discussed are committed to naturalism (although what particular version remains open to debate). This is obviously true of Dewey, Russell, Quine, Rorty and Sellars, who explicitly confess such an allegiance, but it is also true of Wittgenstein and Ryle, which arguably are committed to a mitigated naturalism.

NE is committed to a certain positive attitude towards science, and the most relevant science here seems to be psychology (along, of course, with biology). But what kind of theory in psychology? A behavioristic psychology naturally comes to mind, especially the operant psychology and the behaviorist philosophy of B. F. Skinner (earlier behaviorists who were concerned to do something similar include Clark Hull, E. Tolman, D. Berlyne among others) (See Smith, 1986, who argues for such a claim on historical grounds). Several individuals (Garrett, 1999; Zuriff, 1980) have set forth what kind of EB can be found in Skinner's work. Certainly, Skinner's early interest in constructing a behaviorist epistemology testifies to the fertility of pursuing such an approach. But several issues concerning the precise form of Skinner's EB remain to be clarified, especially his views on privacy and methodological behaviorism (see Moore, 2011).¹⁰

If psychology is to make a contribution to EB, several issues remain to be solved, solved from a naturalistic, especially behaviorist, point of view: the problem of intensionality (belief-states are awareness-that something is the case) vs. the thesis of extensionality; the problem of meaning/ semantics: how can we naturalize semantics (see Kitchener, 1994); and the

¹⁰ Other psychological epistemologies come to mind including the work of Jean Piaget, Joseph Royce, and the almost unknown early work by Max Wertheimer to construct a Gestalt epistemology

fact-norm distinction: how can empirical facts be epistemically germane to norms? All of the authors discussed have made important contributions towards solving these (and related questions) but none have seen the widespread acceptance these epistemological behaviorists would like to have seen. Hopefully the present survey of some possible epistemological behaviorists will provide a first installment on such a task.

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