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MINIMAL FOUNDATIONALISM IN LITERACY STUDIES

VOLUME I

By

Nevin Leder

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ABSTRACT

MINIMAL FOUNDATIONALISM IN LITERACY STUDIES

By

Nevin Leder

Literacy studies—the study of what it is to be literate, how literacy is acquired, and most importantly, how written texts are related to meaning—is currently heavily influenced by antifoundationalist philosophy. According to this perspective, there is nothing "more firm or stable than mere belief or unexamined practice" (Fish, 1989, p. 343). Paradoxically, this position has been taken as axiomatic among numerous literacy theorists, but, by taking this position, these scholars align themselves with classical skepticism, and, therefore, expose themselves to classical refutations of that position, in particular, Kant's argument that human perception is subjective yet informed by *a priori* intuitions that must be accepted as veridical since denying them entails logical contradiction. With these arguments Kant established a minimal foundation for both philosophy and science that can be effectively employed in literacy studies.

Most natural scientists reflexively adopt a Kantian position since their work requires a synthesis of rational analysis and empiricism, but social scientists, literary scholars, and some philosophers, particularly since Wittgenstein, have moved increasingly toward a skeptical position in which thought is equated with language and language is seen as merely "contingent" on, rather than reflective of, reality, a position that has led to extreme skepticism in literary interpretation, indeed to the view that texts are nothing more or less than the discourse community takes them to be.

However, literacy studies is also strongly associated with linguistics, which, since Chomsky, has endorsed the very nativist perspective antifoundatioalists explicitly reject. Moreover, the generative program in linguistics has sparked a "cognitive revolution," which is also strongly nativist.

Although routinely portrayed as Enlightenment dogmatists by antifoundationalists, cognitive scientists are acutely aware of the limitations of computational processes and, some, notably Fodor, have concluded that there must also be an "abductive" mental capacity that allows humans to make appropriate decisions quickly in myriad circumstances, but which cannot be modeled by known computational algorithms. Philosophers of language, particularly Davidson, have made similar observations, arguing that computational models of language cannot explain the sorts of ad hoc adjustments interlocutors constantly make in ordinary conversation; these observations are also pertinent to literary interpretation. Although the ultimate source of this free, abductive capacity remains mysterious—and thus susceptible to antifoundationalist claims—models of interpretation that include computational algorithms along the lines of Chomsky, Katz and others, and pragmatic principles along the lines of Grice, offer a much better explanation of how interpretation is possible than antifoundationalism can, and also provide rational methods for choosing among competing interpretations. Because literacy requires mastery of both computational and abductive processes, a rational approach to literacy studies offers one of the best windows on how the mind integrates these processes, and thus simultaneously provides a potential bridge between literary and scientific study.

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Many others contributed to my education, including all of my professors at MSU, and, of course, my fellow students. The Linguistics Colloquium at MSU was an almost weekly source of inspiration and intellectual nourishment, and the Writing Center fed me, body and soul, for the several years that I worked toward my degree.

My search for foundational principles began long ago when I was still a child, so it is perhaps not surprising that my earliest recollections of conscious reflection on this issue occurred in conversations with my parents, especially with my father, Hans Leder, who, as a professional anthropologist, and consummate reader of philosophy, literature and politics, has more than a passing interest in the subject. Unquestionably, many of my

methods of argumentation are directly traceable to his influence, and I will always be grateful to him for the example he set for me of what it means to think and read critically.

My first taste of formal linguistics was magnificently presented by Christopher J. Hall, whose extraordinary gift for synthesizing generative theory opened my eyes to the vast and fascinating world of linguistics when I was a Master's student in TESOL, some ten years ago in Mexico. It can truly be said that he changed the course of my life thereby. Thank you, Chris!

Last of all, though certainly not least, I want to thank my wife and best friend, Cathy Fleck, for abundant love and support, daily discussions of my work, during which many foggy ideas were clarified, often as a direct result of her ideas, unflinching encouragement, especially when I myself was most dubious—and for grilled cheese sandwiches on demand. I couldn't have done it without you.

PREFACE

The dissertation that follows began its life as a report of a study on the acquisition of science writing skills by graduate students in the sciences. As an applied linguist I was focussed on differences at the structural level, particularly changes in overall organization, since previous research (e.g., Swales, 1990; Bazerman, 1988) has indicated that science writing is uniquely structured and must be learned. I was also planning to evaluate differences in lexicon and syntax, since changes in overall "voice" are bound to be reflected at these lower levels. In short, I was prepared to do a thorough analysis of my subjects' writing under fairly orthodox linguistic assumptions. However, as I researched the literature on literacy, I became increasingly aware of the influence of antifoundationalism on scholarship in literacy. Though I had long been at least peripherally aware of antifoundationalism as expressed by the deconstructionists, I had not realized the extent to which these ideas had been taken up by literacy practitioners and theorists, but as I continued my reading, I realized that I would have to address this issue.

As I began writing the dissertation, I imagined that a few standard examples from linguistics, showing that language users rely on innate knowledge of language would be sufficient to deflect antifoundationalist claims. However, I also realized that these arguments were themselves contingent on other rationalist assumptions—assumptions I knew were explicitly rejected by antifoundationalists. Thus, it became necessary to pursue the discussion into epistemological realms. By this time, I had written the first two chapters of the dissertation and was deeply engrossed in a study of Richard Rorty's

Philosophy and the Mirror of Nature (1979), in which he argues for a rejection of the entire philosophical tradition in favor of "pragmatism." Because of their vast scope, I realized that a full treatment of Rorty's arguments would entail a dissertation length discussion, one that would not fit in my proposed dissertation on science writing. But, by this time, I had also decided that straight-forward linguistic analysis would be naïve in the current antifoundationalist climate. I therefore abandoned the original project in favor of a full-dress treatment of antifoundationalism, including a defense of what I have called "minimal foundationalism." For me, the exercise has been doubly rewarding in that it has given me a firm grounding in antifoundationalism (irony noted), while simultaneously provoking me to integrate various philosophical and linguistic assumptions into a comprehensive, rational, and eminently defensible position on literacy. In my view, everyone working in literacy is obligated to seriously investigate antifoundationalist claims, since such claims have far reaching implications for everything we do. I hope that the discussion that follows will prove useful to some who take up that investigation.

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I: THE LOCUS OF MEANING

One of the few facts about language about which nearly everyone agrees is that language itself is indeterminate. That is, there is broad agreement among linguists, literacy theorists, and literary critics that linguistic tokens—words, sentences, discourses—cannot be accurately interpreted without reference to particular contexts, and particular discourse communities. Common examples of this fact are non-standard uses of words like "bad" for "good," as in sentences like "That sax player was *bad,"* wherein, at least in many discourse communities, the combination of context, and stress tell the listener that, in fact, contrary to the denotative value of the word "bad," in the speaker's opinion the saxophone player was extremely proficient[1]. Probably the most famous example of this sort of thing in literary circles is Stanley Fish's "is there a text in this class," in his essay *Is There a Text in This Class?* (Fish, 1980). In that essay, Fish argues persuasively (though I will argue, not conclusively) that at the moment it was uttered, the question meant both "is there a textbook in this class," and something like, "does the instructor of this class believe in stable texts?" Such examples are easy to find across all areas of language use; collectively, they suggest the rather unsettling conclusion that texts, oral and written, have no inherent meaning. As we shall see later in this chapter, there are still some staunch proponents of the opposite view, i.e., the view that words (if not larger linguistic units) are in fact connected to external reality in

---

[1] In *Philosophical Investigations* Wittgenstein asked rhetorically whether one could "*Say* 'it's cold here' and mean 'it's warm here,'" apparently as an argument for the radical indeterminacy of language that he argued for. Such cases, which are like the "bad" meaning "good" case above, are indeed good evidence for indeterminacy of language itself, at least if "language" is taken to be synonymous with "words spoken."

unambiguous ways; however, even if that position can be defended, it is nonetheless true that the *prima facia* case is strong for language as an indeterminate, context dependent system.

This reliance on context and discourse community makes writing particularly problematic because writing is usually read in the absence of its author(s) and often, if not usually, disconnected from the particular context it refers to, the discourse community it was written in, and the community it was intended for. These peculiar qualities of writing were observed long ago by Plato when writing was still a novelty in educated society (Gee, 1996). Plato complained first that writing was suspect because it could not be interrogated in the dialectical fashion made famous by Socrates. Thus, with writing, it was (is) not possible to clarify the message, because "if you ever ask [written words] about any of the things they say...they just point to the same thing each time" (Plato, 1986; as cited by Gee, 1996, p. 27. Moreover, because writing can be "trundled about," read out of context, and by people who may not be qualified to understand it, it is constantly subject to misinterpretation, and may be "abused" (ibid)[2]. Crucial in Plato's critique is the notion that language is at best a sign system that points to ideas, not a receptacle of ideas *in toto*. Moreover, his comments suggest that he believes ideas are generated independently from language, ostensibly in a richer form, and then translated into language—perhaps as the impoverished "shadows" of ideal semantic forms: . . . "that even the best of writings are but a reminiscence of what we know. . . " (ibid). This thinking, as resonant with modern generative linguistics, as it is foreign to linguistic

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The important question is whether or not there is more to language than this, and if so, whether the "more" is reliably interpretable. (The Wittgenstein example is taken from Hancher, 1972)

relativism and anti-logocentric epistemology (e.g. Derrida, 1976), stakes out a position on writing that is still viable today. Interestingly, despite his misgivings about writing, Plato remains committed to language as the vehicle for conveying ideas and, furthermore, for analysis, but *only in conjunction with perspicacious interpretive practices,* in particular, dialectical interrogation. In these arguments, again, Plato shows a very modern understanding of the interpretive dilemmas that still dominate discussion about writing today, though, no doubt, some might fault him for indulging in "intentional fallacy," since he clearly locates meaning more in the authors of texts and than the texts themselves. Of course, Plato never imagined the modern world, with its millions of books and extensive systems for searching and cross-referencing texts, so it may be that he would soften his stance somewhat in light of the modern context, since, arguably, modern libraries and, perhaps more importantly, modern academic institutions and practices, do make possible, at least to some degree, the sort of interrogation he calls for. But whether or not Plato would have approved, writing is the dominant form for expressing complex ideas in the modern world, and therefore, reliable interpretive practices for it are vital.

The fact that readers do not reliably interpret texts the same way is first and foremost a problem of where meaning resides. Logically, there are four distinct possibilities: 1) in the author 2) in the text itself , 3) in the individual reader, 4) in the discourse community. A fifth possibility would invoke some combination of some or all of these sources. For convenience, I will refer to these perspectives as the 1) author centered view, 2) text centered view, 3) reader centered view, 4) discourse community

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[2] Geisler cites Havelock, who has a very different interpretation of Plato.

centered view, and 5) the hybrid view(s) (which must be individually defined). While there are other problems involved in interpretation, for example, what meaning is, and other issues not concerned with meaning per se, for example, experiential qualities of text, literacy theories remain centered around meaning, and may be differentiated according to the stance they take on these four fundamental possibilities for where meaning resides.

In organizing theoretical approaches to literacy this way, I am departing somewhat from established organizing schemes in the literature, including process/product models (Emig, 1971; Hayes and Flower, 1980, 1983, 1987), cognitive vs. social factors models (e.g. Johns, 2000; Bizzel, 1982; Robinson, 1990), and rhetorical models (Aristotle, 1991; White, 1985; Fish, *in* Bernard-Donaldson and Glazer, 1998) In doing so I do not want to imply in the least that any of the distinctions highlighted in these models are invalid; to the contrary, I believe a comprehensive model of literacy must recognize that literacy has cognitive, social, and rhetorical dimensions, and there is general agreement in the field on this point; moreover, there are real, i.e., natural class, distinctions among these aspects of literacy that justify opposing them in theoretical discussions. The problem, then, is not what to include in a comprehensive model, nor what aspects to contrast, but, rather, to discover the principles and mechanisms that hold these aspects of literacy together, and which, together, make literate practices possible. Currently, the field resembles a smorgasbord from which one picks and chooses those aspects of literacy one finds most compelling. Those with a linguistic/cognitive bent dine on complex psycholinguistic fare, those persuaded by social constructionist arguments, "construct" their plates accordingly, and the rhetorically minded eat word stew. As a

result, much of the debate in the literature lacks traction; instead of compelling arguments for one model over another, or at least arguments that might show why one aspect of literacy should be subordinated to others, what is often served is an elaborate presentation of a particular approach, with little or no justification for why this approach is to be preferred over others. A comprehensive approach must do more. We need to know how cognitive, social, and rhetorical aspects of literacy work together. If, as seems to be the general consensus, these are separate though cooperating domains, there must be principles of correspondence among them, and these should be discovered and elaborated. Moreover, complex cognitive systems generally employ hierarchical structures, and hierarchy entails subordination of some things below others. Literacy is certainly complex, so we should expect such structure. Finally, inasmuch as literacy is a linguistic activity—and there is plenty of evidence to support at least a large if not dominant contribution from linguistic modules—we should expect that there will be separate knowledge and processing domains in literacy. What this means is that there is likely to be an abstract rule system that constrains literate configurations (the knowledge domain), *and* a somewhat separate, though cooperating set of rules for processing and producing written texts (the processing domain)[3]. If any of this is right, the initial move toward discovery is to choose some property of literacy that seems ubiquitous to focus on, since, by doing so, we can hope to find at least one common, and hopefully dominant principle in the system. Meaning seems to me to be by far the best candidate for this

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[3] The division between *knowledge* and *processing* has become fairly standard in generative linguistics, and is overwhelmingly supported by both theoretical models and empirical studies, especially in psycholinguistics, which show that there are good reasons to believe that people routinely use shortcut strategies in processing rather than invoking their entire knowledge of language (Macdonnald Perlmutter and Seidenberg)

quest since, arguably, expression of meaning is the *raison d'être* for all aspects of literacy. Moreover, readers and writers are overwhelmingly oriented toward meaning in their literate practices, and this cannot be ignored. Even if it can be shown, for example, that all literate practices are influenced by, or even controlled by, social factors (as many argue today), these factors are nonetheless auxiliary to the production and recovery of meaning, and therefore, subordinate by definition. This focus on meaning is only possible, however, from within a developed linguistic framework. This is because, whatever meaning is, it is clearly manifested in language through discrete linguistic constituents—phonemes, morphemes, words, phrases, clauses, discourses—and it is only because we can reliably build these constituents into progressively larger semantic units that we can turn our attention to the problem of where meaning resides. Oddly, though everyone in literacy studies seems to accept the existence of lower level processes operating according to regular rules of some kind, this remarkable fact rarely enters into debates on meaning in the literature on literacy. There seems to be an unspoken assumption, perhaps leaking through from literary theory, that the important problems are all at much higher levels and that lower level processes can be assumed and ignored. I think this is mistaken, as will become clear as I develop my model. At this point, however, I want to return to the question of where meaning resides and how that question has been handled historically.

In the following paragraphs, I'll sketch some of the particular instantiations of these views, in the historical order that they have appeared in the schools and professional journals. My survey ignores many hybrid views, since my purpose is to

show where extreme positions on the locus of meaning lead, as part of a theoretical discussion, rather than to comprehensively survey the development and practice of literacy theory. In light of this focus, I have chosen to review only perspectives that clearly illustrate these differences. It will be clear from this brief survey that the general trend has been from greater to ever decreasing certainty, both about the meanings of texts, as well as what interpretive practices and methods should be used.

There is often a considerable gap between theorists and literacy practitioners, with respect to when they adopt and reject these positions; in general, the most theoretically oriented people are always the avant-garde, with literary scholars leading, literacy theorists, college composition teachers and writing specialists following on their heels, and public school teachers often remaining decades behind the leading theorists, and perhaps never adopting some of the more avant-garde theories. This gap is significant for two reasons. First, from a psycholinguistic perspective, it could be argued that the less theoretically oriented one is, the more likely one is to manifest natural linguistic processes; thus, it may be that teachers' intuitions about texts can tell us more about how linguistic processes are related to literacy than theorists can. Second, the gap has obvious and profound pedagogical implications, particularly for advanced literacy practices. Because students are influenced more by teachers than by theorists, they are likely to adopt the attitudes and literary practices of teachers; however, if, as many theorists claim, these attitudes and practices are misleading, then these students can be expected to have great difficulty with advanced literacy. As we will see, several theorists make precisely this claim. However, the value of this criticism is dependent on larger issues,

particularly, our leading question about meaning, so I will withhold comment on these issues until we have developed an answer to that question.

Throughout this discussion I will focus on reading, assuming for the moment, without argument, that whatever holds true of reading, will hold, more or less, in reverse for writing. Difficulties may ensue if this formulae is pushed, but in general I think it holds.

Four Cardinal Perspectives on Meaning in Written Texts

Author Centered Perspectives

This subsection is short because, to my knowledge, no one has ever argued for an exclusively author centered perspective on meaning of written texts. Were such a thing possible, we could dispense with text altogether and rely on telepathy. The point seems silly, because obvious, but it highlights the fact that linguistic meaning is always mediated through text, and that, therefore, interpretation is a necessary part of reading. Though intuitively obvious, philosophically, linguistically, and cognitively, this is not a trivial point. Some, notably Wittgenstein and his followers, have tried to equate thinking with language, or more radically to "language games." If this position can be maintained a likely conclusion is that we are trapped in a sort of "prison house of language" (Jameson, 1972) with no hope of release. Many implications arise from this, including radical linguistic relativism, and general skepticism. On the other hand, if language is just a medium for encoding thought, which itself is prior to natural language[4], these bleak conclusions can be avoided. For literacy, a Wittgenstinian position leads away from

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[4] I use the term "natural language" here to distinguish human languages (English, Japanese, etc.), from internal mental codes, which may also be considered to be languages. Fodor in particular has long maintained that there is a "language of thought" (Fodor, 1975).

authorial intent, since, arguably, the author could not have non-linguistic intentions. But such a move does not lead to clarity about texts. To the contrary, equating thought and language, greatly increases indeterminacy, since language itself is ambiguous. Thus, we begin to see why literacy theories entail broad cognitive/epistemological assumptions.

While there are no purely author centered views, the author/text centered perspective is a strong possibility which I discuss at the end of this section.

Text Centered Perspectives

There are two very different sources for an exclusively text centered view of meaning, one from philosophy and linguistics, and one from literacy and literary theory. The second category, literacy and literary theory can be further subdivided, into two somewhat different sources, each occupied with related but different areas of inquiry, and each with its own distinct literature and traditions. Literacy theory is quite new, drawing its theoretical notions from psychology and psycholinguistics while also incorporating significant contributions from pedagogy, and rhetoric, both of which have strong ties to literature. The study of literature, on the other hand, is ancient, traceable at least to Aristotle, and therefore reflects a complex and varied set of traditions and competing theories. While there are clear theoretical overlaps across all three camps, influence moves almost exclusively in one direction; literacy and literary theorists, especially the latter, occasionally borrow ideas from philosophers of language or from linguists, but it rarely or never happens the other way around. And, likewise, literacy theorists are strongly influenced by literary theorists, but the reverse situation seems rare or non-existent. This perhaps reflects the increasingly abstract nature of these fields' focus as

one moves from literacy, to literature, and finally to linguistics and philosophy. Ultimately, all fields require a basis in abstractions, and are, therefore, related somehow, somewhere to philosophy; therefore, practically oriented fields, like literacy theory, always look to their more philosophical brethren for theoretical grounding.

The philosophical/linguistic source of exclusively text centered meaning[5] originated with Gottlob Frege (1848-1925), and has continued to our day through a chain of philosophers and linguists including Bertrand Russell (1872-1970) Ludwig Wittgenstein (1889- 1951), Andrew Carnap (1891-1970), Willard Quine (1908-2000), and currently Donald Davidson (b. 1917)[6].

Frege, a logician and mathematician, sought to reduce both mathematics and language to logic. For language, a key to this quest was to argue for a stable sign system or a "shared public language." Frege argued that there was a "treasury of thoughts which is transmitted from generation to generation" (Chomsky, 1993). Chomsky summarizes the essence of Frege's system as follows:

> A [linguistic] sign has two aspects. First, it designates an object in the world, its referent; in a logically "perfect language" that will be true for every "well constructed" expression. Second a sign has a "sense" that fixes the reference and is "grasped by everybody" who knows the language; to understand an expression is to know its sense in the shared public language. In addition, each person may have an individual mental image connected with the objective sense. Sign, sense, and referent are external entities outside the mind/brain (ibid).

There are many well known problems with this theory, chiefly, that it is clear that the meaning ("sense") people attach to words varies radically according to context and thus

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[5] Frege didn't distinguish reading from speech, but his system clearly incorporates written language, and some (e.g., Culler, 1987) might argue that his system was really far more applicable to written language than spoken because of its reliance on stable signs.

cannot be contained in the words themselves, a point I will take up in detail in a later section of this chapter. But even if Frege's strong claims regarding a shared treasury of meaning can be abandoned, as they have been by virtually all linguists (Abbot, 2002), some of Frege's observations about language remain central to current theories in semantics and the philosophy of language and are also related to certain claims in literacy theory, though rarely invoked directly. In particular, Frege noticed that *sense* and *reference* can be separated in certain situations, an intolerable problem for a "logically perfect" language. Two ways this can happen, are 1) when a language user believes that an object in the world is really two objects, and 2) when a s/he believes that two objects are really one. Frege's famous example for 1) is the case in which a person believes that the Evening star and the Morning star, which of course are just two names for Venus, based on two perspectives (morning vs. evening), are actually two separate objects. The problem is that the person's sense about Venus disagrees with his/her referential use, i.e., s/he believes that the object s/he contemplates in the sky is really *two* objects, even though, arguably, regardless of what s/he believes, s/he still points to and *refers* to the *one* object, Venus. This divergence between sense and reference is strikingly apparent when one considers plausible versus very improbable sentences about Venus:

1) Wow! the Evening Star and the Morning Star are both Venus!
2) Venus is Venus!

Sentence 1) would be meaningful for a person who had just learned that the Evening Star and the Morning Star were in fact both Venus, but sentence 2) is a tautology that no one would say (except a linguist by way of example). This is problematic for Frege, because

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[6] I don't mean to imply that all of these philosophers hold the same views. To the contrary there are major differences among them; however, they all tend to have a language centric view of meaning.

for him, words that share a single reference should also share a single sense. Thus, since both the Evening Star and the Morning Star refer to Venus, all three terms should be synonymous and interchangeable. However, the examples above show they are not.[7]

The second class of examples covers the opposite case, i.e., when a person attaches one sense to two different objects. An example is the case of a person who can't tell the difference between elm trees and beech trees, and therefore calls both of them "elms." Since, under Fregean semantics, objects in the world are connected to their linguistic referents, this shouldn't happen. In Jerry Fodor's terms (1994), such cases show that more than one "implementing mechanism" (i.e., an elm or a beech tree) can be connected to the same word.[8] Fodor attempts to resolve these cases by arguing that while they are problematic, they are also rare. Actually, however, they are common, perhaps even the rule rather than the exception. Consider, for example, people's sense about such commonplace events as "sunrises" and "sunsets."' Educated people know that, in fact, there are no such things—since actually the pretty colors are caused by the earth turning

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[7] These examples have occurred frequently in one form or another in the semantics and philosophy of language literature, so it is difficult for me to pinpoint the original sources that have informed me. However, Fodor (1994), is certainly one source, and Barbara Abbot (various colloquia and personal discussions at Michigan State University) is another. Any errors in my presentation of these famous semantic puzzles, however, are mine alone.

[8] Though the ideas and even some of the phrases are Fodor's, I have spliced them together from different places in his book (1996), and am actually making a somewhat different point than Fodor does. Fodor's first example of "Twin cases" (i.e., cases where one word is used erroneously for two distinct referents) is of two "parallel worlds," one our own, and the other one in which everything is identical except water, which in the other world is composed of X,Y,Z instead of H2O. Fodor says that this case, which he borrowed from Putnam, shows that "[t]win cases show that more than one *intentional* [sic] *state* can correspond to the *same implementing mechanism*" (my italics). This conclusion seems unwarranted to me since I don't see why our parallel world brethren couldn't have exactly the *same sense* about X,Y,Z as we have about H2O, i.e., that it is water—hence the *same* intensional state as ours. The point *I* am making above is simply that sense and reference can become "unstuck" (Fodor's word), in this case as well as in the Frege case. In fact, the example I use (elms and beeches) is also Fodor's (from a later chapter in his book), but he draws a different conclusion, arguing that elm/beech type cases can be resolved by "deferring to experts' knowledge." While this is true, at least in some cases, I don't think it solves anything. .

toward or away from the sun—yet, because the illusion of a "setting" or "rising" sun is so compelling, we still refer to these events this way. And worse, *uneducated* people, past and present have always done so also. So here we have a case where labels do not match their referents at all, for anyone, and where in the case of the uneducated, sense is also clearly divergent from reference. In the case of the educated, the problem is very unclear; one can claim that, for them, "sunrise and "sunset" are metaphorical terms, but it's hard to know whether they are *represented* that way at the moment of reference. From a mentalist standpoint what matters is the mental state of the speaker at the time of utterance, not what, s/he might know about the world, and it seems plausible that at least some speakers are momentarily overcome by the illusion to the extent that what they internally represent at the time of utterance *is* a setting or rising sun. In any case, the world is full of such cases, especially the technical world[9], so Fodor's "rarity" defense seems lacking. A tempting way out of this entire problem is to jettison the concept of Fregean "reference" and simply adopt a thoroughgoing mentalist framework wherein words have whatever sense (meaning) speakers give them and reference is only connected to the real world insofar as the speaker's representation overlaps the real object. This seems to be Chomsky's position (1993). However, the problem with that position is how to account for the fact that people *do* reliably use the same words to refer to the same things, and worse, how to account for the veridical nature of semantic

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[9] I'm thinking of the many cases where modern people refer to complex machinery without knowing anything about it. No doubt, most people harbor completely erroneous representations of all sorts of things, from computers to cameras, yet they refer to these things all the time.

relations, such as entailment (Dowling, 1999; Katz, 1990; 1998). I'll take these problem up again in detail somewhat later.

The second framework for text centered interpretation comes from literacy theory and is traceable to changes in the use of writing in government and law beginning around the 12th century when written documents began to replace oral testimony (Olson, 1991). According to Olson, these cultural changes are the "link between literacy and modernity—the [origins of a]systematic distinction between something that is taken as given, fixed, autonomous, and objective and something that may be construed as interpretive, inferential, and subjective" (ibid). Olson argues that this increased use of texts in government and law combined symbiotically with advances in science, thereby engendering a radically new perspective on the world.

Tracing the history of this change, Olson notes that by the 16th century, with the rise of new print technologies, many texts that had previously been unavailable to all but the elite were suddenly open to nearly everyone who could read. This created a pressing need, particularly regarding central cultural texts like the Bible, for stable interpretations. But interpretation allows differences, and different interpretations of texts like the Bible were intolerable for leaders of Reformation theology, such as Luther, who were actively fighting against the Church's claim to privileged interpretations:

> The interpretive principle of the Reformation, as expressed for example in Luther's attitude to Scripture, was that Scripture was 'autonomous,' it does not need interpretation, it needs reading; it means what it says. All the rest is made up, a product of fancy or tradition. It was this distinction between the given and the interpreted that launched the Reformation and, a century later, opened 'the book of nature' to modern scientists, to make it readable to anyone 'with a faithful eye' as Robert Hooke (1665/1991), one of the first of the seventeenth-century British empiricists said. (Olson, 1991)

These comments by Luther and Hooke, and many others in the same vein clearly support the reality of a doctrine of "autonomous" texts, reaching back at least to the renaissance; however, it is important to separate people's self-reports about language from their actual practices, a point Olson seems at least peripherally aware of[10]. Though he is known as an advocate of the view that written texts are comparatively autonomous, he also clearly points out that, despite this apparent autonomisity, writing actually invites readers to extrapolate to an author's intentions :

> A written text preserves only part of the language, the form, and the meaning has to be regenerated from that form by the reader. The preserved part we will talk loosely of as being 'given,' 'fixed,' permanent; the reconstructed part we can call, roughly, the meaning, the intention, the interpretation.
>
> That distinction is implicit in speech, but the relation between a text and an interpretation becomes especially problematic in literacy. In oral language the form and meaning form an indissoluble pairing. When we do not understand an utterance, we normally ask, 'What do you mean?' rather than 'What does it mean?' focussing on the person doing the communicating not on the sentence. (ibid)

These comments, reminiscent of Plato's, highlight the interpretive dilemma created by written text, but they also problematize the notion of "autonomous" text. If, as Olson's observations indicate, natural verbal communication employs inferences from (oral) text to speaker, and interrogation, or negotiation strategies, for clarification, it would be reasonable to suppose that people would transfer these natural strategies to written text, at least as far as possible. The fact that people developed a doctrine of autonomous texts, is revealing of folk beliefs about language during the renaissance and after, but, crucially, it

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[10] In the quoted text, above, Olson seems only "peripherally" aware of the gap between folk reports and reality regarding autonomy of texts. In his later writings (e.g., 1994, 2001), however, Olson explicitly refutes the doctrine of autonomous texts, arguing that such a notion is clearly naïve. Probably this shift in his thinking was a result of sustained criticism from other theorists about his early claims for autonomous texts.

does not indicate that these autonomous text strategies were ever actually used, at least not as presented, say, by Luther. People are entirely unaware of the complex linguistic machinery that underlies "simple" speech acts, and no doubt are similarly unaware of the many processing and interpretive strategies actually employed in reading and writing. Nonetheless, it is true that the ideal of the autonomous text has been a major force in literacy theory, and one that still persists to this day.

Olson's observations about the differences between oral and written texts have been disputed by numerous people, though usually what is disputed is the putative effects of autonomous text, not the central claim that autonomous texts exist. Olson regards the development of autonomous texts as a major advance in Western thought, and it is this stance that usually draws fire. These arguments take two lines. Some scholars dispute the claim that autonomous texts confer unique cognitive benefits, arguing that oral societies demonstrate equal cognitive abilities (e.g., Narasimhan, 1991; Feldman, 1991; Denny, 1991); other scholars argue that autonomous texts are actually harmful to thought. Cheryl Geisler, for example, organized her seminal work *Academic Literacy and the Nature of Expertise* (1994) around—or more precisely, against—what she calls the "ideal of the autonomous text" in the academy. Geisler argues that this ideal, more than anything else, has created dysfunctional institutions in academe and calls for "changing the nature of reading and writing practices in the academy" in ways that would overcome this ideal.

Literary theory has also contributed a strong text centered view of meaning, though this is fairly recent. Since antiquity, with the exception of scriptural and legal interpretation, under autonomous text assumptions as outlined above, the default position

on interpretation has generally been author/text. Plato's comments strongly suggest this, as do Aristotle's teachings on poetics and rhetoric. In the *Art of Poetry*, for example Aristotle never explicitly specifies how texts are to be read, but he repeatedly admonishes authors to "represent human beings in action" in ways that will distinguish and emphasize human character traits, and he stresses that it is faithful mimetic representation that gives men pleasure (1951). These admonitions leave no doubt that Aristotle meant for readers to reconstruct the poets' artful intentions. In English literature, Pope's dictum "In every work regard the writer's End, / Since none can compass more than they intend" (quoted in Wimsatt and Beardsly, 1943, by Hancher, 1972) renders these assumptions explicit, and that these assumptions were widespread is evident by examining literary criticism until the modern period. The one glaring exception to this "common sense" author/text approach was in regard to how the Bible should be read. The Medieval Church held that scripture could only be interpreted by accessing its "spiritual aspects" (Olson, 1994). In practice this often meant relegating interpretation to authorities in the Church, who were assumed to have such access, and who, in any case were often the only literate individuals in a community and also the only ones with *physical* access to texts, including scripture. However, during this long period, there was still plenty of disagreement about scriptural interpretation, and there were concerted attempts, notably those of Augustine (354-430) and Aquinas (1225-74) to constrain interpretation through detailed hermeneutical[11] theories (Olson, 1994). Crucially, both Augustine and Aquinas concluded that scripture has a literal meaning that can be read autonomously. However, they also insisted that it had spiritual dimensions that went far

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[11] I will later examine a postmodern "hermeneutics." The two words should not be confused.

beyond these literal meanings, though these spiritual meanings were anchored to the literal meanings in specific ways (ibid). These sophisticated arguments show that people were acutely aware of the complex relationship between text and meaning long before the modern period. Moreover, the odd historical progression of thought regarding Biblical exegesis suggests some interesting relationships between philosophy, science, and literary criticism. During the medieval period to the renaissance, when the Church dominated philosophy (at least in Europe) and science was stagnant,[12] we see a profusion of competing Biblical interpretations and a tendency to admit extra-textual influences of all kinds, including private "inspirations" of readers, and/or privileged methods, such as those of the gnostics, which crucially depended on secret, extra-textual "knowledge." But with the reformation and the rise of science, interpretation becomes text centered and criticism is required to have a basis in either historical or textual facts or both. This empirical orientation toward interpretation and a concomitant assumption that the job of criticism is to discover determinate meaning remained strong enough through the early 1940s to be deemed "axiomatic" by many critics (Dowling, 1999). However, in the 1930s, a movement against authorial intention began, led by none other than Ezra Pound and later T.S. Eliot (Hirsch, 1967). Not coincidentally, this is also a period when philosophy became increasingly skeptical, under the influence of Heidegger, Wittgenstein, and the existentialists.[13] Freud's theories of the unconscious also began to exert great influence in this era, raising many new questions about the relationship

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[12] This assessment of the state of science in the medieval period has been historically verified, but contemporaries were also aware of it. Bacon describes the state of science from the Greek period to his own day as "stand[ing] almost at a stay, without receiving any augmentation worthy of the human race" (1605/1939)

between language, thought, and meaning. Simultaneous with these developments in philosophy and psychology, science became increasingly mysterious for the average person, as familiar and accessible notions of the cosmos gave way to arcane new ideas, like Einstein's theory of relativity. Finally, social catastrophes such as the World Wars and the Great Depression, led many to question the value of science and empiricism and to look for alternatives in the "humanities." In short, in the latter half of the 20th century literary criticism increasingly looked away from empiricism for support, and, not surprisingly, soon became far less deterministic than it had been for centuries. Postmodernism is the culmination of this trend. Though literary criticism's estrangement from science in our time has come about for different reasons than those that kept it separate from science in the medieval period, the results are much the same. As Olson remarks, "reading in Charlamagne's day is somewhat similar to reading the post-modern or reader-response way—what a text means is what a reader takes it to mean" (1994, p. 145). However, we have gotten ahead of ourselves. Reader response theory is the logical culmination of a trend away from science, but is not the only stop on the way. First texts must be separated from authors, and this is the movement we are following at the moment.

According to Hirsch, the central "proposition" of the text-centered movement was "that textual meaning is independent of the author's control [and this proposition] was associated with the literary doctrine that the best poetry is impersonal, objective, and autonomous; that it leads an afterlife of its own totally cut off from it's author" (1976 b., p.87). There are two points to notice here. First, the modern movement for exclusively

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[13] Some critics would classify Heidegger as an existentialist also, but "Heidegger vehemently rejected this

text centered reading originally focussed on poetry, which is arguably an inherently ambiguous genre that calls for more interpretation than most prose; since poetry is generally a non-scientific enterprise, taking a non-scientific approach to interpretation of poetry has at least some initial plausibility to it. Second, though we once again have an appeal to "autonomy," this is an entirely different sort of autonomy than that envisioned by Luther et al. In their case, texts were autonomous *because* they accurately and completely contained the their author(s)' message, in the Bible's case God's. But though the same term "autonomy" is applied to Pound and Eliot's notions of texts, in fact these critics make an opposite argument. For them, texts are autonomous, not because they faithfully reflect authorial intent, but just because they are texts—meaning bearers *regardless* of how anyone may have intended them. Here we clearly see the influence of Wittgenstein, who held that thought and language were one and the same, and that thinking was just a matter of manipulating language. If that's the case, then texts are autonomous, but also and always multiply ambiguous because one can always find new relations among words in a text, and between texts and the world. This is the sort of autonomisity and ambiguity pointed to by Pound and Eliot. Eliot, in particular, was fascinated with the inherent richness of texts, and also by the way texts themselves seem to grow in the mind of authors. In fact, Eliot questioned the entire concept of intentionality, remarking rhetorically to one interviewer:

> I wonder what an 'intention' means!" One wants to get something off one's chest. One doesn't know quite what it is that one wants to get off his chest until one's got it off! But I couldn't apply the word "intention" positively to any of my poems. Or to any *poem* (*Paris Review*, No 21 (1959); cited in Hancher 1972).

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title" (Harris and Levy, 1975).

However, while on the one hand Eliot maintained this line, he also supplied ample critical commentary designed to elucidate the particular textual relations that he himself had in mind when certain of his poems were written. To this end, Eliot wrote extensive and specific commentary on his own works, correlating his poetry, line by line, with numerous works of other authors that influenced him or whose works he was alluding to (e.g., 1930). It's not easy to reconcile this author centered critique with the modern notion of textual autonomy, which is continually linked to him, nor with his denials of intentionality, but there are a few possibilities. On the one hand, Eliot may have believed that texts were ambiguous because different readers would always discover different associations among them—certainly an irrefutable position. Yet, he still apparently held that there was some value in knowing what the author' associations were, and he clearly believed these were recoverable, at least for the author. Moreover, while in some cases the associations he cites are quite idiosyncratic—his association of the Man With The Three Staves with the Fisher King, for example—which Eliot himself says is "quite arbitrary" (1930, p. 69), many if not most of the "associations" he claims, are certainly recoverable by readers who are familiar with certain literature, like the Bible, and classical literature. On this account, then, texts are ambiguous and autonomous only to the degree that readers' "associations" differ from the authors'. Moreover, an author/text centered reading is at least theoretically possible, though perhaps not specially privileged. Another way to reconcile the problem is suggested by Hirsch, who points out that Eliot may have been thinking more about the *significance* of texts in a community rather than their meaning (Hirsch, 1976). As new texts enter a community of readers they exert all sorts of unpredictable influences, particularly over people's interpretations of other texts.

For Eliot, in particular, it was evident that texts were always read against a backdrop of other texts and that this changing backdrop continually alters literary traditions. Thus, all texts' literary and cultural *significance* changes as new texts arise. As Hirsch points out, this observation is "perfectly sound." But significance and meaning are two very different things. Probably no one argues with the premise that a text's significance—its importance to individuals or communities, its historical relationship to other texts and world events, etc.—is highly variable and always dependent on readers and unfolding events, including the publication of still other texts. It seems to me that Eliot and Pound may have started with the first idea—that of a text's significance—and then allowed the idea to be expanded to meaning itself. At any rate, that was the effect in the critical community as the new doctrine of textual autonomy was developed. This, combined with other confusions over similarly subtle points, led to the radically text-centered position known as "New Criticism."

Intentional Fallacy

Wimsatt and Beardsly's seminal article, *The Intentional Fallacy* (1946/1976) was the first widely publicized statement on the position that texts should be read without inferring the authors' intentions. However, the article is far more mild than the position eventually associated with the phrase "intentional fallacy." Extreme New Critics reached the point of completely denying any possibility of recovering authorial intentions, either because these were alleged to be permanently out of the reader's reach, or, in the most radical versions, because the author her/himself did not have a clear intention, had multiple intentions, or perhaps had so many subconscious—and ergo undiscoverable—intentions as to render the term meaningless. These positions echo Eliot's comments. But

Wimsatt and Beardsly were far less ambitious than their followers. In fact, they do not deny authorial intention at all, arguing instead that it simply isn't relevant. But in making this argument, they interjected considerable confusion into the discussion by conflating several senses of "intention." Michael Hancher, points this out, arguing persuasively that

> the concept of 'the author's intention. . . requires analysis into three separate elements: (1) the author's intention to make something or other; (2) the author's intention to be (understood as) acting in some way or other; (3) the author's intention to cause something or other to happen. I further propose that a great deal of the heat of the intentionalist argument has been generated by a fusion or confusion of (2) with (1)—and to a somewhat lesser extent by a confusion of (2) with (3). (1972)

Under Hancher's framework, (1), which he later calls *programmatic intention*, refers merely to the author's intention to write a certain kind of work—a sonnet, epic poem, short story, what have you; category (2), dubbed *active* intention is the "intention that the *thing* one has made *mean* (and be taken to mean) something or other; while category (3) is the "the intention to *cause* an effect of one sort or another; it defines whatever the author wishes to *accomplish by means of* his completed work" (ibid; italics in original).

