Behavior and Philosophy, *32*, 503-524 (2004).

INFERENTIALISM AND TACIT KNOWLEDGE

Jon Cogburn
Louisiana State University

ABSTRACT: A central tenet of cognitivism is that *knowing how* is to be explained in terms of tacitly *knowing that* a theory is true. By critically examining canonical anti-behaviorist arguments and contemporary appeals to tacit knowledge, I have devised a more explicit characterization in which tacitly known theories must act as justifiers for claims that the tacit knower is capable of explicitly endorsing. In this manner the new account is specifically tied to verbal behavior. In addition, if the analysis is correct then it follows that the scope and nature of cognitivist appeals to tacit knowledge are largely mistaken.
Key words: Chomsky, cognitive explanations, Dummett, Evans, Fodor, procedural knowledge, propositional knowledge, tacit knowledge

If there is an epic conflict in the behavioral sciences, it concerns the scope and role of linguaform explanation. One side follows in the footsteps of the early Chomsky, attempting to explain human abilities analogously to the running of a high level (uncompiled) program on a digital computer. Like the computer, the brain is thought of as a central processor and the media of thought and action are analogous to a programming language. On this view, the job of the cognitive scientist is to discern the computational mechanisms relevant to the activity of this "language of thought" (e.g., Fodor, 1980).

Opposed to this linguaform conception are a variety of interrelated theories and approaches stemming from a resurgent behaviorism (e.g., Staddon, 2001), neo-associationist computational models (e.g., Elman et. al., 1997), and the phenomenological tradition (e.g., Petitot et. al., 2000).

The outcomes of this titanic battle should, in the long run, be determined by comparison of developed theories in terms of theoretical virtues such as level of confirmation, scope, fecundity, elegance, fit with other confirmed theories, etc. However, this is not to slight the role that philosophy plays. Philosophical analysis can help in adjudicating debates of this sort, even at the very early stage in which the behavioral sciences find themselves. In particular, epistemic assumptions in which philosophers have traditionally been interested loom large in the relevant debates.

Proponents of linguaform explanations such as Chomsky (1959, 1993, 1995, 1996) and Fodor (1968, 1980) almost uniformly claim a privileged epistemic status for their explanations, one supposedly not shared by competing non-linguaform explanations. In the terminology of analytical philosophy of language, defenders of

AUTHOR'S NOTE: Please address all correspondence to Jon Cogburn, Assistant Professor, Department of Philosophy, Louisiana State University, 106 Coates Hall, Baton Rouge, LA 70802. Email: jcogbu1@lsu.edu; Tel.: 225-578-2492.

linguaform explanations hold that their explanations are not just true but also "tacitly known" by the people of which the explanations are true. In this manner, what would pre-theoretically be thought of as, at best, "know how" is characterized by defenders of linguaform explanation as a species of "knowing that," in this case knowing that the propositions of the linguaform theory are true. In a roundabout way this ends up justifying linguaform approaches.

To best understand this, consider a linguaform versus non-linguaform approach to implementing pronunciation of written words by a computer (see Elman et. al., 1997 for details). The first software to do this successfully took years to design. Programmers explicitly represented a finite set of principles that the program followed. The non-linguaform, neural network implementation of this ability took a few days to train. The generalized non–domain-specific learning algorithm worked to get the machine to correctly pronounce the words. However, the inside of the neural network machine is just a set of weighted nodes that in no clear way represent linguistically representable propositions corresponding to principles of pronunciation. To the extent that such simulations are genuinely explanatory, we can now see the role that tacit knowledge plays. For defenders of the linguaform approach the stated principles programmed into the computer represent the propositions tacitly known by competent readers of English. For defenders of the non-linguaform approach the success of the neural network machine shows that practical ability can be possessed in virtue of instantiation of a domain general learning algorithm and that no propositional knowledge, *a priori* or otherwise, need be involved.

Of course, the debates between these two broad tendencies cannot be settled here. However, given its justificatory role, clarity about the notion of tacit knowledge is a necessary part of settling them. In what follows I start with the canonical (Gareth Evans') characterization of tacit knowledge from the analytical philosophy of language. When generalized to the behavioral sciences, Evans' definition has the counterintuitive result that a crane has tacit knowledge of aerodynamics. I then tie this failure to a more recent worry of the philosopher Michael Dummett, the concern that his own earlier conception of tacit knowledge merely amounted to calling practical abilities a kind of knowledge. Dummett is concerned that his earlier view might be subject to canonical criticisms of behaviorism, the validity of which have largely (until recently) been assumed by the philosophical community. Given the failure of Evans' view, and Dummett's worry, it would seem reasonable to examine the phenomena commonly taken to undermine behaviorism. In this context it is fascinating that the critiques of early forms of behaviorism do not in any way motivate characterizing mental abilities as a species of propositional knowledge, hence they provide no support for linguaform explanations. Given this, phenomena such as domain specificity and the importance of getting inside the "black box" are of no help in characterizing tacit knowledge.

To get out of this impasse I pick up another facet of Dummett's characterization of tacit knowledge, that is, the requirement that a person be able to recognize the tacitly known theory as true. While this is promising, and consonant

with the epistemic talk of many linguists, it too is beset by problems. Most people simply are not able to recognize correct theories in cognitive science. Furthermore, those who are able to do so tend to disagree with one another!

The Dummettian should respond that the requirement only applies to idealized people. However, this form of idealization again renders the definition non-necessary for tacit knowledge. If a rock were much smarter, then that rock would be able to recognize the laws of gravity that govern its behavior.

By examining linguists' actual use of the notion of tacit knowledge I suggest a principled, broadly behavioristic way to limit the Dummettian idealization. I suggest that the theory tacitly known should justify propositions non-tacitly known by the person in question. Though the resulting characterization is free from obvious counterexamples, it ends up being far from trivial because it entails that language of thought and rule-based explanations will work best for cognitive phenomena of which we are relatively articulate (e.g., planning, reasoning, linguistic phenomena) and competing approaches will work best for phenomena of which we are relatively inarticulate (e.g., sensation, bodily movement). Thus, though my proposed characterization is arrived at mostly by philosophical analysis, it has predictive value for empirical theories. In the closing section I demonstrate how it ramifies into other debates concerning knowledge and the behavioral sciences.