There is a strong, though not perfect, overlap between this scheme and the speech act theory of Austin (1965), which divides utterances into three distinct intentional/*performative* "acts": 1) the *locutionary act,* by which denotative meaning is produced through words, phrases and sentences according to the rules of a given language; 2)the *illocutionary act*, by which a particular communicative intention is transmitted; and 3) the *perlocutionary act*, which is the intended effect of the utterance on the interlocutor(s) (Schiffrin, 1994). A standard example which illustrates these three aspects of language nicely is the utterance "is the window open?" which, at the denotative level (locutionary), unambiguously asks a question about the status of a

window, but which also may in many circumstances communicate a request for the interlocutor to close the window (the illocutionary act), and, if successful, also "performs" the act (perlocutionary) of getting the speaker to comply with the request. As Hancher himself points out, his second and third categories correspond fairly closely to Austin's second and third "acts," though there are literary aspects of Hancher's scheme that are usually either entirely absent from oral texts or less important. In particular, generic force looms larger in literary productions. Hancher argues, for example, that some genres such as epics and ballads have a fairly specific conventional "meaning" or "force" associated with them, such that the mere production of either signals a whole constellation of literary intentions (1972). Not all written texts carry such strong generic associations, but most do carry at least some conventional generic associations, and written texts are almost always classifiable by genre.[14] Because these generic associations are well understood, Hancher argues, there is a level of intentionality exclusive to literature—the generic, or *programmatic* level as Hancher calls it. To preserve Austin's theory, Hancher could have simply added this programmatic level to Austin's scheme; logically it would fit between the locutionary and illocutionary levels, but, instead, Hancher simply replaces the "locutionary" level with his programmatic level. He then argues that Wimasatt and Beardsly's original argument against invoking authorial intention, i.e., against "intentional fallacy" referred primarily

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[14] This is a dicey argument though, because oral texts are also subtly marked in ways that invoke complex associations. At a broad level, linguists invoke the concepts of *style* and *register* to explain these associations. *Style* refers to degree of formality, and *registers* "are sets of language items associated with discrete occupational or social groups" (Wardhaugh, 1998). A great deal of social information is conveyed through style and register, so it is clear that the phenomenon of webs of association is not exclusive to literature. However, there may be principled ways to distinguish genre from register and style; in particular, it may be that genre is more consciously controlled than register or style, and there may also be

to this level, and *not* to the illocutionary level ("active intentional" level in Hancher's terms). I'm not sure the evidence for this is as strong as Hancher claims, but it does seem that W&B have programmatic intention in mind to some degree, especially when they warn against evaluating a poem according to whether or not it "achieved the authors design" (1947/1976, p.1). Hancher's point—well taken I think—is that access to illocutionary intentions—arguably the important intentions—is not dependent to any significant degree on recovering programmatic intentions. Hancher goes on to argue that the confusion over intentions in W&B's article results from their failure to separate these different types of intention. Again, this is good criticism. However, Hancher misses the main kind of "intention" W&B were concerned about, which is a level of intention that does not fit in either Austin's or Hancher's schemes.

Careful reading of W&B's essay shows that what they were warning against was not recovery of illocutionary force—but, rather, extrapolation to *private* associations. As for access to illocutionary force, though they don't use these terms, this is not denied at all. Rather, for them, the illocutionary level is a "public" level of language that is simply read off the page:

> There is a difference between internal and external evidence for the meaning of a poem. And the paradox is only verbal and superficial that what is (1) internal is also public; it is discovered through the semantics and syntax of a poem through our habitual knowledge of the language, through grammars, dictionaries, and all the literature which is the source of dictionaries, in general through all that makes a language and culture (1947/1976, p. 6);

This is an exceedingly naïve conception of "public," and also a very naïve view of what it means to read. In fact, access to the kind of "internal" evidence Wimsatt and Beardsly

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qualitative differences in the sorts of knowledge webs genre and registers/styles invoke. The terms are

refer to here, clearly requires extrapolating well beyond the page to some other source of integrated knowledge: "all the literature which is the source of dictionaries. . . all that makes a language and culture." For them, however, this process is apparently held to be obvious, automatic, unproblematic, and, text-centered. Thus, the point about whether or not authors' illocutionary intentions are available reduces to whether the illocutionary level is public and entirely text centered or, whether, at least to some degree, it is idiosyncratic and extra-textual. If, as W&B would have it, these vast and complex webs of meaning are actually public and contained in the text, then there is no need to extrapolate to the author's intentions, and it may even be that these intentions are unavailable. In fact, the question becomes irrelevant to interpretation. Crucially, however, there is no denial of illocutionary force in W&B's essay. What they do deny, emphatically, is the possibility and value of discovering *private* intentions:

> what is (2) external is private or idiosyncratic; not a part of the work as a linguistic fact: it consists of revelations (in journals, for example, or letters or reported conversations) about how or why the poet wrote the poem—to what lady, while sitting on what lawn, or at the death of what friend or brother. (ibid)

and later:

> There is a gross body of life, of sensory and mental experience, which lies behind and in some sense causes every poem, but can never be and need not be known in the verbal and hence intellectual composition which is the poem. For all the objects of our manifold experience, for every unity, there is an action of the mind which cuts off roots, melts away context—or indeed we should never have objects or ideas or anything to talk about. (ibid)

Whether or not it is true is also debatable. It is not hard to imagine cases in which the author's idiosyncratic mental life could indeed have significant bearing on interpretation of his/her work. But, regardless, this restricted sense of intentional fallacy was not long

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slippery though, and there is probably lots of overlap between them.

maintained. Because of the confusions about "intentions" outlined above, the doctrine soon became enlarged to encompass illocutionary intentions as well as the "private" intentions W&B actually warned against. The New Critics, who originally took their theory of textual autonomy from W&B's arguments against intentional fallacy, explicitly denied the sort of stable "public" meanings W&B assumed, and thus radically altered the original sense of the term. For quite a while, this radically enlarged sense of intentional fallacy has carried the day, particularly under postmodern assumptions, which deny any stable ground for language or interpretation.

Various critics have consistently argued against this position, and for an author/text position, including Hirsch (1967) Graff (1979) and others. Recently, William Dowling has resurrected the idea of "public meanings" in his book *The Senses of the Text* (1999), arguing that there are sound linguistic reasons for alleging such a domain, and claiming that it is only through such meanings that determinacy in interpretation can be reclaimed. I will have much more to say about his position later after I have established a linguistic basis for analysis of texts. At this point, however, I will continue to trace the historical separation of authors from texts.

Reader Centered Perspectives

The New Critics' success in alienating authors from texts opened the door for a still more radical departure. If authors' intentions were unavailable and "public" meanings unstable, then it followed that only readers could determine the sense of a text. This perspective gained ground in the 1970's with a movement in literary criticism dubbed "Reader Response theory," which was started quite self-consciously by Stanley Fish, and later picked up by numerous theorists including David Bleich, and Norman

Holland (McCormick, 1994). Philosophically, Reader Response theory is reminiscent of Rousseau's pleas for free exploration in *Emil* and Dewey's many arguments for child-centered instruction; it is also closely allied with the *expressive* model of reading, which is associated with the work of Frank Smith and Kenneth Goodman (ibid). The radical version of this theory holds that there are as many correct interpretations of any given text as there are readers, since meaning is "constructed" by readers. Weaker versions of this theory mix elements of author and text centered views, so that not *all* interpretations can be considered correct, but only those that are in some way supported by the text. Radical Reader Response theory is especially interesting to this discussion because it completely departs from Plato's dictum about the necessity to interrogate texts and/or authors. Its obvious weakness is that it easily slides toward literary nihilism, wherein texts can mean anything or nothing at all. For that reason, and because it is simply too open to allow interesting criticism, radical Reader Response theory is rarely encountered. However, weaker versions are still staunchly defended, both in literary circles (e.g., Fish, 1980), and in pedagogy (Johns, 1997; McCormick 1994; Karolides, 2000). From a theoretical perspective, Reader Response theory is interesting because of the way it intersects with generative (linguistic) theory. If knowledge of language resides only in individuals, as Chomsky would have it (1986), then Reader Response would seem to be inevitable. The way out of this, of course, is to invoke the author's I-language (knowledge of language), as having at least as great a claim on meaning as the reader's. But if this course is taken, how does one adjudicate between the two I-languages? I will argue that, in fact, nearly the whole interpretive dilemma comes down to this question.

The Discourse Community Centered Perspective

Without question this is currently the favorite view, no doubt, because of the influence of "social constructivism" (or "construction") in contemporary culture. The claim here is that meaning arises, or is "constructed" as individuals interact within communities. As with Reader Response theory, discourse community perspectives come in many different varieties, so it is hard to generalize about them. Most are mixed with other views, particularly Text Centered and Reader Response, and weaker versions of interpretive community views focus more on how meanings are derived through interaction than where meanings ultimately reside. These weaker views are hard to argue against, and almost no one does. It seems self-evident that language changes with use, and there is overwhelming empirical evidence that this is true; moreover, since language is always used within some discourse community, the central constructivist claim seems inescapable. Strong constructivism, however, goes much further than this, claiming that meanings actually originate and reside in discourse communities, not individuals. At times Paul James Gee seems to take this view, for example when he writes, "Discourses 'speak' to each other throughout history" (1996, p.90), though it's hard to tell how literally he means these things. Fish, who has migrated from Reader Response theory to interpretive community theory, also strays very close to this dubious territory when he argues that "while there are always mechanisms for ruling out readings, their source is not the text but the presently recognized interpretive strategies for producing the text" and ". . . it is only within the institution that the facts of literary study—texts, authors, periods, genres—become available" (1980). I will take up these arguments in detail, particularly Fish's in the next chapter.

The Traditional View

Along with these "pure' views, one hybrid view cannot be ignored, since it is probably the view most people assume, and the one with the longest history in the schools. This "Traditional" view is a mixture of author centered and text centered perspectives, with most versions emphasizing 2), since a major tenet of Traditionalism is that texts can be autonomous due to the alleged possibility of stable word meanings. Ann Johns (1997) traces the Traditional view back to the "European Enlightenment and the founding of modern science," approvingly citing Cope and Kalintz (1993), whom she says "argue that one of the reasons for the persistence of Traditional views is that they are based on a 'scientific positivism' that originated in eighteenth century Europe and North America and [which] persists to this day." In this synopsis and in her following comments, Johns clearly implies her own disdain for Traditionalism and her general distrust of scientific analysis of language. As will be seen, her assessment is shared by many literacy theorists today, a fact that I will argue is both unwarranted and debilitative for progress in the field.

Traditionalism holds that every text has a "correct" reading (and, ergo, that some readings are wrong). According to this view, meaning is determined first by correct interpretation of words, and then through correct interpretations of larger structures—sentences, paragraphs, whole texts. This view has the advantage of promising precise, unambiguous interpretations; however, it faces an extremely heavy theoretical burden in that it must explain the precise interpretive rules readers should use to arrive at correct readings. Since there are no widely accepted methods to do this, in practice,

traditionalists actually rely for interpretation on "expert readers," like teachers, who are assumed to operate according to these correct rules. But because even "expert readers" frequently arrive at different interpretations, particularly of literary texts, it has been widely rejected by most contemporary theorists, particularly those in literature.

Traditionalism was very influential in American schools through sometime in the 1960's or early 1970's, but it is rarely invoked in any strong form by practitioners anymore, though weak versions of it still remain in schools (Johns, 1997). E.D. Hirsch and Meyer Abrams, among others, have argued for this perspective (though not by that name), but withering critiques from post-modernists, especially Stanley Fish, have ridiculed their arguments by demonstrating that the putatively "stable" texts these authors offered as examples are, at least according to some analyses, ambiguous. Thus, among most theorists today, the Traditional perspective is regarded as hopelessly retrograde and simplistic. I will argue, however, that the problem with the Traditional perspective is not so much that it locates meaning in authors and texts, but, rather, in its simplistic conception of the relationship of authors to texts, and also of elementary terms such as "word definitions."

Though passé among the literati, lay people, including most college students, tend to harbor Traditional views almost reflexively.[15] Indeed, this is one area where folk views of language (Preston, 1998) and professional views are most divergent. Moreover, consistent with its connotations, Traditionalism is often invoked as a rallying point among conservative politicians in discussions of school reform (W. Bush, for example

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[15] Social constructionists would cite this as a perfect example of a socially constructed "paradigm." Cognitivists might counter that it is derivable from innate linguistic resources, reason and experience alone. Still others might find a resolution somewhere in between these positions.

has recently made several public statements about reading that have a distinctly Traditional flavor about them). Traditionalism is thus important as a point of departure for both theorists and practitioners since the implications of rejecting it are very far reaching in both theoretical and practical spheres. In particular, rejection of the Traditional view tends to imply that correct interpretations can never be derived; and from here, many people anxiously retreat from what they perceive as a slippery slope down to the sort of literary nihilism mentioned above. As Hirsch puts it:

> The metaphorical doctrine that a text lives a life of its own is used by modern theorists to express the idea that textual meaning changes in the course of time. This theory of a changing meaning serves to support the fusion of interpretation and criticism and, at the same time, the idea that present relevance forms the basis for textual commentary. But the view should not go unchallenged, since if it were correct, there could be no objective knowledge about texts. (1976a)

Critics on the other side of this debate, like Fish, however, categorically reject the notions of stable meanings and reliable interpretive practices, arguing that meaning is purely a function of "interpretive community" assumptions. Thus, the study of writing presents a formidable epistemological problem in microcosm, one that cuts across several disciplines, including linguistics, psychology, sociology, and history. Although the problem of interpreting text is in many ways restricted to its own world, i.e., the realm of text, and most arguments for particular interpretations remain within that realm, inevitably, larger problems of epistemology, i.e., how it is we can know *anything*, arise in extended discussions of literacy, and it is therefore necessary to address these problems when constructing a theory of literacy.

II: LITERACY AND GENERAL PROBLEMS OF EPISTEMOLOGY

Antifoundationalism

The connection between literacy studies and epistemology proper is generally acknowledged in the literature on literacy, but, unfortunately, rigorously defended positions are rare, probably because there is a dominant epistemological stance among leading scholars in literacy that is often simply assumed, though quite without merit in my view. This dominant stance, a species of skepticism, which is generally referred to in the literature as "nonfoundationalism" or "anti-foundationalism" e.g., (Bizzell, 1992; Fish, 1980; Geisler, 1994; Smit, 1998; Villanueva, 1997) or in some cases "social constructivism,"[16] e.g. (Bizzell, 1992; Johns, 1997; Kukla, 2000; Smit, 1998; Villanueva, 1997) is part of the post-structuralist/post modernist/deconstructionist/social constructivist movement[17] that has arisen in the last thirty years or so, and which is traceable at least to Marx. Kukla, (2000) relates what he calls the "standard story" about the origins of modern constructivism:

> Constructivism stands at the confluence of two streams in the history of sociology; the sociology of knowledge and the sociology of science. The first was shaped by the vision of three seminal thinkers: Marx, Mannheim and Durkheim. All of them emphasized the causal role of social factors in shaping individual belief. Marx famously argued that social class determined various intellectual attitudes (Marx and Engels, 1963). Mannheim (1963) and Durkheim (1915) broadened both the range of causally relevant social factors and the range of intellectual attitudes that fall under

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[16] "Social constructivism" is actually a cover term for a broad range of epistemological stances, only some of which are "nonfoundational." I discuss these various "constructivisms" and their relationship to nonfoundationalism at length below.

[17] These terms appear more or less interchangeably in *some* of the literature, while in other places they are carefully distinguished. Nonetheless it is fair to say that they all share a general skepticism regarding claims to objectivity, particularly in science, and a concomitant commitment to the thesis that our perceptions of reality, or, in extreme versions, reality itself, is a "social construction."

> their sway. But, like Marx before them, they exempted the beliefs generated by mathematics and the natural sciences from their social analysis. Scientific belief was thought to be rationally rather than causally determined, and thus to transcend social and cultural influence. This epistemic dualism is what distinguishes the classical period in sociology from its more modern manifestations.
>
> (2000, p. 7)

As Kukla notes, it is hard to argue with the basic constructivist premises of these "dualists," whom he calls "reasonable social constructivists," given the fact of overwhelming correlation between political and religious beliefs, with region, education, and social class. There is ample evidence that people's perceptions *are* influenced by culture. This has been explicitly acknowledged and warned against, not only in sociology, anthropology and psychology, but also in the philosophy of science literature at least since Bacon's *Novum Organum*[18], and there are numerous modern studies to support the claim (e.g., Khun, 1970) So, point established. However, under reasonable constructivism, particular claims for cultural bias are still amenable to normal scientific evaluation, i.e., through observation, theory construction, data collection, statistical analysis, etc. But radical constructivism seeks to impugn *all* analysis, including, or perhaps, especially, scientific analysis. Proponents of this view, working in a field they call the "the sociology of scientific knowledge" (SSK) argue that *everything*—the very concept of science as well as all supposed scientific facts—are nothing more than "social constructions" (Kukla, 2000, p.11). Kukla cites the following passage by Steve Woolgar,

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[18] Bacon anticipated the postmodern argument by four centuries when he wrote of four classes of "idols" that "beset men's minds" and mislead them: "The Idols of the Cave are the idols of individual man. For everyone has a cave or den of his own, which refracts and discolors the light of nature; owing either to his own proper and peculiar nature or to his education and conversation with others; or to the reading of books, and authority of those whom he esteems and admires; or to the differences of impressions, accordingly as they take place in a mind preoccupied or predisposed or in a mind indifferent and settled; or the like. (1620/1930, p.35)

whom he identifies as "an extremist among constructivists" as an example of this position:

> there is no sense in which we can claim that the phenomenon . . . has an existence independent of its means of expression . . . There is no object beyond discourse . . . the organization of discourse is the object. Facts and objects in the world are inescapably textual constructions.
>
> (Woolgar, 1988:73; cited by Kukla, 2000)

Villanueva (1997, p.391) traces the origins of this movement in composition studies to the 1960s when "anthropological study and different elements of critical theory. . . put serious questions to positivism, to positivisms' search for universal answers, the very idea that we can think in terms of universals" (Villanueva, 1997). Social constructivism, in ever more radical versions, and, eventually, anti-foundationalism, offered alternatives that appealed to many theorists in literacy and composition. But this academic quest for a new approach to composition and literacy studies soon acquired strong social justifications as well. Thus, ironically, these views appear to have become popular in literacy studies and literary theory, not so much because of their inherent virtues, but for social reasons. In other words, these social constructivists socially constructed the academic justification for social constructivism in literacy studies, literary theory, and rhetoric and composition. One way they did this was by "deconstructing" the rationalist enterprise of science, and "demonstrating" that it was anything but enlightened. For example, many social constructivists in literacy studies have argued that patriarchal chauvinism and class inequality, both within the academy itself, and also allegedly perpetrated in the larger society by the academy, are the direct result of (benighted) rationalist ways of comprehending the world (Bizzell,

1992; Brufee, 1982; Geisler, 1994; Johns, 1997)[19]. Nor is this just a reactionary, rationalist critique. Stanley Fish himself, an acknowledged leader of the anti-foundationalist movement, probably *the* leader of the movement, at least within literacy studies, recounts how numerous self-described anti-foundationalists have tried to "redescribe anti-foundationalism so that it becomes a new and fashionable version of democratic liberalism" (1989, p.350). Importantly, the animus behind this "liberal" move has nothing to do with composition, literary theory or criticism, nor, as Fish points out, is it remotely consistent with anti-foundationalism; rather, it is a naked attempt at social engineering under the guise of scholarship (this last comment is mine, not Fish's). Fish cites Bruffee's "Liberal Education and the Social Justification of Belief" as an example of this practice. As Fish notes, in this piece Bruffee urges teachers to use their classrooms as venues for building "self-awareness," of the anti-foundationalist variety:

> we should, according to Bruffee, ask students "to identify" in nonevaluative terms their own beliefs and the beliefs of the local, religious, ethnic, national, supranational, and special-interest communities they are already members of . . .

But, Fish points out—in a rare moment of clarity about the severe limits of anti-foundationalism—

> this goal . . . is incompatible with anti-foundationalism because it assumes the possibility of getting a perspective on one's beliefs, a perspective from which those beliefs can be evaluated and compared with the similarly evaluated beliefs of others. That is, what anti-foundationalism teaches. . . is the inescapability of situatedness, and if situatedness is inescapable, students could not possibly identify in nonevaluative ways their own beliefs, because as situated beings some set of beliefs of which they could not be aware would be

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[19] There is no question that the enlightenment period was marred by terrible abuse of human rights, particularly the rights of women and minorities. And it is also true that "enlightened" white men often "justified" their actions by appeal to "science" in various ways, for example by claiming to have discovered inherent biological inferiorities in those they oppressed. But these are examples of bad science, not a reason for throwing science out the window altogether.

> enabling any identification they might make; and, therefore, the act of identification would from the very first be evaluative through and through (p.350)

If Fish could only see his way to applying this same *tu quoque*[20] criticism to his own arguments, he might work his way out of the anti-foundationalist paradox. But, as we shall see, he has not done so, perhaps because that would compromise his own position too severely.

Another point Fish ignores, but which is also crucial here, is that using literacy studies as a platform for one's political agenda discredits the field in at least two ways. First, those students, teachers, administrators, parents, and other interested parties who do not happen to agree with one's agenda are bound to be offended, and rightly so. Professionals in the field are paid to develop the field and teach it, not hijack it. Second, replacing academic theory with some social agenda just highlights the emptiness of the field. If professionals in literacy really don't have anything unique *from* literacy to offer, then there isn't any reason for the field to exist. Professionals in the field, many of whom bitterly complain about their lack of status and low pay, should look no for further than this for the reason. Either literacy studies is a bona fide academic field with real insights, methods, and knowledge to contribute to the academic enterprise—as I for one certainly believe it *can* be—or it is merely an anachronism—as many in academe already seem to think—and should just blow away.

I would note here, in passing, that I am sympathetic to at least the general spirit of social reform Fish describes (though he himself is not sympathetic to it) and to which, apparently, many in literacy studies are dedicated, that is to the major tenets of liberal

democracy such as equality, representative government, non-violent negotiation, and promotion of education for its own sake. Moreover, though I think it is inappropriate to turn the field into a venue for promoting such ends, I also agree that one cannot teach literacy in such a neutral way as to completely disguise one's political and philosophical convictions. As I've argued throughout this chapter, literacy is an inherently philosophical discipline, and thus, in teaching it, one's colors are bound to show. But in my view, that should happen only as a byproduct of exploring issues in the field, never as the heart of that exploration. Finally, though promoting social justice should not be an immediate goal of literacy studies, there are strong connections between the epistemological questions raised by literacy studies, and social justice. However, the link is opposite from what Bruffee et al imagine. Justice requires *judgment*; indeed, that is Fish's reason for assiduously distancing anti-foundationalism from political reform. I would urge continuing that line of thinking, therefore, to its obvious conclusion: Adopting an anti-foundationalist stance is logically incoherent, and ruinous for both research and social reform precisely because it precludes all possibility of distinguishing true from false, right from wrong.

Occasionally this insight gets through, even to hard core anti-foundationalists. Bizzel, for example, equivocates on this point throughout her entire book, *academic discourse and critical consciousness* (1992). Again and again she "catches" herself making some moral pronouncement, then bravely shakes herself free with the reminder that, no, the comfort of moral truth is not available to anti-foundationalists. Finally, at

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[20] Literally "thou also." I borrow this term from Kukla (2000) who uses it as a label for arguments that impugn themselves or their authors.

the end of this entire, wistful exercise, in her last essay, she makes a desperate attempt to go "beyond anti-foundationalism" (in an essay of that name), arguing absurdly that

> To take the next step in our rhetorical turn [i.e., the turn beyond anti-foundationalism], we will have to be more forthright about the ideologies we support as well as those we attack, and we will have to articulate a positive program legitimated by an authority that is nevertheless nonfoundational. We must help our students, and our fellow citizens, to engage in a rhetorical process that can collectively generate trustworthy knowledge and beliefs conducive to the common good—knowledge and beliefs to displace the repressive ideologies an unjust social order would inscribe in the skeptical void. (p. 271)

Take out the token nods to "nonfoundationalism" and "rhetorical process" and you have a speech William Bennet could applaud. Ostensibly, "going beyond anti-foundationalism" should mean something different than retreating to foundationalism; however, concepts like "forthright," "ideologies," "positive programs," "legitimation," "trustworthy knowledge and beliefs," "the common good" and "unjust social order" are all *entirely,* and very conventionally, foundational—all implicitly appeal to objective notions of truth and justice. Moreover, again, my criticism is not just rationalist grumbling. Fish makes the same point:

> [A]ny claim in which the notion of situatedness is said to be a lever that allows us to get a purchase on situations is finally a claim to have escaped situatedness [i.e., to have "gone beyond anti-foundationalism"], and is therefore nothing more or less than a reinvention of foundationalism by the very form of thought that has supposedly reduced it to ruins.
>
> (Fish, 1989, p.348-349)

Given this *inescapable* conclusion, one wonders why morally conscientious people like Bizzell don't simply throw the anti-foundationalism monkey off their backs and get on with the good works they obviously want to pursue. I suspect that a good part of it is just the desire to remain in fashion, even if that requires subscribing to what is clearly

"fashionable nonsense" (Sokal and Bricmont, 1998). Indeed, Bizzell never *argues* for anti-foundationalism (or "nonfoundationalism" whatever the difference might be); rather, she just continually asserts, oxymoronically, the "truth" of the doctrine, occasionally appealing to authority for support:

> Finally, then, we should see our answers to the question of what we need to know about writing in the light of a new humanistic synthesis. Philosophy has moved to the position that discourse communities are all we have to rely on in our quest for certainty. Literary criticism is analyzing how discourse communities function as historically situated interpretive communities. Composition studies should focus upon practice within interpretive communities—exactly how conventions work in the world and how they are transmitted. If the work of these disciplines continues to converge, a new synthesis will emerge that revivifies rhetoric as the central discipline of human intellectual endeavor. In view of such a synthesis, the project to make composition studies merely scientific looks obsolete. (p.101)

These claims that "philosophy" has given up on rationalism are common in antifoundationalist literature, but they are clearly false. To begin with, what exactly is meant by this term "philosophy"? As many professional philosophers have remarked, including Richard Rorty, *the* antifoundationalist philosopher, philosophy is not a field that is neatly bounded by professional affiliation. Anyone who reads the philosophical literature should be aware that today philosophical argument crosses many disciplines including linguistics, psychology, and the emerging field of cognitive science. And in these fields the question of epistemic "certainty" is a hot topic that is still very much debated. But even if "philosophy" were unanimous on this point, that would hardly be sufficient reason to believe it. "Philosophy" once told us that kings had divine rights, that lead could be "transmuted" into gold, and that rats were spontaneously generated from garbage, so this is not an entirely trustworthy institution. And as for the payoff of this grand vision, the promise to "revivify rhetoric as the central discipline of human

intellectual endeavor," well, I think I'll take my chances with "mere' science, thank you. As I recall, the last time rhetoric reigned over science was during that balmy period in history when the sun revolved around the earth, "philosophers" went around reading falcon entrails and "transmuting" lead, and "witches"' were burned by the dozen at the stake.[21]

These are not exactly compelling arguments against antifoundationalism, of course—they are, well, just rhetoric. But that's the point. If we should take seriously the proposition that science has become obsolete and should be replaced by rhetoric, this is the quality of argumentation that can be expected to ensue.

Fortunately, however, despite incessant anti-foundationalist claims that science is dead and rhetoric is the soon to be crowned king, as David Smit notes (1998) "[m]ost of the varieties of antifoundationalism are still a matter of lively debate in professional philosophy," and "ought to be a be a matter of lively debate in composition/rhetoric too" (1998)[22]

Echoing these sentiments in her brief history of epistemological debates in literacy, Patricia Alexander argues that

> ...multiple perspectives on knowledge have always existed in the research community and the public at large. These epistemological

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[21] Sarcasm aside, it is amazing to hear such pronouncements from educated people who have grown up in a scientific world. Four hundred years ago, Bacon urged the scientific community to escape from philosophical and rhetorical dogmas precisely because these had yielded *no progress* from the time of Aristotle—*the* Philosopher/rhetorician—to his own day. As Bacon so clearly saw then, the only way to escape this trap of ignorance is to subject philosophical and rhetorical conjectures to rigorous scientific analysis. One has only to reflect for a moment on the progress made since people began taking that very good advice to appreciate its merits.

[22] Smit's claim is easily verified by perusing the philosophical journals; epistemological debates are ubiquitous in these journals, and, both rationalists and post-modernists are widely represented.

> differences are nontrivial and help to explain the diverse programs of and approaches to research and practice. (Alexander, 1998)

Alexander expands on this idea by quoting Cunningham and Fitzgerald (1996):

> [F]amiliarity with the variant philosophical positions seems imperative today if one is to understand predominant views of educational research and practice in general and of reading research and instruction in particular. . . . Knowing different epistemological issues and positions forces us collectively to examine the world views implicit in reading organizations,' journals, reading methods, accepted or popular practices. (p.36)

Epistemic Justification

In my view, agreeable though they are, these comments don't go nearly far enough. Epistemological integrity is far more than a nicety, particularly in a field dominated by anti-foundationalists. Naturally, those who would buck the trend by introducing foundational perspectives are obligated to rigorously defend their egregious views. But the status quo should not rest either. They, too, are obligated to explicate their epistemological positions for two reasons. First, although anti-foundationalism may be the dominant stance in the field, in the larger arena of science, that stance is very much in the minority (Sokal and Bricmont, 1998). Some might argue that literacy is not a scientific field, and, therefore, bears no obligations to science. But that position entails a rather narrow view of science, restricted, perhaps, by nominal affiliation or adherence to some set of preconceived notions about "scientific" methods or practices. However, a conceptual view of science is much more inclusive, embracing virtually every field that practices any kind of analysis; as Einstein put it, "The whole of science is nothing more than a refinement of everyday thinking" (1964). But "refined every day thinking" is achieved by reasoning, and reasoning requires a foundation: we must reason *from* some fact, or axiom, *to* our conclusions. Therefore, anti-foundationalists who would do analysis—and literacy theorists all do analysis—are obligated to explain how their

particular brand of anti-foundationalism allows them to reason without contradiction. To let the cat out of the bag, it will be my contention here that there *is* no way out for them—with the possible exception of de-fanging the term *anti-foundationalism*, i.e., of defining it in such a way that it no longer means rejection of foundations. I'll return to this issue in detail later when I examine specific works of self-proclaimed anti-foundational theorists. Second, as argued above, literacy theory is peculiarly dependent on epistemology, more so than perhaps any field other than philosophy itself. Ultimately, all fields depend on epistemological assumptions, but many fields routinely operate at levels far removed from broad epistemological questions, and for most people, beliefs need only be justified "by reference to [their] immediate successor" (Sosa, 1980). Thus, fields not directly concerned with epistemological problems, simply avoid those issues. Most natural scientists, for example, are busy with questions whose scope remains within a dominant paradigm (the immediate successor) (Horgan, 1996; Kuhn, 1970); thus they never have to justify overarching assumptions because these have been expediently dealt with already. But literacy theorists are primarily concerned with the problem of interpretation, and this problem always leads to broad epistemological questions.

Perhaps one reason anti-foundationalism is popular among literacy theorists is because it seems to offer shelter from philosophical exposure. Literacy theorists have learned well from literary critics that strong, unambiguous positions are bound to be attacked, often with embarrassing consequences. Fish, for example, shredded Hirsch for claiming that "simple, straightforward" sentences like "the air is crisp" have a determinate meaning (Fish, 1980). The wrong lesson learned from this, perhaps, was that

the way to explain indeterminacy in language use is to adopt an indeterminate epistemological stance. In the following chapters, I will endeavor to show that, to the contrary, (minimal) foundational epistemology can easily handle linguistic indeterminacy, and that, moreover, only foundationalism can accommodate the kind of systematic complexity that underlies literacy, and, further, that only foundationalism can support the kind of research literacy theory requires to develop.

A Brief Sketch of Central Issues and Solutions to Classical Problems of Epistemology

Developing a firm epistemology foundation is probably the most difficult and important issue in philosophy and science, and also necessary for all other analytical work; however, the problem is not obvious, and in fact, is often ignored or avoided by scientists and academics of all sorts, except philosophers, probably because 1) the reality of knowledge seems natural to us, and 2) we realize intuitively, if not consciously, that no analytical work is possible while the very idea of knowledge itself is under attack. Thus, avoidance is the first "solution" to the problem of epistemology, a solution that dominated philosophy from ancient Greece, well into the renaissance. As one critic notes, "Except for outright Skeptics, Aristotle's solution to the Problem of First Principles, that such propositions are known to be true because they are self-evident, endured well into Modern Philosophy" (Ross, 2000). But appeals to "self-evidence" merely beg the question because skeptics can always ask why we should trust such "evidence." Moreover, "self evidence" is notoriously variable. In the seventeenth and eighteenth centuries this fact prodded philosophers to revisit the issue seriously because "Rationalists like Descartes, Spinoza, and Leibniz, appealed to self-evidence and then came up with radically different theories" (ibid), which showed that "self-evident"

propositions were unreliable. Hume then revived the Skeptics' arguments and the problem became urgent in philosophical circles again (ibid).

Hume's argument, in bare form, was that 1) all our knowledge is derived from our experiences; 2) because we see things happening in succession, repeatedly, like the sun rising and setting, or billiard balls glancing off one another in similar ways, we 3) attribute some cause, to every effect; however 4), we can never anticipate any of these effects originally, but must, rather, wait to see what they will be; thus 5) we must concede that our knowledge is not grounded anywhere in reason, but is, rather, merely a matter of "custom and experience"; however 6), our experience is limited; therefore 7), we cannot know anything with certainty (Hume, 1748/1939).

Kant met this argument in a very crafty way, by first incorporating most of it, and in fact, strengthening it certain respects. He first agrees that "all our knowledge begins with experience" (1781/1965, p.41). He then systematically demonstrates that this experience is always mediated through a cognitive/sensory apparatus, whose relationship to reality is entirely unknown. He called this argument the Copernican Revolution in philosophy because of the way it reverses the relationship between our understanding and the world. Ordinary understanding (OU) assumes sensory input to be isomorphic with reality. Thus under OU we observe such things as planetary motion, and changes in planets' spatial relationships to each other over time, and we thus deduce "facts" about the universe. But Kant says this OU is merely an artifact of our sensory apparatus. Both time and space, he argues, are epiphenomena of our senses, not real things. Moreover, he stresses that this is not merely a matter somewhat faulty senses that perhaps give a

distorted view of reality, as Leibniz, Wolff, and other philosophers had proposed, but, rather, a matter of senses that may not reflect reality at all:

> It is not that by our sensibility we cannot know the nature of things in themselves in any save a confused fashion; we do not apprehend them in any fashion whatever. If our subjective constitution be removed, the represented object, with the qualities which sensible intuition bestows upon it, is nowhere to be found, and cannot possibly be found. for it is this subjective constitution which determines its form and appearance. (ibid, p. 84)

And later:

> It is, therefore, not merely possible or probable, but indubitably certain, that space and time, as the necessary conditions of all outer and inner experience, are merely subjective conditions of all our intuition, and that in relation to these conditions all objects are therefore mere appearances, and not given us as things in themselves which exist in this manner. (p. 86)

This aspect of the argument is as deeply skeptical as any ever proposed, far more skeptical, in fact than Hume's, since Hume reserved his skepticism for pure philosophy while simultaneously admitting, indeed urging, that in practical matters this skepticism "should never undermine the reasonings of common life. . . Nature will always retain her rights and prevail in the end over any abstract reasoning whatsoever" (Hume, 1939, P. 609). Hume's comment could easily be applied to postmodernists and anti-foundationalists as well, since they also argue for an extreme skepticism that they never practice. Regarding Kant's argument, antifoundationalists should also note that Kant outdid them long ago in arguing for extreme "situadedness," though the subjectivity he was concerned with was cognitive/sensory not cultural or linguistic.[23]

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[23] Actually, Kant also considers cultural subjectivity, but dismisses it as a relatively minor problem. he never considers the linguistic problem, but, given his arguments about *a priori* concepts, it is certain that he would have rejected the argument that language causes thought.

But having established this extreme subjectivity of perception, Kant salvages the thinnest of foundations by observing that "though all knowledge begins with experience yet it does not all *arise* from experience" (p. 41; my italics). This subtle move provides a wedge that Kant uses to establish the *a priori*. If, he argues, there are conclusions that we hold about reality *of necessity,* then these cannot have arisen from experience, since, as Hume showed, experience alone cannot give us such convictions. Yet, Kant insists, there are realms of thought—mathematics and logic for example—that are universal and immediately compelling, regardless of experience. Science, then, consists of coordinating these "necessary" suppositions with empirical evidence.

Kantian Rationalism and Science

There are, however, two major problems with this argument. A science that rests on the conjunction of reason and empirical evidence, is only as reliable as they are, and both reason and empiricism have been called into question by the skeptics. First of all, regarding reason, the mere fact, that some judgements are *a priori* doesn't make them true; logically, we must admit that it could be the case that our *a priori* judgements are delusions. Nor can we reason our way out of this problem, for, by definition, once you've reached the *a priori* you cannot look for further support though reasoning. On the other hand, the fact that we cannot prove that *a priori* reason is veridical doesn't mean it isn't. Though we cannot prove that *a priori* judgements aren't delusions, we cannot know that they *are* delusions, either, so we have a philosophical stalemate. Throughout the *Critique* Kant acknowledges this problem, but he argues that stalemate is good enough. In these discussions, he frequently alludes to the ideal skeptic, whom he has modeled after Hume, as "our opponent":

> Now all synthetic propositions of pure reason have this peculiarity, that while in asserting the reality of this or that idea we can never have knowledge sufficient to give certainty to our proposition, our opponent is just as little able to assert the opposite. This equality of fortune [in the ventures] of human reason does not, in speculative modes of knowledge, favor either of the two parties, and it is consequently the fitting battle-ground for their never-ending feuds.

Thus, although rationalists cannot definitively vanquish global skepticism, they need not buckle to it either. For, as rationalists[24] have always insisted, at least since Plato, such skepticism—read, antifoundationalism—is impotent because it always impugns itself: if the skeptic says "all knowledge is in question, and everything is to be doubted" s/he fatally undermines her/his own position. So, the validity of *a priori* reasoning is saved, albeit by stalemate.

The other problem—how to save empiricism—is more difficult. In fact, many critics argue that Kant fatally undermined his entire project by contradicting himself regarding the status of empirical data (Ross, 2000). I will argue here that Kant didn't contradict himself, though it certainly appears that way at prima facia. Unfortunately, Kant's elliptical, abstract style is often cryptic, but it is possible to infer a consistent, non-contradictory, though extremely subtle, position regarding reason and empirical data.

Let us first recall the problem we have. Kant argues continuously throughout the *Critique*, that, "through the senses we do not apprehend [the world] in any fashion whatever "[25] (p. 84). But if this is true, then how can we use empirical evidence in

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[24] Throughout this dissertation I will use the term "rationalist" in the post-Kantian sense, i.e., in the sense that does not exclude empiricism. Thus, for me, Chomsky and Fodor are rationalists, even though they both appeal to empirical data.

[25] This position may be too strong. If we don't know how are senses are related to reality, then we can't know that there is *no* relationship between what we sense and what is really there. I think this somewhat weaker position is really what Kant was after, but he wanted to stress the fact that we can't deduce reality directly from our sensory perceptions.

conjunction with reason to reach true conclusions about the world? Even if we are lucky, and reason *is* veridical, our conclusions still might have nothing to do with the world as it really is; as Ross puts it, "to the extent that knowledge depends on the structure of the mind and not on the world, knowledge would have no connection to the world and is not even true *representation*, just a solipsistic or intersubjective fantasy" (2000).