Evans' Proposal

The clearest explication of the notion of tacit knowledge in the philosophical literature remains the discussion in "Semantic Theory and Tacit Knowledge," in which Gareth Evans (1985, p. 336) explicates the notion in this manner:

> (1) At the level of output, one who possesses the state of tacit knowledge that *p* is disposed to do and think some of the things which one who had the ordinary belief that *p* would be inclined to do and think (given the same desires).
>
> (2) At the level of input, one who possesses the state of tacit knowledge that *p* will very probably have acquired that state as a result of exposure to usage which supports or confirms (though far from conclusively) the proposition that *p*, and hence in circumstances which might well induce in a rational person the ordinary belief that *p*.

This is a roundabout way of saying that (1) the tacit knower behaves as if *P* were true without verbally acknowledging that it is true, and (2) one could learn that *P* were true from the behavior to which the tacit knower has been exposed. We can perhaps best understand this if we consider syntax. A correct syntax for a language will generate all and only the grammatical sentences of that language. At the level of output, this matches up with the ability of competent language users to discriminate between grammatical and ungrammatical sentences. For the case of input, the usage to which a competent speaker is exposed as a child is the same as that used by the syntactician in testing his theory.

Thus, we can start to make good sense of how the attribution of tacit knowledge of a correct syntax to competent speakers is both explanatory and testable. Given Evans' characterization of tacit knowledge, we can represent it in this manner:

(1) At the level of output, one who possesses the state of tacit knowledge that a correct syntax *S* for a language *L* is correct is disposed to make the same grammaticality and distributional judgments as one who has the ordinary belief that *S* is a correct syntax for *L* (given relevantly similar desires).

(2) At the level of input, one who possesses tacit knowledge that *S* is a correct syntax for *L* will very probably have acquired that state as a result of exposure to grammaticality and distributional judgments that support or confirm (though far from conclusively) the proposition that *S* is a correct syntax for *L*, hence in circumstances that might well induce in a rational person the ordinary belief that *S* is a correct syntax for *L*.

It should be somewhat clear that characterization of tacit knowledge does provide plausible (if vague) necessary conditions for the correct attribution of tacit knowledge (at least when concerned with syntax) of the truth of a proposition to a person. However, Evans' characterization certainly does not provide adequate sufficient conditions.

It is relatively easy to come up with situations in which we would not say that a person has tacit knowledge of a set of propositions, while both the input and output conditions of Evans' definition of tacit knowledge are satisfied. It is easy to show cases of procedural knowledge that satisfy Evans' description.

Consider a crane—call him Leonard—who is quite proficient at flying. We don not want to say that Leonard has tacit knowledge of the textbook *Principles of Aeronautical Engineering*, but Evan's criterion, taken as a sufficient condition, forces us to say that Leonard does. Consider:

(1) At the level of output, one who possesses the state of tacit knowledge that *Principles of Aeronautical Engineering* is correct is disposed to do and think some of the things which one who had the ordinary belief that *Principles of Aeronautical Engineering* is correct would be inclined to do and think (given the same desires).

(2) At the level of input, one who possesses the state of tacit knowledge that *Principles of Aeronautical Engineering* is correct will very probably have acquired that state as a result of exposure to usage which supports or confirms (though far from conclusively) the proposition that *Principles of Aeronautical Engineering* is correct, hence in circumstances that might well induce in a rational person the ordinary belief that *Principles of Aeronautical Engineering* is correct.

To see why this does not work, let Amelia be the author of *Principles of Aeronautical Engineering*. Also note that Amelia had wanted to design and fly

planes since she was a child. Again, at the level of output, both Leonard and Amelia end up behaving the same way; given that Amelia desires to fly, *Principles of Aeronautical Engineering* functions as a how-to book. The clause for input is satisfied because Leonard must cope with the same physical conditions Amelia must when testing her theories. *Principles of Aeronautical Engineering* explains these conditions by predicting them from a rich and inventive combination of physical law and mathematical approximation theory.

Thus, if we take Evans' characterization of tacit knowledge to be correct we are forced to say that Leonard has tacit knowledge of the propositions expressed in *Principles of Aeronautical Engineering*. However, it is very strange to say that Leonard has tacit knowledge of a physical theory rich enough to adequately describe and predict his flying behavior.

The insufficiency of Evans' account suggests that propositional knowledge may in fact be irrelevant to large swaths of the behavioral sciences. Perhaps the cognitive revolution rests upon a simple mistake, and the relationship between a correct behavioral science and our own cognitive abilities is no more direct than that between *Principles of Aeronautical Engineering* and Leonard's flying abilities.

Some of Michael Dummett's earlier writings about the theory of meaning tended to suggest this view, as admitted in this more recent passage (1991, p. 105):

> If linguistic competence could be straightforwardly classified as a practical ability, we could say, as I once did say, that in framing a meaning–theory we are giving a theoretical representation of a practical ability—the ability to speak the language. We are representing this complex ability as consisting in the knowledge of a theory, that is of an articulated structure of propositions. On this account, we are analyzing a complex of practical abilities by feigning to attribute to who has these abilities a knowledge of the theory.

Dummett takes it to be clear that this earlier dogged insistence on seeing linguistic competence as only a practical ability is an implausible form of behaviorism, though he nowhere says why it is implausible. Rather, he seems to have been swept up in the anti-behaviorist *zeitgeist* of Universal Grammar's heyday. Following Dummett's suggestion, one should examine the alleged problems with radical behaviorism as a guide to characterizing the notion of tacit knowledge.

Evans' view is problematic because it entails that we have tacit knowledge in situations in which no propositional knowledge is involved. Starting with Chomsky's influential "Critique of Skinner," the cognitivist reaction to the alleged failures of radical behaviorism is to see the behaviorist as committing the opposite sin, not countenancing tacit knowledge in cases in which people clearly do have tacit knowledge, the canonical case of this being Chomsky's own example of language learning. If this is correct, then an examination of the problems with behaviorism should be fruitful in amending Evans' definition.

Radical Behaviorism and Tacit Knowledge

Two tenets of radical behaviorism can be given in this manner: (1) acquisition of psychological abilities can be described in terms of generalized learning strategies that make no special reference to the subject matter being learned, and (2) possession of psychological abilities can be described in terms of functions from stimuli to responses that make no mention of internal mechanisms "computing" these functions. The first tenet asserts that learning is domain-general, whereas the second asserts that issues concerning the realizability of psychological abilities are irrelevant.

A cursory examination of the issues of domain generality versus domain specificity and physical realizability shows that, contrary to expectation, they do not help in discerning a plausible characterization of tacit knowledge. The falsity of radical behaviorism does not entail the truth of cognitivism. One can consistently deny the tenets of radical behaviorism at dispute while holding that psychological abilities such as language use and intentional action do not in any deep way involve propositional knowledge.

Domain Specificity

Given that Chomsky's poverty-of-stimulus argument is the most influential anti-behaviorist argument for domain specificity, it must be given some attention. I will not argue that Chomsky was wrong in criticizing the view that a language can be characterized in terms of simple stimulus–response pairs. However, it is easy to show that his argument in no way entails that we must characterize linguistic ability in terms of tacit knowledge, as he often seems to assume it does. The concern is very similar to the concern I have with Evans' characterization of tacit knowledge. As I will show, the poverty-of-stimulus argument applies equally to the acquisition of a spider's ability to weave a web. Again, we would not normally say that a spider has knowledge of a theory of web building.

Chomsky claims that the stimulatory input available to a child while learning a language could only be sufficient to predict the child's later linguistic dispositions if we assume that there are domain-specific constraints on the learning process. More recent work (e.g., Keil, 1994) has provided empirical evidence for these claims, albeit not as applied to learning of natural language syntax. Following Chomsky, when concerned with linguistic abilities the learning task is probably most clear if we think of the child as a "little linguist," learning how to speak a language by observing adult behavior and making inductions about what is and is not grammatical.

In classical behaviorism this inductive process was thought to be completely domain-general, in that the child was just thought of as perceiving whatever similarities to which he was exposed to, then (given an appropriate form of conditioning) generalizing from this exposure. *Pace* this view, Chomsky argued that learning mechanisms are highly domain-specific; inductions made by learners can only be regarded as being guided by true principles specific to that which they

are learning. For example, a very young child will not apply the same count noun to both inanimate and animate objects.

It is unfortunate that the fascinating and relevant current work that does not involve acquisition of syntax has been so little noticed by philosophers. Keil (1990, 1994) has presented astute and philosophically interesting discussions of the kind of domain-specific information that children are best represented as utilizing. He presents an overwhelming amount of data to argue convincingly both that children are hardwired with a distinction between material objects, biological objects, artifacts, and people, and that these distinctions not only play a strong role in acquisition of words for types of objects but also determine a set of *a priori* beliefs children bring to the world. For example, children will accept that a coffee can is able to become a tree house but reject that a tiger can become a cactus. Also, the domain-specific distinctions concerning types of causality have been shown to exist in children as young as four months old.

Chomsky often suggests a dichotomy between Skinnerian behaviorist accounts of acquisition (in which the mechanisms are non-innate and domain-general) and cognitivist accounts (in which the child is thought of as having *a priori* tacit knowledge). For example, in a recent paper, after a discussion of the poverty-of-stimulus argument that contains the surprising suggestion that we possess an innate grasp of the concept CARBURETOR, Chomsky (1996, p. 574) writes,

> However surprising the conclusion may be that nature has provided us with an innate stock of concepts, and that the child's task is to discover their labels, the empirical facts appear to leave open few other possibilities. Other possibilities (say, in terms of "generalized learning mechanisms") have not, to my knowledge, been coherently formulated.. . . .Since these facts are known essentially without evidence, it must be that the child approaches language with an intuitive understanding of concepts involving intending, causation, goal of action, event, and so on, and places the words that are heard in a nexus that is permitted by principles of universal grammar, which provide the framework for thought and language, and are common to human languages as conceptual systems that enter into various aspects of human life.

The possibility that Chomsky does not explore involves the affirmation (with Chomsky the rationalist) that innate learning mechanisms are domain-specific, coupled with the refusal (*pace* Chomsky) to take this as evidence of propositional knowledge.

Even Frank Keil, perhaps the leading researcher in the current wave of developmental research relevant to the issue of domain specificity, is very clear that the dichotomy between radical behaviorism and Chomskyan cognitivism is a false one. He (1990, p. 152) writes,

> I am reluctant to grant spiders intuitive theories of the mechanisms of physical lattices like webs, even though their behavior displays a precise honoring of such principles. Similarly, cockroaches and other cognitively "simple" creatures also seem to pick out objects and follow their trajectories and the like, yet one

> cringes at calling them object theorists. . . .Thus, there are some unresolved questions concerning what constitutes evidence for theory versus less belief-laden systems of representation.

Thus, the proper anti-cognitivist response to Chomsky is to argue that what Chomsky takes to be innate knowledge of so-called "universal grammar" and *a priori* possession of concepts are really just (to the extent that we have knowledge for them) innate stimulus–response dispositions instinctually "hard wired" into people with a high degree of domain specificity. Then if (a big if, see below) universal grammar exists, it should be treated as an abstract description of mechanisms that effect a severe limitation on the kind of dispositions human beings can bring to the acquisition process—likewise for a correct theory of concepts.

Therefore, Chomsky's poverty-of-stimulus argument is of surprisingly little help in discerning a notion of tacit knowledge. The falsity of some of the tenets of radical behaviorism does not entail the truth of cognitivism. Perfectly consistent with the falsity of old-school behaviorism is (following Keil's worry) the view that language acquisition is the result of highly domain-specific instincts, instincts of the sort that do not require attribution of any kind of propositional knowledge, tacit or otherwise.

Realizability

One might take the second primary challenge to behaviorism in the sciences of the mind to be relevant in helping to discern a better characterization of tacit knowledge. Early behaviorists like Skinner (1957) did not just deny domain specific constraints; they also maintained a dogged insistence that any mechanisms involved in producing dispositions were strictly irrelevant to psychological characterizations. The behaviorist was to treat mechanisms causally efficacious for these dispositions as irrelevant. Thus, along with Chomsky's anti-empiricist rejection of radical behaviorism, we also have the computational rejection of behaviorism.

The computationalist's guiding metaphor is the digital computer. Thus, for example, linguistic behavior is characterized as being the "output" of a program realized in the human brain. While a computationalist could relocate Skinner's "black box" one level down by holding that the proper study of linguistic competence does not involve studying the relevant hardware (presumably the human brain), he might also hold that study of the relevant hardware is a good guide to the computational architecture involved in linguistic competence. For example, certain kinds of brain dysfunction are best characterized as leading to computational dysfunction of our abilities.