Yet, it is clear that Kant did not regard the radical subjectivity of our senses as a bar to science. To the contrary, throughout the *Critique,* he unequivocally supports empirical science:

> Natural science was very much longer [than geometry] in entering upon the highway of science. It is indeed only about a century and a half since Bacon, by his ingenious proposals, partly initiated this discovery, partly inspired fresh vigor in those who were already on the way to it
>
> (p. 19-20)

Undoubtedly, the work Kant refers to here, is *Novum Organum* (1620/1939), in which Bacon argues for the superiority of methodical empirical research over pure logic and conjecture in science. In making this argument Bacon first reviews the history of science, demonstrating thereby that despite the efforts of many brilliant men, little had been accomplished in the natural sciences during more than two thousand years of effort:

> The more ancient of the Greeks took up with better judgement a position between these two extremes—between the presumption of pronouncing on everything, and the despair of comprehending anything . . . And yet they too, trusting entirely to the force of their understanding, applied no rule, but made everything turn upon hard thinking and perpetual working and exercise of the mind.
>
> Now my method, though hard to practice, is easy to explain; and it is this. I propose to establish progressive stages of certainty the evidence of the sense, helped and guarded by a certain process of correction, I retain. But the mental operation which follows the act of sense, for the most part I reject; and instead of it, I open and lay out a new and certain path for the mind to proceed in, starting directly from sensuous perception.

(1620/1939, p. 25)

Bacon was writing in direct opposition to the Aristotelian tradition, which had dominated science for two thousand years, under which men tried to fit the world to Aristotle's elaborate preconceived ideas of how the universe should be, given certain assumptions about "perfection," "beauty," etc. What is so striking, then, about Kant's position, is that although his major project is to establish the reality of the *a priori* and determine its characteristics—a project that has a superficial resemblance to Aristotle's preconceived "categories"—he nonetheless endorses Bacon's virulently anti-Aristotelian approach. Again, this is either a monumental contradiction, or there must be some way of harmonizing Bacon's empiricism with Kant's metaphysics. My approach will be to find harmony. However, I would first like to briefly pursue the logical alternatives engendered by a contradictory reading, of which there are three. If the text is taken as simply blatantly contradictory, we may be led to ignore Kant altogether, an option that some have taken, but which I will not pursue here. The two remaining options entail overlooking the contradiction and ignoring one or the other of Kant's stipulations; i.e., we can either ignore his oft repeated statements that "through the senses we do not apprehend the world," or his endorsements of normal science. Taking the former option means treating Kant's statements either as hyperbole, or simply as mistaken. Let us call this the "hyperbolic reading." Such a reading trivializes Kant's claims about the limits of our senses, but it allows the reader to concentrate on Kant's treatment of reason, which, after all is the real focus of the *Critique.* I think this approach seriously misconstrues the work and produces an impoverished understanding of the importance of the *a priori* in Kant's system. I will return to this, but first let us consider the other non-harmonizing approach, since it is interesting in itself and will also promote a greater understanding of

Kant's startling claims. Under this second approach, Kant's endorsements of Baconian science are simply ignored, and a transcendent science is urged. Let us call this the "transcendental" reading." A particularly clear exposition of this position is given by the Russian "mystic"[26] Ouspensky:

> . . . people do not know how to read Kant at all. . . Kant simply put the question, threw to the world the problem, demanding the solution but not pointing the way to it. . . Positive philosophy stands in a very ambiguous relation to Kant's views. It accepts them and it does not accept them: it accepts, and considers them correct in their relation to the direct experience of the organs of sense. . . [but] when it concerns itself with scientific experience. . . in which precise instruments and calculations are used, positive philosophy evidently considers Kant's views in relation to that invalid, assuming that "scientific experience" makes known to us the very substance of things, the true causes of our sensations—or if it does not do so now, it brings us closer to the truth of things, and can inform us later (1921/1970).

However, Ouspensky argues, if we take Kant seriously, we must regard scientific perceptions of reality as equally unreliable as original sense perceptions. His solution to the conundrum is to change the way we perceive reality, focussing especially on the primary points of subjective perception that Kant identified, space and time. Regarding space, Ouspensky observes that we perceive the world in three dimensions, but that it may have more dimensions than just these three. What would happen if we tried to perceive these other dimensions, and what would these other dimensions be? To get us

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[26] "Mystic" has become a pejorative term in academic circles (or perhaps always was). In scientific disciplines this is reasonable since, by definition, science can accept only empirically validated ideas. However, in philosophy, particularly in epistemology, the validity of science and empiricism itself is in question, so any line of rational thought is legitimate. Ouspensky eventually argues that only transcendent ideas, i.e., ideas from "higher" sources outside ourselves, can lead us to real knowledge. By taking this turn, he departs from the rationalist tradition, which, perhaps is why he is generally excluded from academic discussions. However, the arguments he uses to justify the transcendental turn are completely rational and in fact remarkably clear and compelling. Thus, these foundational arguments for transcendentalism should not be dismissed out of hand as mystical, even if the transcendental turn itself is rejected.

accustomed to this mode of thought, Ouspensky first asks us to consider what life would be like if we only perceived the world in two dimensions or in one[27]. He further observes that each higher dimension can be defined in terms of the dimension below it. Thus, the zero dimension is a point; one dimension is a line, or the "tracing" of the movement of a point; two dimensions is a plane, or the tracing of the sideward movement of a line; and three dimensions is a cube, or the tracing of the orthogonal movement of a plane. A regular rule of dimensions is suggested by this, namely, that each higher dimension is the tracing of an object of the next lower dimension, moving in a new direction. Given these relations, we might then suppose that the fourth dimension would be the tracing of the movement of a cube. But here we face a problem for we do not know how to move a cube in a direction not already described within it, though the law of higher dimensions seems to require that to reveal the next higher dimension one move the lower dimensioned object in a direction it does not already possess. Ouspensky suggests that this fourth dimension is *time*, since, like space, time is ever present, and every movement in space is also a movement through time, though we do not know what time is:

> Time is the most formidable and difficult problem that confronts humanity.
>
> Kant regards time as he does space; as a subjective form of our receptivity; i.e., he says that we *create time* ourselves, as a function of our receptive apparatus, for convenience in perceiving the outside world. Reality is continuous and constant, but in order to make possible the perception of it, we must dissever it into separate moments; imagine it as an infinite series of separate moments out of which there exists for us only one. In other words, we perceive reality as if through a narrow slit, and what we are seeing through this slit, we call the present; what we did see and now do not see—the past; and what we do not quite see but are expecting—the

[27] Ouspensky cites C.H. Hinton's *New Era of Thought* (1888) in connection with these ideas. Edwin Abbot's *Flatland* (1884) is another early work that treats the fourth dimension in similar fashion.

> future...But our perception concerning our "being in time" is entangled and misty up to improbability.
>
> First of all let us analyze our relation toward the past, present, and future. *Usually we think that the past already does not exist.* It has passed, disappeared, altered, transformed itself into something else. The future also does not exist—it does not exist *as yet.* It has not arrived, has not been formed. By the present we mean the moment of transition of the future into the past, i.e, *the moment of a transition of a phenomenon from one non-existence into another non-existence.* For that moment only does the phenomenon exist for us in reality; before it existed in potentiality, afterward it will exist in remembrance. But this short moment is after all only a fiction; it has no measurement. We have a full right to say that the present does not exist. We can never catch it. That which we did catch *is always the past!*
>
> If we are to stop at that we must admit that the world does not exist, or exists only in some phantasmagoria of illusions, flashing and disappearing.
>
> Usually we take no account of this, and do not reflect that our customary view of time leads to utter absurdity.
>
> (Ibid, p.33).

This is an exceedingly interesting discussion for several reasons. First of all, despite its "mystical" flavor, it is rationally compelling, displaying no clear fallacies. Time *is* universally construed as a tripartite continuum of past, present, and future, only the middle part of which exists; and this *does* lead to the "absurdity" of *nothing* existing, if the terms past, present, and future, are taken as postulates. On the other hand, escaping this absurdity seems to entail strange "mystical" leaps into four dimensional worlds, leaps that we can't easily approve of and which we can't execute in any case even if we did, because we don't know how to think in those terms. Moreover, though such notions as time travel and hyperdimensionality are ignominiously associated with science fiction, they actually do get considerable support from modern science, especially Einstein's theory of relativity, under which space and time are merged as spacetime. It is surely noteworthy that Kant identified precisely these two phenomena—space and time—

as completely subjective constructs, and that he deduced this from pure philosophy one hundred and forty years before Einstein and other physicists showed that Newtonian—i.e, common sense concepts of time and space—were flawed via mathematical reasoning. Finally, the fact that empirical science later vindicated Einstein's theories through experiments suggests that pure philosophical reasoning, mathematical reasoning, empirical science, and perhaps even some aspects of "mysticism" are not arbitrarily related. On the other hand, though modern physics shows that the common sense notions about time and space are defective, it doesn't reject them utterly, as a pure Kantian perspective might seem to demand. How then, can Kant's extreme position vis-a-vis "sensibility" be supported, and why would he take such a radical position anyway?

To answer this question, it is important to remember that Kant's primary purpose in the *Critique* is to develop a defensible metaphysics, which he defines as "a completely isolated speculative science of reason" (1929, p.21), not to develop a methodology for science itself. In fact, as we've seen, Kant seems entirely satisfied with Bacon's scientific methodology, so much so that he wants to use it as a basis for his metaphysics. In explaining this decision, Kant notes that successful natural scientists "learned that reason has insight only into that which it produces after a plan of its own, and that it must not be kept, as it were in, nature's leading strings" (ibid, p. 20). Kant requires the metaphysician to apply this same insight to metaphysics. According to Kant, metaphysics has two objects of study, first reason itself, and then reason's connection to reality. So, if we apply the analogy of natural science that Kant suggests, we see that just as the natural scientist must "compel the witness [nature] to answer questions which he himself has formulated" (ibid), so the metaphysician must compel *reason* to answer

his questions. By drawing this rather abstruse analogy, Kant argues that metaphysics has failed precisely because it has allowed itself to be led by nature, in that metaphysicians hitherto assumed an isomorphic relationship between sensible impressions and reality. Hence, Kant's "Copernican revolution" : by interpreting sensations not as impressions of reality, but, rather, as indications of our sensory apparatus, Kant attempts to separate pure reason from subjective impressions. His argument is that just as natural science must isolate some object of analysis by "compelling nature" to answer well chosen questions, so the metaphysician must isolate reason and then compel *it* to answer well chosen questions about the nature of reason and the relation of reason to reality. However, Kant also argues that reason cannot consider itself without some admixture of experience. Somewhat paradoxically, according to Kant, reason only becomes visible in conjunction with experience, even though sensible impressions themselves are purely subjective. This argument recalls the subtle move Kant made in his Introduction to the *Critique* when he wrote that though "all our knowledge *begins* with experience. . .it does not follow that it all *arises* from experience" (p.41; my italics). The common line that Kant pursues in both of these arguments is that reason and experience interact to produce true *knowledge* even though neither could do so by itself: ". . . [T]houghts without content are empty, intuitions without concepts are blind. . . only through their union can knowledge arise" (Ibid, p. 93). Hasty critics see this as contradictory because they assume that *subjective* sensory impressions should bear no relation to reality. But close reading of the *Critique* shows that this is not Kant's position. To the contrary, he is careful to point out that the subjectivity of sense impressions does not entail illusion:

> When I say that the intuition of outer objects and the self-intuition of the mind alike represent the objects and the mind, in space and time,

> as they affect our senses, that is, as they appear, I do not mean to say that these objects are a mere *illusion*. For in an appearance the objects, nay even the properties that we ascribe to them, are always regarded as something actually given. Since, however, in the relation of the given object to the subject, such properties depend upon the mode of intuition of the subject, this object as *appearance* is to be distinguished from itself as object *in itself*.
>
> (Ibid, p.88; italics original)

The key phrase here is "relation of the given object to the subject"; though "appearances" depend on "the mode of intuition of the subject," the *relation* between this mode and the object is *not* dependent on the subject[28]. An example may help. We see the world in color, meaning that we perceive objects as bearing color in themselves. However, empirical work on light and the human visual system tells us that color is just a mode of perceiving varying wave lengths, or frequencies, of light which are reflected from objects. Moreover, we know that we perceive only a small portion of the spectrum, from about 4000 angstroms, to about 7000 angstroms, and this fact is also consistent with color being a mode of perception rather than an inherent property of objects, since the limited range we see is consistent with our visual system, not the objects. And there are many other effects in vision that we can explain in terms of interactions between light frequencies and our visual apparatus, including various pathologies and illusions. But this would not be expected if either our sensible perception, or our extended empirical perception (i.e., "scientific" perception) were mere illusions[29]. Assuming a steady state world, the best way to explain a regular relation between different modes of perception is

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[28] Such relations should not be confused with relations *among* perceived objects. The latter, Kant insists, like the perception of objects themselves, are purely subjective. The difference is important.

[29] Visual illusions are not what Kant referred to when he said that sensible perceptions were not *illusions*. This is a problem of vocabulary. By *illusion* Kant meant something entirely disconnected from the source. Visual illusions, by contrast are not disconnected, which is why we can explain how they work.

to suppose that both modes are in some *real,* though unknown relation to the world. Crucially, however, the fact that there is a regular relationship between sensible and extended modes of perception does not mean that either mode allows us to see things in themselves. Just as wave lengths are transformed by our visual system into colors, some *other* mode of perception might transform wave lengths into yet another form, perhaps one unimaginable by us. Nonetheless, empirical work gives us true *knowledge* of the world, albeit always in terms of our modes of perception, which is evident from the fact that such knowledge can be used to make predictions and solve problems. It is important to note here that though the knowledge we acquire of the world through scientific investigation is always "subjective" in the sense that it is given in terms of our sensory apparatus, it is nonetheless real. Moreover, it follows from this that as scientific knowledge accumulates, particularly in cases where more and more data is brought under a system of principles, we can say that we are making real progress, in the sense that we really do know more about the world than we did before. This last point is often disputed by antifoundationalists on the confused ground that if all perceptions are subjective, then perception B, is, by definition, just as subjective as perception A (e.g., Rorty, 1998). From this axiom, it is then argued that we never get closer to the real nature of the world. But in making such pronouncements, antifoundationalists confuse "subjectivity" with "illusion." Illusions can be in any arbitrary relation to reality, but, presumably, our sensory apparatus is in some regular, rule governed relation to reality. Thus, Kant's insistence on the subjectivity of sensory perceptions does not foreclose scientific progress. To the contrary, under Kant's system the only way to acquire knowledge of the world is through scientific exploration since it is only through science that we can

triangulate our subjective perceptions, and it is only through triangulation that we can transcend these perceptions. Although the accumulated findings are still given to us in forms determined by our sensory apparatus—whether extended or not—and it is therefore true that we don't ever see things as they really are, we are nonetheless justified to believe that we are making progress in the sense that our models really are becoming closer to reality[30]. Ultimately, of course, we cannot prove that we are making such progress for that would require a comparison between our models and reality, and this we cannot provide. Nonetheless, it is a reasonable supposition that scientific models that are better in terms of our limited sensory apparatus—i.e., better in the sense that they unify and explain more data—are also *objectively* better. Suggestive analogies abound. Suppose for example that a blind man is asked to describe a room. For us, the room has certain qualities that are forever beyond him—the colors, the play of light and shadow, the overall visual effect of the décor. Nonetheless, he can make progress from banal descriptions, toward a description that more nearly approximates our impressions. His first cursory description might be limited to rough dimensions and major pieces of furniture, for example. But, if pressed, he could do much better, noting the textures of the materials, the area devoted to windows, etc. Though he will never tell us about light and shadow, colors and overall visual effects, his later descriptions would very likely conform more to our own descriptions—"reality" in this analogy—than his early, or "prescientific" descriptions. This is how Kant conceives of our position vis-à-vis reality; we can never see things as they are because we simply don't have the right apparatus. However, increasingly accurate descriptions, given in our own terms, do bring us closer

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[30] However, this is much disputed in the philosophy of science. I take this up at length in a later chapter.

to the way things really are. Later we will see that antifoundationalists badly misconstrue this point.

The skeptic can still raise several doubts here, however. For one thing, everything I've just said depends on a reasonably steady state world, but we have no guarantee of such. As Bertrand Russell once observed, in principle, there is no way to rule out the possibility that every morning we wake up either much larger or smaller than the day before. The universe and everything in it could grow or shrink by any arbitrary factor every day, and we would have no way to detect it. Yet, this fantasy is restricted in that everything would have to grow or shrink at once and at the same rate, else we would detect differences. Of course, one can posit a universe ruled by a mischievous God who adjusts our brains every day in just such a way as to prevent us from detecting the wild changes that are actually going on around us[31], but such skeptical fantasies can be ruled out on the same grounds that we rule out the possibility that reason itself is delusional, i.e., by the argument that if such is the case, no argument including the argument for skepticism can have validity. So, once again we are brought to a familiar stalemate; skeptical fantasies cannot be absolutely ruled out, but can never be ruled in either.

But it is at this point that the rationalist achieves a very thin victory over the skeptic. Though s/he can neither definitively establish the veridical nature of reason, nor that God doesn't play dice, to quote Einstein, yet s/he can go about reason's business without contradiction, whereas the skeptic who entertains either skeptical position lives in permanent and inescapable contradiction. Thus, Kant argues:

> . . . reason has, in respect of it's practical employment, the right to postulate what in the field of mere speculation it can have no kind

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[31] Descartes considers and discards this possibility.

> of right to assume without sufficient proof. For while all such assumptions do violence to [the principle of] completeness of speculation, that is a principle with which the practical interest is not at all concerned. In the practical sphere reason has rights of possession, of which it does not require to offer proof, and of which, in fact, it could not supply proof. The burden of proof accordingly rests upon the opponent.
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> (ibid, p. 617)

With these arguments, Kant provides an epistemological position I call "minimal foundationalism[32]." It is minimal because it assumes as little as possible—indeed no more that the radical skeptic assumes. Yet, as anyone who has ever climbed mountains knows, the difference between even the thinnest flake of a foundation, if it yet be secure, and nothing is profound. All science and analysis of every kind ultimately rests on this tiny ledge, whether or not the fact is acknowledged.

Minimal Foundationalism Versus The Antifoundationalist Paradigm

Since foundationalists are usually accused of claiming much more given territory than I am claiming here, I would like to position minimal foundationalism carefully within the paradigm of our "opponent," who in literacy theory, is certainly best personified by Stanley Fish. Here then is Fish's outline of the two major choices available to literacy theorists:

> . . . in general theory comes in two forms: although even as I say that I want to qualify it by saying that by some arguments the second form of theory is not properly theory at all. Be that as it may, the two forms of discourse that at least announce themselves as theory are foundationalism and antifoundationalism. By foundationalism I mean any attempt to ground inquiry and communication in something more firm and stable than mere belief or unexamined practice. The foundationalist strategy is first to

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[32] In the ensuing discussion I will freely alternate between the terms "minimal foundationalism" and "rationalism," simply because the latter term is often more felicitous. I hope I have established the point that rationalists must be minimal foundationalists.

> identify that ground and then so to order our activities that they become anchored to it and are thereby rendered objective and principled. The ground so identified must have certain (related) characteristics: it must be invariant across contexts and even cultures; it must stand apart from political, partisan, and "subjective" concerns in relation to which it must act as a constraint; and it must provide a reference point or checkpoint against which claims to knowledge and success can be measured and adjudicated. In the long history of what Derrida has called the logocentric tradition of Western metaphysics, candidates for the status or position of "ground" have included God, the material or "brute act" world, rationality in general and logic in particular, a neutral-observation language, the set of eternal values, and the free and independent self. Every foundationalist project assumes the existence and availability of one of more of these grounds, and it is from this assumption that its program of research proceeds in the direction of building and elaborating a model.
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> (1989, p. 343)

There are some devious rhetorical maneuvers in this passage that should be noted. Fish's project is to get us to accept antifoundationalism, but to do that he must show that that all foundations of any kind are faulty. Thus he begins by defining foundationalism in minimal terms—"[anything] more firm and stable than mere belief or unexamined practice"—thereby implicitly promising to refute that level of foundationalism. However, by the end of the passage he has modified his definition of foundationalism to include much more: "The ground so identified must have certain (related) characteristics: it must be invariant across contexts and even cultures; it must stand apart from political, partisan, and "subjective" concerns. . ." He thus substantively alters his own criteria in two ways, first by raising the level of certainty from "anything stronger than mere belief," to "invariance," and second by imposing a psychological aspect on the "ground" such that it is now no longer "anything," a principle or abstraction, for example, but rather a psychological mechanism that "stands apart from political, partisan, and "subjective" concerns. . ." Now, as it happens we can probably meet the demand for

invariance—though we wouldn't have to since even a strong trend would certainly be "more firm and stable than mere belief"—but we reject the psychological strictures out of hand as going far beyond minimal foundationalism. As to invariance, we will claim, following Kant, and a long line of rationalists up to the present day that proper reasoning *invariably* leads to truth provided that you start with true premises (Samuels, Stich, and Faucher, 2000). However, we insist that reason is an abstraction, the process of applying logic to problems, which is neither exhaustively nor consistently displayed in human behavior. Thus, although all normal humans have a capacity to reason (Denny, 1991; Samuels, Stich, and Faucher, 2002), they do not do so flawlessly. To the contrary, cognitive psychologists are "rather pessimistic" about the average human's reasoning ability (ibid).

Admittedly, there are some major leaps here. First of all, our opponent might well demand that we rigorously define "proper reasoning." Now, in a general investigation of epistemology, this would be a fair demand; however, again following Kant, we will maintain here that it is *not* a demand the minimal foundationalist must meet in order to nullify antifoundationalist critique. If it were, the antifoundationalist could always raise the bar, demanding ever greater levels of evidence and analysis for every proposition put forth. But with respect to evidence, there is radical asymmetry between foundationalists and antifoundationalists: the former accept evidence as part of normal research, but the latter *reject evidentiary procedures out of hand.* If they didn't they wouldn't qualify as antifoundationalists for they would be relying on something more firm than "mere belief or unexamined practice." Thus, though a vaguely defined "sense of reason" is not at all adequate for a fully developed epistemology, it is perfectly

adequate as the basis of minimal foundationalism and also an adequate response to antifoundationalists[33]. Minimal foundationalism simply asserts that there is some property of the mind/brain, which is commonly called "reason," that reliably leads to true conclusions *if properly applied.* The evidence for this is 1) the fact that all normal people have *a priori* convictions about the world, which seem *necessarily* to be true; and 2) the fact that application of this reasoning ability combined with empirical research reliably leads to valid conclusions about the world. The first claim is easily verified through introspection along the lines that Kant argued, and the second is verified by the tremendous success of science, particularly over the last four hundred years since Bacon and others initiated the empirical research practices of modern science. Crucially, it isn't necessary that the applications of reason in either case 1) or 2) be perfect, in the sense of *always* leading to *absolutely* true conclusions. All that is necessary to establish minimal foundationalism is to show that reason is "more firm and stable than mere belief or unexamined practice." I submit that there is overwhelming evidence that this is so, and that therefore, the burden is on the antifoundationalist to show, *through specific case analysis* that it is not. But, to belabor a point I've made many times already, this places the antifoundationalist in an irresolvable paradox, for the only way to show that reason is unreliable is to present reasonable analysis and evidence that supports that claim. In fact, there have been some attempts along these lines, but, though they do support the conclusion that people don't *always* behave reasonably, they do nothing to impugn

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[33] Again, I am borrowing this entire line of reasoning from Kant, in this case a passage toward the end of the *Critique* in which he reiterates the point that "for our complete equipment we require among other things the hypotheses of pure reason. For although they are but leaden weapons, since they are not steeled by any law of experience they are yet as effective as those which our opponents can employ against us (p.618).

reason itself. The most famous and oft cited example of this kind is Kuhn's monograph, *The Structure of Scientific Revolutions* (1970), which argues that science, as an institution, does not always proceed according to the best evidence of science. Rather, numerous social factors intervene in the selection of theories, and in the promotion of scientists themselves, such that there is not a straight line of scientific development predicated solely by scientific evidence. But all this shows is that social factors are also influential, and this is a finding that is in no way incompatible with minimal foundationalism. To the contrary, reasonable social constructivism—and Kuhn's findings[34] easily fit within that paradigm—is not only compatible with minimal foundationalism but is required by it, and this fact has been recognized at least since Bacon. This is probably the greatest point of misunderstanding among the antifoundationalists. They continually pose the conflict as an either/or issue: either *everything* is a social construction, or *nothing* is. But there is no reason to accept that sort of dichotomy. Social factors are just that, *factors*, which, combined with other factors, such as facts about the material world and cognitive processes, including reason, collectively explain human behavior. Crucially, however, this does not entail that foundationalist analysis can always, or for that matter ever, exhaustively explain human behavior or anything else for that matter. *Theoretically*, according to foundationalist tenets, all phenomena are explicable according to nomological processes. However, we do not know all of these processes, nor can we collect all the data necessary to trace them even if we did. Again, this is a point antifoundationalists continually confuse, or perhaps deliberately misrepresent in caricatures of foundationalism. Thus, for example, Fish

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[34] Kuhn seems sometimes to have interpreted his own findings in a strong constructivist way, but the

triumphantly quotes Scholes' characterization of foundationalist epistemology: "a complete self confronts a solid world, perceiving it directly and accurately, always capable of capturing it perfectly in a transparent language" (in Fish, 1989, p. 344). But this is a description of some bizarre brand of naïve dogmatism, which is far removed from minimal foundationalism and also completely at odds with science. To the contrary, pursuant to Kantian notions of subjectivity, it is *because* we know that we do *not* see things as they are, that we have science in the first place. Antifoundationalists continually confuse and misrepresent this point. They present science as a naïve perspective that imagines itself to be immune to distortions, particularly cultural and linguistic distortions, and they then go on to claim that all perspectives are nothing more than reflections of cultural belief systems, mutually reinforced by intrinsically biased "vocabularies[35]", with no objective substance. In effect, then, they argue that we are perpetually caught in a vicious circle with respect to discovering reality. Since we begin from a distorted position, all subsequent extrapolations from that position will be equally distorted. But this would only be true if 1) we were unaware of the possibility of original distortions, and 2) we lacked the ability to gradually correct our perspective by adjusting our models to fit new discoveries.

The Virtuous Circle in Science

But it is possible to build such awareness and readiness to correct for error into research. This second perspective on the problems of subjectivity, which is precisely the perspective Bacon argued for, is sometimes referred to as a "virtuous circle" by way of

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findings themselves are amenable to reasonable constructivist analysis.

direct comparison to antifoundationalists' vicious circles[36]. Mary Hesse cites Habermas's "learning machine" as a typical example of a virtuous circle:

> In discussing natural science Habermas makes frequent use of the concepts of successful prediction, feedback, and self correction. In effect this is to appeal to a model of natural science as a learning machine. . . The presence of feedback loops in a learning machine allows for the circular self-correction of theory by experience and experience by theory that is demanded by interpretation of science as theory-laden. 'Experience' must be regarded in the model as the input or physical stimuli impinging upon the machine from its environment. The process of describing experience in intersubjective language by the scientific community is representable as the coding of the input into machine language according to whatever categories have been programmed into the machine from the current natural language. Doubtless the coding devices will *also* be subject to modification in the light of feedback from successful and unsuccessful learning by the machine of its environment, just as the natural descriptive language of a human learner may be modified. Thus the physical stimuli themselves need not be expressible in any stable language, and it must be a *hypothesis* that they themselves remain sufficiently stable for what is learned by the machine to be applicable and testable on future occasions.
>
> (Hesse, 1980, p. 174)

This description shows a virtuous circle to be dependent on two principles, both of which we have already derived from Kant's system. First, there must be stability—a steady state world—and second there must be some mechanism of self correction in the system. Hesse's account is explicit regarding the need for stability, but it scants the details on self correction. In her account the machine self corrects, but this begs the question, "How?" In fact, a machine cannot correct for anything it has not been *programmed* to correct for, as we all know well from experience. Humans, however, can self correct, by virtue of

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[35] This is a favorite word of Rorty's, which when used by him, refers to much more than words. Rorty takes a radical Wittgenstinian view of language, so, for him, language and thought are inseparable and "vocabularies" carry within them entire ready made systems of thought.

[36] Hirsch (1967, p. 259)) cites Dilthey as the originator of the idea of virtuous hermeneutic circles.

their *a priori* property of reason. Admittedly, this does not move the question along very far. One has a right to press further and ask how it is that reason functions, and how it came to be so conveniently installed in humans to begin with. However, I will not pursue these questions here. Kant's *Critique* is an attempt to answer the question about function, and there is a vast literature that debates to what degree he succeeded and much more literature that attempts to explain reason in other terms. As for the question about origins, the suggested answers and endless debates are familiar—God, evolution, some undreamed of force—but it is not necessary for our purposes to answer such questions; my contention is simply that humans do have an *a priori* reasoning ability and that denial of this fact inevitably leads to incoherence. Antifoundationalists may complain that resorting to a foundation which itself is ungrounded is contradictory. However, rationalists would note first, that this complaint itself is rationally motivated[37], and hence paradoxical, and second that there is a difference between ungrounded and unexplained foundations. Logically, everything must have some etiology, including reason itself, but we do not have to know a thing's etiology to establish the fact of its existence. Philosophers and scientists alike have long conceded that questions about ultimate origins are probably forever unanswerable. On the other hand, there is a literature on "evolutionary psychology" that does attempt to show how cognitive functions such as general reasoning ability could have evolved (e.g., Pinker, 1997). So far, however, this literature seems to be nothing more than speculation; if humans evolved from lower

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[37] By definition, any objections to contradictions are *rational* objections. In ordinary conversation, courts of law, scientific discourse, and even self proclaimed antifoundational discourse, contradiction is recognized as illegitimate, and this is true universally. But when we ask why, we find that all we can say is that contradiction violates reason.

primate forms, then reason must be a mutation, or a series of mutations. However, no one has any good story about the specific steps that could have led from animal cognition to human, nor any evidence to support the conjectures. Nonetheless, among cognitive psychologists human reasoning ability is regarded as a fact.

In a scientific community, it is this inexplicable property of reason that makes the research circle virtuous rather than vicious. Problems are typically addressed by many minds working together or independently on the same issues. Although every step of the process, i.e., original theory construction, data collection, data coding, and interpretation, is subject to error, the community as a whole can successfully adjust the model by focussing on areas that seem anomalous. In such a virtuous circle there is an assumption that the system is never overwhelmed by error, but there is no assumption of perfection. Moreover, it is certainly possible for there to be all kinds of outside interference, including interference from social factors[38], along the lines that Kuhn described, and there is no guarantee that reason will win out in the end. Indeed, there have been long periods in history where reason has been violently suppressed and no progress was made. Sometimes this fact is used to argue for strong social constructivism; however, such arguments conflate large scale social movements with individual cognition, which is a mistake. Society may be irrational on the whole, in the sense that it does irrational things, but this does not mean that individuals are. Moreover, though suppressing rational discovery is irrational from a macro point of view, from an individual perspective it may not be, at least not in the short term. For example, the church hieracrchy in the

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[38] Although social factors emanate from individuals they are "outside" the theoretical circle of research Hesse describes.

middle ages understood quite well that their power was threatened by the rise of science and so they suppressed science for the *rational* if supremely selfish reason that they desired to retain power. Such suppression and subversion of reason continues everywhere, but that fact does not impugn reason itself. To the contrary, history supports the view that when reason prevails at the macro level in society, progress is made and life generally improves for most people.

Returning to the question of knowledge, the difference between antifoundationalists and rationalists can be described as a difference between those who believe in the inevitability of vicious circles and those who believe that virtuous circles are possible. Antifoundationalists point to the fact of continued "situatedness" as an insurmountable bar to objectivity, whereas rationalists acknowledge the impossibility of complete objectivity but focus on overall models of the world, in the belief that continued research and adjustment generally leads to models that are increasingly powerful in terms of their ability to unify and explain data. This criterion, however, could also be questioned. Why should we prefer models that are more "powerful" in these terms? the standard answer is Occam's Razor: "that which can be explained by recourse to fewer assumptions is explained in vain by more." Yet this principle too can be questioned, and so on. Thus, at some point we have to admit that it just seems more *reasonable* to prefer elegant, or powerful models to haphazard or weak ones. The entire debate, then, comes down to whether or not we should accept rationality as a foundation for knowledge.

Since this is the crux of the debate, it is important to establish that antifoundationalists do, in fact, reject rationality. Actually there is a lot of squeamishness about this, and no doubt many nominal antifoundationalists would not

readily admit to rejecting rationality. However, according to antifoundationalist definitions, they *must* reject it or renounce antifoundationalism. As I've already argued and will continue to demonstrate, antifoundationalist arguments always fail to maintain consistency in this respect, which should be enough to discredit antifoundationalist "theory." It is a curious fact, though, that antifoundationalists continue to publish obviously contradictory articles which receive acclaim from the philosophical and literary community. Perhaps the reason for this is that most people do not fully comprehend that antifoundationalism entails the rejection of reason. The proclivity of humans to reason is so strong and our expectation that others will be rational so overwhelming that it is natural to forget this underlying rejection when reading antifoundationalist literature. Thus, antifoundationalists are able to smuggle reasoned argument into their articles while simultaneously claiming no foundations. But this is an outright contradiction since they explicitly reject reason as a foundation. We must be clear about this; either we accept reason as reliable and therefore foundational, or we have to reject it entirely. There is no half-way point where we can pick and choose to apply reason some of the time and attack it as a social construction at other times, though this is what antifoundationalists often do. For example, by including "rationality in general and logic in particular" in his list of "discredited" foundations (see quote, p.65), Fish explicitly rejects rationality[39], yet throughout the same essay he employs common principles and devices of reasoned argument, such as inferring cause and effect relations, stipulation of historical facts as a preliminary to conjecture, presentation and analysis of contradictory statements, etc., all while simultaneously accusing other self-described antifoundationalists of departing from

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[39] He later squeaks out of this statement, by claiming a "community rationality" that the individual can

strict antifoundationalism through "reinvention of foundationalism by the very form of thought that has reduced it to ruins" (1992, p. 349). A veritable feast of paradoxes.

Pure Empiricism

As we've seen, antifoundationalist attacks on the inviolability of reason are nearly always launched from a social constructivist position. Since this is the main point of confrontation for rationalists, I have devoted all of my discussion to this quarrel. However, occasionally, similar doubts are raised from within the scientific community itself, based on quite different premises. In particular, some materialist cognitivists, i.e., cognitive scientists who assume a material basis for all cognitive functions, have trouble with *a priori* claims because they do not know how to account for them in a materialist model. If reason is to stand as a foundation, then, we must answer these objections as well. Let us take a quick look, therefore, at one such discussion, in this case, one by Patricia Churchland and Terrence Sejnowski, both prominent cognitive scientists:

> The central epistemological question, from Plato on is this: *how is representation of the world by a self possible?* So far as we can tell, there is a reality existing external to ourselves and it appears that we do come to represent that reality, and sometimes even to know that its initial appearance to our senses differs from how it actually is.[40] How is this accomplished, and how is knowledge itself possible? How is science itself possible?
>
> The dominant philosophical tradition has been to try to resolve the epistemological puzzles by invoking mainly intuition and logic... Plato, Descartes, and Kant are some of the major historical figures in that tradition. . .
>
> In a recent departure from this venerable tradition of *a priori* philosophy, some philosophers have argued that epistemology itself

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access. I comment on this later.

[40] Note that Kantians would not agree with the implication of this statement, i.e., that we come to see things as they are, though they could agree that we get closer to that ideal.

> must be informed by the psychological and neurobiological data that bear upon how in fact we represent and model the world...
>
> If, as it seems, acquiring knowledge is an essentially biological phenomenon... then there is no reason to expect that brains should have evolved to have *a priori* knowledge of the true nature of things: not of fire, not of light, not of the heart and blood, and certainly not of knowledge or of its own microstructure and microfunction... Innateness is no guarantee of truth, and it is the truth that *a priori* reflections are presumed to reveal...
>
> Whether our intuitive understanding of the nature of our inner world is at all adequate is an empirical question, not an *a priori* one.
>
> (Churchland and Sejnowski, 1990)

Here again we have a deliciously paradoxical discussion in that it uses *a priori* reason to question the possibility of *a priori* reason. Note also, however, that C&S seriously misconstrue Kant when they allude to *a priori* "reflections." As Kant made clear, *a priori* convictions ("reflections") must be combined with empirical evidence; reflections by themselves lead nowhere. Thus, he would have agreed with C&S that questions about the adequacy of our representations are empirical, though he would have insisted that empiricism without reason was *blind.* Nonetheless, C&S pinpoint the fundamental problem. True enough, there is no reason to suppose that reason is veridical—*but, at the same time, it is impossible to doubt it!* C&S have conflated two problems here that need to be kept separate. It is one thing to ask what the physiological basis for reason is, but quite another to ask whether it is a reliable function. The first question we might hope to eventually answer through the sorts of empirical research they suggest, but the second question will remain forever out of bounds. For to answer it in the negative would entail contradiction and endless paradox, since it is only through reason that such an answer could come.

Though C&S miss this point, they concede that at present they cannot do without the concept of *a priori* reason, though they denigrate it to the status of a "folk theory":

> The methodological point is that in science we cannot proceed with no theoretical framework, so even intuitive folk theory [i.e., Kant's theory[41] of the *a priori*]is better than nothing as the scientific enterprise gets underway.
>
> (P. 345)

Note that C&S here employ the notion of the virtuous circle, thereby validating Hesse's and Habermas's theoretical notions of how science proceeds. However, they don't seem to appreciate that reason itself stands in a unique position in this circle. In fact, reason is the corrective mechanism in the circle, a position that is never occupied by anything else[42]. We can ask how reason operates, how it originates, and so forth, but we cannot ask whether it is valid. For that would make the circle vicious: reason will forever proclaim itself valid and so we must accept that it is.

In Chinese mythology there is a famous character called the Monkey King, who through wit, pluck, and divine favor develops extraordinary magical powers: he can leap over mountains, transfigure himself into various animal shapes, and vanquish armies single-handedly with his cudgel. Not surprisingly, this monkey acquires a rather inflated view of himself and his importance, so much so, in fact, that the Buddha himself

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[41] That they could label Kant's system a "folk theory" is especially revealing of C&S unwarranted faith in naked empiricism. Kant's framework may be wrong, but it was certainly no "folk theory," since by definition, folk theories are theories held by the common people, the "folk," and Kant's system has never been known outside limited academic circles. C&S seem to be equating anything that is not based on "modern empiricism" with "folk theories" a serious error in my opinion.

[42] In conscious learning I think this is correct. However, there may be cases like first language learning where some other mechanism—UG—in this case drives the circle. Language theorists often invoke the notion of "hypothesis formation and testing" in language learning, but what they refer to is an unconscious process. This raises the interesting question whether reason can operate within individuals without their conscious participation. In the language learning case, perhaps UG, which can be thought of as specialized knowledge of linguistic principles interacts with general reason at an unconscious level.

eventually notices him and decides to interview him. Summoning the Monkey King to his palace, the Buddha requests to see a demonstration of his powers. So, the Monkey King leaps high into the air from whence his is able to survey a mountain range composed of five long ridges and five corresponding valleys. Flying with all his might, he crosses two of these and lands on the neighboring ridge, where he cleverly marks a spot with an "X" in his own urine; he then leaps back into the air and returns to the Buddha to whom he boasts of his achievement and the proof that he left behind. But the Buddha only smiles and holds out his hand. And there on his third finger, unmistakable by its stench and shape, is the mark of the Monkey King, who then realizes that it is vain indeed to attempt to escape the very power that sustains him.

Relying heavily on Kant, I have argued here that reason is to knowledge and argument, as the Buddha was to the Monkey King. Though it's conceivable to reject reason on extreme skeptical grounds, it is not possible to escape it through argument, and all attempts to do so end in paradox. Pursuit of knowledge begins with this understanding while rejection of this premise—read "foundation"—leads inevitably to confusion. I would hasten to add here, however, that metaphysics is not entailed by this conclusion. In this sense, the Monkey King myth is misleading since it suggests that only transcendent sources could serve as foundations. But I do not mean to suggest this. Rather, the tale is used only to emphasize the impossibility of escaping reason through argument. Crucially, however, this does not entail transcendence or any kind or reliance on the supernatural. It is conceivable that reason is simply part of our genetic inheritance due to evolution, and this, in fact, is a common argument among "evolutionary psychologists," like Pinker (1997), Plotkin (1997), and many others.

A final remark regarding the term "antifoundationalism" and its use throughout philosophy, linguistics, and literacy studies is in order here. In the above discussion I have carefully defined "antifoundationalism" in Fish's terms, since he is somewhat of an authority on the doctrine. As we've seen, for Fish, antifoundationalism extends to every domain of human experience, including "rationality in general and logic in particular." However, there are others who use the term in a far less encompassing manner. Chomsky, for example, endorses a mild form of antifoundationalism:

> We may simply adopt the standard outlook of modern science, in essence, the anti-foundationalism of the seventeenth century reaction to the Cartesian skeptical crisis, as Richard Popkin describes it: "the recognition that absolute certain grounds could not be given for our knowledge, and yet that we possess standards for evaluating the reliability and applicability of what we have found out about the world," thus "accepting and increasing the knowledge itself while recognizing that "the secrets of nature, of things-in-themselves, are forever hidden from us (Popkin 1979: 139ff)
>
> (Chomsky, 2000, p. 77)

Crucially, this "anti-foundationalism" is limited to facts about the world, in keeping with the Kantian perspective I've outlined above, while leaving intact appeals to *a priori* faculties of reason—thus the "standards" Popkin refers to. Unfortunately, there are many instances of the terms "antifoundationalism" and "non-foundationalism" in the literature which are not so carefully defined, which leads to significant ambiguity in many cases. At any rate, it should be clear that that Chomsky's "anti-foundationalism" is essentially the same as the "minimal foundationalism" I have developed here. In my view, the latter term is far more appropriate, since, as I've argued, reason— provides a firm foundation for science and rationalism is therefore not antifoundational. But nothing substantive is at stake here.

Summary

In this chapter I have developed a general epistemological position that stands in sharp contrast to antifoundationalism, though it shares some of the same assumptions, particularly the point that direct knowledge is impossible because everything is mediated through our particular human sensory apparatus, which itself is further influenced by social factors. Though agreeing with antifoundationalists on these points, minimal foundationalists assert that knowledge of the world is nevertheless possible through the application of reason—which is a universal, *a priori* human function—in conjunction with sensory experience, particularly through the process of hypothesis formation and empirical investigation known as science. Although we cannot prove that reason is valid, it cannot be disproved either, so we, therefore, can dispense with radical skepticism. Ultimately, then, that is from a purely abstract position, minimal foundationalists are deadlocked in an eternal stalemate with antifoundationalists. However, in practice, minimal foundationalists have the advantage, for they alone have consistency between their epistemological claims and their actions. As Hume noted, even skeptics follow reason in their ordinary activities, reserving skepticism for "philosophy." Thus the skeptic lives in continual contradiction. But minimal foundationalists are in the happy position of being able to live the philosophy they espouse. Minimal foundationalists can also point to vast amounts of data compiled over many centuries that shows precisely how the application of reason and empirical investigation, according to foundationalist assumptions[43], led to thousands of apparently

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[43] Usually not *minimal* foundational assumptions, which is why deconstructionist critics have some legitimate work to do. Nontheless, because all brands of foundationalism encompass minimal

valid discoveries. Moreover, everywhere in human discourse, we find common appeals to reason, and this is strong evidence that reason is universal and *a priori.* Our opponent, the antifoundationalist, on the other hand, rejects all foundations, arguing instead for a vicious circle: "in every case these entities [previously identified as "the world, language, and the self"] along with the procedures by which they are identified, will be inextricable from the social and historical circumstances in which they do their work [sic] "[44] (1989, p.345). Although antifoundationalists include "rationality in general" among the foundations they reject, they also claim that the community, which supposedly creates the inalienable "situation" that every person is "possessed by" "enables [the "subjects"] rational acts" (Fish, 1992, p.346). However, they vehemently deny that individuals can act or think outside of their communities. Note what a strange claim this is and what bizarre entities are entailed by it if it is taken literally. Individuals cannot reason, yet, somehow, "communities" can, though only *through* individuals. A literal interpretation would have to posit some community mechanism that "reasons" independently of individuals and then speaks through individuals. But no one has ever found any evidence of such an entity, and the idea is contrary to everything we know about thought, not to mention biology and physics. Currently in psychology and cognitive science, everyone agrees that whatever thought is, it occurs solely within individuals, as a result of some at present very poorly understood neuronal activities. Thus, the only way to reconcile Fish's claims with cognitive science is to interpret them

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foundationalist tenets—i.e., the legitimacy of reason and empirical research—they have often been quite successful despite the baggage of unsupported assumptions.

[44] I doubt that Fish really believes that "the world" or "language" does work and is affected by social factors, so probably he means "their" to refer only to "selves."

metaphorically; i.e., to say that he means that individuals are always *influenced* by their communities. But then the claim is reduced to a garden variety claim of social influences, which no one disagrees with. Minimal foundationalists agree that social factors (as well as cognitive factors) alter the perception of reality; they disagree that this is an insoluble problem. To the contrary, minimal foundationalists insist that science is the answer to that problem. Moreover, we insist that it isn't necessary to be completely free of social factors, or "situatedness" to discover truths. To the contrary, the power of science is that it allows us to discover how things are[45] *despite* how they appear. Finally, we allege that it is precisely here—in this fundamental misunderstanding of science—that antifoundationalists are most seriously mistaken. In the next chapter I will explore how these radically different epistemologies affect theories of language.

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[45] Though, per Kant, only in terms of our human apparatus

III: NATIVISM

In the last forty years or so there has been a pronounced shift in psychology and linguistics away from behaviorist theories—which theories are in fact compatible with radical social constructivism—and toward innatist or nativist theories. Obviously, should nativist theories take hold in literacy studies that would seriously undermine the tenets of antifoundationalism because such theories tend to show that individuals are not solely, or perhaps not even primarily, "constructed" by social factors, but are, rather, the product of the interaction between rich, innate cognitive mechanisms and sensory data, including, but not limited to social data. Interestingly, while for the most part cognitive scientists simply ignore postmodernism and strong social constructivism, in this case they too have noticed the conflict:

> Rationalists are nativists by definition; and nativism is where cognitive science touches the real world. As both Pinker and Plotkin rightly emphasize, the standard view in current social science—and what's called "literary theory"—takes a form of empiricism for granted: human nature is arbitrarily plastic, and minds are social constructs. By contrast, the evidence from cognitive science is that a lot of what's in the modules[46] seems to be there innately. Pinker and Plotkin both review a fair sample of this evidence, including some of the lovely experimental work on infant cognition that psychologists have done in the last couple of years, a lot of indirect evidence that points to much the same conclusion: All human languages appear to be structurally similar in profound and surprising ways. There may be an alternative to the nativist explanation that linguistic structure is genetically specified; but, if there is, nobody thus far has had a glimpse of it.
>
> Cultural relativism is widely held to be politically correct. If so, then, sooner or later political correctness and cognitive science are going to collide. Many tears will be shed and many hands will be wrung in public. So be it. If there is a human nature, and if it is to

[46] Fodor refers here to "modules of the mind"; for those unfamiliar with this idea, Fodor has long held that the mind is composed of 'encapsulated' modules that each do a specific kind of cognitive work, according to specific and exclusive algorithms. See, especially, Fodor, 1983.

> some interesting extent genetically determined, it is folly for humanists to ignore it.
>
> (Fodor, 1998, p. 207-208)

Fodor's comment that "rationalists are nativists by definition" underscores the tight connection between Kantian philosophy and contemporary cognitive science and psychology. Kant derived his notions of the *a priori* from pure philosophy; now these ideas are receiving considerable support from empirical studies of the brain/mind. There is now massive evidence that human beings are innately endowed with a lot of cognitive structure.

Antifoundationalists' Rejection of Nativism

Yet, as Fodor points out, the antifoundationalist camp seems determined to ignore these facts. Bizzell, makes this clear when she admiringly quotes Fish asking a characteristically rhetorical question in response to this remark by Lynn Cheney:

> The humanities are about more than politics, more than social power. What gives them their abiding worth are truths that pass beyond time and circumstance—truths that, transcending accidents of class, race, and gender, speak to us all.

By which remark, as Bizzell notes, "Cheney asserts that there are texts which contain true knowledge. She assumes that everybody has the capabilities to discern the importance of these texts, simply by virtue of being human—they speak to us all" (Bizzell, 1992, p.264). Rational criticism of Cheney's comment would involve demonstrating, through extensive presentation and analysis that the universal themes Cheney alleges, do not in fact exist, but Bizzell provides none of that. Rather she quotes Fish making what she apparently considers to be a show stopping rhetorical rejoinder to Cheney's assertions:

> Once you have subtracted from the accidents of class, race, gender, and political circumstance, what is it that you have left?

to which, Bizzell supplies the answer: "The correct antifoundationalist answer to this question is, nothing" (ibid, p. 264).

The odd thing about claims like this in the antifoundationalist literature is that though they are usually delivered rhetorically, yet they accorded the status of evidence. In this case for example, it is clear from the surrounding arguments that Bizzell regards Fish's *tabula rasa* claims as credible, and moreover, that she expects her readers to accept them also. However, she offers no empirical evidence of any kind to support this extremely controversial position, relying completely instead on Fish's disparaging remark as if that were enough to settle the very important and perennial question of whether there is an innate foundation for human thought. In presenting the argument this way, she is not only making a substantive claim, but also attempting to establish rhetoric itself as a legitimate alternative to reasoned argument, a position that is explicitly endorsed by other antifoundationalists, notably Fish (1998) White (1985, p.45) and Rorty (1979; 1995; 1998). But adopting such a position is bound to lead to contradictions because what is rhetorically "useful" in one context may be damaging in another. Bizzel appears to have been led into just such a contradiction regarding nativism, for in another essay in this same collection, she completely contradicts the antifoundationalist, *tabula rasa* position by endorsing Chomsky:

> Composition specialists generally agree about some fundamental elements in the development of language and thought. We agree that the normal human individual possesses innate mental capacities to learn a language and to assemble complex conceptual structures
> (1992, p. 76)

Note that Bizzell claims "general agreement" about this unmistakably Chomskyan doctrine, not just personal opinion; thus, even if she changed her mind later on—which is certainly her privilege—her later claim about implicitly understood *non*nativism, i.e.,

strong social constructivism, within the same community that, according to her, previously endorsed Chomskyan nativism demands an explanation, but none is ever offered.

To round out this set of contradictions, I quote Fish on Chomsky and Fish on Bizzell. First here's Fish giving an "example" of a "foundationalist model," indeed the very model Bizzell claimed general agreement about:

> As an example of such a [foundationalist] model, it is convenient to recall the linguistics of Noam Chomsky, at least in its early or pure form.[47] As Chomsky himself describes it, the goal of his theory is the construction of a "system of rules that in some explicit and well-defined way assigns structural descriptions to sentences" and does so in a way that "does not rely on the intelligence" of the assigning agent, of whose beliefs, cultural situation, educational experience, etc., the system remains independent. What it does rely on is a set of formal constraints which have the characteristics I have already cited: abstractness, generality, invariance across contexts
>
> (Fish, 1992, p. 343).

Fish provides no direct commentary on this passage, merely extrapolating from it to his conclusion about the "foundationalist project":

> In short, the successful foundationalist project will have provided us with a "method," a recipe with premeasured ingredients which when ordered and combined according to absolutely explicit instructions—and the possibility of explicitness is another foundationalist assumption—will *produce*, all by itself, the correct results. In linguistics that result would be the assigning of correct structural descriptions to sentences; in literary studies the result would be the assigning of valid interpretation to poems and novels; and in the teaching of composition the result would be the "discovery of rules that are so fundamental as to be universal," rules that if followed would lead directly to coherence, intelligibility, readability, persuasiveness, etc.

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[47] Fish gives no explanation of what he means by "pure" forms of Chomskyan theory, nor just when, and how, Chomsky allegedly descended into "impure" versions of his early work. Chomsky has been remarkably consistent over the years regarding his central claims, as is evident, for example, by the fact he continues to quote his early work approvingly to the present day. Thus, Fish's allegations are mysterious.

(Fish, 1989, p. 343)

Here, Fish once again employs the ubiquitous antifoundationalist strategy of presenting any claims to scientifically derived knowledge, however limited in scope, as total, and therefore, ipso facto, not credible. But any fair reading of Chomsky shows that he has generally been scrupulous about limiting the scope of his claims to particular, well supported facts, and, furthermore, that he continually reminds his readers that the search for explanatory principles behind those facts is one that, like all scientific models of reality, changes as new discoveries are made. True, the *goal* of generative linguistics is a complete descriptive/explanatory model that explicitly defines the rules behind language use—just as the goal of other sciences is a complete explanatory model of whatever is the focus of these sciences—but Fish conflates these rationalist/scientific goals with putatively claimed results, thereby falsely imbuing the scientists he maligns with dogmatism, and scientistic myopia. But a fair reading of the primary works tells a very different story. Here's a typical example of Chomsky's oft repeated remarks about the scope, goals, and limits of his mode of inquiry into the nature of language and mind:

> We observed that it is the task for the brain sciences to explain the properties and principles discovered in the study of mind. More accurately, the interdependency of the brain sciences and the study of mind is reciprocal. The theory of mind aims to determine the properties of the initial state S_0 and each attainable state S_L of the language faculty, and the brain sciences seek to discover the mechanism of the brain that are the physical realizations of these states. . . In principle, discoveries about the brain should influence the theory of mind, and at the same time the abstract study of states of the language faculty should formulate properties to be explained by the theory of the brain and it is likely to be indispensable in the search for mechanisms. To the extent that such connections can be established, the study of mind—in particular of I-lanauge[48]—will be assimilated to the mainstream of the natural sciences.

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[48] "internal language" I explicate this term and its ramifications for literacy studies later in this chapter.

> So little is now known about the relevant aspects of the brain that we can barely even speculate about what the connections might be. We can, however, imagine how they might be established in principle, however remote the goal.
>
> (1986, p. 39)

In other passages Chomsky is similarly modest about our current state of knowledge. Even about syntax, his own area of expertise, he continually characterizes our collective knowledge as preliminary and provisionary, and even possibly altogether wrong:

> Of course there is no guarantee that this way of approaching the problems of (1)[49] in Chapter 1 is the correct one. This approach may turn out to be thoroughly misguided, even if it achieves substantial success—just as a theory of valence, etc. might have turned out to be completely off the track, despite its substantial success in nineteenth-century chemistry. It is always reasonable to consider alternative approaches, if they can be devised, and this will remain true no matter what successes are achieved. The situation does not seem different in principle from what we find in other areas of empirical inquiry.
>
> (ibid, p. 24)

This sort of rational theory construction, tempered by empirical investigation, i.e., normal science, in no way resembles the dogmatic "foundationalism" Fish lampoons, yet Fish categorically identifies Chomskyan linguistics as a typical foundationalist theory that "fails, lies in ruins, because it is from the first implicated in everything it claims to transcend" (1992, p. 345). As far as the accusations of dogmatism go, it is clear from examining the literature, as we've done briefly above, that Fish is attacking a straw man. The other charge, about being "implicated in everything it claims to transcend," is just a fancy way of alleging a vicious theoretical circle. But as I've already argued at length, such charges can't be made *en bloc* because to do so is to assume the conclusion one is

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[49] The problems referred to are: " (i) What constitutes knowledge of langauge? (ii) How is knowledge of language acquired? (iii) How is knowledge of language put to use?" I take up these "problems" again later in this chapter.

supposedly arguing for. If someone's theory is viciously "implicated" the critic is obligated to show precisely where and how. Regarding linguistic nativism, Fish fails to do either, so his criticism is vacuous.

I will conclude my survey of Fish's and Bizzel's perspective on nativism by speculating a bit about the underlying reasons for the inconsistencies in their remarks. As to the inconsistencies themselves, this is what we find. Literally half a page before his pronouncement of the "failed and ruined" state of foundationalism, Fish praises Bizzel's essay *Cognition, Convention and Certainty: What We Need To Know About Writing*—the very same essay, quoted above, in which Bizzel declares her agreement with fundamental nativist—i.e., Chomskyan—tenets about language[50]. Here's a synopsis of these exchanges: 1) Bizzell endorses linguistic nativism (1992, p. 76) ; 2) in the same essay, Bizzell implicitly rejects linguistic nativism when she declares that "all knowledge. . . is of human origin," i.e., is a social construction (p.98)[51] 3) Fish praises Bizell's essay as "excellent" while at the same time denouncing nativism (first published in 1987); 4) Bizzell implicitly rejects nativism again, by approvingly citing Fish's "correct antifoundationalist answer" to the question about what is left after subtracting race, class, etc.

While this kind of inconsistency is to be expected from antifoundationalists because of the intrinsically paradoxical nature of their position vis-à-vis rational argumentation, there is an additional twist regarding nativism and literacy here, at least

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[50] about this essay Fish writes: "I have quoted from an excellent essay by Patricia Bizzel, who goes on to rehearse some of the assumptions informing the work of foundationalist theories of composition" (Fish, 1989 p.345)

for Bizzell. In general, nativist claims of all kinds are a threat to antifoundationalism because they contradict the doctrine that all thoughts are socially constructed. However, linguistic nativism is also an inherently egalitarian idea that has attractive aspects for liberal democratic theorists like Bizzell. Few ideas are as class leveling as the notion that all dialects are inherently equivalent in terms of their structure and ability to encode human thoughts. Champions of diversity have seized on this idea and in fact have used it very effectively to combat racism and dialect discrimination in the schools (Wolfram, 1998). However, as noted, the claim also undermines strong social constructivism because it posits inherent cognitive structure and because it strongly implies that language merely *encodes* thought rather than exhaustively containing it, or, more radically, creating it. Bizzell tries to ameliorate this conflict by suggesting that humans start with innate structures that are later converted to socially constrained ones, but this opens up an even more serious problem for her. The following passage picks up from the end of her previous passage (quoted above) about innate language capacities:

> As the individual develops, these capacities are realized in her learning her native tongue and forming thought patterns that organize and interpret experience. The mature exercise of these thought and language capacities takes place in a society, in interaction with other individuals, and this interaction modifies the individual's reasoning, speaking, and writing within society.
>
> (1992, p. 76)

This is a highly ambiguous passage because it leaves the extent of the social influence claimed undefined. On a strong social constructivist reading Bizzell could be equating the "native tongue" with "thought patterns," although it is also possible to construe those

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[51] in the previous line Bizzell uses the term "human construction" to refer to knowledge, and it is clear in the context that she means this in the sense of "social construction" not the innately driven "construction" Chomsky talks about.

phrases as independent, with "thought patterns" predicated not by language but by some unnamed force, perhaps "society" in general. She also talks about how "interaction modifies the individual's reasoning." Again, there is significant ambiguity because we don't know what "modify" means; it could be anything from a small change to a complete makeover. But no matter how it is (reasonably) interpreted, the passage is problematic for strong social constructivism/antifoundationalism. On a strong reading it is deeply paradoxical, undermining all its own claims, and the many further claims Bizzell will make in the rest of the essay. If all thought patterns and reasoning merely reflect social "conventions," then the passage also is merely reflective and therefore not compelling. Weaker readings (there's an infinite range), may avoid this paradox, but then they contradict the central claims of strong constructivism. If reason is merely "modified" without being totally changed by social interaction, then, 1) there must be some non-socially originating reasoning capacity—i.e., an *a priori* capacity—in humans, and 2) there is hope that this capacity can be freed, at least at times, from the constraints of social conditioning. In fact, that claim is implicit in the entire essay, which continually asks us to consider reasoned argument and evidence, though, paradoxically, in this same essay Bizzell also argues that "we cannot look at reality in an unfiltered way—reality only makes sense when organized by the interpretive conventions of a discourse community" (1992, p.94). Now in a certain sense this is undeniably true, i.e., in the sense that we never see *everything* for what it is at one time; but it had better *not* be true in the sense that Bizzell uses it to make her argument for social constructivism and against objective reasoning, i.e., in the sense that *all* judgements about the world are *always* compromised. For if it is, then we—and especially Bizzell—are back in a vicious

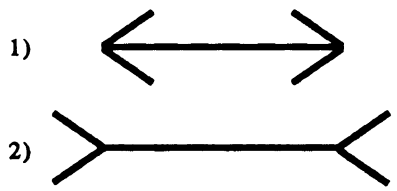
paradox. This is the same problem we've been dealing with all along, but now the root of it begins to show. The problem is that strong social constructivists conceive of thought and perception in a gestalt way such that if one aspect of a "situated self" is implicated, all are, whereas cognitive/nativists implicitly—and often explicitly—argue for a "modular" mind/brain that can attain a certain distance from its own processes, though only bit by bit, and only by employing carefully designed procedures over time. That is, though we cannot see things as they really are, we can at least know that that is the position we are in, and we can devise ways to correct for cognitive and social biases, and also to check on the accuracy of these procedures and their results. In fact, as previously argued, this is the entire basis of, and reason for, science, and the fundamental difference between strong social constructivists and rationalists: the latter believe that we can escape the vicious circle of "situatedness" whereas the former say we can't, though they continually contradict themselves by making statements about reality that can have no validity if it is also correct that we cannot escape our situatedness. Interestingly, cognitivists also have a "situatedness" problem. The difference is, they perceive it as an unexplained "mystery" of the mind, that will have to be resolved since it conflicts with certain fundamental notions, such as cognitive materialism, rather than a postmodern (anti)fact that trumps everything else, as the antifoundationalists insist. A comparison of how cognitivist/rationalists and antifoundationalist/strong constructivists approach this problem dramatically illustrates the ever-widening gulf between them, alluded to by Fodor above, and also reveals that, paradoxically, antifoundationalism/strong constructivism is not only committed to particular epistemological positions, as argued in

the previous chapter, but also to a set of very dubious ontological premises, contrary to its claims to be free of such commitments.

The Problem of Cognitive Integration

Antifoundationalists are perennially worried—or exultant—about the problem of situatedness, but, there is a prior, related problem that must be solved first, and that is the problem of cognitive integration. However, this problem is hidden in antifoundationalist rhetoric because antifoundationalists implicitly assume a unified mind that is somehow "possessed" of whatever community beliefs it happens to be associated with. As long as you don't ask any troublesome questions about *how* such a global system could possibly work, it serves as an "explanation," but if you begin to look a little more carefully at cognition, it is obvious and undeniable that there are independent systems at work. Thus, for example, we know that vision is largely a cognitive experience, controlled by specific regions of the brain that "compute" visual stimuli according to specific algorithms (Marr, 1982). We know also that it is possible to lose visual function as a result of brain damage, with no necessary consequences to other cognitive functions. In light of massive evidence of this sort for all kinds of cognitive functions, including long and short term memory, various aspects of language, vision, hearing, speech, and even such specifics as facial recognition, many cognitivists have concluded that the mind is composed of nearly independent "modules" each dedicated to a specific task. According to this "massive modularity hypothesis" (Fodor, 1983), each module does its work independently and then sends the information to some "executive processor," which integrates the information. We can get a good sense of this, experientially, by observing our reactions to certain illusions, like the Muller-Lyre illusion, in which two straight

lines of equal length are perceived to be of different lengths, regardless of whether we know the truth about them or not :



The fact that we continue to perceive the lines as unequal even when we know that this is false, is strong evidence of mental modularity: apparently, the visual system processes information according to its own rules and sends them to some poorly defined "executive" region of mind we identify with the self. Crucially, this "self" is not overwhelmed or dominated by the visual module. If other evidence is strong enough, the self will ignore the visual module's messages, treating them, quite rightly, as illusions. Now, if the mind is massively modular we might hope to escape all sorts of illusions and delusions, including socially imposed ones, and also to explain the mind itself. The beauty of modules is that they can operate mechanically—i.e, they can be modeled by mechanical devices that do their work by following syntactic algorithms (Fodor, 1983). We thus have a way of escaping both situatedness and Cartesian dualism—the doctrine that there is a fundamental mind/body split such that the body is a mechanical device whereas the mind is "metaphysical"—and, voila! we explain cognition. Well, but not quite. The problem, as Fodor points out, is that while mental modules are "local," each restricted to some narrow function that can be accommodated (i.e., modeled) by computational algorithms that we understand, the "executive" processes are "global," and therefore do not fit current computational models. In particular, consciousness and free will are completely inexplicable because both seem to have original or creative

aspects that cannot be accommodated by computational models, since the latter operate only on predefined constituents according to predefined algorithms. Because their operative processes and constituents are pre-defined, computational mechanisms are completely oblivious to context and that makes them incapable of creative, intelligent "executive" adjustment. Obviously, however, this is contrary to experience; in cognition we seem to find both rigid algorithmic processing and creative adjustment. Thus, in the Muller-Lyre illusion our visual system "computes" the length of the lines according to predetermined algorithms, yet some other part of the mind/brain is able to overrule those computations. The visual part is explicable according to computational models, but the part that overrules the illusion is mysterious. Fodor humorously sums up the problem as follows:

> If the mind is massively modular, then maybe the notion of computation that Turing[52] gave us is, after all, the only one that cognitive science needs. It would be nice to be able to believe that...
>
> But it seems to me one can't. Eventually the mind has to integrate the results of all those modular computations, and I don't see how there could a module for doing *that*. The moon looks bigger when it's on the horizon; but I know perfectly well it's not. My visual perception module gets fooled, but I don't. The question is: who is this I? And by what—presumably global—computational process does it use what I know about the astronomical facts to correct the misleading appearances that my visual perception module insists on computing? If, in short, there is a community of computers living in my head, there had also better be somebody who is in charge; and, by God, it had better be me. The old rationalists, like Kant, thought that the integration of information is a lot of what's required to turn it into knowledge. If that's right, then a cognitive science that hasn't faced the integration problem has scarcely gotten off the ground. Probably, modular computation

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[52] Turing showed that a simple machine could be programmed to "recognize any argument that is valid in virtue of its syntax (Fodor, 2000).

> doesn't explain how minds are rational; it's just a sort of precursor. It's what you have to work through to get a view of how horribly hard our rationality is to understand.
>
> (1998, p. 207)

Here, again, note the non-dogmatic nature of the discussion—completely unlike antifoundationalists' caricatures of rationalists' claims about perception and cognition. Moreover, note that once the problem is framed in terms of integrating modules and representations, it is the antifoundationalists' position that is revealed to be unreasonably dogmatic, rather than the rationalists'. Despite massive evidence that cognition is, at least to some degree, modular, and that modules are dependent on innate processes and mechanisms, antifoundationalists mock rationalist models of mind as hopelessly retrograde products of enlightenment thinking and insist that their strong social constructionist view is the only possible option.

As Fodor noted, most literary theorists—and by extension all antifoundationalists—are committed to the view that minds are "arbitrarily plastic," a position that lets them "explain" everything in terms of the discourse community. Moreover, there is a campaign among antifoundationalists to discredit the "atomistic" practices of science, to deny science's special position as a tool for accumulating knowledge, and even to refute the concept of knowledge itself. Two ontological premises support these arguments, first that mental processes are ineluctably holistic, and second that language is inseparable from thought. Under these assumptions, knowledge systems are made to look like acquired tastes, and epistemology is rendered irrelevant since there is no longer a legitimate object of study.

Antifoundationalist Argumentation

In his much celebrated book, *Philosophy and the Mirror of Nature* (1979), Richard Rorty, who is widely considered to be the preeminent antifoundationalist philosopher, makes this argument when he advocates "hermeneutics," as an alternative to epistemology. Over the next three chapters, I will look closely at these claims and the perspective on knowledge associated with them, and then compare them to rationalists' thoeries about language and mind, specifically Fodor's and Chomsky's frameworks, both of which are classic examples of scientific theory construction under rationalist epistemological assumptions. Ordinarily, a discussion like this proceeds by presentation of claims, supporting evidence, counter claims and evidence, all augmented with critical discussion. However, as we've seen, antifoundationalists categorically reject these discursive procedures, arguing that discussion can only occur from within a strong social constructivist framework:

> It is not just that antifoundationalism replaces the components of the foundationalist world picture with other components; instead, it denies to those components the stability and independence and even the identity that is so necessary if they are to be thought of as grounds or anchors. Entities like the world, language, and the self can still be named; and value judgements having to do with validity, factuality, accuracy, and propriety can still be made; but in every case these entities and values, along with the procedures by which they are identified and marshaled, will be inextricable from the social and historical circumstances in which they do their work"
>
> (Fish, 1989, p.345).

It is interesting to note that Fish left "rationality in general and logic in particular" out of this list, though, recall, these are two of the "ruined" foundationalist elements he previously identified; however, "the procedures" probably includes these elements, or so I will assume since it is clear from the antifoundationalist literature on the whole that both are under attack.

Given these preemptive strictures, it is necessary for me to discuss the rhetoric used in the discussion as well as the substantive claims raised, since antifoundationalists, like Rorty, explicitly eschew evidence in favor of rhetoric, and also attempt to discredit rationalist argument *ex ante* on the grounds of its "illicit" assumptions alone. I thus have a double purpose here: first to examine the diametrically opposed claims made by these two theoretical camps, with an eye toward deciding between them, and second to defend rational discourse itself, which goal will also necessarily involve exposing and analyzing the fallacies and rhetorical tricks used in *irrational* discourse. In making this choice to discuss rhetoric as well as substance, I am departing significantly from norms one might infer from the literature. In general, one could infer, at least from most academic literature, that it is bad form to question published writers' motives for, and methods of, exposition. There is, in other words, a tacit gentlepersons' code to which academics are supposed to subscribe. This code includes the assumption that writers will focus on content—as opposed to rhetoric—and that their arguments will be presented in good faith, in as clear and accessible a manner as possible. I would claim further, that, at least traditionally, the entire code was underwritten by the categorical imperative to seek truth in one's arguments. However, antifoundationalists explicitly reject this code. In fact, they argue that

> The need to justify our beliefs and desires to ourselves and to our fellow-agents subjects *us* to norms, and obedience to these norms produces a behavioral pattern which we must detect in others before attributing any beliefs to them. But there seems no occasion to look for obedience to an *additional* norm, the commandment to seek the truth.
>
> (Rorty, 1995)

As we shall see, this argument hinges on the claim that humans are not *equipped* to detect truth—that the whole concept of truth is severely misguided. I will of course argue that

the claim that humans cannot detect truth is itself a substantial and extremely significant *truth claim* and that, therefore, among other reasons, the antifoundationalist position is incoherent. These arguments will unfold as we go. But in making them, it would be foolish for me to ignore the rhetorical aspects of antifoundationalist arguments, for, quite often, rhetorical sleight of pen *is* the only substance in such arguments.

In general, then, I will discuss the rhetoric first, and then move to the substantive claims. As to the latter, the questions we will be looking at, are 1) Is epistemology, defined here as the study of how knowledge arises in humans, and how humans can verify that knowledge, a possible and useful field? 2) How is language related to epistemology in general 3) What is the status of mental representations vis-à-vis these epistemological questions? and, finally 4) What particular theories of language and literacy seem most compatible with what we know about mind, especially how we acquire knowledge? We will find that there are virtually no areas of agreement between the rationalists and Rorty on these issues. In fact, it is even difficult to see that they are discussing the same topics. The reason for this is that the disagreements arise at such a profound level. It is not just that they have theoretical or factual disagreements about language, knowledge, or mind; rather, they disagree even about what these words refer to and what would constitute a decent argument about them. This fact in itself—the fact that rationalists and antifoundationalists have different "vocabularies"—is frequently cited triumphantly by Rorty as evidence for his strong social constructionist position. Most rationalists, however (*this* rationalist in any case), would counter that when two models of reality contradict each other so completely, one or the other or both must simply be wrong. Of course, paradoxically, Rorty continually claims that he isn't

offering models of reality—he's only introducing a new "vocabulary"—but he makes plenty of strong claims about knowledge, mind, and language that contradict rationalist positions, and which, therefore, invite investigation. Nonetheless, because there are so few points of contact between the two positions, I will not move back and forth between them often. Rather, I will develop major portions of each position before overtly comparing them. Because Rorty's position is less well known, and very exotic, I will spend a lot more time dealing with his position than with the rationalists,' and I will begin with him.

IV: RORTY'S COUNSEL OF DESPAIR

In a July 1998 review of Rorty's *Truth and Progress,* Carlin Romano of *The Nation,* hailed Rorty (1931—), as "the great American philosopher," and went on to cite several other generally laudatory epithets from diverse sources, including Roger Kimball's "happy nihilist" and "official philosopher of postmodern academic liberals." One may question the intrinsic value of " happy nihilism," and, according to Romano, Rorty himself rejected the "postmodern" appellation on the grounds that "postmodernism is a word that pretends to stand for an idea," yet there is little doubt that both quips were apt based on the attention Rorty receives in postmodern/antifoundationalist literature, and on his own antifoundationalist polemics. In any case, Rorty's presentation of antifoundationalism is probably the most extensive case in print, and I will consider it to be representative of antifoundationalist thought. Thus, just as Kant abstracted a "philosophical opponent" based on Hume, so I will treat Rorty as a type of the rationalists' antifoundationalist opponent.

Rorty's most important work is *Philosophy and the Mirror of Nature* (1979), an ambitious historiographical survey of philosophical thought since Descartes, "from the point of view of the anti-Cartesian and anti-Kantian revolution" (ibid, p. 7), consciously written as the philosophical counterpart to Kuhn's famous critique of science, *The Structure of Scientific Revolutions* (1962). The major thesis of *The Mirror*, which title itself is meant to mock the "naïve" representational view of knowledge and mind that allegedly still "constrains" humanity, is that representational theories of mind are badly mistaken, and that it is the job of what Rorty calls "edifying philosophy" to expose or deconstruct these systems so that we can free ourselves from the "false" assumptions that

undergird them. According to Rorty, the greatest delusion of all is the notion that humans have minds that are capable of accurately representing, or "mirroring" reality:

> The aim of [The Mirror of Nature] is to undermine the reader's confidence in "the mind" as something about which one should have a "philosophical" view, in "knowledge" as something about which there ought to be a "theory" and which has "foundations," and in philosophy as it has been conceived since Kant.
>
> (1979, p. 7)

Working from this anti-thesis Rorty alternates between historiographical analysis of the ways in which philosophy allegedly perpetuated the "myth" of veridical mental representations, and "Wittgensteinian" arguments to the effect that all our psychological theories, from Plato to the present day[53] are merely "language games," which we can change whenever we find a "pragmatic" reason to do so. In a typical comment, Rorty sums up this perspective, arguing that, from the

> Wittgensteinian view, an intuition [theory, etc.] is never anything more than familiarity with a language-game, so to discover the source of our intuitions is to relive the history of the philosophical language-game we find ourselves playing.
>
> (ibid, p. 34).

Since Rorty takes this extremely dubious position as axiomatic, it is not surprising that all of the historiographical analysis that follows reinforces it. A long complicated analysis of his baroque argumentation, showing how he combines questionable interpretations of various philosophers' work[54] with repeated assertions of the "fact" of the Wittgensteinian position would be a worthy endeavor. However, it is

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[53] However, Rorty avoids tangling with current theories of linguistics and cognitive science, a suspicious lapse especially given that the theories he relies on have been amply refuted, especially by Chomsky and Fodor.

[54] Rorty is frequently accused in published papers of egregiously misrepresenting the work of various philosophers, including Descartes, Dewey, and Davidson. See Farrell (1995), Gouinlock (1995), Haack (1995) and Levine (1995), all in Saatkamp Jr. (1995), for detailed criticisms along these lines.

not one I will attempt here. Rather, I will focus on specific claims he makes regarding language and mind, and also on his "therapeutic" approach to the "problem." Since Rorty's approach is historiographical his "narrative" jumps around a lot conceptually; therefore, since I am doing a conceptual analysis, the reader will find that I jump around a lot in the text, frequently linking ideas that are hundreds of pages apart. However, I have been scrupulous about matching ideas carefully and in accurately interpreting Rorty's claims, something we will find Rorty does not do in his own analysis of other authors' work.

According to Rorty, "hermeneutics" is the requisite therapy [for culturally ingrained mirrorism], designed to "help readers, or society as a whole break free from outworn vocabularies and attitudes..." (ibid, p.12). I will therefore begin with that concept, one that has endured in Rorty's later work and which in my view, of all antifoundationalist doctrines, holds the most promise for reconciliation with rationalism, although only in its "virtuous" version. The difference between this version of hermeneutics and the other "vicious" version Rorty ultimately advocates will become clear as the discussion unfolds.

Epistemology and Hermeneutics

I have titled this section "Epistemology *and* Hermeneutics," rather than "Epistemology *versus* Hermeneutics," because I do not see a conflict between these concepts. At first blush it would appear then that I am in half-agreement with Rorty, since he also claims that hermeneutics is not to be taken as a "successor" to epistemology. However, he is equivocal about the relationship between these concepts,

as we shall see. At any rate, his comments are not motivated, as they might at first appear to be, by philosophical tolerance. To the contrary, he most definitely does want to vanquish epistemology, so his "no successor" rhetoric should not be taken as benevolence toward rationalists, but, rather, as a sneak attack, not only on epistemology but also on the entire rationalist tradition that is associated with it:

> I am *not* putting hermeneutics forward as a "successor subject" to epistemology, as an activity which fills the cultural vacancy once filled by epistemologically centered philosophy. In the interpretation I shall be offering, "hermeneutics" is not the name for a discipline, nor for a method of achieving the sort of results which epistemology failed to achieve, nor for a program of research. On the contrary, 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.
>
> (1979, p.315)

This is classic Rorty. He bundles five or six dogmatically asserted though completely unsubstantiated, mutually reinforcing claims together in such a way as to make all of them seem much more credible than any of them would separately. He's 1) not trying to "replace epistemology with hermeneutics" because 2) that would only perpetuate the "confrontation and constraint" wrought by the former's 3) "dead" perspective; rather, he will merely "express his hope through hermeneutics" that the 4) "cultural vacuum" left by the 5) "failure of epistemology," *not* be filled, so that "our culture" will finally be relieved of the 6) "demand," imposed by epistemology, "for confrontation and constraint."

The first claim (1) is the most absurd in a chain of absurd claims. Rorty pushes his "hermeneutics" with a vengeance, as we shall see. As for "confrontation and constraint" this is an irrelevant appeal to emotions, achieved by conflating questions of

truth with "pragmatic" concerns. The difficulty in discussing this problem is that, for Rorty, there *are* no questions of truth—everything supposedly reduces to "pragmatics." However, this as an incoherent claim since it is evident that all pragmatic claims rest, eventually, on truth claims—you cannot have pragmatic turtles all the way down. Thus, for example, the "hope" that the "dead" space left by epistemology not be filled is "therapeutic" and "pragmatic" only if it is *true* that epistemology was harmful and is now dead. Regarding confrontation and constraint, the implication here is that people have been prevented from thinking in gentler more productive ways, but, again, that is a truth claim, and one that has to be shown not merely asserted.

As for the "demise" of epistemology, this is pure wishful thinking on Rorty's part. Rorty associates the epistemology he hopes will disappear with the "view of knowledge as an assemblage of representations—a view of knowledge which I've been arguing is a product of the seventeenth century" (1979, p. 136). Actually, as Churchland and Sejnowski noted (re: quote in chapter 2), representational theories of knowledge are at least as old as Plato, but it is true that they were substantially developed, especially by Descartes, Locke, and Hume, in the seventeenth and eighteenth centuries. What is decidedly false, however, is that they are now "dead." To the contrary, representational theories of knowledge have never been more lively. Today they are central in linguistics, cognitive science, psychology, psycholinguistics, and recently even in anthropology (see for e.g., Chomsky, 1957, 1986, 1995; Fodor, 1975, 1983, 1998, 2000; Jackendoff, 1976, 1987, 1992, 1997; Katz, 1990, 1998; Marr, 1982; Pinker, 1979, 1988, 1994, 1997; Sperber, 1996) . Indeed, it would not be an understatement to say that none of these fields (except anthropology) would even be possible in their modern forms without

substantial and detailed theories of representation—you could even say that all these fields are just the study of how representations work in different areas of cognition.

Now, Rorty's major thesis is that the entire representational view of mind is a misguided social construction; moreover, his 'therapeutic' alternative—that we should adopt the attitude that belief systems are serendipitous epiphenomena, the detritus of "blind contingent forces" that affect people through the "vocabularies" that they acquire —is entirely dependent on this thesis; if it turns out that representational theories of cognition are substantially correct, i.e., that our comprehension of the world and each other is a function of the interaction of representations from different sensory and psychological sources, then his pragmatics is in jeopardy. Thus, one would think that he would have a detailed defense of his position and substantial evidence against representational theories of mind. However, in the nearly four hundred pages of *The Mirror* there is none of this. For the most part, Rorty simply ignores the arguments in the thousands of books and articles from generative linguistics and cognitive science that unequivocally support representational theories of mind. The one exception to this is even worse; in that case he brazenly co-opts some of Fodor's cognitive theories for his own purposes, cavalierly quoting Fodor out of context in such a way as to completely change his meaning. We will look at this in detail in a later section of this chapter. In any case, it is evident from Rorty's manipulations, that he realizes that he can't plausibly completely deny representations, so he tries instead to minimize their importance. For example, buried in *The Mirror* we find this peculiar disclaimer:

> ...once explanation [of how we know what we know] and justification [how we justify our beliefs] are held apart there is no reason to object to explanation of the acquisition of knowledge in terms of representations.