Aphasics are people whose linguistic abilities are defective in various ways because of trauma to the brain. As discussed by Pinker (1994, pp. 297-331), these defects can be highly specific. Some are localizable in common places for most people. People with damage to Broca's area typically have severe difficulty constructing sentences. Damage to Wernicke's area does not cause problems with

the construction of syntactically well-formed sentences. However, such aphasics typically compulsively speak word-salad and also have great difficulty naming objects. People with damage to the connection between Broca and Wernicke's areas typically cannot repeat sentences when asked. People with the Broca and Wernicke's area intact, but severed from the rest of the cortex, can typically only repeat sentences they've heard, with no sign of comprehension. Damage to the area between Wernicke's area and the angular and supramarginal gyri often causes loss of the ability to speak or understand most nouns (exceptions being gerunds, pronouns, and a few generic nouns). In addition to localization common to most speakers, there is much evidence that other grammatical functions are localized, but in areas of the brain very different for different speakers. A wide variety of extraordinarily specific malfunctioning has been observed, including: lack of ability to understand speech coupled with retention of the ability to read, speak, and write, and lack of ability to speak or process very specific syntactic constructions or word groups (such as verbs, classes of nouns, function words, and so-called trace constructions such as "The man who the woman kissed hugged the child").

While such evidence does support the claim that human brains, in some sense, "compute" language in characteristic ways, this form of anti-behaviorism is no more helpful for the cognitivist. Our questions involve the extent to which such data in particular, and computationalist metaphors in general, are helpful both in defending the claim that competence is a species of propositional knowledge and in successfully characterizing criteria for something counting as tacit knowledge. The answer to both questions is that it is not very helpful.

As with the poverty-of-stimulus argument, there is no reason to characterize these phenomena as involving propositional knowledge rather than simply practical abilities. For example, consider the linguaform verses neural net approaches to modeling pronunciation of written text. While the set of true propositions programmed in the linguaform version did describe the ability, it was in no way represented in the computational machinery of the neural net version. It is very possible that something similar holds of cognitive abilities in the human brain. Consider, for example, those involving the sensory modalities, which are also significantly localized in the brain. A computationalist model of vision is at least as defensible as a computationalist model of language abilities, and, again, it is very misleading to characterize people as having tacit knowledge of a theory of, say, the physical and biological principles involved in color discrimination and depth perception. Once again, the possible limitations of some of the tenets of radical behaviorism do not entail the truth of cognitivism.

At this point one might wonder whether there are *any* principled reasons to adhere to cognitivist explanations. Perhaps we should stop here and conclude that the notion of tacit knowledge need not be explicated because the thought that we have tacit knowledge relies on such duplicitous dichotomies. Alas, this would be too quick. First, it misconstrues the proper relationship between philosophy and the sciences. With some tragic exceptions (see Huck & Goldsmith, 1996) scientific theories rise (and fall) with their empirical successes (and failures). Philosophical

pretensions should play a minor role. Second, even if tacit knowledge is a hopelessly confused notion, the philosopher would still have the task of showing why it has (at least sometimes) been a fruitful metaphor. Thus it behooves us to keep seeking a characterization both not subject to obvious counterexample and such that it applies to successful cognitively motivated theories.

Further Behavioral Constraints

John Searle (1992) argues that a necessary constraint on the attribution of individual subconscious beliefs is that such beliefs must in some sense be accessible to consciousness. While Searle does not apply this constraint to issues relevant to sorting out cognitivist and non-cognitivist approaches to mental abilities, we shall see that it has a large resonance.

Recognitional Capacities

In his discussions of linguistic competence, Michael Dummett often gives the distinguishing feature of tacit knowledge as Searle's (later) constraint on subconscious belief. On Dummett's early view, one can only properly be attributed tacit knowledge of a proposition *P* if one is able to recognize *P* as correct when presented with an explicit statement of *P*. For example, he argues that

> . . .we have to take more seriously the ascription of knowledge to someone who possesses the practical ability in question: "knows how to do it" is not here a mere idiomatic equivalent of "can do it". Rather, we may say of the agent that he knows *that* certain things are the case, that he knows certain propositions about how the operation is to be performed; but we need to qualify this by conceding that his knowledge is not *explicit* knowledge, that is, knowledge which may be immediately elicited on request. It is, rather, *implicit* knowledge: knowledge which shows itself partly by manifestation of the practical ability, and partly by a readiness to acknowledge as correct a formulation of that which is known when it is presented. (1996, p. 96)

This recognitional ability is best explained by positing a kind of knowledge. In *For Truth in Semantics* Anthony Appiah (1986, p. 7) discusses Dummett's views and argues that most of our everyday beliefs have this property to which Dummett calls attention.

> . . .someone can believe something—that there is, now, a war in Namibia—even when they are currently unaware that they believe it. Not only do I think this is obviously correct—consider a sleeping member of the South African Defense Force, or a waking British Prime Minister—but I think many of us have beliefs that are not only thus unconscious sometimes, but unconscious all our lives. If I had never thought about the matter, I would not know that I believe that the car I drove last week had a hand-brake. I used the brake in parking, and released it in starting; and that use was guided by my belief. But at the time, I did not need to bring this belief to consciousness; if I had not been looking for a philosophical example I might never have done so. . .

Appiah is right; most of our beliefs are not brought to consciousness, but are such that if queried we would confess to them. But then, if principles true of our language had this property, we would have good evidence that we in some non-conscious manner believe the true theoretical claims about of our language.

However, some caveats are in order. It cannot be denied that many people who are certainly competent language users are probably incapable of learning enough linguistic theory to even understand many of the claims true of their language. Both Donald Davidson (1984) and Dummett have held, in much of their work, the conjunction of the views that (1) people are linguistically competent in virtue of tacit knowledge of a correct theory of meaning for their language, and (2) a necessary constraint on attribution of tacit knowledge of a proposition *P* is the ability to recognize that *P* is true when presented with an explicit statement of *P*. These two views together have the consequence that people who cannot learn linguistics are not linguistically competent. If we mean to apply "linguistically competent" to everyday speakers (who possess the ability we are trying to explain) then this is an absurd result. Therefore, this characterization of tacit knowledge is unacceptable as it stands.

Unlike Davidson, Dummett (1991, pp. 95-96) himself came to recognize the problem with this view. For example:

> The concept of implicit knowledge is of little assistance here. The term should properly be reserved for knowledge which its possessor is incapable, unaided, of formulating verbally, but of which he can recognise a formulation when presented with one.