(ibid, p. 210)

This is a momentous passage that demands substantial development since it directly contradicts the main thesis of *The Mirror*, but, again, none of this is forthcoming. Rorty passes this admission off as if it were a minor point, while claiming that—somehow—once explanation and justification are held apart the contradiction disappears. But why should it? Even if Rorty could show that representations have *nothing* to do with "justification," his primary thesis—that representational theories of mind are "optional," ultimately just the product of Descartes' "invention of the mind" (ibid, p. 136)—is potentially contradicted[55]. Moreover, the relationship between representations and justification of belief is extremely complex, not to be waived aside as if it were self-evident and firmly established that they are separate. To the contrary, as we've seen from Fodor's discussion of illusions, it is clear that they are *not* separate, though at this point we don't know precisely how particular representations are related to each other or to our overall comprehension of the world. Clearly, humans have ways to adjudicate among representations, overruling some and accepting others; interestingly, it seems that in cases of rejection, what occurs is that *other* representations are brought to bear in their place. Thus, in the case of the 'big' moon on the horizon, educated people substitute a representation of the solar system that tells them that celestial objects do not change size—a representation that itself is integrated with thousand of other more or less "justified" beliefs and representations—and then 'veto' the information from their visual systems. Interestingly, in this case, and in many cases like it, most people cannot give a reasonable explanation for what they are seeing. That is, they know they are experiencing

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[55] Only "potentially" because Rorty equivocates on this point; "there is no reason to object..."

an illusion but they can't explain how it works. And, as Fodor points out, though cognitive psychologists can explain the mechanics of the illusions, they don't have any real idea how humans are able to arrive 'abductively' (to use Fodor's term) at conclusions that seem to require nearly instantaneous integration of information from many different modules, some of which contradict each other, or how people exercise the 'executive' ability to sort this information and form beliefs. Even more difficult is the question of how people, both individually and institutionally, are able to interrogate their belief systems and improve the degree of correspondence between their beliefs and reality, though it is evident from our general success in negotiating our way in a complex world that we do so. These are formidable problems that are unlikely to have facile answers and which cry out for systematic investigation, i.e., for epistemology. But Rorty dismisses these fascinating problems and the entire world of linguistics, cognitive science, and psychology that currently provides our best glimpses of what may be going on, by opting for a "rhetorical alternative":

> It is so much a part of "thinking philosophically" to be impressed with the special character of mathematical truth that it is hard to shake off the grip of the Platonic Principle ["that differences in certainty must correspond to differences in the objects known"[56]] If, however, we think of "rational certainty" as a matter of victory in argument rather than of relation to an object known, we shall look toward interlocutors rather than to our faculties for the explanation of the phenomenon. If we think of our certainty about the Pythagorean Theorem as our confidence, based on experience with arguments on such matters, that nobody will find an objection to the premises from which we infer it, then we shall not seek to explain it by the relation of reason to triangularity. Our certainty will be a matter of conversation between persons, rather than a matter of interaction with nonhuman reality. So we shall not see differences

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[56] ibid, from the previous paragraph. Note that rationalists do not have to accept this positivist notion, and that scientific realists would emphatically reject it since it conflates belief with reality (Bhaskar, 1997).

> in kind between necessary and contingent truths. At most, we shall see differences in degree of ease in objecting to our beliefs. We shall, in short, be where the Sophists were before Plato brought his principle to bear and invented philosophical thinking.
>
> (ibid, p. 157)

There are obvious objections to all this, beginning with the fact that there are liars, fools, and demagogues in the world and that even if there weren't, sincere "conversationalists" often disagree about things. Rorty might say that that's OK because, by and large, the community arrives at true beliefs, but, to begin with that just ain't true—X-million religious and political fanatics *can* be wrong, and in fact most of them *must* be since they contradict each other. And even in the felicitous cases, i.e., cases where the "community" gets something right, explaining true belief through "conversation" just passes the buck. *How* does conversation "justify"? Even a superficial look at any debate quickly reveals that people invariably invoke *representations of the world*, which representations are defended by complex webs of other representations, in their efforts to persuade others. Moreover, in many cases, the interlocutors resort to empirical methods of verification, and in nearly every case, they cite *someone* who lays claim to empirical evidence. Thus, in the Pythagorean example, it just isn't true that geometry students come to believe the theorem based on "conversation." Contra Rorty, their certainty is very much the result of their own reasoning ability in relation to "triangularity." Moreover, it is well known that if their certainty *isn't* based on personal investigation, they wind up not being so certain, and also not knowing anything much about the theorem. Further support for the superiority of a correspondence theory of truth (though not a naïve Fregean one) over a conversational one is the fact that there is an overwhelmingly high correlation between false beliefs and *lack* of contact with empirical research, either direct or second hand.

Now of course all of the above claims rest on the notion that we can discover true things about the world—a notion Rorty generally rejects, though he is often equivocal about it. Visions of Mexican stand-offs may dance in some heads at this point, but they shouldn't. It is the central argument of this dissertation that there is profound asymmetry between antifoundationalists and minimal foundationalists, with the latter enjoying a comfortable advantage, though not one that can ever produce a decisive victory. First, as Hume conceded, and as the above discussion supports, reason must reign in practical affairs. And now, in his claims about justification through conversation, Rorty has revealed another interesting chink in the antifoundationalist armor, though one that must be prized apart carefully.

As noted above, the weakness in conversational schemes for justification of belief is the fact that conversation requires representations of things in the world and relations among things, as well as representations of linguistic structure. Linguists and psycholinguists are virtually unanimous about this. Other than a few hold-outs in the connectionist camp, no linguist or cognitive scientist even considers non-representational theories of language since there is no way to model them and since representational models explain linguistic data superbly. So if conversation requires representations, doesn't this mean that justification through conversation winds up just being justification through representations—and reason—after all? If, as I've argued here, it does, then that leaves the need for epistemology intact, particularly since we know we don't always get things right.

Psychological Nominalism

Now, Rorty realizes that the greatest danger to his pragmatic philosophy is representational theories of mind—that's why refuting them is his main task in *The Mirror*. However, because the evidence overwhelmingly supports such theories, he grudgingly concedes that they may have some place in our knowledge webs, but he desperately wants to exclude them from "conversation," since the latter is to be the engine of his alternative ontology. But this leads him into utter absurdity, both in commonsensical realms as we see above, and in theoretical realms as well, as we'll see.

To save conversation from representational theories, Rorty adopts a "Wittgensteinian view[57]" of language under which "vocabularies" are primaries, and words, sentences and even complete "discourses" are assimilated wholesale. Under this fantastic conjecture it is argued that prior to language there is no awareness of any kind. Rorty approvingly quotes Sellars' exposition of this position, which Sellars calls "psychological nominalism":

> *all* awareness of *sorts, resemblances, facts,* etc. in short all awareness of abstract entities—indeed all awareness even of particulars—is a linguistic affair. According to it, not even the awareness of such sorts, resemblances, and facts as pertain to so-called immediate experience is presupposed by the process of acquiring the language.
>
> (Sellars, no specific citation; in Rorty, 1979, p. 182).

To Rorty's credit, he recognizes that "the existence of raw feels—pains, whatever feelings babies have when looking at colored objects, etc.—is the obvious objection to this doctrine" (ibid). However, the credit is short lived, since Rorty accepts Sellars'

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[57] It requires prejudicial selectivity to derive this position from Wittgenstein. On the whole, Wittgenstein seems confused about the relation of non-linguistic concepts to language rather than convinced that there is only language, although certain of his aphorisms can be used to support that position.

counter to the "raw feels" argument, i.e., that there are two kinds of awareness, one that even rats and amoebas share with us, which Sellars calls "awareness-as-discriminatve-behavior," and a second, exclusively linguistic awareness, called "being 'in the logical space of reasons'" (ibid). Rorty apparently finds this a compelling rebuttal, for without any supporting commentary he builds on Sellars' work:

> Awareness in the second sense is manifested only by beings whose behavior we construe as the utterance of sentences with the intention of justifying the utterance of other sentences. In this latter sense awareness is justified true belief—knowledge—but in the former sense it is ability to respond to stimuli. The bulk of [Sellars'] book is an argument that such ability is a causal condition for knowledge but not a *ground* for knowledge. This view has a corollary that knowledge of particulars or of concepts is not temporally prior to knowledge of propositions (but always an abstraction from the latter) and thus the empiricist accounts of language-learning and of the nonpropositional basis for propositional knowledge are inevitably misguided. The crucial premise of this argument is that there is no such thing as a justified belief which is nonpropositional, and no such thing as a justification which is not a relation between propositions.
>
> (ibid, p. 183)

It takes some work to see that there is a total lack of credible substance in this passage. Some of the claims are idiosyncratic innovations that require development, though none is provided, and others have been more than amply refuted in the cognitive science/linguistics literature. Regarding idiosyncrata, on what basis can "awareness" be construed as "justified true belief," and why is it only possible among interlocutors who are engaged in "justifying utterances"? These bizarre definitions do not accord with anything in the psychological literature on awareness (usually subsumed under "attention"; see, for e.g., Carr and Curran, 1994; Schmidt, 1995), and they are strongly counter intuitive. Worse, they introduce considerable confusion into the discussion. We

start out with a reasonably standard notion of "awareness," albeit stated in vague behavioral terms ("awareness-as-discriminative behavior"), but we are then eased into a "definition" that has almost nothing to do with the standard term. If the innovative definition is accepted, then the startling conclusions that follow, follow "by definition," but nothing other than obfuscatory tautology has been accomplished. For example, one startling conclusion in the above passage is that "knowledge of particulars or of concepts is not temporally prior to knowledge of propositions." If the innovative definition of awareness is accepted, the conclusion is inescapable: Since "awareness is justified true belief," justified true belief is knowledge, and awareness can be "manifested only by beings whose behavior we construe as the utterance of sentences with the intention of justifying the utterance of other sentences," then it "follows" that there is no knowledge of particulars without knowledge of propositions, since only beings who have mastered the utterance of propositions could be aware, and, recall, awareness *is* knowledge. But this entire absurd argument is a house of cards supported by nothing other than bald-faced, counter-intuitive assertions, which are logically and empirically incredible. Logically, it is impossible for set A, which includes elements b, c, d, to *precede* elements b, c, or d. At most, the set and the elements could arrive together. Now, on any account, propositions *entail* "particulars" and one cannot "know" a proposition without knowing its constituent parts, so propositions can't *precede* "particulars." Logically, they could arrive together (ontologically), but there is no evidence that they do and fairly good evidence that they don't. It is well known, for example, that children follow a developmental path in language acquisition, from babbling, to one word nominal utterances, to two word sequences, usually nominal plus predicate, to truncated clauses,

and finally to full sentences, with each stage supporting a corollary "discourse" style composed of "sentential" units strung together (Gleitman, 1994). The very reasonable and widely accepted inference from this acquisition pattern is that knowledge of "particulars" *is* prior to knowledge of propositions. Now, there are lots of theorists who also believe that a good deal of conceptual knowledge is innate. Indeed most generativists assume that this must be so because children learn things like category boundaries so easily (Lakoff, 1987). But the last thing that Sellars and Rorty would endorse is linguistic nativism, because along with that comes the whole flood of representational theories of mind that they oppose. No, what they *seem* to be selling here is the absurd notion that children start with blank slates and then at some magic moment acquire discourses wholesale:

> We may balk at the claim that knowledge, awareness, concepts, language, inference, justification, and the logical space of reasons all descend on the shoulders of the bright child somewhere around the age of four, without having existed in even the most primitive form hitherto. But we do not balk at the thought that a cluster of rights and responsibilities will descend on him on his eighteenth birthday, without having been present in even the most primitive form hitherto.
>
> (ibid, p. 187)

At this point, the reader would certainly be justified to wonder where Rorty and Sellars were on their fourth birthdays when that logical space of reasoning was descending. However, further reading intimates that this entire Sellarian excursion is one of Rortian pragmatic hyberbole, intended to support Rorty's arguments for "epistemological behaviorism," not the absurd theory of mind it *seems* to be:

> ...in both cases [acquisition of rights, acquisition of understanding] what has happened is a shift in a person's relations with others, not a shift inside the person which now *suits* him to enter such new relationships... Thus Sellars should not be expected to offer

> arguments for a "theory of the relation between language and thought," for thoughts are inner episodes which may or may not (depending on the needs of empirical psychology) be though of as necessarily linked to language, to brain-states, or to various other things. As an *epistemologist,* Sellars is not offering a theory about inner episodes. Rather, he is noting the traditional, nonbehaviorist notion of "epistemology" is the confusion of an account of such episodes with an account of the right to make certain assertions. This is to adopt the view that philosophy (and, specifically, "philosophy of mind") cannot, by supplying a loftier critical view, reinforce or diminish the confidence in our own assertions which the approval of our peers gives us. Sellars' psychological nominalism does not stem from behaviorism as a thesis about what the mind is or is not. It stems from epistemological behaviorism in the sense defined above, a sense indistinguishable from epistemological holism. To be behaviorist in this sense is simply to "divide through" by any and all mental events and faculties and to view our practices of justifying assertions as not needing empirical or "ontological" ground.
>
> (ibid, p. 188)

The gambit here is similar to the non-replacement-of epistemology-by-hermeneutics gambit above. In both cases, the goal is to manipulate the reader out of his/her reasonable expectation that theories of mind will be discussed and then to refocus the discussion on "pragmatic" alternatives, while leaving the seed of an indefensible "theory" behind—in this case the holistic acquisition of language and "reason" theory. Since the only way to follow the argument is to enter Rorty's weird "pragmatic" space, Rorty achieves his goal of forcing us to see the world in a-epistemological terms, or he nearly does anyway. There is a way to avoid the coercion, but it requires ferreting out the ontology that *is* presupposed by his pragmatism—despite his denials of having any such presuppositions—and this in turn requires an understanding of his "epistemological behaviorism."

Epistemological Behaviorism

"Epistemological behaviorism" is one of those odd compounds that winds up denying both its elementary parts—contrary to the sound of it, EB is neither an epistemological theory, nor a behaviorist doctrine, at least not in Watson's or Skinner's sense. Rather, EB turns out to be a "pragmatic" denial of the very possibility of credible theories of mind. But, as is so often the case with Rorty and his terms, in some places he presents a mild, inoffensive sounding version of EB, which masks the "vulgar" hidden aspects that lurk beneath—to borrow a memorable locution from Susan Haack (1995), who denounces Rorty's "vulgar pragmatism"—so it is only by paying careful attention to the doctrine on the whole that we can assess it.

> ...the question of whether we can be behaviorist in our attitude toward knowledge is not a matter of the "adequacy" of behaviorist "analyses" of knowledge-claims or of mental states. Epistemological behaviorism (which might simply be called "pragmatism" were this term not a bit overladen), has nothing to do with Watson or with Ryle. Rather, it is the claim that philosophy will have no more to offer than common sense (supplemented by biology, history, etc.) about knowledge and truth.
>
> (ibid, p. 176)

The definition above, for example, seems to be a simple plea for "common sense" and science, over dusty old philosophy, with all its unwieldy metaphysics and historical baggage. If that's all there were to it, then EB would seem to be a good candidate for a modern epistemology based on psycho-cognitive theory and empirical research—certainly nothing wrong with that. Moreover, Rorty encourages this view as much as possible, even to the point of *seeming* to endorse foundational, rationalist principles of mind:

> We can take the Sellars-Quine [EB] attitude toward knowledge while cheerfully "countenancing" raw feels, a priori concepts, innate

> ideas, sense-data, propositions, and anything else which a causal explanation of human behavior might find it helpful to postulate.
>
> (ibid, p. 177)

Again, this sounds fine, though we might wonder why EB is necessary, given that we already have a well-developed theory of mind along the lines "cheerfully countenanced" above. However, we soon see that it's all a trick; the nice definition is just bait meant to get us to swallow the real EB, which is indeed "vulgar" in intent and empty of content:

> What we cannot do is to take knowledge of these "inner" or "abstract" entities as *premises* from which our knowledge of other entities is normally inferred, and without which the latter knowledge would be ungrounded.
>
> (ibid, 177)

Apparently "cheerfully countenancing" is meant quite literally here, i.e., in the sense of not taking any of these ideas seriously. For, to *seriously* countenance a priori concepts, innate ideas, and so forth is precisely to use them as premises for further extrapolation. That's what it means to be a rationalist. Rorty achieves the brief illusion of being less encumbered than thou, when he bracingly rejects all preconceived ideas, but it *is* an illusion. For, we should ask, on what has he grounded *his* assertion, i.e., the assertion that we *cannot* take knowledge of such things, or at least of *something* as a premise? As I hope I've made clear by now (especially in Chapter Two), we don't have a choice—unless we're insane, we'll *always* stand on something. As for what Rorty wants to use as a ground, we soon find out that it's one of the old antifoundationalist favorites, historiography:

> ...the Sellars-Quine approach differs from the empiricist and rationalist tradition about whether, once we understand (as historians of knowledge do) when and why various beliefs have been adopted or discarded, there is something called "the relation of knowledge to reality" left over to be understood.
>
> (ibid, p. 178)

This is very cheerful indeed, downright hilarious in fact. How could one simultaneously "countenance" something like the theory of innate ideas and at the same time "countenance" the idea that there is no relation between knowledge and reality? This is simply incoherent. Theoretical proposals like the existence of innate "ideas" have been proposed as *facts* by Chomsky and others, and, therefore, by definition they *must* relate to reality or be ruled out as *false*. Let's make no mistake, then, about the content of EB; this doctrine declares, categorically—and paradoxically—that there is no knowledge that corresponds to reality.

Rorty derives EB, in a circuitous fashion, from the assertion that theories of mind entail Cartesian dualism. As with much of his analysis, Rorty begins his exposition of this position by presenting a historical case against the theory that allegedly misleads us, in this case dualism (of any variety). Rorty asserts that

> Discussion in the philosophy of mind usually start off by assuming that everybody has always known how to divide the world into the mental and the physical—that this distinction is common-sensical and intuitive, even if that between two sorts of "stuff," material and immaterial, is philosophical and baffling.
>
> (ibid, p. 17)

Opening with this claim, that Cartesian dualism—a well known fallacy denied by nearly everyone in cognitive science—is still firmly entrenched in philosophy, allows Rorty to build the case that epistemology has been misguided since Descartes. Then, assuming that we buy that premise, he is able to argue against epistemology by arguing against dualism. This he does by cleaving intentional states from phenomenal states, arguing that they do not form a natural class since phenomenal states like pain "are not intentional"... "not *about* anything" (p.22). Yet, Rorty argues, phenomenal states and intentional states have traditionally been bundled together as "mental," because both have been conceived

as "immaterial," a fact, which, according to Rorty, just goes to show the depths of confusion dualism has gotten us into. The first confusion is to imagine that there are immaterial properties of mind, and the second is to group all things "mental" together. As for immateriality, Rorty argues that this fallacy follows from the fact that we cannot deduce intentional states by examining the physical structure of the brain. While this may be true, that is it may be true that some people have been led to countenance immateriality because they are flummoxed by the opacity of the brain/mind connection, the argument itself is quite weak. The fact that we do not understand how minds supervene on brains, to use Fodor's terminology, is certainly not a reason to reject the hypothesis that they do. Rorty, of course, is not offering the immateriality argument qua argument, but rather as a historiographical explanation of our alleged current confusion. The flaw in this is that he fails to analyze the problem itself in any depth or to discuss the reasonable alternatives that have been presented in the literature (see comments below). Rather, he presents the case in an either/or fashion; either we accept these (ludicrous) old Cartesian confusions, or we embrace "pragmatism." Moreover, it is always a bit unclear just where Rorty draws the line between historical confusion and what he himself actually believes. In the current case, for example, it is clear enough that he rejects immateriality, but he seems to accept the opacity argument itself as good evidence against straight-forward materialism. That is, though he rejects the dualistic doctrine of physical stuff versus mental stuff, he nonetheless seems to think that mental properties cannot be fully explained as products of neurology:

> If we take "the material" to be "the neural," for example, we can say that no amount of inspection will reveal the intentional character of the pictures and inscriptions found there. Suppose that all persons struck by the thought "I left my wallet on a café-table in Vienna," *in*

> *those very words,* have an identical series of neural events concomitant with the thought. This seems a plausible (though probably false) hypothesis. But it is *not* plausible that all those who acquire the belief that they have left their wallet on a cafe-table in Vienna have this series of events, for they may formulate their belief in quite other words or in quite other language. It would be odd if a Japanese and an English thought should have the same neural correlate.
>
> (ibid, p. 24-25)

Once again, it is only by ignoring the central findings of linguistics and cognitive science that Rorty can even hazard such an absurd argument. If you make the prodigious assumption that language and thought are one and the same, then the rest may follow, but the assumption itself is not intuitively compelling, and it has been strongly disavowed by most linguists and cognitive psychologists. As for intuitions, we are all familiar with intensional states for which we can't find words—"tip of the tongue" phenomena ; this alone should raise serious doubts about thought/language isomorphism. Again, consider the many intentional activities we all engage in that are non-linguisitic. Driving, playing sports, doing mechanical tasks—all of these activities require definite intentions; we must make the lane change *now* while simultaneously watching out for the truck on our bumper; dodge our opponent and make that backhand shot to the far corner, *without* smacking into the wall in the racquetball court; apply sufficient torque to the wrench to turn the nut, while lying on our backs in that impossible position, *without* allowing the damn thing to slip and leave us with bloody knuckles. Does anyone suppose that these activities, all of which have an undeniable mental dimension, are in any important way linguistic? If they were we would fail miserably at them I'm sure, simply because, often, there isn't enough time to *translate* our thoughts into words, and no words that would do the trick even if there were time. Just what words would you use, say, to command your body to move in just the right way, in those few nano seconds between the time you see

the ball racing toward you and you make that shot? In my view, these sorts of examples alone should be enough to falsify the thought/language claim, but there is also a large and very well known literature on the issue. Probably the most famous book on the topic is Fodor's widely acclaimed *The Language of Thought,* which he published in 1975, four years before Rorty published *The Mirror.* In this book, Fodor discusses several well-known reasons for rejecting the idea that people think in natural languages, such as the fact that animals and babies both think without it, and then begins making a logical argument for why there "must be a language of thought":

> The proximal stimulus is a proximal *representation* of the *distal* stimulus and the 'proximal response' *stands for* an overt act. But representation presupposes a medium of representation, and there is no symbolization without symbols. In particular, there is no internal representation without an internal language.
>
> (Fodor, 1975, p. 55)

Fodor goes on to refute various objections to this claim including the supposed problem of an infinite regress of presupposed languages (not a problem because the language of thought is not a *learned* language). The rest of the book outlines plausible features and architecture for a language of thought and the implications for psychology and linguistics if we assume there is one. This extensive and very tightly argued discussion, combined with the intuitions outlined above, and the fact of very rapid category acquisition and near universality of semantic and syntactic categories across languages, provides a very strong initial argument for a language of thought, and *against* the language *is* thought theory. Moreover, as we'll see, Rorty quotes extensively from this very book, so he simply has no excuse for ignoring all these arguments. He is of course free to argue against all this, but he isn't free to simply ignore it, at least not if he wants his own position to be taken seriously. This is one of the most egregious aspects of *The Mirror*; it

purports to be a comprehensive account of fallacies in contemporary philosophy, yet it mostly ignores the numerous important philosophical questions and solutions that have been raised regarding these problems in the cognitive and linguistic literature over the last fifty years or so. Rorty might argue that as an antifoundationalist he doesn't' have to follow normal academic research practices, but, interestingly, he doesn't. To the contrary, in a recent article about academic responsibility, he says:

> When we say that good historians accurately represent what they find in the archives, we mean that they look hard for relevant documents, do not discard documents tending to discredit the historical thesis they are propounding, do not misleadingly quote passages out of context, tell the same historical story among themselves that they tell us, and so on.
>
> (1998, p. 73)

Good rational sentiments, yet Rorty violates all these strictures on a regular basis, as I will continue to demonstrate. Moreover, though he sounds rational in the above passage, in other cases he advocates "abnormal discourse" practices, which, as we'll see can include misrepresentation of all kinds. Logically, antifoundationalism *must* lead to abandonment of normal scholarship for otherwise it wouldn't be antifoundational. After all why *should* scholars attempt to honestly represent the texts they draw from or the data they gather if they don't believe accurate representation is possible? This is a central dilemma posed by antifoundationalism that the academic community must confront.

In this case, by ignoring the cognitive science and linguistics literature, Rorty manages to present a dichotomous choice: either we remain moribund in the "bad old Cartesian ways" or we take the only alternative and adopt epistemological behaviorism. But since the whole purpose of such "behaviorism" is to avoid exploration and explanation of mind, all we get are vague descriptions with no theoretical justification:

> Explaining rationality and epistemic authority by reference to what society lets us say, rather than the latter by the former, is the essence of what I shall call "epistemological behaviorism," an attitude common to Dewey and Wittgenstein. This sort of behaviorism can best be seen as a species of holism—but one which requires no idealist metaphysical underpinnings. It claims that if we understand the rules of a language-game, we understand all that there is to understand about why moves in that language-game are made.
>
> (ibid, p. 174)

But denying the importance of theory doesn't relieve one of theoretical entailments. In this case, the ludicrous specter of "rationality descending at age four" or something very much like it is implicit. Moreover, "understanding" here is a completely mysterious term. In a representational theory of mind, there are ways to account for how we understand various things, and one can work at sharpening the concept by subjecting competing models to psychological tests. But under Rorty's non-theory, in which everything is stated in maximally vague terms, there is never any way to test the specific non-hypotheses.

Rorty's entire argument for the language-game position consists in his arguments *against* dualism; i.e., he argues that the only alternative to dualism is epistemological behaviorism. But, again, this argument has been specifically refuted in the literature, in fact, again in the same book referred to above, from which Rorty quotes. Speaking of a philosopher who takes a position similar to Rorty's—and whom Rorty often cites approvingly—Fodor remarks:

> Ryle assumes (as most psychologists who take a Realistic view of the designata of mental terms in psychological theories would not) that a mentalist must be a dualist; in particular, that mentalism and materialism are mutually exclusive. I have argued elsewhere that confusing mentalism with dualism is the original sin of the Wittgensteinian tradition...Suffice it to remark here that one result of this confusion is the tendency to see the options of dualism and behaviorism as exhaustive in the philosophy of mind.

(Fodor, 1975, p. 4).

The degree to which Fodor's remarks match Rorty's arguments is almost uncanny—almost as if Rorty based his thesis for *The Mirror* on the positions Fodor warned against. Fodor's original comments against behaviorism as an alternative for dualism (the "elsewhere" arguments) fully anticipate the first chapter of *The Mirror*:

> If there is a received view about psychological explanation among Anglo-American philosophers and experimental psychologists, it is surely that one or another form of behaviorism is true. At first glance this is surprising, since behaviorism by no means recommends itself on grounds of inherent plausibility. On the contrary, its adherents are forever warning us to distrust our intuitions, which, they say, have been corrupted by Cartesianism or by misunderstandings of ordinary language or by both. It is only through prolonged and sophisticated philosophical therapy, according to this view, that these influences can be mitigated and an innocent ear regained.
>
> (1968, p. 49)

But, as Fodor argues at length, if we reject the either/or choice, and consider the possibility of a thoroughgoing materialistic mentalism, we find that it is far superior in terms of explanatory power than any behaviorist theory. I will present some of the arguments against Skinnerian behaviorism in a later chapter. At present, however, we are concerned with Rorty's epistemological behaviorism, which is quite different. Unlike Skinnerian behaviorism, which does offer a theory of language—however vapid—epistemological behaviorism may be best described as the determination *not* to have a theory, a position that is licensed by the putative "fact" that mental theories entail dualism. But if it can be shown that mentalism does not entail dualism, then the justification for EB disappears. Put another way, if it can be shown that mental properties, including rationalism and language, can supervene on neural states, then dualism can be dismissed. Of course the best evidence would be a detailed neurological

to psychological mapping of the brain, showing precisely how brain states correlate to particular mental states. However, we are far from being able to show any such thing. Thus, the arguments come down to what is or is not logically possible, and even here it must be admitted—is admitted quite frankly by everyone in cognitive science—that we simply don't know enough about either neurology or cognition to say whether the neurology we are beginning to map in the brain can support our models of cognition. But, again, non-understanding of a complex mechanism is not a good reason to dismiss the possibility of that mechanism. Given the astounding complexity of the brain—one hundred *billion* neurons, each with as many as sixty-thousand synaptic connections to other neurons—there is no reason to reject materialist theories of mind out of hand. On the contrary, given what we *do* know about the world, i.e., that complex and apparently "mysterious" biological phenomena, such as sexual reproduction, infectious diseases, plant growth and regeneration, etc, consistently wind up being explicable in materialist terms, we have good reason to suppose that mental properties also have material causes.

Epistemological Behaviorism versus Fodor's Naturalized Epistemology

Although Rorty generally ignores cognitive science, and completely ignores linguistics (other than Wittgenstein, who really wasn't a linguist, and Davidson, who is more a philosopher of language than a linguist), as for example when he ignores Fodor's refutation of the dualism-or-behaviorism doctrine, he does address two prominent issues in cognitive science 1) psychological reductionism, and 2) how subjects' internal representations represent reality. Actually, as we'll see, Rorty subtly changes question 2) to "How *well* do the subject's representations represent reality?" a substantially different

question that allows him to shift the discussion toward pragmatics. But we will deal with this as it comes up. Of the two, the second issue is by far the more important one (in either version), but they are closely related issues, because developing a theory for the second issue is largely dependent on how one deals with the first issue. If psychology is sensibly reducible to neurology, then there is not much point in constructing psychological theories of mind, so we will look at this issue first.

Initially, Rorty sides with Ryle, who takes the reductionist position, while Fodor refutes Ryle. Since Rorty addresses the psychological non-reductionism issue directly, it seems that, for once, we have an opportunity to evaluate his arguments against representational theories qua theories instead of having to settle for his usual non-argument through narrative historiography. However, once again we will be disappointed. Rorty starts by defending Ryle, but by the end of the discussion, he takes up the pragmatic line again, after obliquely conceding that "the notion of psychological states as inner representations is unobjectionable..." , and then torpedoing this admission with the added comment, "but fairly uninteresting" (1979, 242). This use of attributive "uninteresting" is a favorite of Rorty's, which he hauls out whenever he's in a corner; the opposing position might be correct, but it's "uninteresting." But since it clearly *is* interesting to thousands of linguists and cognitive scientists, and since it obviously bears on the larger argument Rorty is making against epistemology, the comment can be disregarded, though it should be noted for its chauvinism. But enough of this, let's briefly examine the issue. Since, in the end, Rorty agrees with Fodor on this issue, I won't consider the arguments for reductionism, but will rather simply present the case for non-reductionism, i.e., for psychology. In any case, the arguments for reductionism are

nothing other than the refutation of these arguments, so there really is no need to cover both sides.

Conceptually the reduction issue is not terribly difficult, though the computational/architectural ramifications are extraordinarily complex. Psychological reductionists say that there are no intermediate levels of representation between stimulus reception and response, whereas psychological non-reductionists say there are. It is worth noting that this is the sort of problem that can't be solved—at least not yet, and probably not for a very long time—by direct investigation. Rorty is right about that part; looking directly at neurons gets us almost nowhere because the neuronal structures are horribly complex and we have no idea what to look for. So, the only thing we can do is try to infer systems by analyzing patterns of thought, as revealed by behavior[58]. The temptation to become reductionist is a logical one stemming from the seeming fact that either the mind is like other things, i.e., a physical apparatus of some kind, or it is something entirely 'other'—non-material 'mind stuff." Since the latter option raises the specter of dualism again, we are supposedly forced into reductionism. But there is a third alternative, which is to analyze the mind in terms of systems, as defined by rules, and to treat these rule-governed systems as real and *not* reducible to the matrix they are instantiated in. This third option, which is the option Fodor, Chomsky, Jackendoff, Pinker, and most other cognitivists take, assumes that mental systems are instantiated neurologically, but insists, nonetheless, that the systems themselves must be analyzed in their own terms. Some of the clearest examples and evidence for this position are from

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[58] Note that there is world of difference between inferring states of mind *from* behavior, and studying behavior itself as an explanaans. The first case is an instance of cognitive psychology, the second of behaviorism

linguistics. Thus, for example, English speaking readers will perceive the bracketed words: [red, RED, *red,* R*e*d], as identical at the lexical level even though they also perceive the typeface differences. Crucially, it can't be the case that we have memorized a finite set of typefaces, since we can also recognize 'red' as written by any number of individuals, each with unique penmanship. The only reasonable explanation for this is that we have some abstract, orthographic representation, or *type* (not to be confused with "typeface"), for each of the individual letters r-e-d, and that we use those representations to identify unique tokens of each type. Other (phonological, and reading) rules are used to recover the phonological representation rɛd, and still others to recover the lexical item 'red,' with its associated "subcategorization" information (adjective, color term, etc.), and finally some representation of the concept RED[59]. There are competing models among linguists for how the details of such a scheme works, but there is wide agreement that there must be a representational scheme and that it would minimally have to include the levels listed above, to handle cases such as reading alphabetic script.

Representationalists argue that all aspects of mental processing are only explicable by reference to such rule systems, and that there must be different rule systems for different aspects of cognition. As it turns out, Rorty doesn't disagree with this. What he does disagree with, however, is that the structure and content of mental representations could or should have anything to do with determining truths about the world. I find the conjunction of these two positions—acceptance of representations; rejection of their fundamental importance to justification of beliefs—nearly incomprehensible, and

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[59] I've borrowed the example and typeface conventions from Fodor (1995)

definitely incoherent. Since the opposite position—that representations have *everything* to do with how we determine truth about the world—is axiomatic for cognitivists, in particular for Fodor, we have a clearly defined debate to analyze. Unfortunately, however, Rorty adopts the devious strategy of trying to co-opt Fodor's position for his own side of the debate, rather than refuting the representationalist argument directly. So, as usual, the discussion that follows, will have a double purpose, first to expose Rorty's tricks, and second to refute his argument.

Intellectual Piracy

Rorty selects several passages from Fodor to support his position, and they do appear to do so if one accepts Rorty's interpretations of them, and particularly if one is unfamiliar with Fodor's overall position. However, if Fodor's passages are read in their full context, and if one is familiar with other arguments Fodor frequently makes, Rorty's reading is untenable. The following passages, also quoted by Rorty, are from Fodor; however, Rorty leaves some portions out. I will indicate the portions Rorty leaves out by setting them off in separate blocks and italicizing them; italics within other blocks are original to Fodor. Fodor is discussing the problem of how to model perception under a theory that assumes that the distal stimulus is transformed during the process of perception (i.e., a theory of intermediate representations). He begins by criticizing non-articulated models:

> ...this way trivialization lies. If one is allowed to use the notion of stimulus so as to distinguish the input to the retina (light energy) from the input to the optic system (patterns of light energy which exhibit invariances relevant, e.g., to the explanation of perceptual constancies), why not also talk about the stimulus for the *whole organism* (viz. perceptibles). Thus the answer to 'How do we perceive bottles?" would go: 'It is necessary and sufficient for the

> perception of a bottle that one detect the presence of the stimulus invariant *bottle'*.
>
> *The trouble with this answer (which by the way has a curiously Rylean sound to my ears) is, of course, that the problem of how one detects the relevant stimulus invariant is the same problem as how one perceives a bottle, so no ground has been gained overall.*
>
> What this shows, I think is not that the psychological problem of perception is a muddle, but that *stating* the problem requires choosing (and motivating) a proprietary vocabulary for the representation of inputs. I have argued that the vocabulary of values of physical parameters *is* appropriate on the plausible assumption that sensory transducers detect values of physical parameters and that all perceptual knowledge is mediated by the activity of sensory transducers.
>
> (Fodor, p. 49n. ; Rorty, 1979, p. 244-245)

Fodor is arguing here that the reason for positing a representational system in the first place is that it gives us a way to explain how stimuli can be accurately interpreted. We know that the light array reflected from objects is both impoverished and ambiguous and that, therefore, the organism must select relevant features to interpolate. Fodor's point is that if we want to understand how this works, we have to "motivate" a choice of relevant features, or "vocabulary," for the visual module to work on, and then try to model our choice to see if it will work. Rorty, however, takes the discussion in a different direction, focussing not on the cognitive problem, but, rather on how, or if, it relates to problems of justification. He plants the seeds for this argument by recalling some points about processing:

> To my earlier rhetorical question "Could psychology discover that the input to the cognitive mechanism is not at the retina, but rather half-way down the optic nerve?" Fodor would presumably answer: Yes, for doing it one way rather than another depends on which way of drawing lines around black boxes best splits the organism into the

> sort of transducers and processors whose descriptions make up a general and fruitful theory of cognitive processing.
>
> (ibid, p. 245)

It's not at all clear what Fodor would say about this. There are issues regarding what counts as "cognitive" and what counts as optical. If we consider the series of transformations that occur in the eye as strictly "optical" then, by definition the input to the cognitive system is not the retina, but these are terminological issues not conceptual ones. The important point is that the light array is transformed by both the eye itself and, even more significantly, by the visual cortex, in specific rule governed ways, and that without these very complex transformations, no useful information about the world would be transmitted to the organism (Marr, 1982; Stillings, et al, 1995). The reason for claiming that the cognitive input is not at the retina is because we need a site for interpolation, and it is how we model that site, or sites, and its mechanisms that determines whether we have a "fruitful theory of cognitive processing." The rest of Rorty's paragraph, the "drawing lines" part is irrelevant. But these are minor points. In the next paragraph, however, Rorty forces a connection to the issue of justification, which issue is completely off topic:

> Notice that this answer takes away any connection between the question "How do we recognize bottles?" and the question "what is indubitably given by the mind, such as to serve as an infallible touchstone for inference?" For the question "What does the subject have the right to believe without conscious inference?" or more precisely, "What sort of thing can he justify merely by such remarks as 'I saw it clear as I see you now' or 'I know English'?" has nothing to do with the question "What bit of the organism shall we pick as interface with the world?" or more precisely, "What choice shall we make of a 'proprietary vocabulary for the representation of inputs'?"
>
> (Rorty, 1979, p. 246)

Several things are mixed up here. First of all, the justification questions, as posed, are not questions any contemporary rationalist would ask; rather, they are Rorty's versions of classical (and "inappropriate") Cartesian "mirrorist" questions. In fact, it is unlikely that Descartes or his followers would have asked these questions either. So, once again, Rorty is a building a straw man. Then, his rendering of "What choice of vocabulary..." as "What bit of the organism ..." is misleading. The "vocabulary" Fodor is talking about is a theoretical vocabulary that *we* (cognitive scientists) construct and test, and which will presumably change as necessary to accommodate new data, not some intrinsic part of the organism. As for the main claim, that these theoretical choices have nothing to do with the justification choices, well, that's trivially true, because no one in cognitive science—certainly not Fodor—is framing the issues that way, though Fodor does ask related questions in other places. But Rorty tries to make Fodor his spokesman for this justification issue by blending this strange discussion into another passage from Fodor's text that is a few pages away. Directly following the above Rorty quote, Rorty introduces this Fodor passage (below) with the words: "Fodor is admirably explicit on this point" :

> But whatever relevance the distinction between states of the organism and states of its nervous system may have for *some* purposes, there is no particular reason to suppose that it is relevant to the purposes of cognitive psychology.
>
> (Fodor, p. 52; Rorty, p. 246)

But Rorty's "this point" has nothing to do with the point Fodor is making! Rorty's "this point" is the point about whether our choice of "vocabularies" for models of perception has anything to do with justification of belief. But Fodor's point is that the *distinction* between states of the organism as a whole and states of the nervous system is not "relevant to the purposes of cognitive psychology." In no way, shape, or form is Fodor

saying or implying that cognitive states themselves are irrelevant to justification of belief; NOR, is he saying that cognitive psychology is irrelevant to states of the organism as a whole, though if you're not careful to focus on the word "distinction" you could misread him that way. Fodor is making *his* point in the context of a discussion about the differences between local, or modular processes, and consciousness. What he is working at here is a principled way to abstract the boundaries of different cognitive systems according to "natural domains," and as part of that discussion he has noted that such domains may very well not respect the conscious/unconscious distinction. With this background, let's take another look at the quote, but this time with some surrounding context:

> If our psychological theories commit us to a language of thought, we had better take the commitment seriously and find out what the language of thought is like.
>
> My second point is that, while I have argued for a language of thought, what I have really shown is at best that there is a language of computation; for thinking is something that *organisms* do. But the sorts of data processes I have been discussing, though they may well go on in the nervous systems of organisms, are presumably not, in the most direct sense, attributable to the organisms themselves.
>
> There is, obviously a horribly difficult problem about what determines what a person (as distinct from his body or parts of his body) did. Many philosophers care terrifically about drawing this distinction, and so they should: It can be crucial in such contexts as the assessment of legal or moral responsibility. It can also be crucial where the goal is phenomenology; i.e., the systematic characterization of the *conscious* states of the organism. But whatever relevance the distinction between states of the organism and states of its nervous system may have for *some* purposes, there is no particular reason to suppose that it is relevant to the purposes of cognitive psychology.
>
> (Fodor, p. 52)

In a footnote prior to the "But" that begins the last sentence, Fodor adds some comments to the effect that it is questionable whether conscious states form a theoretical domain,

especially since Freud and others discovered the world of the subconscious. The thrust of these comments is against behaviorism, the main topic of the chapter. The point he's trying to drive home is that the same "information states" can arise for different reasons. For example, reading the word 'dog' hearing a dog bark, seeing a dog, or thinking of some particular episode in my past, may all cause me to have "dog thoughts." Now, Fodor argues that "what psychologists typically try to do is to characterize the etiology of behavior in terms of a series of transformations of information" (ibid, p. 52), so if having "dog thoughts" is a behavior (it is according to Fodor), then theoretically, I should go through the same series of transformations of information (processing of "dog thoughts") for any of the above different reasons[60]. Thus, for that series, these divergent phenomena would form a natural kind. The generalization that follows is that

> the natural kinds, for purposes of theory construction, appear to include some things that the organism does, some things that happen in the nervous system of the organism, and some things that happen in the environment.
>
> (p. 53).

Returning to Rorty, we will see that he has completely ignored Fodor's topic, and continues to misapply Fodor's text to his justification argument. The Fodor text that follows originally appears one paragraph after the above Fodor quote, and is still focussed on that topic. But in Rorty's text, the following Fodor text appears (with an intervening ellipse) after the above Fodor quote that ends with the clause, "there is no particular reason to suppose that it is relevant to the purposes of cognitive psychology." :

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[60] I'm not at all sure that Fodor's theory is right. For one thing, I'm somewhat dubious about the stability of such things as "dog thoughts." Fodor needs to have absolute redundancy for such thoughts to make his system work, and this seems unlikely to me.

> ...the states of the organism postulated in theories of cognition would not count as states of the organism for purposes of say , a theory of legal or moral responsibility. But so what? What matters is that they should count as states of the organism for *some* useful purpose. In particular what matters is that they should count as states of the organism for purposes of constructing psychological theories that are true.
>
> (Fodor, p. 53; Rorty, p. 246).

Since we have the context, we know that what Fodor is talking about here is the fact that these postulated "states" cut across the conscious/unconscious border, and that, therefore, they couldn't be used to construct theories of legal or moral responsibility because *those* theories (on virtually all accounts) refer *only* to conscious decisions, ergo only conscious states. But Rorty appropriates the quote for his own purposes. The comments below follow directly from Fodor's last quote:

> We need only add that what goes for moral and legal responsibility goes also for epistemic responsibility—the organism's being justified in believing this or that. No roads lead from the discovery of the organism's various interfaces with the world to criticisms of the organism's views about the world, or, more generally, from psychology to epistemology.
>
> (Rorty, p. 246)

These inferences are completely spurious. In the first place, Fodor carefully limited his comments to the domain of legal and moral responsibility, and it *doesn't* automatically follow that what "goes" for that domain "goes" for epistemology. At the very least we would need to see some arguments as to why and how these domains overlap in this way. Rorty makes his claim appear a little more plausible by adding the word "responsible" to "epistemic," thereby forcing a conceptual and lexical overlap with Fodor's "legal or moral responsibility," but, in fact, epistemology is pointedly *not* part of the moral realm, though how we deal with knowledge certainly is. The same inappropriate juxtaposition

of senses occurs in the next line where "justification" now resonates with "responsibility," as if forming "justified beliefs" were primarily a moral choice rather than a matter of becoming convinced. In fact, in Rorty's pragmatics these senses do overlap significantly, but since he's arguing about the connection, or alleged lack thereof, between Fodor's system and classical epistemology, he is not *justified* (I use the term advisedly), to impose his pragmatics on these systems. We need to understand what's at stake here. Rorty is trying to create an impassible gulf between cognitive processing and theories of reality:

> We can now see that Fodor's picture of the mind as a system of inner representations has nothing to do with the image of the Mirror of Nature I have been criticizing. The crucial point is that there is no way to raise the skeptical question "How well do the subject's internal representations represent reality?" about Fodor's "language of thought."
>
> (ibid, p. 246)

Now in one sense it is true that Fodor's system has nothing to do with Rorty's Mirror—i.e., in the sense that Fodor wouldn't accept Rorty's Mirror story as a legitimate *reflection* (again used advisedly) of the history of epistemology, and certainly not as a legitimate description of contemporary epistemology. As we've seen, for example, Fodor specifically refutes the dualism-or-behaviorism doctrine, which doctrine is a major part of the Mirror story. Moreover, Fodor most definitely *does* see a direct connection between theories of cognitive processing and epistemology. In fact, the "skeptical question," alluded to above, is part of a set of classical questions and assumptions that are central to his work, though he would frame that question differently. Whereas Rorty asks "How

well do the subject's internal representations represent reality?" Fodor begins with the assumption—based on the obvious fact that "subjects" function successfully most of the time—that the answer to the "skeptical question" is, "pretty well." The real work for Fodor, then, begins with the question, "*How* do the subject's representations represent reality?" and more importantly, "How do those representations become causal factors in the subject's behavior?" Before we look at this, a few generalizations, made against the context of Rorty's Mirror story, are in order.

Rorty's strategy throughout *The Mirror* is to present classical epistemology as a grotesque accretion of mistaken notions about mind and reality, heavily infused with various idealistic notions, borrowed from diverse sources, ranging from ancient Greek teleology to renaissance Christianity. The goal is to get us to flee from this confusion into the welcoming arms of Rorty's pragmatism, which is presented as the only possible alternative. Fodor, on the other hand, is not much interested in historiography, and though he does discuss the history of his field—"speculative psychology" as he call it—when he does so it is always in the context of specific ideas or problems related to his own theories, never in the grand interpretive/narrative style Rorty favors. This is much more than a stylistic difference. In Rorty's case, historiography is a major part of the argument; if we don't buy his Mirror story, his philosophy, what there is of it, will founder too. But Fodor's theories stand or fall only according to empirical and theoretical evidence. Now, these differences have everything to do with conceptions of epistemology. From a Rortian perspective epistemology is inextricable from its history, and in fact cannot be understood a-historically. But the world's libraries could be destroyed tomorrow, along with our collective memory of everything that happened, say,

before the twentieth century, and it wouldn't make much difference for Fodor's theories (which is not to say that Fodor himself wouldn't be appalled at the loss). Thus, ironically, one of Rorty's goals regarding epistemology—to purge it of its illegitimate historical baggage—is accomplished incidentally by Fodor simply as a consequence of Fodor's philosophical orientation to problems of mind. Fodor is a thoroughgoing rationalist/naturalist/empiricist, and this orientation extends right through to his epistemology. For Fodor, then, there are no metaphysical problems associated with epistemology simply because there are no metaphysical aspects of mind, though there may still be metaphysical issues. And this naturalism extends to other classically "metaphysical" concepts including truth, and indeed, reality itself. The result is that the three (knowledge, truth, and reality) can be felicitously joined in a naturalistic philosophy. Rorty on the other hand, continues to insist that comprehensive systems of epistemology are ineluctably infected with Mirrorism and that, therefore, the only fix is to slay epistemology and become pragmatists. But because cognitive psychology is obviously a serious contender for a modern epistemology he must find a way to alienate the former from the big epistemological issues, which issues he dreams of subjugating to his ghastly pragmatism. Capisca? Good. So let's take a look at Fodor's naturalized epistemology[61].

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[61] Rorty recognizes the option of "naturalized epistemology," and in fact raises the issue in light of Quine's version of it. He then compares Quine's notions to Fodor's points about motivating an input vocabulary (though never to anything Fodor himself says about epistemology), and from there makes some more questionable inferences about Fodor's position. There are complex issues here, but I will leave them for another day.

Fodor's Naturalized Epistemology

In the preface to *The Language of Thought*— the same book Rorty quotes from above—Fodor discusses the links between his methodology and philosophical orientation, and the old discipline of speculative psychology (SP), which he wants to revive. The key feature of SP is that it "used the methods of both philosophy and psychology because it was dedicated to the notion that scientific theories should be both conceptually disciplined and empirically constrained" (p. vii). He then goes on to describe why this orientation became difficult to sustain:

> The elucidation of general concepts is a typical philosophical concern, so speculative psychology tended to merge with the philosophy of mind...
>
> ...There were, moreover, fashionable epistemological theories—theories about the proper conduct of science—which suggested that no respectable inquiry *could* be partly conceptual and partly empirical in the way that speculative psychology was supposed to be. According to such theories, matters of fact are distinct in principle from relations of ideas, and their elucidation ought thus to be distinct in scientific practice. Philosophers who accepted this epistemology could accuse speculative psychologists of psychologizing, and psychologists who accepted it could accuse them of philosophizing. Since according to the epistemologists, psychologizing and philosophizing are mutually incompatible activities these accusations were received with grave concern.
>
> (ibid, vii-viii)

If you know a little about the history of psychology, and are familiar with Fodor, it's not hard to deduce that the puritans referred to above were the Skinnerian behaviorists. In any case, it is clear that Fodor regards the separation of philosophy and psychology, as, well, *unnatural*, in his sense of the term. He therefore promises to *do* some SP in his book, and he makes the point that SP is a comparatively risky venture because SP theories are subject to falsification. Finally, wrapping up the preface, he returns very briefly to the issue of the union of psychology and philosophy:

> Finally, *qua* speculative psychologist, one seeks to elaborate empirical theories of mind which are, if not philosophically untendentious, at least philosophically respectable. But of course there is more than one view of what constitutes philosophical respectability, and one has to choose. I have pursued the discussion in this book on the assumption that philosophical realism is a better philosophy of science that reductionism, and that in general, it is unadvisable for philosophers to try to make ontological points by using epistemological arguments.
>
> (ibid, p. ix)

The comments about realism are meant to license reasonable extrapolation from empirical data to unseen and perhaps unseeable structures. Fodor is repudiating the positivist idea that one must confine one's analysis to immediately verifiable things. This position is very much in line with the Kantian rationalism developed in the previous chapter. However, his comments regarding the relationship of ontology to epistemology could be interpreted as a departure from that tradition because they seem to license a pure ontology, separated from epistemology. In this respect, the comments are reminiscent of Churchland and Sejnowski's[62] comments, quoted in chapter two, about deriving theories of mind from pure empirical research. However, based on other comments Fodor makes, I don't think this is what he means. The point is more a methodological point than a philosophical one. What he is saying is that theories of mind—ontology—should stand or fall only according to empirical findings and theoretical integrity, not because they are or are not harmonious with anyone's epistemological deductions. Nonetheless, Fodor is a rationalist, and, as he comments (see quote beginning of chapter 3), "rationalists are nativists by definition." What this means is that he assumes that rationality is an a priori function, in the Kantian sense, and that, therefore, it is a given that ontological research

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[62] Only in this limited way. Fodor is generally dismissive of connectionism.

will have an epistemological base, albeit a minimal one. Moreover, a constant theme in Fodor's writing is the problem of accounting for rationality. Like Churchland and Sejnowski he wants to be able to account for rationality in cognitive/psychological terms—in fact for him this is the Holy Grail of cognitive science—but he is frankly realistic (a more cheerful way of saying "pessimistic") about the prospects for such an account. This is another crucial point that Rorty either missed, or more probably ignored. Fodor continually reminds his readers that the processing theories he is suggesting are extremely limited in scope—that they probably can only explain a small part of cognition. The reason for this, is that the processing theories Fodor has been working on for nearly forty years now, are all based on computational algorithms, which he himself very much doubts can explain major aspects of cognition, because they are too rigid to allow the sort of "abductive" states of mind that are so characteristic of humans. Nonetheless, he thinks the mind must rely on such computational routines to some extent—however minimal—and since we don't even have the beginning of a plausible theory for non-computational processing[63], he thinks the best we can do at present is learn as much as possible about the computational aspects of mind. This is important for our purposes because it explains some of the insularity between the processes Fodor works on and his views of the human mind as a whole. Fodor typically spends the majority of his books discussing the details of various cognitive, semantic, and computational theories, but in the background there is always the Quest, the desire to

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[63] In particular, Fodor considers connectionism to be an implausible non-computational theory. I'm personally not so sure. It seems to me that the mind might make use of connectionist strategies in conjunction with computational ones, but, of course many people think this. As Fodor would point out, it's easy to imagine such a marriage but very hard to model it.

somehow pull all the threads together and explain the big issues about mind that Rorty says are Cartesian induced phantasmagoria.

One of Fodor's most substantial discussions of this type, appears in *The Elm and the Expert* (1995). In his final chapter, aptly titled "Why We Are So Clever: An Epistemological Postlude," Fodor succinctly lays out the larger territory of SP, demonstrating unequivocally, contra Rorty, that not only are there "*roads* between cognitive psychology and epistemology," but that there is a veritable highway:

> One of the reasons it would be nice to have a respectable philosophy of mind is that one's treatment of other philosophical issues, from metaphysics to epistemology, and, no doubt, on beyond to ethics, aesthetics and philosophical anthropology, depends a lot on what one thinks that minds are like...I'll argue in particular, that the notion that causation of behavior is mediated by mental representations may be just what's needed to solve the mind/body interaction problem; and the notion that the content of mental representations is constituted by their etiology may be just what's needed to connect computational psychology to naturalized epistemology.
>
> (p. 81-82)

Before laying out the details of this naturalized epistemology, Fodor groups the historical approaches to the epistemological quandry of how representations do their work, into three categories:

> There are, roughly speaking, three kinds of philosophical opinions about how thoughts could interact with things that aren't themselves mind dependent.
>
> ♦ There is the view that the problem doesn't arise, either because, as behaviorists and eliminativists say there aren't any thoughts, or because as Idealists say, *everything* is mind dependent.
>
> ♦ There is the Cartesian view, which is that the interaction problem does arise but is unsolvable because interaction is miraculous.
>
> ♦ And there is the mental representation story, a version of which I have been trying in the course of these lectures to construct.

> Behaviorism, eliminativism and Idealism are preposterous on the face of them. They are councils [sic] of despair and deserve to be spurned as such.
>
> (p. 82-83)

These comments, together with Fodor's succinct summary of the philosophical positions associated with representations, show that Fodor's views are diametrically opposed to Rorty's. Although it's true that these comments were written long after *The Mirror* was published, they are entirely in line with the major theses of *The Language of Thought*, and, therefore, it is clear that Rorty's use of Fodor's quotes to bolster his own argument for epistemological behaviorism is an egregious and blatant example of intellectual piracy[64].

Continuing with Fodor's naturalized epistemology, there are two principle parts of the theory, each of which is further analyzable into problems and solutions. I will abstract the key components in outline form below. Wherever possible I have stayed with Fodor's original locutions, but for the sake of readability I eschew quotes. There are also many points that would no doubt be contested, especially by strong social constructivists. No doubt, Fodor is aware of these as well, but he ignores them because he is presenting the theory in a somewhat idealized form.

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[64] Some readers may be dismayed by this harsh judgement, perhaps thinking that Rorty may simply have a "different interpretation" of Fodor. There are two issues here. First whether or not a text like Fodor's can be confidently confined to a narrow range of interpretations such as the one I've offered here; and, second, if so, whether Rorty should be held accountable for retrieving a compatible interpretation. As for the first question, it is the purpose of this dissertation to argue "yes," as will become clear as we proceed. As to the second question, given Rorty's extensive background in philosophy, and his general cleverness with text, I find it vanishingly unlikely that he could simply have misread Fodor. Perhaps if the points were minor, that would be possible, but anyone who is even minimally familiar with Fodor would know that the interpretations Rorty presents are completely incompatible with Fodor's thought. Moreover, the sorts of misrepresentations Rorty presented require deliberate manipulation; they couldn't just be innocent mistakes.

An Outline of Fodor's Naturalized Epistemology

I. Thoughts are part of the causal structure of the world.

A) To be effective, thoughts must connect to the world, *and* to selves.
   1) This seemingly impossible task is solved by mental representations.

B) Mental representations are special because they can face both ways:
   1) to the world by representing it, largely veridically
   2) to behavior, by being its typical proximal cause
      a) This Janus trick is accomplished by *semantics*
      b) "vocabulary" of proximal representations converts to semantics through computational steps.

C) Major mystery for cognitive science here: *How* does the firing of neurons become semantic?
   1) Hume's solution[65] was through *associations* but that is ruled out because such a model would induce random connections. We need *appropriate* associations, i.e., *selections*.
   2) Crucial point is that thinking generally takes us from truths to truths, and we need to account for this.

D) Fodor's solution to the neuron to semantics problem:
   1) Mental processes that are the proximal causes of behavior are *computations* rather than associations.
   2) The properties of mental representations that determine their computational roles also carry information about the world
   3) Computation is syntactic (in the sense that it works by manipulating symbols according to specific, innate algorithms)
   4) thought is *productive* in that it is the possible rather than the actual etiology that counts semantically (thus we can *imagine* things not just reflect)

II. Truth

A) Connecting representations in two directions is only half the problem. We also need to ensure that the information the organism uses is veridical
   1) could let the world rule directly, but the organism clearly needs to *select* which bits of information become instructions for behavior.

B) Humans are *clever*—fact demands explanation—"clever" attributes include:
   1) we are able to have thoughts that are generally true
   2) we can *reflect* on our thoughts, i.e., have thoughts *about* our thoughts
   3) we can analyze thoughts *etiologically*, i.e., trace them to their causes

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[65] Of course Hume didn't put it in these terms, but, translated into contemporary terminology, this is what he thought.

C) Acquisition of knowledge through "managed cognition"
   1) although we cannot *decide* what to believe, we can put ourselves in the position to be convinced of a given proposition *P* iff certain conditions are met.
      a) example—consulting "experts": we can ask "experts" for information and can pre-decide to believe what we hear (within certain parameters). So, we can ask someone the time, and then *believe* the answer, if we know the person has a watch, etc.
   2) Can use same principle with nature through experiments (science)

This brief outline doesn't do justice to Fodor's treatment, but it distills the main ideas. Crucially, the theory assumes that humans have a truth detecting capacity that is innate and which is directly hooked to the world through representations of it. This much of the story is ancient, traceable to Aristotle, though his conceptions were different in some important ways. What is new is the computational aspects. Social constructivists may be tempted to seize on the point about experts, but they should realize that Fodor assumes that our assumptions about experts are also "managed" by contact with the world. That is, in many situations at least, we demand some proof of expertise, and we also engage complex webs of checks and balances to verify information. This doesn't imply that we are infallible—obviously that's not true—but it does say that we are not just social automata, doomed to be "possessed" by whatever discourse community we find ourselves in. A moment's thought about this should also reveal that some humans are more free than others with respect to how they deal with information; that is, some people believe a lot more false things than others, and some people are much less able to exchange false beliefs for true ones than others. The standard rationalist explanation for this, which is amply supported by studies, is that innate ability and education are the two principle factors that enable perspicacity. Educated people know more about the world, and they also know more about how to question, analyze, and verify claims. Since

"truths lead to truths," the larger the base one has, the better one is able to veridically extrapolate. Thus, rationalism, though "constraining" in some ways, is a counsel of hope and a commitment to individual freedom and dignity, in sharp contrast to Rorty's behaviorist "counsel of despair." In his closing comments Fodor speaks of the intellectual waste that such counsel has engendered through absurd doctrines like Rorty's Mirror story:

> The only philosophers who really *were* doubtful about the existence of other minds were relentless *anti*-Cartesians like Wittgenstein, Dewey, Ryle, Quine, and Rorty, and they were equally doubtful about the existence of their own. What we got for our efforts was mostly decades of behaviorism and the persistent bad habit of trying to run epistemological or semantic arguments for metaphysical conclusions. The end of this, I fear, is still not with us.
>
> (ibid, p. 102)

Yet, at least in psychology, Fodor sees signs of change:

> But although I try to be as pessimistic as the facts allow, I think this fiasco really is less that the whole story. There are, increasingly, signs of an emerging naturalist consensus that is Realist in ontology and epistemology, externalist in semantics, and computational in cognitive psychology...It used to be that the price of a scientific world view was going to be a pugnacious denial of the importance of minds in human affairs. ...You had to be very credulous to be prepared to swallow that. If Positivists found it bracingly tough-minded it struck nearly everybody else as crude, implausible, inhuman and, worst of all, irrelevant to their concerns...No wonder if the deconstruction of scientific realism became a matter of priority in philosophy [and literacy studies!] departments from the University of Paris to Emerson Hall and on to Berkeley...I think, that perhaps the very worst may now be over. One can begin to imagine a psychology of cognition that, at a minimum, doesn't imply its own impossibility. The productivity, spontaneity and rational coherence of behavior, which most naturalists in psychology used to feel called upon to denigrate...are not embarrassments for the kind of cognitive theory that I've been endorsing....Naturalism might turn out to be more of a humanism than, until quite recently, anyone had dared to hope.
>
> (ibid, p. 103)

I hope that this excursus has proved my point that Rorty's position is completely incompatible with Fodor's, and that it has sharpened the issues at stake. According to Rorty, we are to accept a radical separation between cognitive processes and knowledge formation as a whole within individuals. If such a position is accepted we are left with the cynical notion that our beliefs are wholly dependent on "conversation" and not capable of interrogation through personal empirical analysis guided by reason. Fodor, on the other hand, assumes that reason is innate, sensible representations usually veridical, and that humans have the unique capacity to use these two abilities in conjunction to extrapolate "from truth to truth."

Theoretically, Fodor's position is far more coherent since it makes only the conservative assumption that local cognitive processes contribute to cognition as a whole, and, hence, to the organism's beliefs, whereas Rorty must somehow account for his assumption that local processes—which he doesn't deny occur—are somehow radically disjoined from cognition as a whole, particularly from the organisms beliefs about the world. The evidence from science unequivocally supports Fodor's position. Work in cognitive science and linguistics over the last fifty years or so, confirms that representations are a central fact about the human mind, and that most of the processing rules for handling representations are innate. Moreover, in the two cases that have been most extensively investigated, vision and language, there is massive evidence that local transformations of information are integrated by the organism into coherent macro representations, which the organism uses consciously. Thus, in vision, the organism transforms an impoverished light array, which initially consists only of "intensity values [at particular points on the retina]" into veridical images of things in the world, probably

in a several stage process that includes integration of information from other sources (Stillings, et. al., 1995). In the case of language, the acoustic signal that arrives at the ear is radically under-specified in terms of the features that native speakers recover, and this is true at several distinct levels, including phonology, morphology and syntax, as well as at the discourse level as a whole. As in vision, what we find with language is that the lower level processes are beyond introspection, but at the macro level, the integrated representation is available to the organism for conscious analysis and use. That is, though speakers are entirely unaware of the complex phonological, morphological, and syntactic computations and transformations that occur almost instantaneously during language use, they have conscious access to surface structure and meaning (Chomsky, 1986; Kenstowicz, 1994; Pinker; 1994). The logical inference from these facts is that further macro-level integration of other sorts of representations and, indeed other macro representations—"intuitions," particular scraps of received knowledge, particular beliefs, cultural ideals, etc.—are also somehow integrated within the organism in the formation of beliefs (Sperber, 1996). If these ideas are roughly correct, and, again, there is massive empirical evidence that supports them, as well as compelling theoretical reasons to accept them, then theories of representation should occupy a central position in any discipline—like literacy studies—for example that is concerned with how we process information. By the same token, if representationalism is correct, then we should expect massive confusion and incoherence from approaches that attempt to deny it. Antifoundationalist literature is often deliberately vague and evasive about specific cognitive premises associated with antifoundationalism, but when they are teased out, as I've done above, they are revealed to be completely incoherent. Moreover, the very fact that

antifoundationalists have ontological and epistemological doctrines is a direct contradiction of the central tenet of antifoundationalism, i.e., that "there is nothing more secure than mere belief or unexamined practice." Whatever their factual status, Rorty's claims about the relation of cognitive processes to belief formation *are* ontological/epistemological claims and very substantial ones at that. So, here again, we have blatant contradiction, both of the general antifoundationalist claim to not hold foundational beliefs, and also of Rorty's claims against epistemology.

Extended arguments could easily be made here against holistic models of mind and language, through presentation of evidence from psychology, linguistics, and child language acquisition, but this literature is widely known and easily accessible, so I will remain focussed on the task of explicating and refuting particular points in Rorty's exposition of antifoundationalist philosophy.

Definitions of Knowledge

At this juncture I want return to Rorty's arguments about the justification of beliefs and actions. Recall that Rorty claims—inexplicably—that "once explanation and justification are held apart there is no reason to object to explanation of knowledge in terms of representations" (ibid, p. 210). As is so often the case with Rorty, there are hidden assumptions in this claim that need to be addressed before the central claim can be analyzed. In this case, Rorty assumes, in the tradition of analytic philosophy, that knowledge is "justified belief." Sometimes, as in the definition given above for awareness, he includes "true" in the definition ("justified *true* belief"), but he is not consistent in this, and the general drift of his arguments is decidedly for "justified belief" without "truth." And, in any case, even where he does include "truth" it's not clear that

that anything changes since he also argues that "truth" is [only] a property of sentences" (Rorty, 1988, p. 83) ; so we can assume that for Rorty knowledge is "justified belief" without the "truth" attached. There are two problems with this. First, such a definition removes "congruence with reality" from the definition of "knowledge," since mere "justification" does not entail such congruence. Second, once truth is removed from justification, representations disappear from language as well. Rorty is explicit about this, although he puts the implication the other way around:

> To drop the idea of languages as representations, and to be thoroughly Wittgensteinian in our approach to language, would be to de-divinize the world. Only if we do that can we fully accept the argument I offered earlier—the argument that since truth is a property of sentences, since sentences are dependent for their existence on vocabularies, and since vocabularies are made by human beings, so are truths.
>
> (ibid, p. 83)

Once again we are confronted with a welter of interlaced claims that need to be carefully disentangled and analyzed. All of these claims will be addressed in time, but, at the moment let's stay with the problem of separating "truth" from knowledge. Odd results follow from such separation, even at mundane levels. For example, the astute though hapless victim of a clever con may have had plenty of "justified belief" when he accepts various *false* propositions—that the credentials displayed by the con man are genuine, that reports of his impeccable reputation are true, that the discount he offers for pre-payment is reasonable considering his need to purchase materials, etc. If the con man is good, he can provide all sorts of convincing "justification" to believe these things, though of course they are all false. But, under a "justified belief" definition of knowledge, we will have to say that the victim "knew" the con man was telling the truth, even after the victim has faced up to the fact of being swindled. Clearly there is something wrong with

this; we want to be able to distinguish states of mind that correspond with reality from states of mind that do not. Thus, in such cases, people do not say the victim *knew* the false claims, but rather only that he *believed* them.

Until the advent of analytic philosophy, toward the end of the nineteenth century, this common sense definition of knowledge—"beliefs that correspond with reality,"—was maintained, both in science and philosophy. Ironically, it was the attempt to formally define this relationship that undermined it. Beginning with Frege, and continuing through Russell and the "early" Wittgenstein,[66] a massive effort was made to account for meaning as well-defined relations between linguistic entities and the world. But as this effort continued, intractable logical problems emerged because semantics is not ultimately reducible to logical relations. As Katz puts it, "the logical powers expressed in the formulas of calculi developed on the basis of Frege"s or Russell's work diverge significantly from intuitively clear logical powers of the sentences that are their obvious counterparts" (Katz, 1990, p.68). As an example of this difference, Katz cites the problem of negation of simple propositions. In the logical systems' "calculi," negation is an all or nothing matter that operates at the propositional level, and in Wittgenstein's *Tractatus* system, such propositions in language had to be tied to real things in the world. Thus proposition *P* would be true just in case *P* was true and ~*P* would have to be the *absence* of *P*. But, in natural languages we have all sorts of simple sentences that contain no negative elements and yet implicitly negate other simple

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[66] Wittgenstein's philosophy is commonly divided between the *Tractutus Logico-Philosophicus* period (1913-1929) during which he tried to perfect the logical semantic system Frege and Russell initiated, and the later period of his *Philosophical Investigations* (1929-1951), during which he largely repudiated the *Tractutus* philosophy and adopted a "meaning defined by use" orientation toward language.

sentences. Thus, the sentence *the spot is blue* implicitly negates the sentence *the spot is red* since, as Wittgenstein noted, "for two colors to be at one place in the visual field is impossible." However, according to the formal rules of logic Wittgenstein relied on, proposition *P* cannot contradict proposition *P'* unless there is a negative operator:

> (It is clear that the logical product of two elementary propositions can neither be a tautology nor a contradiction. The assertion that a point in the visual field has two different colors at the same time is a contradiction.)
>
> (Wittgenstein, 1921/ 1999; parentheses original)

The problem, then, is the fact that the two elementary propositions (*the spot is blue; the spot is red*) clearly do contradict each other. In fact, each *implicitly* contains the proposition *the spot is not red/blue,* as well as an infinite range of other propositions. Wittgenstein tried to resolve this problem by claiming that "Atomic propositions, though they cannot contradict, may exclude each other." However, as Katz notes, "This doesn't help, since in the logical framework within which he was still working, there is no content to the notion of a relation of exclusion which is not logical incompatibility" (ibid, p. 69)

Eventually, as Wittgenstein confronted more and more problems of this nature, he came to see the *Tractatus* project as hopeless. His response to this dilemma was to withdraw from philosophy for several years, after which he reemerged with a completely new "language game"/use orientation toward language. But both approaches undermined the common sense understanding of knowledge—the logical program because it failed and therefore led philosophers to think that perhaps correspondence between language and reality was a chimera, and the language game/use approach because it encouraged a hermetic view of language in which propositions are "verified" either by their use, or by their relationship to other propositions, but not by their relation to the world.

It is at this juncture (one that occurred gradually over at least half a century) that analytic philosophy completely diverged from classical philosophy and also from science and common sense notions of rationality. Though Rorty criticizes analytic philosophy for other reasons, he wholeheartedly accepts the technical space that it has created for hermeticism in language. In my view, this technical space and nearly everything derived from it is a serious mistake, the result of accepting certain basic premises that are simply false. At the bottom of analytic philosophy is the notion that external manifestations of language, i.e., sounds, scribblings, must either have a direct and fully formalizable relation to things in the world, OR be written off as unregulated 'games' that interlocutors mysteriously understand and/or make up as they go along, and that can therefore only be described and analyzed behavioristically. But there is a third alternative, the one adopted by generative linguistics, which considers sounds and scribbles to be manifestations of intensional[67] states of mind, which must be recovered through a computational process along the lines of what Fodor proposes above. We will consider this alternative at length in a later chapter. At the moment what is important is to understand that analytic philosophy became alienated from the concerns of most scientists, including many linguists, which is a momentous event in philosophy since,

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[67] "Intension" is a poorly glossed term in the literature, and one that is used in several different ways. Frege had a formal notion of intension that was non-psychological, an idealized "sense" connected to an idealized language such that the intension designated its "extension" or reference. Thus, for Frege, "the morning star" is one of the intensions of the extension "venus" and "the evening star" is another (Green, 1996). By contrast, Katz replaces the formal relation between sense and reference (i.e., between intension and extension) with a "mediated" relation that must be worked out for each token. For Katz, then, "intension" includes a complex array of grammatical (including "semantic") attributes that "provide identifying properties necessary to fix their extensions," but only with the application of mediating rules on a case by case basis (Katz, 1990, p. 89). I adopt Katz' use of intension, with the reservation that I am not sure about his insistence on "realism" in language, though I find the idea intriguing. But, so far as intensions are concerned, it seems to me that Katz' system is amenable to a "natural" position on language just as well as to a "realist" position (though, ironically, I think he would disagree with this as well).

hitherto, the concerns of scientists and philosophers were always closely intertwined. One result of this, as we see here, is that the commonsense notion of knowledge as representations of the world that correspond with reality, a notion that is assumed by many scientists (although some instrumentalists might want to replace "reality" with something less comprehensive, say "demonstrated facts") and by nearly all lay people[68] (i.e., non-specialists in analytic philosophy or some postmodern enterprise) can no longer be treated as a common supposition. However, this is a fact that should cut both ways. If we have reached the point where rationalists must exhaustively defend every foundational notion they rely on, antifoundationalists should do the reverse, i.e., exhaustively and systematically critique these foundations and show why they are unreliable before assuming that they are. Rorty purports to have done so in *The Mirror* and elsewhere, but analysis of his criticism shows that in every case it consists either of doubtful historiographical conjecture about the alleged fallacies that various philosophers fell into and the "harm" that classical epistemology has done, egregious misrepresentations of the literature in cognitive science, or, in many cases, fantastic ontological assertions, like the one above about "language and reason descending" wholesale at age four, which, even if true, would only enhance the need for epistemology, since we would certainly need some pretty fancy theories of knowledge to explain how that could occur. Of course the reason for the lapse is not hard to find. Any systematic attack on epistemology entails a new system of knowledge, i.e., a new epistemology, and,

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[68] Even Rorty agrees with this. Regarding the common notions of truth, Rorty says, "The non-philosophers...probably took the idea of truth to mean something like 'the idea of an accurate representation of the intrinsic nature of reality.' Most people still take it to mean something like that." (1998A, p. 79).

thus, paradox. So, instead, Rorty engages in rhetorical subterfuge; for example, by offering "hermeneutics" as a non-substitute for epistemology, above, and here, covertly introducing the "justified belief " definition of knowledge, without discussion, thereby setting up grounds for accepting the analytic philosopher's notion that knowledge is not determined by reality but, rather, only by coherence among propositions, i.e., "conversation," without having to argue for that position.

Returning to Rorty's major claim about the need to separate explanation of knowledge and justification of belief, I don't see how this is possible under either definition of "knowledge." Correspondence definitions of knowledge necessarily intersect with "justification" since under such definitions, "knowledge" is the synthetic concept "true belief[69]," but it seems to me that "justified belief" definitions are also synthetic in the same sense, the only difference being that they substitute "justified" for "true." It is an oft remarked psycholinguistic fact that "belief in X" cannot contradict "knowledge of X," which is demonstrated by such "impossible" conjoined propositions as *I believe it's raining, but I know it's not.* Clearly, this impossible conjunct is equally impossible in reverse order—*I know it's not raining, but I believe it is.* What these examples demonstrate is that, under either definition, knowledge is inseparable from belief. Now, belief itself expresses the analytic concept, "that which is held to be true,"

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[69] An interesting problem is whether *unjustified* true belief should count as knowledge. Should the gambler who "knows" that the dice always come up snake eyes at five seconds to midnight be credited with "knowledge" if the dice coincidentally accommodate him? I'm strongly inclined to say no, and thus to argue for a "*justified* true belief" definition of knowledge, rather than just a "true belief" one. But that choice opens up the difficult space of what should count as justification. It is tempting to argue for justification in terms of true causal analysis, but then we won't be able to accept justification by authority, which also seems indispensable. I think a chain of reasoning can connect authority to causes, but I leave the details unresolved here.

or, in verbal form, the mental state of "holding X to be true." Thus our two definitions of knowledge come down to either 1) "that which is held to be true, and is true" (correspondence), or 2) "that which is held to be true and is justified" (justified belief). So, on either account, any explanation of "how knowledge arises" will entail an explanation of how "that which is held to be true" arises, and therefore explanations of how knowledge arises are inseparable from justification. Some interesting evidence of this entailment is the fact that despite his admonishments to separate explanations of knowledge and justification, Rorty fails to do so himself. In his model, knowledge is acquired *solely* through conversation, but so is justification. So, he contradicts himself on this point as well.

V: KNOWLEDGE

Learning and Hermeneutics

Thus far, we have seen that there are radical differences between Rorty's position on knowledge and the rationalists. In general, rationalists argue that knowledge requires a synthesis of representations and reason, whereas Rorty argues for a holistic transfer of knowledge—which term is significantly different from the rationalists' version—via conversation. However, he is not consistent in this, particularly not in his initial descriptions of hermeneutics:

> My use of the terms *epistemology* and *hermeneutics* to stand for these ideal opposites may seem forced. I shall try to justify it by noting the connections between holism and the "hermeneutic circle." The notion of knowledge as accurate representation lends itself naturally to the notion that certain sorts of representations, certain expressions, certain processes are "basic," "privileged," and "foundational." The criticisms of this notion... are of the form: We will not be able to isolate basic elements except on the basis of a prior knowledge of the whole fabric within which these elements occur...This holist line of argument says that we will never be able to avoid the "hermeneutic circle"—the fact that we cannot understand the parts of a strange culture, practice, theory, language, or whatever, unless we know something about how the whole thing works, whereas we cannot get a grasp on how the thing works until we have some understanding of its parts.
>
> (1979, p. 319)

There is nothing objectionable in this passage other than the notion that rationalists are somehow excluded from thinking in these reciprocal terms. But there is no reason to think they should be, based on the notions of rationalism developed in the previous chapter. For rationalists, the interesting question is whether the hermeneutic circle is to be interpreted viciously or virtuously, and if the latter, what the result of the process is, and, most importantly, what mechanism guides the process. Rorty answers the first

question affirmatively, but he is decidedly vague about the second question, and downright evasive about the third:

> This notion of interpretation suggests that coming to understand is more like getting acquainted with a person than like following a demonstration. In both cases we play back and forth between guesses about how to characterize particular statements or other events, and guesses about the point of the whole situation, until gradually we feel at ease with what was hitherto strange.
>
> (p.319)

According to this description, hermeneutics promotes knowledge, but this begs both epistemological questions: 1) What is the ultimate source of this new knowledge and 2) How is it verified? Thus, any claim for the operation of hermeneutics entails some underlying epistemology, contrary to Rorty's declaration of epistemology's "demise." Rorty disguises this by substituting the terms "characterize" and "feel at ease" for "understand" and "develop knowledge of," but if we apply these terms in any meaningful way to the examples he lists—" [a]strange culture, practice, theory, language, or whatever"—it's pretty hard to deny that knowledge is involved. This might seem to be a trifling argument over terminology, but there's more at stake here than that, for there are meaningful distinctions that we could make between "knowledge" and "feeling at ease." Though it's probably true that knowledge of X and feeling at ease with X are often congruent, they aren't necessarily so. I may feel pretty comfortable about, say, my bagpipe playing, even though it is painfully obvious to the neighbors and professional pipers that "playing" is a wildly optimistic euphemism for what I actually do with the pipes. But under Rorty's "hermeneutics" we must either lose that useful distinction—between how I feel and what I actually know how to do—or admit that knowledge is an essential goal of the hermeneutic process. Then there's the issue of how one virtuously

moves through this circle. Rorty talks vaguely about "guesses," guesses about "events" and about the "point of the whole thing." But what's behind a guess? Arguably, guessing is a rational process wherein we evaluate the factors and choose the solution that appears best, so it begins to look like there's no great difference between Rorty's "hermeneutic circles" and Habermas's virtuous circles, although he is clearly trying to claim that there is. Moreover, it's pretty hard to exclude standard reasoning procedures associated with epistemology—observation, representation, hypothesis formation, data collection, analysis, reformulation of hypotheses, etc.—from such a process. Perhaps Rorty has the idea that "hermeneutic" learning occurs unconsciously, and perhaps he thinks that would exclude these processes. But we don't know that that's what he thinks because he remains vague, and even if he did think such things many questions remain. For example, what exactly does it mean to learn something unconsciously? Can there be unconscious guesses? Can reason operate unconsciously? Can the mind integrate representations unconsciously? How would we know? Actually, there is a large literature in psycholinguistics, child language learning, second language acquisition (SLA), and education on these issues, and some insight. For example, we know that all normal children (say under age five) "acquire" full grammatical competence in the language(s) of the surrounding community, whereas adult learners virtually never do. This is a very robust fact that demands some explanation. The best explanation that we have is that children have access to abstract, universal grammatical principles and representations of linguistic structure (UG), which they employ during language acquisition, whereas adults either have no access to UG or perhaps very limited access and therefore fail to achieve full competence. If we pursued the literature in the fields mentioned above, we would

find many competing theories about learning and acquisition, nearly all full of various representational theories, and a range of evidence to support them. The point is, scientists who actually study learning are not locked into a narrow "epistemological" perspective that excludes "hermeneutical systems," as Rorty describes them.