Dummett then goes on to admit that this kind of knowledge of linguistic theory should not be attributed to competent speakers. He writes (ibid.),

> A piece of *implicit* knowledge may perhaps be attributed to someone who has only an implicit grasp of the concepts involved. If a speaker always uses the pairs "I"/"me", "he"/"him", "she"/"her", "who"/"whom" correctly, but, never having been taught the rudiments of formal grammar, has never heard the words "nominative" and "accusative", can he be said to have an implicit grasp of the concepts they express? A statement of the rule he tacitly follows will involve an explicit formulation of those concepts and will necessarily be somewhat lengthy. Still we may credit the speaker with an implicit knowledge of that rule, provided that, when he understands the statement of it, he acknowledges it as accurately describing his existing practice.
>
> The concept of implicit knowledge is not infinitely elastic, however: if we try to stretch it to cover our whole knowledge of our native tongue, it will snap. An explicit statement of the principles governing the use of the language will amount to a meaning–theory. It would be preposterous to suggest that all competent speakers would recognize such a theory as correct if it were presented to them. Most would not understand it: those who did would probably engage in disputes, far from easy to resolve, over whether it was correct.

Unfortunately, after stating this misgiving in *The Logical Basis of Metaphysics*, Dummett's comments on what kind of knowledge constitutes linguistic competence are extraordinarily sketchy. However, the problem is that the purported definition of tacit knowledge does not specify necessary conditions (when we understand tacit knowledge of a meaning theory to be necessary and sufficient for linguistic competence).

On the other hand, people who are sufficiently cognitively situated are capable of recognizing truths about their language. This fact both licenses the introspective method of much contemporary linguistics (as practiced by Chomskyans and non-Chomskyans) and supports the claim that some form of knowledge is involved in linguistic competence. Of those who are not capable of recognizing true linguistic claims, it is still sensible to maintain that, were they better cognitively endowed, they would be able to recognize the generalizations.

As recognition is a success verb, we must be very careful. It should be clear that the process involved in recognition is defeasible, meaning that evidence supporting such recognition can sometimes be defeated by stronger counter-evidence. Thus, competent speakers might falsely take themselves to recognize a false claim as being true. Though Dummett does not discuss this issue, we can cope with it by understanding our attribution of the recognitional capacity to involve holding our idealized speakers to be such that, were they presented with a false generalization and a true generalization attempting to account for some range of phenomena, they could distinguish the false from the true.

This recognitional capacity suggests amending our definition of tacit knowledge in this manner:

(1) At the level of output, one who possesses the state of tacit knowledge that *P* is disposed to do and think some of the things which one who had the ordinary belief that *P* would be inclined to do and think (given the same desires).

(2) At the level of input, one who possesses the state of tacit knowledge that *P* will very probably have acquired that state as a result of exposure to usage that supports or confirms (though far from conclusively) the proposition that *P*, hence in circumstances that might well induce in a rational person the ordinary belief that *P*.

(3) One who possesses the state of tacit knowledge that *P* is such that, with some finite (possibly null) extension of their cognitive capacities, he can recognize that *P* is true when presented with an explicit statement of *P*.

Now, I believe something like this is right, as far as it goes, and that it is suggested by Dummett's problem with his earlier view.

However, this definition is not without its own problems. Moreover, these problems are once again precariously similar to the problems that beset Evans' original characterization. Consider a crane. If its cognitive capacities were extended enough, it would be able to recognize that propositions of aerodynamics

were true. So this characterization again seems to fail to provide sufficient conditions for possession of tacit knowledge.

Thus, from Dummett we have that if the third clause in the above definition is not idealized, then the definition does not provide necessary conditions. Our problem is that if the third clause admits too much idealization, then the definition is again not sufficient.

Clearly, one who wants to use the above characterization to claim that linguistic competence is a species of knowledge must say something informative and not circular about which kinds of idealizations are appropriate.

Inference

We have seen that our first two considerations (domain specificity and internal mechanisms) did not help us characterize tacit knowledge, and that our third (privileged access) does, but at the price of a radical instability, depending upon how the recognitional ability is idealized. The fourth consideration to which I shall call attention constrains the idealization in a way that does support and help make sense of the view that some cognitive abilities involve tacit propositional knowledge. To do this we will attend to clear cases in which certain kinds of linguistic competence uncontroversially involve an epistemic component. For example, as Quine (1960) convincingly argues, lexical competence concerning word meaning is so intertwined with theoretical knowledge of true sentences involving the words that one could reasonably argue that it is impossible to make any kind of principled distinction between lexical competence and encyclopedic knowledge. Another kind of example concerns the role of beliefs in actions. Intentional actions are paradigmatically explained by appeal to a set of desires and a set of beliefs held by the agent. As Grice (1989) first showed, analyzing conversational contributions as intentional actions requires attributing to the actors several beliefs concerning both their interlocutor and their language.

The relevance of these sorts of epistemic components is that they are very close to standard, everyday sorts of non-conscious knowledge or belief (e.g., Appiah's car example), and thus make it more plausible to attribute tacit knowledge of other, less easily retrievable facts. Returning to our discussion of syntax and semantics makes this clear.

Where *v* ranges over English verbs, *vp* ranges over verb phrases, *np* ranges over English noun phrases, $(p)np_1$ ranges over the result of pluralizing the head noun in np_1, and vp_e ranges over extensional verb phrases, nearly any competent speaker of English will recognize instances of the following schema:

I know that "*v*" cannot come before "*vp*" in a sentence.

(e.g., "I know that 'happened' cannot come before 'went to the store before lunch feeling very upset about the crisis in Kashmir' in a sentence.")

I know that if "np_1" and "np_2" name the same thing(s), then $(p)np_1\ vp_e$ if and only if $(p)np_2$ do/does too.

(e.g., "I know that if 'rational animal' and 'featherless biped' name the same things, then rational animals walk if and only if featherless bipeds do too.")

Of course, not all such everyday "meta-linguistic" behavior of competent speakers concerns discussion of specific word and phrasal categories. Another important example of this is ambiguity resolution, at which the vast majority of competent speakers are skilled, and which also involves quoting whole sentences. Consider the following two monologues, which are not at all implausible:

He said "Latoya and Michael washed ten cars." I know that means either that ten cars got washed or twenty cars got washed. Dang! Which one is it?

He said "Frank signed the papers on the boat." I know that means either that the papers on the boat now are the ones Frank signed, or that Frank was on the boat when he signed some papers. Dang! Which one is it?

Furthermore, speakers do not only make correct knowledge claims about language, they also remonstrate with other speakers who doubt such claims. But now we can ask: with what justification do speakers engage in such remonstration? What justifies each of the above knowledge claims, both transparently (certainly at least in part) about language? A correct syntax and semantics for English will predict all instances, respectively, of the above two embedded schemas and predict the distinct truth conditions corresponding to the ambiguous sentences. Thus, one plausible thing to say is that speakers who make such knowledge claims and state instances of the embedded claims while remonstrating with others are justified in doing so by the fact that the correct syntax and semantics for English predict they are correct in making the claims they make about their language. This does provide some evidence for taking people to have tacit knowledge of syntax and semantics.

This directly suggests a manner to license the idealization of cognitive capacities in the proper definition of tacit knowledge. The proposition that would be known under the idealization must function as a justifier for normative assessments and correct knowledge claims that the tacit knower makes, or is willing to make. Therefore, we can say that *A* tacitly knows *P* if and only if:

(1) At the level of output, one who possesses the state of tacit knowledge that *P* is disposed to do and think some of the things which one who had the ordinary belief that *P* would be inclined to do and think (given the same desires).

(2) At the level of input, one who possesses the state of tacit knowledge that *P* will very probably have acquired that state as a result of exposure to usage that supports or confirms (though far from conclusively) the proposition that *P*, hence in circumstances that might well induce in a rational person the ordinary belief that *P*.

(3) One who possesses the state of tacit knowledge that *P* is such that, with some finite (possibly null) extension of their cognitive capacities, he can recognize that *P* is true when presented with an explicit statement of *P*.

(4) The idealization of cognitive capacities in (3) must be licensed by the tacit knower's behavior, in that *P* must function as a justifier for normative assessments and knowledge claims that the tacit knower does make, or is prepared to make.

I think that this does limit the idealization in a reasonable way. Moreover, I take it that this added component is what moves many linguists to characterize linguistic ability as a species of knowledge. A typical example is the introductory section of Peter Cullicover's syntax textbook (1997, pp. 3-4), which begins with the poverty-of-stimulus concern:

> It is hard to suppose that knowledge of a given language, such as English, is present at birth. Why wouldn't everyone grow up speaking the same language, then? On the other hand, no one has yet been able to demonstrate that general learning mechanisms that are not specific to language acquisition could acquire human languages in all their richness.

However, he goes on to appeal to the fact that linguistic theory justifies many propositions about language held true by competent speakers:

> One main reason for this is that there are many things that we know, as native speakers, for which there appears to be no evidence in the experiences that we have as language learners. For instance, we know that the question in (*2b*) is ungrammatical, while the question in (*2a*) is grammatical.
>
> (*2a*) Who did you buy a picture of?
> (*2b*) *Who did you buy Mary's picture of?
>
> It does not appear that children are provided with specific information during the course of language learning that will indicate to them the relative grammaticality of such examples. In fact, it does not appear that children are provided with *any* systematic information about the ungrammaticality of particular examples.

Thus, nestled into a statement of the poverty of stimulus argument is the role of nontheoretical claims about language that competent speakers do know.

Possible Objections

By examining possible counterarguments to the analysis we shall see that even though the characterization was arrived at by philosophical analysis, it does have non-trivial empirical ramifications. Thus, to the extent that these ramifications can plausibly be thought to be coming to pass, there is empirical support for the analysis.

Linguistic Deficits

One might argue that the above *definiens* are not necessary for tacit knowledge attributions by claiming that some linguistically capable people are unable to either make knowledge claims about language or correct other's usage. Such a response is slightly odd; I think we would characterize people who are incapable of correcting other's usage or making true knowledge claims about their language to be linguistically deficient. Perhaps more to the point, though, I am not convinced that a characterization of tacit knowledge needs to prevent counterexamples such as this. It is enough if our characterization holds of the vast majority of people whom we call linguistically competent. Then, family resemblance considerations can pick up wherever the criterion fails. If my characterization does hold of the vast majority of people who are linguistically competent, and if a small minority are relevantly similar to those characterized but lack one of the criterial features in the definition, then the characterization is successful enough. Deviant cases, in which it is clear that something relevant to the *definiendum* is deviant, do not normally count against the correctness of the *definiens*.

The counterexamples to the earlier analysis were not borderline. All of the examples that showed given characterizations of tacit knowledge to be insufficient were such that it was very clear that the creature in question did not have any kind of propositional knowledge, tacit or not, of propositions of the theory under consideration. Evans' characterization entails that people have tacit knowledge of psychological theory and that cranes have tacit knowledge of aerodynamics.

We considered whether or not the fact that linguistic abilities were domain-specific and dependent upon physical realization could be used in reformulating Evans' characterization. Given that instincts such as a spider's ability to weave webs are also domain-specific and dependent on physical realization, it was totally unclear how early critiques of radical behaviorism provided evidence for construing linguistic abilities as a species of knowledge. Any change of Evans' definition motivated by these arguments would thus seem to also have clear counterexamples showing the new definitions to be insufficient for tacit knowledge (e.g., the spider's ability to weave webs).

The envisioned requirement that speakers have implicit knowledge (in Dummett's sense) forced the definition to be non-necessary. Very few competent speakers have implicit knowledge of contemporary linguistics. Idealizing the notion (requiring speakers to be such that if their capacities were idealized, then they would be able to recognize the truth of the tacitly known claim) again rendered the definition non-sufficient. Again, if a spider were *much* smarter, then it would understand the theory of web building.

What is important is that none of these cases is borderline; each is a clear counterexample to a proposed definition. On the other hand, if there really could be a speaker who communicates and uses language fairly well but is unable to engage in normative assessment of language use, this should (at best) be

considered to be a borderline case of someone semi-competent—but that is what the definition predicts.

Intended Actions

One might make a similar sort of objection about non-linguistic intentional actions. Sometimes we are quite articulate about facets of our actions, such as why we have decided on a course of action given our beliefs and desires. However, we are extremely inarticulate about other facets of our actions. For example, people do not make very many explicit knowledge claims about how they manage to navigate rooms, walk upright, and catch baseballs. But then, my account of tacit knowledge would, if correct, entail that such people do not have tacit knowledge of a theory of how to do these things.

If it is incorrect to say that a baseball player has tacit knowledge of a theory of baseball catching, then we have less reason to expect a "language of thought" explanation of human activities such as baseball catching. Therefore, the defender of the language of thought is likely to argue that my definition fails to provide necessary conditions for tacit knowledge.

Pace Fodor, if we have independent reasons to be wary of such explanations of human action, then we have empirical evidence for my analysis of tacit knowledge. We do have such reasons. Contemporary attempts to build robots that can do things such as catch baseballs are successful largely to the extent that they bypass rule-based artificial intelligence.