Rorty and Kuhn

Probably, Rorty tries to make this split because he wants to get away from standard notions of knowledge, since these notions involve a chain of reasoning that leads to a foundational philosophy. Once we admit that knowledge is possible, we have to ask how we are able to distinguish knowledge from nonsense, and this leads to empiricism, reason, and all the other "traditional" ideas Rorty wants to reject. This leaves Rorty in an untenable position, however, regarding public knowledge, particularly scientific and technical knowledge, for, according to Rorty's favorite historian of science—Thomas Kuhn—it is an uncontroversial fact that much of this knowledge is acquired through systematic, consciously designed empirical research, i.e., "normal science" (Kuhn, 1979, p. 52). Moreover, though philosophers may debate the ultimate meaning of "knowledge," arguing as to whether it is to be understood transcendentally, i.e., as information about how the world "really is," or only instrumentally, or perhaps in terms of our sensory apparatus, i.e., in Kantian terms,[70] it is again uncontroversial that knowledge leads to the "ability to account for a wider range of...phenomena[71] or to account with greater

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[70] Instrumentalism is not the same thing as Kantian subjectivity. Instrumentalists are neutral regarding ultimate reality, focussing only on observations. Kantians on the other hand, are free to extrapolate from observations to the "objective" world. I explicate this notion at length below.

[71] The ellipsised word is "natural." I have removed it because I am making a more general point than Kuhn was (he was specifically referring to natural science); however, I think the remark is still consistent with his argument.

precision for some of those previously known" (Khun, 1970, p. 66). That is, new knowledge is not just another way of "constructing" reality; rather, to qualify as *knowledge* it must explain reality in ways that are measurably better than the old theories, and in ways that are independent of the observers. Thus, for example, if a new theory can more accurately predict the details of some regularly recurring phenomenon, like the weather, or the movement of the planets, we can say we have new knowledge. As for how new knowledge is acquired, Rorty repeatedly claims to have adopted Kuhn's framework, but that framework does not justify Rorty's rejection of epistemology. Kuhn is famous for having shown that science doesn't always proceed according to the neat and tidy procedures of "scientific method"—a notion Rorty applauds—but what is often overlooked—or in Rorty's case avoided—in Kuhn's work, is his stress on the extent to which revolutionary science *depends* on normal science. For, according to Kuhn, it is only when we have a normal paradigm that anomaly becomes visible. Kuhn's framework, then, places hermeneutics in a complementary relationship to epistemology; epistemology—or, rather, paradigmatic assumptions derived from epistemology, e.g. Bacon's dicta[72] of scientific method—provides stability for normal science and this environment makes possible the intuitive, "hermeneutic" leaps that open the way for new paradigms. Kuhn's view of the history of science could even be seen as a macro-hermeneutic circle through which the entire culture of science moves, a view that is

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[72] I refer to Bacon's rules for science as elaborated in *Novum Organum*. See p.54 for brief exposition and analysis of this position. Bacon was perhaps excessively empiricist in his formulation of these rules, and many philosophers of science criticize him for this (e.g. Bhaskar, 1997). However, this is matter of degree not kind. Bacon called for an interaction between theory and empirical data. Scientists may debate the relative proportion that each part should play, but no one denies the principle that there must be such an interaction.

supported by his comment that "research under a paradigm must be a particularly effective way of inducing paradigm shift" (ibid, p. 52).

This analysis of Kuhn's work strongly supports rationalism and classical notions of epistemology. However, it is also true that Kuhn is often cited by strong social constructionists, including Rorty, in support of the opposite, radically subjective view of science, the view that our experience of the world is so socially conditioned that paradigms are only relevant to the culture they appear in. According to this perspective there is a radical "incommensurability of paradigms," which makes objective comparisons among them impossible (Sokal and Bricmont, 1997). Nor is this a misreading of Kuhn, for he does make comments that support these strong social constructivist views. Nonetheless, much of his work supports the rational view developed above. Reading Kuhn is thus something of a puzzle. Sokal and Bricmont explain how it is possible to derive these conflicting perspectives from the same work:

> The basic problem is that there are, as the philosopher Tim Maudlin has eloquently pointed out, *two* Kuhns—a moderate Kuhn and his immoderate brother...The moderate Kuhn admits that scientific debates of the past were settled correctly, but emphasizes that the evidence available at the time was weaker than is generally thought and that non-scientific considerations played a role...By contrast, the immoderate Kuhn—who became, perhaps involuntarily, one of the founding fathers of relativism—thinks that changes of paradigm are due principally to non-empirical factors and that, once accepted, they condition our perception of the world to such an extent that they can *only* be confirmed by our subsequent experiences. Maudlin eloquently refutes this idea:
>
> "If presented with a moon rock, Aristotle would experience it as a rock, and as an object with a tendency to fall...Similarly, ever better telescopes revealed more clearly the phases of Venus irrespective of one's preferred cosmology. . ."
>
> (Sokal and Bricmont, 1997, p.75; internal quote from Maudlin, 1996)

Thus, as S&B conclude, "while it is true that scientific experiments do not provide their own interpretation, it is also true that the theory does not determine the perception of the results" (ibid, p. 76).

Put in the terms we've been using throughout this discussion, i.e., in terms of learning circles, whether "constrained" or "hermeneutic," the "moderate Kuhn" believes in virtuous circles whereas the "immoderate Kuhn" believes in vicious circles. But the choice between these perspectives should be easy to make. If the immoderate Kuhn were correct, we would not expect to see the steady advance of science that is so undeniable over the last four hundred years; moreover, if the immoderate Kuhn were correct, then his own theories would also be subject to immoderate Kuhnism and hence not believable either. Many people have pointed this out including Sokal and Bricmont (1997), Kukla (2000), Naess (1972), and even Feyerabend (1993), someone who is generally associated with relativism himself.

Rorty's treatment of "hermeneutics" reveals the same moderate/immoderate split that Maudlin discovered in Kuhn's work. As we've seen, Rorty's initial descriptions of hermeneutics are entirely compatible with rational epistemology—they are virtuous circles that end in greater knowledge. However, these descriptions are made in the context of strong denunciations of epistemology, which, as I've argued above, contradicts virtuous hermeneutics. Thus from the inception, there is a conflict in Rorty's position, one that becomes ever more pronounced as Rorty develops his theory of hermeneutics in the concluding two chapters of *The Mirror.*

Since Rorty's argument against representational theories of knowledge, i.e., classical epistemology, is supported by the allegation that philosophical systems are

nothing more than elaborate "language games," he needs to demonstrate that what is normally regarded as successful use of representational theories to extend knowledge, such as progress in science and other areas of intellectual endeavor, is really just a shifting of "vocabularies," and to do that he needs to show a repeated pattern of such shifts throughout history. Not surprisingly, he relies heavily on the "immoderate Kuhn" for the latter, both by selectively citing Kuhn's work, and by adopting his historiographical methods, but this puts him in the same *tu quoque* bind—self-refuting immoderate Kuhnism—as Kuhn's "immoderate brother." Moreover, by linking his arguments to Kuhn's, Rorty implicitly makes the claim that the shift he is advocating, away from epistemology and toward the hermeneutics, is just another in a long line of paradigm shifts, and this opens him up to criticism by comparison. Kuhn's argument is that paradigm shifting is a revolutionary but regular process that occurs in science "when...the profession can no longer evade the anomalies that subvert the existing tradition of scientific practice" (Kuhn, 1970, p.6). Therefore, to make the comparison stick, Rorty has to show that there are "anomalies" in the philosophical paradigm that are "subverting" the progress of philosophy. His charges of dualism is one such attempt, but we have seen that cognitive science[73] rejects that position, so philosophy is *not* being so subverted, though perhaps we are still in a paradigm shift toward a cognitive/psychological version of epistemology, which explains why some philosophers like Rorty are still fighting against the old paradigm.

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[73] Once again, I would argue that psychology, linguistics and cognitive science all regularly deal with themes that have classically been considered "philosophical," so one must consider ideas from all these fields when one considers contemporary philosophy. I don't think Rorty would disagree with this, in principle, but, as I've shown, he has done a poor job of practicing it in his criticism of philosophy.

Moreover, it is apparent that the kind of shift Rorty is advocating is not a normal Kuhnian shift. What is usually stressed in Kuhn's work is the revolutionary aspects of these paradigm shifts, but the *regularity* of these "revolutions" may be just as important. Kuhn stressed that his was an "extended conception," in which even relatively minor discoveries are treated as "revolutionary," not so much because of their impact on the larger community, or even on the scientific community, but, rather, because they have a similar, though much smaller, transformative effect on the practice of science as the major revolutions (ibid, p. 8). Thus, his work was aptly titled "the *structure* of scientific revolutions," not just "scientific revolutions." As noted above, Kuhn stressed throughout his work that this scientific structure requires a complementary relationship between normal and revolutionary science. Crucially, the "revolutionaries" nearly always come from within the dominant paradigm, and, in general, they use the same methodology and rely on many of the same assumptions as the normal science they seek to replace. For example, Copernicus relied heavily on Tycho Brae's observations, all of which were made under Ptolomaic assumptions. Moreover, these same observations continued to serve the scientific community well, long after the Copernican Revolution, just as Pre-Copernican mathematics, telescopes, and methods of recording did. In sum, Kuhn's work shows that although each revolution in science disrupts a previously stable paradigm, the macro-paradigm of science articulated so clearly by Bacon has remained steady for four hundred years and is still current today. For this entire time many if not most scientists have seen and continue to see their work as the job of discovering objective truths about a reality that is indeed "out there" (Sokal and Bricmont, 1998, p.266; Bhaskar, 1997; Einstein, 1964, p. 50; Penrose, 1989, 299), and there has been

substantial agreement about what constitutes acceptable scientific methods during all that time and into the present. Moreover, even before Bacon, during the entire Airstotelian/Catholic period people were still dedicated to discovering the nature of reality, which was overwhelmingly assumed to be real and separate from human imagination; the difference was they started from a large "revealed" body of assumptions. Kuhn describes the pre-Baconian period, between Aristotle and Bacon, not as *pre-scientific* but, rather, as *pre-paradigmatic*. In making this point, Kuhn briefly analyzes the work of pre-Newotonian and post-Newtonian optics, observing that

> no period between remote antiquity and the end of the seventeenth century exhibited a single generally accepted view about the nature of light. Instead there were a number of competing schools...most of them espousing one variant or another of Aristotelian or Platonic theory. One group took light to be particles emanating from material bodies; for another it was a modification of the medium that intervened between and the body the eye; still another explained light in terms of an interaction of the medium with an emanation from the eye...Each of the corresponding schools derived strength from its relation to some particular metaphysic, and each emphasized, as paradigmatic observations, the particular cluster of optical phenomena that its own theory could do most to explain.
>
> (Kuhn, 1970, p. 12-13)

What stands out from this history is that despite the multiplicity of foundational premises, each derived from a different "metaphysic," all of these "schools" assumed a common reasoning process and rules of scientific evidence: success in theories was measured by how well they explained the data, and the theory deemed the best was the one that explained the most data. Although the metaphysically derived premises seem quaint to us—as does the very idea of deriving premises of natural science through metaphysics—there is nothing foreign in the reasoning. Moreover, despite the false premises, the process of attempting to fit theory to observations had positive results:

> At various time all these schools made significant contributions to the body of concepts, phenomena, and techniques from which Newton drew the first nearly uniformly accepted paradigm for physical optics. Any definition of the scientist that excludes at least the more creative members of these various schools will exclude their modern successors as well. Those men were scientists.
>
> (ibid, p.13)

Again, Kuhn shows that in science, perspicacious theory can emerge even from a welter of wrong theories and presuppositions as long as rational investigation is a significant part of the mix. Kuhn's postscript in which he discusses the confusion and controversy generated by moderate versus immoderate interpretations of his work indicates that this is really the dominant thesis he wanted to articulate in his work:

> Taken as a group or in groups, practitioners of the developed sciences are, I have argued, fundamentally puzzle-solvers. Though the values that they deploy at times of theory-choice derive from other aspect of their work as well, the demonstrated ability to set up and to solve puzzles presented by nature is, in case of value conflict, the dominant criterion for most members of a scientific group...In the sciences, I believe, the value accorded to puzzle-solving ability has the following consequences.
>
> Imagine an evolutionary tree representing the development of the modern scientific specialties from their common origins in, say, primitive natural philosophy and the crafts. A line drawn up that tree, never doubling back, from the trunk to the tip of some branch would trace a succession of theories related by descent. Considering any two such theories, chosen from points not too near the origin, it should be easy to design a list of criteria that would enable an uncommitted observer to distinguish the earlier from the more recent theory time after time. Among the most useful would be: accuracy of prediction; the balance between esoteric and everyday subject matter; and the number of different problems solved...Later scientific theories are better than earlier ones for solving puzzles in the often quite different environments to which they are applied. That is not a relativist's position and it displays the sense in which I am a convinced believer in scientific progress.
>
> (Kuhn, 1970, p. 206)

Two points should be taken from these comprehensive reflections. First, and most important, Kuhn is unequivocal about the centrality of reason in science, and likewise

about the relation of reason to nature. According to Kuhn, Bacon's dicta *are* axiomatic in science, and that has not changed. Second, progress can be measured by the ability of models to solve problems. Were these reflections complete, it would be hard to imagine how anyone could have derived immoderate Kuhnism from *The Structure*; however, the immoderate Kuhn steps in forcefully in the next passage:

> Compared with the notion of progress most prevalent among both philosophers of science and laymen, however, this position lacks a an essential element. A scientific theory is usually felt to be better than its predecessors not only in the sense that it is a better instrument for discovering and solving puzzles but also because it is a better representation of what nature is really like. One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth. Apparently generalizations like that refer not to the puzzle-solutions and the concrete predictions derived from a theory but rather to its ontology, to the match, that is, between the entities with which the theory populates nature and what is "really there."
>
> Perhaps there is some other way of salvaging the notion of 'truth' for application to whole theories, but this one will not do. There is, I think, no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its "real" counterpart in nature now seems to me illusive in principle. Besides, as a historian, I am impressed with the implausibility of the view. I do not doubt, for example, that Newton's mechanics improves on Aristole's and that Einstein's improves on Newton's instruments for puzzle solving. But I can see in their succession no coherent direction of ontological development. On the contrary, in some important respects, though by no means in all, Einstein's general theory of relativity is closer to Aristotle's than either of them is to Newton's.
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> (ibid, p. 206).

Behold the source of antifoundationalism. From this strongly counter-intuitive assertion, that there is no reason to assume that models that are better at solving problems are also closer to reality, all manner of "constructionist" doctrines flow. I will return to this immoderate Kuhnian argument, which, henceforth, I will refer to as "strong instrumentalism," in detail, in a following section. However, I first want to trace the

influence of immoderate Kuhnism in Rorty's work, and also note some important differences between what immoderate Kuhnism "licenses" and what Rorty advocates.

Immoderate Kuhnism licenses several antifoundationalist premises, specifically:

1) that all models of reality are equal in terms of accuracy (strong instrumentalism)

2) that truth is a naïve concept (and hence the antifoundationalist practice of enclosing the word *truth* in contemptuous scare quotes ('truth') whenever referring to it)

3) that all perspectives are equally and detrimentally "theory laden"

On the other hand, there are other tenets of antifoundationalism, often traced to Kuhn, that are *not* licensed, even by immoderate Kuhnism:

A) that individual perspectives depend entirely on the discourse community.

B) that scientific reasoning itself is just another, learned cultural mode of thinking.

C) that there are no independent (i.e., from the discourse community) methods of choosing among theories.

Tenets A) and C) are contradicted by Kuhn's insistence on the scientific community's reliance on empirical data. Whatever social conditioning may be operating, it is a fact that the scientific community places a very heavy value on empirical data and that scientists go to great lengths to collect and present such data 'objectively,' and, moreover, that very often when push comes to shove, the data is the deciding factor when choosing among theories. Note that this need not *always* be true for the general principle to hold; much hay is made of the fact that *sometimes* scientists seem to ignore the data, but the fact remains that data is generally central in science. This is clearly not the case in many other fields—conspicuously philosophy and literature—so comparisons that ignore this

fact should be treated with extreme skepticism. In any case, we can say that science does not rely exclusively on the discourse community for its "justification" of belief, an opinion that Kuhn agrees with wholeheartedly.

As for tenet B)—that scientific reasoning is merely a cultural convention—this is trickier. Kuhn does remark that "only the civilizations that descend from Hellenic Greece have possessed more than the most rudimentary science" (ibid, p.168). However, this can be interpreted in a number of ways. From a strong social constructivist position, in which *everything* is deemed "conventional," the narrow distribution of scientific practices across cultures can be interpreted as support for constructivism, since, it is argued, if scientific ways were objectively true, other cultures would have discovered them too. But this is not the only way to reconcile the historical facts. Scientific realists would argue that Baconian methods are not obvious and, further, that it takes a rather large cultural commitment of resources to sustain scientific research. Given this, it is not surprising that it took people a long time to discover the methodology of science and to apply it successfully; moreover, it is not surprising that this method of discovery was made only once in one part of the world[74]. Scientific realists would also point to the fact that although these methods and practices originated locally, they have now been exported all over the world with great success, and that they are practiced within many different cultural frameworks.

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[74] This is surely an exaggeration, though. No doubt people have been "discovering" empirical methods of verification and theory construction since the earliest pre-historic times. What is different about the Baconian revolution is that for the first time in history the principles were clearly and completely articulated and a major cultural commitment to them was made.

In sum, only one of Kuhn's claims—the claim that there is no relationship between the ability of a model to solve problems and the proximity of models to reality—supports antifoundationalism, and this claim is by far his weakest. Moreover, not even immoderate Kuhnism licenses all forms of antifoundationalism. In particular, no version of Kuhn supports the idea that paradigm change can happen outside the context of "puzzle solving," though he does suggest that such puzzles are only solved instrumentally. Finally, and most importantly, none of the paradigm shifts Kuhn describes change the fundamental Baconian notions of science. Although there are disagreements within scientific circles about the relative weight observation and theory should be given, since Bacon it has been axiomatic among scientists that both are necessary and that theories unchecked by data are, by definition, not scientific. Hidden under all of this in Kuhn's treatment of science is the fact that scientific principles and methods also require a commitment, albeit one that is usually tacit, to reason as an *a priori* veridical human function. Global skepticism is completely incompatible with science and is therefore not found there. Only in disciplines like philosophy, which can operate without empirical checks, can skepticism take hold. A major conclusion to be drawn from all of this, and which Kuhn's analysis fully supports, is that science is *structure preserving* at the macro level, though it evolves through "revolutions" at the paradigm level.

Vicious Hermeneutics

All of this is problematic for Rorty, or should be, since what he advocates in his hermeneutics is not a shift from within the rational macro-structure, but, rather, the complete destruction of that structure—a shift that has never been seen in the history of

science. This is also completely contrary to his original description of hermeneutics. Whereas the original version was virtuous and ended in learning, this new version is essentially destructive, and also contemptuous of learning, at least as that term is commonly understood, i.e., as the acquisition of new knowledge (in terms of correspondence) about the world. In fact, according to this new perspective, we can never get closer to "things in themselves," but only acquire new "vocabularies" (Rorty, 1988). Thus, this hermeneutics is "vicious" in the sense that it does not allow progress. However, Rorty attempts to disguise this by appropriating Kuhn's idea of paradigm shifts for his theory of hermeneutics and then arguing slyly, contra Kuhn, that paradigm shifts are brought about more by changes in "vocabulary" than theory:

> Kuhn's lessons from the history of science suggested that controversy within the physical sciences was rather more like ordinary conversation (on the blameworthiness of an action, the qualifications of an officeseeker, the value of a poem, the desirability of legislation) than the Enlightenment had suggested.
>
> (ibid, p. 322)

Maximizing these claims, Rorty argues that theory choice in science is more, or perhaps only, a matter of "conversation" among interested parties, and that the argumentation used in science is not different from what we find in literature or politics. Notice, though, that this idea is entirely contrary to Kuhn (both of them)[75]. Whether or not successive scientific models bring us closer to reality as it really is, Kuhn insisted that scientific argumentation is fundamentally constrained by its relation to problem solving and data

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[75] Kuhn has frequently sought to ameliorate his immoderate claims. Fifteen years after *The Structure* was published he wrote that "every individual choice between competing theories [in science] depends on a mixture of objective and subjective factors" (1988), in an attempt to separate himself from the radical relativism sometimes associated with him by such writers as Rorty. On the whole, as I have argued above, his work supports a moderate view in which social factors figure, but do not dominate. Only by ignoring the total thrust of his work is it possible to interpret him as Rorty does here.

from outside the human community. While literary and political arguments *can* resemble this 'objective' style, it is clear that they do not have to. Quite often literary and political arguments are based on highly subjective criteria, such as esthetic values, and are thus *qualitatively* different from scientific arguments. Rorty totally ignores this. Instead he places all the emphasis on the "revolutionary" aspects of paradigm shifting proposals, arguing that is not so much the content and quality of the new arguments brought forth, but, rather, simply the fact that they are "abnormal" and therefore provocative:

> Abnormal discourse is what happens when someone joins in the discourse who is ignorant of [the prevailing] conventions or sets them aside. Επιστημη [aquaintance with a matter, understanding, skill][76] 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." The product of abnormal discourse can be anything from nonsense to intellectual revolution, and there is no discipline which describes it, any more than there is a discipline devoted to the study of the unpredictable, or of "creativity." But hermeneutics is the study of an abnormal discourse from the point of view of some normal discourse.
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> (ibid, p. 320)

Once again, note the major differences between Kuhn's structured revolutions and what Rorty describes here. In Kuhn's descriptions, paradigm shifters are never "ignorant of [the prevailing] conventions," nor do they ever "set them aside," at least not the *macro* "conventions," i.e., the commitment to "puzzle solving" through the process of matching theory and data. Moreover, not just any sort of "abnormal discourse" will do; rather, only discourses that are rational and scientific in that they demonstrate understanding of and adherence to fundamental assumptions and methods of science, as described above, *and*

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[76] This Greek word, which can be rendered phonemically in English as "episteme," and which is glossed by the American Heritage as "knowledge" is the etymological ancestor of our word "epistemology." The bracketed gloss above is from Collins' Greek/English dictionary.

that demonstrate advanced understanding of the particular problems addressed, will even be considered; else, scientists would be caught in never ending analyses of the bizarre theories which abound in popular culture. Rorty again ignores this. However, although his focus is on the alleged transformative powers of "abnormal discourse," it is notable that Rorty does concede here that such discourse can produce anything from "nonsense to intellectual revolution." Once again, the interesting question of what moves a hermeneutic circle along arises. In this case there is the strong suggestion that if the circle is motivated by inferior ideas, the result will be nonsense, whereas good ideas would produce intellectual revolution—presumably favorable. Now, if the chain of ideas in science or any other intellectual sphere--"intellectual history" as Rorty refers to it—had to rely exclusively on chance for change, then we might well expect that no progress would occur—we would be locked in a vicious hermeneutical circle. Indeed, Rorty's warnings about possible bad effects of abnormal discourse support this position, and they beg the question of how we can tell the difference between good abnormal discourse, and bad. However, Rorty immediately forecloses the possibility of investigating this difference, implying that "creativity" is simply beyond description and study. But later, he warns again that "abnormal discourse" should only be practiced by those who are fully acculturated into the discourse they want to disrupt:

> Education has to start from acculturation...we cannot be educated without finding out a lot about the descriptions of the world offered by our culture (e.g., by learning the results of the natural sciences)... I raise this banal point that education—even the education of the prophet—needs to begin with acculturation and conformity merely to provide a cautionary complement to the "existentialist" claim that normal participation in normal discourse is merely one project, one way of being in the world. The caution amounts to saying that abnormal and "existential" discourse is always parasitic upon normal discourse, that the possibility of

> hermeneutics is always parasitic upon the possibility (and perhaps upon the actuality) of epistemology... To abnormal discourse *de novo*, without being able to recognize our own abnormality, is madness in the most literal and terrible sense.
>
> (ibid, 365-366)

These conservative warnings are very strange since they radically undercut Rorty's other claims about the "demise" of epistemology and the social construction of knowledge. Why bother with traditional education if such education is based on the same "misguided" epistemology that we supposedly need to escape? Rorty's only answer to this is his comment that "abnormal discourse is always parasitic upon... epistemology." Again, this seems to be an attempt to present "abnormal discourse" as a Kuhnian paradigm shifting catalyst, but, as I've pointed out, his "abnormal discourse" is incompatible with Kuhnism because it attacks the macro-structure of rational thought itself and also because it seems to have no end other than to disrupt rational discourse.

Grice's Theory of Cooperation

These contradictions in Rorty's writing can themselves be regarded as part of the "parasitic abnormality" he advocates. "Normal discourse, as Rorty acknowledges, depends upon numerous assumptions about the world and about one's interlocutor(s), including adherence to certain cooperative principles. Paul Grice, who first formalized these principles in 1967, in a series of lectures at Harvard, lists the following minimal conditions for successful discourse:

> Our talk exchanges do not normally consist of a succession of disconnected remarks, and would not be rational if they did. They are characteristically, to some degree at least, cooperative efforts; and each participant recognizes in them, to some extent, a common purpose or set of purposes, or at least a mutually accepted direction...We might then formulate a rough general principle which participants will be expected (ceteris paribus) to observe: Make your conversational contribution such as it is required, at the

stage at which it occurs...One might label this the Cooperative Principle.

(1989. p. 26)

To explicate this idea, Grice goes on to distinguish four categories—Quantity, Quality, Relation, and Manner—and their associated "maxims"—which comprise the Cooperative Principle. I summarize[77] these below:

Quantity:

1) Make your contribution as informative as is required (for the current purposes of the exchange).
2) Do not maker your contribution more informative that is required.

Quality:

1) Do not say what you believe to be false.
2) Do not say that for which you lack evidence.

Relation:

1) Be relevant.

Manner:

1) Avoid obscurity of expression.
2) Avoid ambiguity.
3) Be brief (avoid unnecessary prolixity.
4) Be orderly.

(P. 26-27)

Importantly, these are *descriptive* principles not normative ones. Grice's point is not that we *should* observe these principles, but, rather, that, in general, we do, and that if we didn't conversation would be impossible[78]. Now, Grice was focussed on oral text, and it is true that written text is substantially different in some ways; however, I would argue

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[77] The summarized text is all verbatim from Grice, but I have excised his intervening commentary.

[78] Later, Grice adds a normative, theoretical element to the discussion, explaining that "I am, however, enough of a rationalist to want to find a basis that underlies these facts...I would like to be able to think of the standard type of conversational practice not merely as something that all or most do *in fact* follow but as something that it is *reasonable* to follow, that we *should not* abandon.

that the differences that exist are superficial—that, fundamentally, readers depend on authors' cooperation in much the same way as listeners. In particular, I would claim that the maxim of Quality must be adhered to at some level, else text becomes meaningless. Crucially, it is always possible, in both oral and written environments, for elaborate styles or genres to develop that appear to flout these maxims; however, again, I would argue that such flouting is usually superficial, and that actually, once the moves in the discourse are properly understood, the flouting itself is revealed to be cooperative. If your interlocutor is familiar with "jive talk" or Elizabethan parody, then you can say or write things, which at least at denotative levels, flout all the maxims; however, because your interlocutor knows the "code" s/he can recover your meaning, and, indeed, if done skillfully, the jive or parodic elements of your discourse will enhance communication, not detract from it.

On the other hand, there are notable exceptions to cooperation in discourse. Lying is the prime example. In such discourse one or more interlocutors deliberately breaks the maxim of Quality while simultaneously projecting sincerity. The example of a successful con, above, is illustrative. Crucially, such cases are "parasitic," to borrow Rorty's term, on normal, i.e., cooperative, discourse. Obviously, however, there are limits to how much parasiticism a community can stand, and, indeed, limits for the parasites as well. You can only con people who are essentially trusting, but trust is fragile, and once it's gone, your game goes too.

It is instructive to consider Rorty's "abnormal discourse" in this light. As he says, it is parasitic upon normal discourse, and it is easy to see how. In *The Mirror*, for

example, Rorty exploits the maxim of Quality by offering an extensive and complex historiographical narrative that derives all of its coherence and power from our "normal" expectation that the facts therein are true, or at the very least, held true by Rorty. Thus, his entire polemic against "mirrorism" is only coherent and persuasive (if it is), if we believe that psychological materialism entails dualism, that Descartes "invented the mind", that Wittgenstein showed definitively that "intuition is never anything more than familiarity with a language game", that epistemology is wrongheaded residual Platonism[79]. Rorty's expressed goal in all these arguments is to bring us to the point of accepting the "abnormal" view that we cannot improve our representations of reality and should therefore give up on "representationalism"; yet, if we were to accept that view, his argument would collapse since the only thing that supports it is his historiographical argument that *his* representation of history and philosophy—i.e., *his* "mirror" of reality—is better than what we formerly had. Rorty is clearly aware of this problem, but instead of attempting to rectify it, he embraces it:

> We do not escape Platonism by saying that "our essence is to have no essence" if we then try to use this insight as the basis for a constructive and systematic attempt to find out further truths about human beings. That is why "existentialism"—and more generally, edifying philosophy[80]—can be *only* reactive [sic][81], why it falls into self-deception whenever it tries to do more that send the conversation off in new directions. Such new directions may, perhaps, engender new normal discourses, new sciences, new philosophical research programs, and thus new objective truths. But they are not the point of edifying philosophy, only accidental byproducts. The point is always the same—to perform the social function which Dewey called "breaking the crust of convention,"

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[79] Rorty makes all these claims repeatedly throughout *The Mirror*.
[80] Rorty uses "edifying philosophy" as a synonym for "abnormal discourse."

[81] I think Rorty means "can *only* be reactive." The scope of "only" is significantly affected by this change.

> preventing man from deluding himself with the notion that he knows himself, or anything else, except under optional descriptions.
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> (ibid, p. 378-379).

So far as I can see, this leaves him in blatant contradiction. Either the many historical and ontological claims he made throughout *The Mirror* are true, in which case, these comments about "deluding ourselves" are false, or the claims in *The Mirror* are false. Rorty may think that the word "optional" gets him out of the paradox, but it doesn't. If all "descriptions" are equally "optional" then a description that claims to be better than other descriptions is no more or less valid than any other description—it's just another option. On the other hand, if some descriptions *can* be ruled out on the grounds that "no description is allowed to make truth claims," then the rule against truth claims rules *itself* out since it is a truth claim. Again, however, part of what Rorty apparently regards as "edifying" is to roil around in these absurd paradoxes, and perhaps that's all he intends for us to do.

Raving Platonism versus Kantian Rationalism

Thus far, through a rather torturous route, I have established several foundational points in Rorty's philosophy of language and mind—all in direct opposition of course to his claims to antifoundationalism—and also several glaring points of contradiction. Though he avoids making direct claims, always cleverly couching them as a non-claims, I have shown that Rorty's epistemological behaviorism rests on the doctrine of holistically acquired language skills, which skills somehow "contain" rationality. Since, in his scheme, thinking is reducible to manipulating language, i.e., playing "language games," rationality itself is simply whatever games are currently licensed by one's discourse community. Likewise, knowledge is reducible to justified belief, and, in perfect circularity, beliefs are justified by the discourse community through

"conversation." Conspicuously absent from this picture is any Reality outside the discourse community, a lapse that is explained by the "fact" that representationalism was a myth foisted on us by Descartes, along with other fallacies such as dualism. Since this new "intersubjective" world is not compatible with the "conflict and constraint" wrought by epistemology, the latter is to be non-replaced by hermeneutics, which is, variously, either a virtuous learning circle, or "abnormal discourse" meant to "break the crust of conventionality."

Because the mechanism of change—abnormal discourse—is essentially negative, it is necessary for Rorty to show that the rationalist's epistemological view of the world is deeply flawed in order to get us to consider his pragmatic alternative. The way he does this is to ridicule rationalism by conflating it with ancient, discreditable philosophies. Charles Taylor analyzes the technique:

> Now, there is one principal line of argument, which Rorty returns to again and again in different forms, to establish his status claim [i.e., "that disputes between alternative language games are unarbitrable"], and hence the thesis of closure. This is that the only alternative to his Pragmatism is some belief in a correspondence theory, the belief that one's 'philosophy corresponds to the way things really are.' This is represented as being quite untenable, even laughable; Pragmatism is the only believable alternative.
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> Certain suppositions seem to be made in the various invocations of this argument (i) that the only candidate for a general account of truth is in terms of correspondence; (ii) that correspondence is to be understood in a rather simple-minded way, approaching at times a picture theory; (iii) that believers in the correspondence theory are Raving Platonists. Underlying all of this is a continuing imprisonment in the model basic to the whole epistemological tradition, which understands thinking in terms of representations.
>
> (Taylor, 1990. p. 269)

These invidious rhetorical techniques, which we have already seen quite a bit of in *The Mirror*, are prominently on display in Rorty's essay, "The Contingency of Language"

(1998). The essay also contains some clear statements of the immoderate Kuhnian doctrine of strong instrumentalism, or non-correspondence between scientific theories and reality. Therefore, an analysis of a passage from that essay provides an opportunity for me to refute "Raving Platonism" and also to redeem my pledge to critically analyze Kuhn's strong instrumentalist doctrine.

In the essay, Rorty lays out his argument, as if following a Taylor script. He speaks of two kinds of philosophers, one committed to the old epistemological model, "faithful to the Enlightenment," "identified with the cause of science," "insistent that natural science discovers truth rather than makes it" ; while the other, the antifoundationalist philosophers

> realize that the world as it is described by the physical sciences teaches no moral lesson and offers no spiritual comfort, [and] have concluded that science is no more than the servant of technology. . .
>
> Whereas the first kind of philosopher contrasts "hard scientific fact" with the "subjective" or with "metaphor" the second sees science as one more human activity...On this view great scientists invent descriptions of the world that are useful for purposes of predicting and controlling what happens, just as poets and political thinkers invent other descriptions for other purposes. But there is no sense in which *any* of these descriptions is an accurate representation of the way the world is in itself.
>
> (1998, p. 66)

True to the script, Rorty applies the old antifoundationalist trick of painting all rationalists as retrograde dogmatists—they accept *everything* associated with the Enlightenment; they believe their models to be complete and "accurate representations of the way the world is in itself, " and, further, that "the world teaches moral lessons and offers spiritual comfort." By now I have refuted these straw man arguments so often that no commentary should be necessary. However, redundancy may be a virtue. Minimal foundationalists (Kantian rationalists) do not believe that science reveals the world "as it

is in itself," and they certainly need not be committed to any doctrines regarding morality or spirituality, as Rorty should know from reading rationalists like Chomsky and Fodor. Rather, most rationalists believe that science gives us models of the world only in terms of the human sensory apparatus (HSA) (where HSA is understood to include whatever extensions technology allows). As for whether more successful models bring us closer to "reality as it really is," there is a genuine split on this. Some rationalists adopt Kuhn's instrumentalist stance, defining "better" only in terms of how well a model solves "puzzles" in the field, and avoid the question of "closer." But other rationalists, including many professional scientists, adopt one or another "realist" stance, arguing that better models are quite probably also "closer" models.

The first point to realize in deciding between these perspectives is that the question cannot be solved empirically. That is, nearly everyone concerned agrees that we don't have direct access to reality *in toto* and probably never will. On this at least, minimal foundationalists and antifoundationalists agree. Therefore, we can never solve the question by direct comparison—we'll never be able to compare various models of reality with the "real thing" to see whether some models are closer than others. Thus the answer will have to turn on logical inferences and whatever indirect evidence we may be able to gather. Kuhn's comments are very interesting in this respect. For convenience I repeat them here, with the addition of italics in some crucial passages:

> Compared with the notion of progress most prevalent among both philosophers of science and laymen, however, this position lacks a an essential element. *A scientific theory is usually felt to be better than its predecessors not only in the sense that it is a better instrument for discovering and solving puzzles but also because it is a better representation of what nature is really like.* One often hears that successive theories grow ever closer to, or approximate more and more closely to, the truth. Apparently generalizations like that

> refer not to the puzzle-solutions and the concrete predictions derived from a theory but rather to its ontology, to the match, that is, between the entities with which the theory populates nature and what is "really there."
>
> Perhaps there is some other way of salvaging the notion of 'truth' for application to whole theories, but this one will not do. There is, I think, no theory-independent way to reconstruct phrases like 'really there'; the notion of a match between the ontology of a theory and its "real" counterpart in nature now seems to me illusive in principle. Besides, as a historian, I am impressed with the implausibility of the view. I do not doubt, for example, that Newton's mechanics improves on Aristotle's and that Einstein's improves on Newton's instruments for puzzle solving. But I can see in their succession no coherent direction of ontological development. On the contrary, in some important respects, though by no means in all, Einstein's general theory of relativity is closer to Aristotle's than either of them is to Newton's.
>
> (ibid, p. 206).

Kuhn offers exactly one inferential justification for his very strong opinion, viz., that there is *no theory-independent way to reconstruct phrases like 'really there,'* and one scant bit of indirect evidence, viz., that, *in some important respects, though by no means in all, Einstein's general theory of relativity is closer to Aristotle's than either of them is to Newton's.* Now, regarding the inference, suppose we agree that "really there," or perhaps, more sensibly, "what is really there" cannot be "reconstructed" independently of some theory. I suppose that 'reconstruct' means "conceived of," so that the comment means that we cannot conceive of 'what is really there' independently of some theory. Well, this is certainly correct; again, Kantians and antifoundationalists would agree. But why does it follow from this that some conceptions couldn't be closer to reality than others? What *does* follow is that no conception can capture reality unencumbered by preconceived structures and categories, i.e., theory, but, logically, there is no reason that one conception couldn't be *closer* than another; in fact, logically, it is far easier to suppose that some conceptions *must* be so, than that all conceptions could somehow be

equally "far away." Reality must have *some* shape, *some* qualities, however fantastic and unimaginable they may be. Consider for a moment what the strong instrumentalist doctrine entails. Suppose, for fun, that reality is ten-dimensional (whatever that means). Now consider Ptolemy's model of the planets and the sun versus Copernicus's. Is it *more* sensible to say that *neither* model is closer to the reality of that ten-dimensional universe than to say that one (probably Copernicus's) is? I just can't see how. In fact, I can't imagine a universe, at least not one with reasonably steady state properties, for which that could be true. Now, of course the fact that I can't imagine such a universe is no bar to its existence, but why should we *presume* that the universe would have such bizarre and, for us, unimaginable properties as to render all models of it equally "far away"? I submit, in accord with Occam, that the model that requires fewer assumptions is to be preferred over the one that requires more. And a universe that is similar to the world we know, i.e., a world in which different models or representations are *necessarily* closer, further away, or in very rare cases exactly the same distance, in terms of verisimilitude, to the real thing (think of maps and territories, for example), requires far fewer assumptions than the universe of the strong instrumentalist, in which, somehow, though we can't dream how, all models are equidistant.

Now, regarding the indirect empirical evidence, first of all notice that this is evidence of an entirely different kind. The inference considered above is *deductive*, and thus potentially conclusive, whereas the indirect, empirical evidence is *inductive*, and thus, by definition, cannot be conclusive—no matter how many cows with one head you see, you cannot absolutely rule out the possibility of a cow with two heads, three heads, or none. Nonetheless, inductive evidence is good evidence, especially if you have a lot of

it. But how much has Kuhn presented here? He gives only *one* example, the three way comparison of Aristotle, Newton, and Einstein, with no detail, and he hedges that example significantly—"*in some important respects, though by no means in all.*" Who knows what he means by this? I have a vague notion that he is probably referring to some of Aristotle's metaphysical suppositions about matter, such as his insistence that nothing is every ultimately destroyed, but rather, only changes form (1951, p. 11), or perhaps to doctrines that have a relativistic flavor (in Einstein's sense) about them, such as his discourse on "Motion, Change, and the Infinite" (ibid, p. 43-46), but, since Kuhn doesn't exegete his claim, we can't evaluate it. And, in any case, even if it's true that Aristotle's theories *are* closer to Einstein's in "important respects," how does that fact lead to strong instrumentalism? If the universe is thought of as a gigantic puzzle (from our point of view), one that has aspects that are forever beyond our understanding; and if, as curious puzzle solvers we have gone about trying to model it, through pure intuition, pure empiricism, and lately, refined intuition checked by empiricism (science), all mixed with great heapings of "social situatedness," it would hardly be surprising that, at least occasionally, "in some important respects," some ancient puzzle solvers would be closer to the unreachable target (reality) than those in middle history, and also closer to the moderns. Nothing at all counter-intuitive about that. On the other hand, if it turned out that there was *no trend* in the puzzle solving enterprise overall, i.e., that better puzzle solutions and general models of reality, didn't tend to show convergence over time, *on the whole,* then that *would* be good inductive evidence. But Kuhn alleges no such general trend. To the contrary, in the previous paragraph, he painted the standard picture

of ever better convergence and greater puzzle solving ability as the puzzle solving enterprise moves on.