Robots designed in the framework of Situated Agency do not accomplish tasks by having their parts following linguaform instructions dictated by a program running in a central processing unit. Instead, as Andrew Clark (1998, p. 22) describes it,

> . . .the subsumption architecture puts multiple quasi-independent devices, each of which constitutes a self-contained pathway linking sensory input to action. As a result, the behaviors of such systems are not mediated by any integrated knowledge base depicting the current state of the overall environment.

The Fodorian might (and Chomskyan would) retort that this is merely engineering and not reflective of how humans accomplish things. Such a response is particularly ill-motivated. The two relevant facts are that (a) machines designed with the situated agency approach can accomplish simple tasks like negotiating their way through difficult environments, and (b) machines designed with rule-based architectures can not. This is surely presumptive evidence against the language of thought hypothesis as an explanation of our ability to negotiate environments. Again, the empirical facts provide evidence for the definition.

Universal Grammar

The Chomskyan linguist is likely to challenge our definition as being insufficient. Chomsky's (1995) terminology "language in extension" versus

"language in intension" at least suggests that intensionally distinct grammars could be extensionally equivalent. For the sake of argument, assume that grammar *A* does correctly model the human acquisition process in terms of setting parameters of a universal grammar while grammar *B* does not, but that both grammars generate the same set of sentences in a way consistent with the distributional judgments made by competent speakers. It is possible that, by my definition of tacit knowledge, we would have to say that competent speakers have tacit knowledge of both grammars when (if Chomsky's acquisition story is correct) speakers do not have tacit knowledge of grammar *B*.

If this criticism is valid, then I think it is clear that the definition would need to be amended. However, I do not think that the criticism is compelling. First, the general thesis being appealed to is suspect. Could there really be two non-trivially distinct grammars that generate the same set of sentences in a way consistent with the distributional judgments made by competent speakers? When we factor in the consideration that grammars are answerable not just to syntactic distributional data but to morphological, pragmatic, and semantic evidence as well, there is absolutely no reason to think that two grammars could be non-trivially distinct while generating the same data.

Of course, it is easy to come up with trivial instances of the underdetermination thesis. Add the sentence "Das nichts nichts" to one theory and the denial of that claim to the other. Slightly less trivial examples come from formulating the theories in different mathematical frameworks (e.g., the metric system in one and English measurements for the other). Physicists respond to these examples, as well as significantly less trivial ones (e.g., the wave and matrix formulations of quantum mechanics were not initially known to be derivable from each other) by saying that there is only one theory formulated in different ways.

Even if we assume (falsely, I believe) that the underdetermination thesis did work in this context (for influential critiques see Davidson, 1979 and Wilson, 1980) this objection would then only be as strong as there is independent reason to believe that syntax should be answerable to Chomsky's acquisition story in the way Chomsky dictates. Psychologists such as Palmer (1986), philosophers such as Cowie (1999), and generative linguists such as Pullum and Scholz (2002), who have critically examined the arguments for Chomsky's assumptions, have recently produced an extraordinarily compelling body of literature undermining Chomsky's philosophical claims. Likewise, Johnson and Lappin (1998) have produced an influential and devastating critique of the way Chomsky attempts to use his philosophical views to motivate Minimalist syntax.

I noted earlier that the success of situated agency approaches to robotics gives us *prima facie* reason to doubt language of thought kinds of explanations for abilities that we are relatively inarticulate about. Similarly, the fact that *every* successful application in computational linguistics has involved non-transformational approaches to generative grammar such as Head Driven Phrase Structure Grammar (Pollard & Sag, 1994) and Categorial Grammar (Carpenter, 1997; Morril, 1994) give us *prima facie* reason to doubt the psychological reality of transformational approaches such as Minimalism.

While this issue cannot be settled here except by appeal to authority (both the critiques and the current boon of computationally tractable, non-transformational approaches to grammar footnoted earlier), I would suggest that the whole concept of an I language is the result of a monumental equivocation between *how* we come to know something and *what* we know. *What* we know is a grammar of the sort that computational linguists are able to implement successfully. *How we come to know* that grammar is a question that connectionists and "third wave" cognitive scientists like Clark are attempting to answer. This is quite similar to Daniel Dennett's characterization of consciousness as "a serial process running on a parallel architecture." It also has great resonance with a major component of connectionist research, attempts such as Ron Sun's (2001) to get neural nets to implement rule-based systems.

Independent of the philosophical, linguistic, and psychological bankruptcy of current transformational syntax is the broader issue raised here. Even when linguaform theorizing is predicted to be appropriate to a domain, as my theory holds of the syntactic and compositional semantic parts of grammar, this does *not* have anything to do with whether generalized learning algorithms are utilized in the way humans come to tacitly know that that theory is true. It also has nothing to do with whether inner mechanisms have any special relation to the tacitly known theory. It has nothing to do with whether the tacitly known theory is in some sense *a priori* known. It has nothing to do with whether an *a priori* story of learning (one actually involving the *a priori* knowledge for Chomsky and Fodor) should be used to constrain the structure of the syntactic theory in question (as Chomskyans use their story to defend Minimalism and attack competing, computationally tractable frameworks).

That is, one of the pleasantly surprising aspects of my theory is just how little is being claimed when we hold that a theory is tacitly known. Again, the empirical significance of my analysis is merely that a linguaform analysis of *what* is known is appropriate in a number of cases (albeit a vastly more limited number than Chomsky and Fodor would allow). This is completely consistent with radical behaviorist views of how that knowledge was acquired and completely consistent with neo-behaviorist hypotheses of how that knowledge is embodied in the human organism. Thus, while my theory is independent of the bankruptcy of Minimalism, the fact that it is consistent with such bankruptcy does provide evidence for it over such views that are logically implicated in Minimalism.

Thus, if I am right then the whole endeavor of Universal Grammar (understood as a syntax invariant over all languages, known to pre-linguistic children, and such that specific language's syntaxes are derived from it by transformational mechanisms) rests on a category mistake, a colossal confusion between what is known and how it is learned. Again, it must be noted that I do not think that any kind of philosopher's definition by itself is strong enough to falsify a scientific theory. However, the much greater empirical scope and computational success of non-transformational approaches to syntax, plus recent developments in artificial intelligence, plus overwhelmingly persuasive recent critiques of Chomsky in psychology, linguistics, and philosophy, do show Universal Grammar to be

fundamentally misconceived. If my definition of tacit knowledge is consistent with this, then this is further support for my definition.

Externalism

Finally, one might argue that *P*'s playing a justificatory role *vis a vis* true knowledge claims that a person makes is not good evidence that that person knows that *P*. One could even cite philosophers' externalist theories of justification (e.g., Goldman, 1986) for other kinds of knowledge claims in epistemology as evidence that it is mistaken to assume that a person has tacit knowledge of the justifiers of their beliefs.

Linguists' epistemological talk is somewhat at odds with such a position. For example, linguists describe a sentence as being grammatical only if an ideally competent speaker would judge it so. This is extraordinarily similar to the Aristotelian heuristic of an action being good only if an ideally intelligent agent would judge it so. Thus, the philosopher of language who wants to "save the phenomena" and make sense of epistemological talk of linguists is likely to be drawn to virtue theory epistemology, such as that recently developed by Zagzebski (1998). However, this is a very weak argument against the externalist unless one could independently argue that an externalist epistemology cannot in some manner play the same justificatory role that the linguist's virtue–theoretic talk does. Pending detailed application of externalist frameworks, no such argument can be given.

On the other hand, if such an externalist account of knowledge about our abilities could be motivated, it might make no difference for our purposes. Remember that what is ultimately at issue is the applicability of linguaform explanations in the behavioral sciences, and there are no *prima facie* reasons to think that an externalist recasting of my analysis would undermine the analysis's substantive predictions: (1) that linguaform approaches to the behavioral sciences will fail when applied to abilities about which people are relatively inarticulate, and (2) that successful linguaform explanation need not be cognitivist in the Chomsky/Fodor strong sense. In addition, if successful, an externalist account would likely to be if anything *less* congenial to cognitivist approaches to the behavioral sciences. That is, if this paper served to motivate such a development, it will have served a useful purpose.

References

Appiah, A. (1986). *For truth in semantics*. Oxford, UK: Basil Blackwell Press.

Carpenter, B. (1997). *Type logical semantics*. Cambridge, MA: MIT Press.

Chomsky, N. (1959). A review of B.F. Skinner's verbal behavior. *Language*, *35*, 26-58. Reprinted in N. Block (Ed.), *Readings in philosophy of psychology*, Vol. 1. (1980, pp. 48-63). Cambridge, MA: Harvard University Press.

Chomsky, N. (1993). *Language and thought*. Wakefield, RI: Moyer Bell.

Chomsky, N. (1995). *The minimalist program*. Cambridge, MA: MIT Press.

Chomsky, N. (1996). Language and problems of knowledge. In A. Martinich (Ed.), *The philosophy of language* (3rd Ed., pp. 558-577). Oxford, UK: Oxford University Press.
Clark, A. (1998). *Being there*. Cambridge, MA: MIT Press.
Cowie, F. (1999). *What's within? Nativism reconsidered*. Oxford: Oxford University Press.
Cullicover, P. (1997). *Principles and parameters: An introduction to syntactic theory*. Oxford: Oxford University Press.
Davidson, D. (1979). The inscrutibility of reference. *The Southwestern Journal of Philosophy*, *10*, 7-19.
Davidson, D. (1984). *Inquiries into truth & interpretation*. Oxford, UK: Clarendon Press.
Dummett, M. (1991). *The logical basis of metaphysics*. Cambridge, MA: Harvard University Press.
Dummett, M. (1996). What do I know when I know a language? In M. Dummett (Ed.), *Seas of language* (pp. 322-342). Oxford, UK: Clarendon Press.
Elman, J., Bates, E., Johnson, M., Karmiloff-Smith, A., Parisi, D., & Plunkett, K. (1997). *Rethinking innateness: a connectionist perspective on development (neural networks and connectionist modeling)*. Cambridge, MA: Bradford Books.
Evans, G. (1985). Semantic theory and tacit knowledge. In G. Evans (Ed.), *Collected papers* (pp. 322-342). London: Routledge & Kegan Paul.
Fodor, J. (1968). The appeal to tacit knowledge in psychological explanation. *Journal of Philosophy*, *65,* 627-640.
Fodor, J. (1980). *The language of thought*. Cambridge, MA: Harvard University Press.
Goldman, A. (1986). *Epistemology and cognition*. Cambridge, MA: Harvard University Press.
Grice, P. (1989). *Studies in the ways of words*. Cambridge, MA: Harvard University Press.
Huck, G., & Goldsmith, J. (1996). *Ideology in linguistic theory: Noam Chomsky and the deep structure debates*. London: Routledge.
Johnson, D., & Lappin, S. (1998). *Local constraints vs. economy*. Cambridge: Cambridge University Press.
Keil, F. (1990). Constraints on constraints: Surveying the epigenetic landscape. *Cognitive Science*, *14*, 135-168.
Keil, F. (1994). Explanation, association, and the acquisition of word meaning. *Lingua*, *92*, 169-196.
Morril, G. (1994). *Type logical grammar, categorical logic of signs*. Dordrecht, Holland: Kluwer.
Palmer, D. C. (1986). Chomsky's nativism: A critical review. In P. N. Chase &. L. Parrott (Eds.), *Psychological aspects of language* (pp. 44-60). Springfield, IL: Thomas.
Petitot, J., Varela, F., Pachoud, B., & Roy, J. (2000). *Naturalizing phenomenology: Issues in contemporary phenomenology and cognitive science*. Stanford, CA: Stanford University Press.
Pinker, S. (1994). *The language instinct: How the mind creates grammar*. New York: Harper-Collins.
Pollard, C., & Sag, I. (1994). *Head driven phrase structure grammar*. Chicago, IL: University of Chicago Press.
Pullum, G., & Scholz, B. (2002). Empirical assessment of stimulus poverty arguments. *The Linguistic Review*, *19*, 9-50.
Quine, W. (1960). *Word and object*. Cambridge, MA: MIT Press.
Searle, J. (1992). *The rediscovery of mind*. Cambridge, MA: MIT Press.
Staddon, J. (2001). *The new behaviorism: mind, mechanism, and society*. New York: Psychology Press, Taylor and Francis Group.

Sun, R. (2001). *Duality of the mind: A bottom up approach toward cognition*. Mahwah, NJ: Lawrence Erlbaum Associates.

Skinner, B. F. (1957). *Verbal behavior*. New York: Appleton-Century-Crofts.

Wilson, M. (1980). The observational uniqueness of some theories. *Journal of Philosophy*, *77*, 208-233.

Zagzebski, L. (1998). *Virtues of the mind: An inquiry into the nature of virtue and the ethical foundations of knowledge*. Cambridge: Cambridge University Press.