Another aspect of Kuhn's strong instrumentalist claim is salient here, and may help to explain and deflate the claim. It is notable that Kuhn chooses to focus the claim on physics, an area of science that has always had, and still does have, extremely mysterious edges. Physics is both the ultimate science, in the sense that all other natural sciences are in some way reducible to it, and also the most comprehensive, endeavoring to unify and explain events and structures from the cosmic to the quantum level. Hence, because of this vast domain, it is not surprising that Grand Unifying Theories (GUTS), which people are fond of, vary widely and are still completely unsatisfactory in terms of coherence and explanatory power. The point is, such theories are the least scientific that we have in some ways since they simply range far beyond our collective knowledge. Another way to put this is that of all the theories that we have about the universe, GUTS are the least well checked by data. Now, it so happens that, outside radical postmodern or antifoundationalist writing—in which *everything* is deemed to be a social construction—most writers who want to make strong instrumentalist claims, tend to make them about far-reaching theories of physics, never about well-established, redundantly verified theories like heliocentrism. This is also why the most famous instrumentalists in science are people like Bohr and Heisenberg, people working on the fringes of the edge of physics, though, it should also be noted that the most famous physicist, Einstein, was a realist, not an instrumentalist. Notice how quickly a typical instrumentalist perspective of science resorts to examples from physics:

> The conceptual foundations and premises of theories undergo continuous and sometimes revolutionary change, and this occurs not

> merely *before* the so-called scientific revolution in method of the seventeenth century, but subsequently, when the method of science remained comparatively stable. The succession of theories of the atom, and hence of the fundamental nature of matter, for example, exhibits no convergence but oscillates between continuity and discontinuity, field conceptions and particle conceptions, and even speculatively among different topologies of space.
>
> The empiricist response to this instability of theory has been the positivist or *instrumentalist* view of science as constituted essentially by accumulating knowledge of phenomena or observables, rather than of the fundamental but hidden nature of things.
>
> (Hesse, 1980, p. 174; original italics)

From this point, it is easy to slide into full-blown strong instrumentalism, wherein a theory is suspected behind every bush and not even one's own lawn can be viewed in a theory neutral way; or to put it in the sort of jargon Rorty prefers:

> Instrumentalism can no longer interpret the truth claims of science as a body of empirical statements, but can at best point to the pragmatic effects of science to indicate its form of empirical objectivity.
>
> (Hesse, p. 175)

However, Hesse notes that this view has come under attack by post-empiricists "whose role of scientific theory also reopens the old debate between realism and instrumentalism" (ibid, p. 175). In a perspicacious analysis of this debate, Hesse discovers a principle that I think does much to resolve the issue:

> There is, however, a third possibility, which does more justice to the sequence of theory systems as we actually find them in the history of science. This is the view that successive theories supersede and reinterpret their predecessors, but without rejecting the empirical discoveries that they embody...This implies that something remains constant from theory to theory. What that something is can best be expressed by pointing to classifications of what count as similar systems subject to the same laws, and the forms of those laws or approximations to them. For example, that the planets, the earth, and stones falling on the earth, are similar types of body and satisfy the same laws within certain empirical limits. Such discoveries have not been affected by subsequent radical conceptual changes in the

> theory of space and time, or in the understanding of mass and its natural motions, which affect all these bodies alike. Lawlike structures and similarities of nature between physical systems have been maintained and are cumulative. Theoretical interpretations of what the natures of these systems absolutely are, are not.
>
> (ibid, p. 177)

According to this uncontroversial analysis, which is strongly supported by history, a great many scientific "descriptions" do reach stability, which, at least for these descriptions, obviates Kuhn's strong instrumentalism. Realist*ic* interpretations of these "lawlike structures" is entirely reasonable, then, and, in fact, far more logical than instrumentalist ones, for the reasons outlined above. The crucial question here, one not fully addressed by Hesse, is how one differentiates between "lawlike structures" and "theoretical interpretations." Perhaps because she doesn't carefully distinguish one from the other, Hesse comes to a rather surprising conclusion (this conclusion directly follows from the above):

> Hence even on such a moderate interpretation of post-empiricism science must still be said to yield phenomenal or instrumental rather than theoretical knowledge.
>
> (ibid, p. 177)

But there is danger of circularity here. If theories are, by definition, "descriptions that are still in flux," and "lawlike structures" are "descriptions that are no longer in flux," then Hesse's conclusion will always be trivially true. We need to remember that at one time, many, if not all of our "lawlike structures" were debated theories ("theoretical interpretations" in Hesse's terms). The heliocentric system, for example, —a paradigmatic example of a "lawlike structure"—began as a much debated theory. When Copernicus proposed it, Tychoe Brae's observations, and the moon and the planets themselves were considered "lawlike structures," but the heliocentric system itself had to mature a while and defeat competing theories before it could be elevated from a theory to

that status. Today the heliocentric system seems to us to be infinitely superior to the geocentric one, and it has been verified in so many ways that we can refer to it off-handedly as a "lawlike structure," as if there were something intrinsically different about it from our theoretical interpretations of, say, sub-atomic particles. But the difference appears to be a function of our differing degrees of knowledge about these systems rather than anything intrinsic in the systems themselves. In it's infancy, Copernicus's theory was no better and in some ways worse than Ptolomy's in terms of its power to predict celestial objects' positions. Not until it was "drastically revised more than sixty years after Copernicus's death" (Kuhn, 1988) did Copernicus's model prove itself to be a better predictor. From an instrumentalist perspective, then, we would have to say that geocentrism and heliocentrism were roughly equal through the late 17th century, and *by definition* (i.e., instrumentalist definition) that means that they were roughly equidistant to reality as it really was. But I doubt that you could find an astronomer who would say this today, and that shows the absurdity of strong instrumentalism. If, today, geocentrism and heliocentrism are *not* equally distant from reality as it really is, then how can it be that they *were* equidistant only three hundred years ago? Clearly, it's only our perspective that makes instrumentalism appear reasonable. Today, astronomers may be instrumentalists about the Big Bang, or curved space-time, but they're realists about the solar system; thus, if you take Hesse's line, you will say that the solar system is a lawlike structure, and heliocentrism *is* closer to reality than geocentrism, yet you would still maintain that the Big Bang *isn't* closer than, say Hoyle's steady state system. But why is this any less absurd than it is to be instrumentalist about the geocentric versus heliocentric case? Only because we're not as sure about the Big Bang as we are about

heliocentrism. But, then, why not just state things that way? Why not just say theory X is tentative, but if it is not falsified and continues to fit our developing models of the universe, then we will place more trust in it? Taking that stance sensibly separates our limited human perspective from what we assume must be true, i.e., that reality has some shape independent of us.

This maturation from theory to lawlike structure is not unique to astronomy. Similar situations obtain across other areas of science as well. For example, Newton's descriptions of the visual spectrum and its relation to sunlight, have been retained, with minor alterations, and can be regarded today as lawlike structures, but it took many years for them to reach that status, and during those years many radically different proposals were circulating (Gillispie, 1964). Had Hesse and Kuhn done their historiographical analysis in the late 17th century, they would have to have adopted an instrumentalist stance toward Newton's optics, though perhaps, by then, they could have called the solar system "lawlike."

In a nice discussion of the problem of separating observations from theory, a problem that is similar to the "lawlike structures" versus "theoretical interpretation" problem, Grover Maxwell invokes a visual metaphor (amount of separation between observer and observed) to make the point that there aren't any sharp ontological differences between observations and theory:

> The point I am making is that there is, in principle, a continuous series beginning with looking through a vacuum and containing these as members: looking through a windowpane, looking through glasses, looking through a high-power microscope, etc., in the order given. The important consequence is that, so far, we are left without criteria which would enable us to draw a nonarbitrary line between "observation" and "theory."
>
> (Maxwell, 1988)

It seems to me that the "lawlike structure" versus "theoretical interpretation" is much the same, and that a probable reason for the seeming instability is simply the fact that science is young and theoreticians are ambitious. My guess is that some of today's "interpretations" will become tomorrow's "lawlike structures," and that others, like the optical theories Newton competed against, will become obscure notes in some future historiographical account. This is what's wrong with instrumentalism; it confuses human perspective with reality, and we should understand why. The choice between instrumentalism and realism will forever be a philosophical not an empirical one because we will never be in the position to see reality as it is and settle the question empirically.

Now as to what sort of stance is most reasonable, from very strong realism, wherein observables themselves are said to be "real" (e.g., Bhaskar, 1978), to strong instrumentalism, minimal foundationalism takes a middle view. Kantian epistemology, at least as I interpret it, excludes strong realism because of inalienable subjectivity, but it nontheless licenses a realist*ic* view. As noted in the previous chapter, there is a lot of confusion about how both of these claims could be true. How can Kant claim *both* that our perceptions are ineluctably "subjective" *and* that they can be used to discover reality? Doesn't it follow that subjective perceptions can only be used to build subjective models and that therefore we can never get closer to reality? The short answer is yes and no. Yes, all models are "subjective," but no, it doesn't follow that all subjective models are equal in terms of closeness to reality. The reason is threefold. First, although sensory perceptions are "subjective," in the sense that they are totally a function of our sensory apparatus, this subjectivity does not operate at the individual level. Rather, the "subjectivity" of perception is a species-wide attribute that is extremely stable. *All*

humans experience time and space in approximately the same ways because these modes of perception are given to us *a priori*; thus the HSA can be treated as a constant, which allows us to extrapolate from observations. Second, we also have a species-wide *a priori* reasoning ability which is veridical, or at the very least unavoidable. Of course, if reason *isn't* veridical, then all bets are off, but thinking that way is a non-choice since we cannot escape reason. Antifoundationalism is the result of opting for this non-choice, which is why it is endlessly paradoxical. Third, we can use reason to create models of the world and we can test these models by triangulating our subjective perceptions, especially through the use of technology. Though we don't expect to ever see reality *in toto* as it "really is," we do believe that models that explain more data *are* in fact closer to the way things really are because that is the best way to explain why better models are better. To put this the other way, if all models were equally bad, i.e., equally inaccurate, it would be a mystery why some models are so much better than others at explaining the data. The *reasonable* answer is that the models are *not* equal, and that the ones that explain more data *are* more accurate.

I have rehearsed these ideas here because I want to carefully distinguish between Kantian subjectivity and the loose, all encompassing subjectivity Rorty evokes. Let us refer to these as K-subjectivity and P-subjectivity, respectively, where P stands for personal (with the understanding that personal includes cultural). Of course, P-subjectivity includes K-subjectivity since K-subjectivity is species-wide. The reason it is so important to distinguish among these different levels of subjectivity is that it gives us a way to formally distinguish science from other activities, on principled grounds. By definition, science, at least since Bacon, is that activity that consciously seeks to reduce

the subjective element to K, and then to transcend K by triangulation of observations, guided by reason. In common terminology science seeks an "objective" perspective of the world—one that leads to explanation of phenomena. Unfortunately, the terms "objective" and "subjective" both have at least two meanings, but these are not well understood and there is therefore a great deal of confusion around these terms. If we accept Kant's framework it should be clear that ultimate objectivity is an impossibility for humans, since that would mean seeing the world "as it really is," not filtered through our sensory apparatus. However, it is still useful to have a word for the concept, and it is also useful to have a term for the concept "objective except for K-subjectivity." Thus, to stay with our system of abbreviations, it seems we need four terms: $U_{ltimate}$-objectivity , K-objectivity, K-subjectivity, and P-subjectivity. Actually, K-objectivity and K-subjectivity will turn out to mean the same thing, since both mean the view of the world when all but the K elements of distortion are removed. However, it may be useful to retain both terms since each stresses a different aspect of perspective.

It should now be clear why Rorty's comments are obtuse. The philosophers he mocks for believing in contrasts between "hard scientific facts" and the "subjective" [world] are really contrasting carefully triangulated K-objectivity with P-subjectivity, a contrast that is eminently defensible and sensible. However, Rorty refuses to recognize that contrast, instead conflating P-subjectivity and K-subjectivity, under the catch-all term "subjectivity." Since K-subjectivity and K-objectivity are the same thing, he can then claim—implicitly in this passage—that "hard scientific facts" are no more "objective"—a term that always means U-objective in his usage—than "subjective" ideas, and, as long as the terms aren't explicated, he'll technically be right.

Antifoundationalists have a field day with this. They are forever "pointing out" that nothing is ever truly "objective" as if that fact ended science or placed science at the same level of subjectivity as, say, abstract art. But if you distinguish the important concepts carefully, it is clear that what scientists commonly mean by "objective" is "K-objective," and that science can and does achieve that level of objectivity regularly[82].

Rorty's attempt to put everything in pragmatic terms is also disingenuous. Although it's true that scientific findings, poetry, and politics are all "useful" to humans, it doesn't follow from that that they have the same etiology. Moreover, not even the immoderate Kuhn would agree that scientific descriptions are "inventions" in the same sense that poets' and politicians' "descriptions" of the world are. Actually, I think "invention" is a misnomer in all three cases, but that's a side issue. What is really important is the fact—and this *is* a fact—that scientists' descriptions of the world arise in a fundamentally different way than poets' or politicians'. As Sokal and Bricmont have been at pains to point out, scientists' descriptions are constrained by nature itself, OR, at the very least, they (scientists) *believe* that they are so constrained. Either way, it should be clear that scientists' descriptions are different from poets' and politicians' in essential ways. If we accept the notion of K-objectivity, which again is an eminently defensible notion, then we can say with confidence that in fact scientists' descriptions are constrained by nature, albeit at the K-objective level. Virtually all rationalists would agree with this. However, even if you are an immoderate Kuhnian, you should still agree that scientists *think* they are so constraining their descriptions, and you should also agree

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[82] Someone might object that I'm holding Rorty accountable for terms that I just made up. But the conceptual distinctions are there in the literature, and Rorty knows that literature as well as anyone, so that objection is moot.

that they go to great lengths to do so—literally billions of dollars per year are spent on technology to assist in this effort, and scientists will spend months or even years to eke out triangulated descriptions that hew to these constraints. Therefore it is simply a monumental falsehood to equate scientific with poetic or political descriptions, and Rorty must know it.

Another problem with Rorty's "pragmatic" account of the etiology of scientific descriptions is that it renders their utility mysterious. Though everyone will certainly agree that it is "useful" to be able to predict the weather, cure diseases, and so forth, explaining how scientists achieve such "descriptions" by appeal to utility is vacuous, since it is clear that merely *wishing* to achieve such results won't do the trick. There are lots of serious problems that scientists would love to solve; on Rorty's account, then, it is inexplicable why they don't simply "invent" some new descriptions and take care of it. Of course we all know the answer: they don't do this because they don't know how! By contrast, the standard scientific account—that new descriptions of nature arise only through discovery of hitherto unrealized *real* properties of nature—explains all of these relations. Scientific descriptions are useful because they explain real things about the world, but such descriptions are in limited supply because they are difficult to derive.

Importantly, it is also evident that a moderate realist*ic* scientific account coincides much better with the "hermeneutic circle," as described by Rorty himself, than his own pragmatic account. In fact, as we have seen, the hermeneutic account is in no way antithetical to rational epistemology, and, moreover, is also consistent with Kuhn's

account of scientific development, at least the moderate Kuhnian account[83]. It even appeared for a moment that hermeneutics could provide a rapprochement between rationalists and antifoundationalists. However, Rorty demolishes this possibility when he moves to eliminate epistemology in favor of "pragmatics." He simply can't have it both ways. He can't coherently argue *both* that hermeneutics promotes understanding of the world (of "a strange culture, practice, theory, language, or whatever"), *and* that scientific theories bring us no closer to the way things are. Unfortunately, it seems that he has opted to sacrifice scientific coherence for pragmatism, a most *impractical* move in my opinion.

In the next chapter we will look at some normal scientific discourse, specifically Chomsky's, and then return to Rorty again for the purpose of examining some further claims he makes about science and language. The goal of all this is to clearly and specifically delineate the substantive differences between scientific views of language and mind and antifoundationalist ones.

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[83] Kuhn's account is crucial here, not so much because it is correct, but because Rorty explicitly and repeatedly endorses it. Of course, one could argue that Rorty only endorses the immoderate Kuhn, which is probably correct; however, as I've argued above, that account also conflicts with hermeneutics, so this doesn't save Rorty from contradiction.

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MINIMAL FOUNDATIONALISM IN LITERACY STUDIES

VOLUME II

By

Nevin Leder

A DISSERTATION

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VI: CHOMSKY

An interesting point of comparison between Chomsky and Rorty is their mutual focus on Descartes as a landmark philosopher—both identify him as the philosopher who first clearly articulated the view of mind that is now identified with representationalism. However, agreement stops at this point. Rorty regards Cartesianism as the beginning of a long thoroughly misguided and deleterious tradition from which we must liberate ourselves, whereas Chomsky credits Descartes as a man of great intelligence and insight who opened a certain avenue of investigation that we have only recently—say in the last fifty years—begun to appreciate.

Chomsky's focus on the mind rather than external aspects of language, i.e., sounds, scribblings, or linguistic behavior, initiated the most important and fruitful shift in the history of linguistics and also provoked psychologists to reconsider their commitment to behaviorism. Initiated in the 1950s, Chomsky has consistently described this shift as a shift from "external language" or "E-language," to "internal language" or "I-language." Considered from Chomsky's perspective, this shift in focus can be regarded as a single very powerful epistemological move that enabled a unified research program in linguistics, one that he has maintained for nearly fifty years and which has informed at least two generations of linguists. However, from an outside perspective, Chomsky's revolution cannot properly be seen as a single shift in focus, for it profoundly affected at least three areas of scholarship: linguistics, psychology and sociology; moreover, there are remnants of the previous schools of thought from each of these areas still surviving today. In fact, I will argue that antifoundationalism is a last bastion of several of these remnants and that in this respect, ironically, antifoundationalism can be

viewed as a counter-revolutionary movement, stubbornly hanging on to a set of thoroughly discredited ideas, yet presenting itself as avant guard, while it attempts to smear rationalism as a remnant of the Enlightenment. A thorough survey of these theoretical and historical connections would take us far afield, so I will give only a brief summary here, beginning with Saussure.

A Brief History of Linguistcs from Saussure to Chomsky

Ferdinand de Saussure (1857-1913)

Many historians of linguistics consider Ferdinand de Suassure to be the "father of modern linguistics." Jonathan Culler takes this position when he notes that

> together with his two contemporaries, Emile Durkheim in sociology and Sigmund Freud in psychology, [Saussure] helped to set the study of human behavior on a new footing. These three men realized that one could not approach an adequate understanding of man and his institutions if one treated human behavior as a series of events similar to events in the physical world . . . [With physical things, the scientist] can describe what happens and try to explain why without paying attention to ordinary people's impressions. . . But human behavior is different. When studying human behavior the investigator cannot simply dismiss as subjective impressions the meaning behavior has for members of a society. . . Moreover Saussure, Freud, and Durkheim saw that the study of human behavior. . . must focus primarily on the functions which events have within a general social framework. It must treat social facts as part of a system of conventions and values.
>
> (Culler, 1976, pps. xiv-xiv)

This focus on "underlying systems" rather than external phenomena is somewhat similar to Chomsky's focus on I-langauge, and indeed, aspects of Saussure's system are often compared with Chomsky's. However, the approaches are different in one crucial respect. Saussure located the underlying system primarily in the speech community rather than in individuals, whereas Chomsky posits innate "knowledge," or Universal Grammar (UG) in interaction with language data as the source of I-lanaguge. Saussure

assumed that the systematicity he observed in language, was a cultural phenomenon, whereas Chomsky cites innate cognitive factors almost exclusively. Nonetheless, if this difference is ignored, Saussure's distinction between *langue* (the underlying system) and *parole* (language in use), is very similar to Chomsky's *competence* versus *performance* (Anderson, 1985). For some aspects of linguistic research, the locus and origin of the underlying system may be relatively unimportant. For example, *in principle* both models could discern the critically important difference between phones and phonemes, and both *could* recognize phonemic differences as by far the more important kind of difference, though as it happens Saussure did not achieve this realization, at least not clearly (Anderson, 1985, p. 55). On the other hand, when larger epistemological or ontological questions arise, as they constantly do in postmodern discussions, the question of origins becomes crucial. For, if, as Saussure would have it, linguistic systems are cultural inventions, then we could reasonably expect them to vary widely and to affect individuals and societies in unpredictable ways. We will see such notions cropping up in Rorty's writing as we proceed.

Saussure is also famous for clearly articulating the principle of arbitrary sound/symbol relationships. This principle, which has been maintained in virtually every linguistic model since Saussure would seem to be unproblematic; however, here again, the question of the locus of the underlying system becomes crucial. If linguistic systems "belong" to discourse communities, then we can argue for "public" meanings. On the other hand, if we take a firm Chomskian I-langauge perspective, then meanings *must* be individual, and public meanings, to the degree that they can be defended at all, must be regarded as epiphenomena, created by the overlap among individual I-languages. Of

course, the relationship between public and private is still reciprocal, since new language learners—typically children—infer most of their word meanings from older members of their discourse communities. Nonetheless, there are good reasons to argue for individual meanings and to question the reality of public ones.

A final important difference between Saussure and contemporary generative linguists is that Saussure regarded linguistics not as a well defined discipline, clearly distinct from other types of human behavior, but, rather, as part of a broader study of semiology, though, as he said, that science had not yet been invented (Culler, 1976, p. 97). Saussure imagined this new science—semiology—to cover every "communicative" aspect of behavior that humans engage in, "rituals, customs, etc" (ibid, p. 97). The problem with this is that with a little imagination, *everything* people do or make can be regarded as a "sign," though in many cases a multiply ambiguous one. Another, even more serious problem, is that under an "imperialistic semiology" (Culler's term), in which all systems are analyzed as systems of signs, important fundamental distinctions get lost. For example, Culler shows how various natural and social sciences can be analyzed semiologically: in meteorology, atmospheric conditions are "signs"; in medicine, symptoms; in economics, economic "signs." Further, he talks about how a science such as botany can be analyzed "not as the sum of true statements about plants, but as a system of discourse" (ibid, p. 111). But lost in all this is the crucial concept of *intention.* Surely it makes a difference whether a "sign" is used *intentionally* as a sign or not. Dark clouds are a reliable predictor of rain to come, but few believe that the sky (or gods, God, what have you) is *intentionally* arranging the atmosphere for the purpose of *telling* us something. In fact, it's pretty clear that our word "sign" is a homophone/graph

with at least two quite distinct meanings, one which requires intention on the part of a signer, and the other which means "reliable, but unintentional indicator." Conflating these two kinds of "sign" leads only to confusion. In fact, as Culler also notes, Suassure was well aware of the problem of the need to connect possible signs to signers. For example, one of his passions was the search for anagrams scattered in the letters of the words in Latin poetry; however, he was troubled by the difficulty of determining which patterns were intentional, if any, and which were coincidental (ibid, p117). Because of this very rational doubt, Culler places Saussure "squarely within" "the central tradition of Western Culture" (ibid, 120). Elaborating on this tradition, Culler identifies "logocentrism, which he defines as "the belief that sounds are simply a representation of meanings which are present in the mind of the speaker," as one of its main tenets, and he notes that Saussure's "signs" were clearly meant in this way, although, the locus of the Saussurean sign is primarily in the discourse community and only secondarily in the speaker's mind. But in any case, for Saussure, signs must be invested with meaning by signers; otherwise, they do not qualify as signs. Ironically, however, certain later readers of Saussure, seized on his work in anagrams as an early example of a new way to read, a way to break out of the "oppressive logocentric tradition" (ibid, 119). According to Culler, these iconoclasts had two reasons for attempting to break away from the Western tradition,

> . . . one logical the other moral and political. The moral and political argument is that meaning should not be something that we simply recover but something that we produce or create. . . No one can ever grasp what another person might have had in mind...Therefore, rather than guiltily attempt an impossible task, one should welcome the necessity of creative interpretation.
>
> (ibid, p. 120)

Notice the similarity between this argument and Rorty's. The argument is not at all that postmodern readings (or whatever school makes these claims) are more accurate—in fact it is frankly admitted that they are not—but rather, that they are "necessary," because accurate reading is "impossible." If one has this orientation it's not hard to imagine also wanting to deny truth altogether, since, if that's accomplished, one can also dispense with "guilt" for having abandoned what has always been understood to be the first responsibility of reading—to get the author's intended meaning. Note also, the paradoxical assertion about what is and is not possible in interpretation. To know that interpretation is impossible one would have to know a good deal about language, and that entails theory. But the interpretation-is-impossible camp usually also denies the possibility of scientific knowledge, so they have no coherent theory of linguistics to support their claims. Again, all of this is consistent with antifoundationalism, as we've seen.

The "logical" reason for abandoning logocentrism, which Culler also refers to as the "philosophical" reason is that Saussure taught that sign systems derived meaning from relationships—from a system of differences—not symbols. Thus, the argument goes, there *are* no fixed signs. Rather,

> every sign in some sense bears the traces of all the other signs; they are co-present with it as the entities which define it. This means that one should not think, as logocentrism would like to, of the presence in consciousness of a single, autonomous signified. What is present is a network of differences. . . The whole notion of a linguistic system, the whole notion of *la langue* as Saussure defines it, is that of networks of differences at the level of both signifier and signified—networks which are already in place, already inscribed or written in the mind of the subject.
>
> (ibid, p. 122)

This is certainly a much more reasonable objection than the "moral" one. In fact, an I-language/cognitive perspective leads to much the same conclusions about how individuals represent concepts. However, even if concepts are represented in some distributed fashion throughout a neural network, it doesn't necessarily follow that words couldn't be discrete semantic units with fairly restricted boundaries. It may be that each individual represents concepts in a somewhat unique fashion, yet accesses a lexicon that is remarkably similar to the lexicons of other native speakers. This is a point where nativism looms large. If there are innate semantic primitives that humans use to construct I-langauge, as most generative linguists believe (see, esp. Jackendoff, 1995, 1997), then the semantic associations or *intensions* connected to words would be quite restricted, and one would expect strong overlap among speakers. On the other hand, if intensions are culturally constructed, then one would expect more variability and perhaps even untranslatability from discourse community to discourse community. A case can be made for both positions. On the nativist side, there are extensive studies of common lexical items and their intensional and combinatory properties (e.g., Cruse, 1986; Levin, ?; Pustejovsky, 1995), which demonstrate very convincingly that, at least for an extensive, commonly used vocabulary, there are robust properties that are widely shared and which are very hard to account for without positing innate semantics. On the other hand, it is well known that specialized vocabularies, or jargons—which in some cases can be quite extensive—develop in particular discourse communities and it is also certain that many members of such communities acquire these specialized jargons as adults. However, both of these facts, i.e., strong overlap among most lexical items, specialization of "jargon" within particular communities can be accommodated within a generative (i.e.,

I-language) framework, just by leaving the lexicon "open" in comparison to other linguistic systems, which are comparatively "closed." What this means is that the innate language component hypothesized to be devoted to the lexicon remains flexible throughout life, which allows the organism to continually acquire new lexical items; whereas, grammatical components appear to "close" some time before puberty. In any case, rejection of a naïve logocentrism is an area where postmodernism and cognitive science/generative linguistics overlaps. We will see some specific examples of this overlap somewhat later.

Edward Sapir (1884-1939)

Edward Sapir was a student of one of the first American linguists, Franz Boaz (1858-1942), who had developed an anthropological approach to linguistics in the late nineteenth century in the United States, focussed on American Indian languages (Andereson, 1985). As Boas's student Sapir continued working on American Indian languages in the ethnographic tradition that Boas had developed. However, whereas Boas's energies were devoted primarily to the development of a rigorous, phonetically oriented field research methodology, Sapir became interested in psycho-social aspects of language, particularly the "relation between personality and culture" (ibid, p. 219). Importantly, Sapir's perception of this "relation" was richly cognitive, though ultimately based on social rather than biological constraints. Thus, while he clearly recognized that phonemes, for example, must be abstractly represented in the minds of individual speakers, he traced the source of phomemic contrast to culture, not any innate property of mind. Perhaps the supreme expression of this theoretical orientation is Sapir's famous declaration that "speech is a human activity that varies without assignable limit as we

pass from social group to social group" (in Anderson, 1985, p. 222). On the other hand, Sapir recognized that, in fact, there do seem to be limits to the variation, as was evident by the success of typological approaches to language in his day (ibid). He seems, therefore, to have adopted an uneasy compromise position between the extreme arbitrariness of language espoused by linguists like Joos, and the in-his-day discredited universalist notions of the previous century. The most original aspect of Sapir's universalism was the stress he put on the relation of his putative linguistic universals to concepts:

> He assumed that every language must express a certain range of basic (concrete) concepts corresponding to the reference of simple lexical items, especially nouns and verbs. In addition, every language must express a certain range of *pure relational* concepts, which "serve to relate the concrete elements of the propositions to each other, thus giving it definite syntactic form." The positing of such categories as *necessary* is already a significant departure from the strong view that languages are in principle arbitrarily different from one another.
>
> (ibid, p. 225)

Looking back on this from a contemporary generative perspective, it seems odd that Sapir did not move from these generalizations about universal linguistic structure toward a biological explanation. It is hard to account for *necessary* relations and categories in strictly social terms. At some point, one asks the question, where do the social relations and categories of language come from? If they are creatively developed from scratch in each culture, one would not expect universals. Yet Sapir strictly maintained that linguistic structure was culturally based. Therefore, for him the "fundamental problem of linguistics was not the construction of a 'theory of grammar' but the elucidation of the relationship of language on the one hand and culture and personality on the other" (ibid,

p.228). In fact, Sapir's theory seems to be a good example of Kuhnian blindness[84]. Because of his training under Boaz, who was a confirmed environmentalist, and because nativist theories were strongly disavowed by most scholars from the 1920s through at least the 1950s, Sapir couldn't or wouldn't consider the possibility of innate universals, even though such universals offer a much better solution to the problems he saw in linguistics than culture did.

Whorf (1847-1941)

Sapir's conceptual-cultural perspective became enormously influential in the 1920s and 1930's, attracting numerous students, the most famous of whom was Benjamin Lee Whorf (1847-1941). Whorf was a professional fire insurance inspector who dabbled intensely in many areas unrelated to his profession including botany, theology, and later linguistics (Carroll, 1956). Brilliant and curious, he was also a bit of a mystic with strong teleological leanings. In the 1920s he got the idea that the much debated discrepancies between Biblical and scientific views of the world might ultimately be linguistic rather than substantive, and he therefore began studying Hebrew with the goal of performing his own exegesis of the Bible, which he hoped might reconcile scripture and science (ibid, p. 7). During this quest he became acquainted through reading, with various mystical/linguistic systems and he eventually began applying these to other languages, particularly American Indian languages including Aztec, Mayan, Nahautl, and Hopi. Whorf was apparently a gifted language learner, although it is difficult to tell how

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[84] By this I mean the tendency that Khun pointed out for scientists to remain within a dominant paradigm even when the evidence points in another direction. As discussed in Chapter 6, there is a range of Kuhnian perspectives, from the very strong perspective, wherein dominant paradigms and other social factors completely obscure rationality, to weaker views in which social factors and rationality interact. I, of course, take a weak view.

proficient he actually was with any of the languages he studied. In any case, Whorf pursued his studies of language and linguistics energetically, attending occasional lectures and reading books on linguistics, and without any formal training he began publishing papers on Indian languages (ibid). During this period Whorf tried to develop the theory of "oligosynthesis," which he defined as the "name for the type of language structure in which all or nearly all of the vocabulary may be reduced to a very small number of roots or significant elements" (in Carroll, 1956, p. 12). This theory, which Whorf later dropped after he had studied under Sapir (Carroll, 1956, p.16) is more notable for what it says about Whorf than about language. What is consistently evident in Whorf's work is that he approached language study with a set of very strong preconceived ideas which were part of a comprehensive cosmology designed to unify science, history, linguistics and theology. This makes for rather fascinating reading, especially since Whorf was a bright and imaginative man; however, Whorf's extensive claims abut particular languages and the nature of language in general should be read with extreme caution, since there is good reason to believe these conclusions were driven more by his preconceived and quite unfounded theories than by the data themselves.

Whorf's most famous conclusion is that language structure *determines* thought, to the degree that different language communities literally cannot think the same thoughts since they must express them differently This position, referred to, variously either as linguistic relativity (in most commentaries), or linguistic determinism (Pinker, 1994) originated in a much weaker form with Sapir, with whom Whorf studied during the early 1930s. For someone predisposed toward grand unifying theories, the idea that language

shapes thought was attractive since, if true, such a theory could unify and explain psychology, history, culture, literature, and probably politics and theology as well. So, Whorf enthusiastically searched for evidence of this theory, and he claimed to have found a lot. He "found" for example, that Eskimos have many more words for snow than other people and thus conceive of the landscape differently; that the Hopi have no words for time, and thus, presumably no clear concept of time; that cultures have different sets of color terms, and thus must see the world in different sets of colors. The trouble is, all of these strong claims have been refuted. Geoffry Pullum, among other linguists, demolished the Eskimo claim in his article "The Great Eskimo Hoax" (1991). In this article, Pullum first shows that, in fact, the Eskimo languages Whorf made claims about do not have more words than, say, English does for snow, and he then argues that even if they did, that wouldn't be an argument for linguistic determinism:

> Would anyone think of writing about printers the same sort of slop we find written about Eskimos in bad linguistics textbooks? Take [the following] random textbook. . ., with its earnest assertion "it is quite obvious that in the culture of the Eskimos. . . snow is of great enough importance to split up the conceptual sphere that corresponds to one word and one thought in English into several distinct classes. . . " Imagine reading: "It is quite obvious that in the culture of the printers. . . fonts are of great enough importance to split up the conceptual sphere that corresponds to one world and one thought among non-printers into several distinct classes. . . " Utterly boring, even if true.
>
> (Pullum, 1991; cited in Pinker, 1994)

Pullum's point is that we do not think the printers' language is remarkable because we know that it simply reflects their working environment, one that just happens to be concerned with fonts. We know that we can easily expand our own repertoire of technical terms, without any major shift in conceptual categories, so why should we assume that Eskimos would have to have a radically different conceptual categories if

they have more words for snow? I myself have a snow story which relates to this issue. I lived for several years in Yosemite National Park, where I spent my winters cross country skiing. In those days many of us still used waxable skis, which meant that we had to learn to match the right wax—designated by color—with the snow at any particular time. If you choose a wax that is too hard, you'll get no traction, whereas a wax that is too soft will stick to the snow. The colder and drier the snow, the harder the wax you must use. As a result of this, a spontaneous vocabulary sprang up among the skiers. We would ask one another "How's the snow?" and the reply would be something like, "It's red, with a purple kicker," meaning you should use red wax on the tips and heels of your skis and purple on the portion under your feet. Since we used seven different waxes, we could describe the snow in at least fourteen different ways with this system. Now it may be that having this system helped us to focus more carefully on the clues in the snow that reveal it's temperature and moisture content, but it surely couldn't have been the case that the vocabulary *enabled* us to see those differences; we saw the differences first, and then developed the vocabulary to suit our observations.

The Hopi myth about time is really fantastic. Can anyone really believe that an entire culture could survive without being able to communicate references to events in time? In any case, it too has been solidly refuted by analysis and translation of Hopi. Pinker provides this passage, translated from Hopi as evidence:

> Then indeed, the following day, quite early in the morning at the hour when people pray to the sun, around that time, then he woke up.
>
> (1994, p. 53)

The translation is ample evidence, but Pinker goes on to add that "the anthropologist Ekkehart Malokti, who reported this sentence, also showed that Hopi speech contains

tense, metaphors for time, units of time (including days, number of days, parts of the day, [etc]."

As for the color myths, numerous studies have been done on this, none of which support strong linguistic determinism (Lakoff, 1984). It is true that different languages employ different ranges of color terms, some with as few as two and others with eleven or more, and in special situations (like a paint store) hundreds. However, researchers have found that all languages reflect certain color facts, and, moreover, that individuals' perception of color is not restricted to the terms in their language. Thus, in the most restrictive case, a two color language like Dani, native speakers have no trouble detecting differences among many different colors, even though they have no words for those colors.

So, a strong Whorfian perspective (strong linguistic determinism) is not supported by the evidence. Moreover, note that we would have to reject very strong linguistic determinism on the same grounds of irresoluble paradox as we've previously used to reject antifoundationalism. For, if language really did determine thought in a strong sense, we could never think a thought our language hadn't already licensed, and so could never compare one language's conceptual structure to another's, since a fortiori, for another language to have a different conceptual structure would entail its having a structure not licensed by our own language. However, a much weaker Whorfian theory might be sustainable, if language structure is construed as just one more factor in the mix of many factors that influence us. Logically, this would be the appropriate starting point, since it is undeniable that languages do have different structures, at least at surface levels, and since, presumably, we are affected by all the factors in our environment, however

minor some of these influences may be. The hypothesis itself is easy to come by, but it is difficult to test and qualify. But this is what a responsible social constructionist theory must do if it is to add at all to our knowledge and understanding of thought and language.

It is ironic to note that Whorf, a man committed to a grand totalizing vision of rational/spiritual cosmology, was later picked up by people who seek to undermine rationality and who categorically deny the possibility of comprehending reality. Yet, though it is ironic, it is not difficult to understand. Whorf made the fundamental mistake of positing a theory that undermined itself though it is clear that this was not his intention. He writes constantly, for example, of "different logics," existing in different grammars, a proposition which, if possible, would render other language speakers completely and forever incomprehensible to us, yet he emphatically did not believe in such mutual incomprehensibility. In his analyses, such as they are, we find a man who believes that *he* at least *can* read the peculiar logic of the "other" and make it clear to the rest of us. Somehow it doesn't seem to occur to him that if he can do this it can't really be "other." Or perhaps, although he talks about different logics, he really just meant different emphases on the same logical system. But, again, if *he* was able to see this difference, there must be some part of mind that is independent of language that allowed him to do so; again this doesn't seem to have occurred to him.

Bloomfield (1887-1949)

Although Bloomfield was Sapir's contemporary and, in fact, interacted with Sapir and even worked briefly in the same university (Illinois), his thinking about language was completely different. Whereas Sapir was interested in intangible bases of language such as "society and culture," Bloomfield was determined to excise anything that was not

"scientific" from his theories of language. As Anderson puts it, this orientation was "a product of the times, [a desire] for scientific explanation based solely on propositions relating observable factors and influences by principles of logic and mathematics alone" (1985, p.254). Given this orientation, one might expect that Bloomfield would have relegated himself to the study of "sound laws" or other phonetic principles. However, Bloomfield came to linguistics during an age when the phoneme was central to the field, and phonemes are necessarily related to meaning. Thus, Bloomfield had to incorporate some notion of meaning in his system even though he felt that abstractions like meaning were far beyond "scientific analysis." His answer to this conundrum was to adopt a strictly behaviorist definition of meaning: "the disposition of a speaker to respond in particular ways to particular stimuli" (ibid, 255). Meaning thus defined could then be treated as a postulate and then ignored, enabling the real work of the linguist, which for Bloomfield was the systematic evaluation of sound changes in languages. Bloomfield's work in this area is complex and important for the development of phonology; however, for our purposes here, what is mostly important is Bloomfield's contribution to a widespread rejection of "mentalism" prior to Chomsky. In this respect Bloomfield was very much like Skinner, the last of the major figures I will look at here.

Skinner (1904-1990)

Like Bloomfield, Skinner was profoundly influenced by the theories of Willhelm Wundt (1832-1920), who is sometimes referred to a the father of psychology, or one of the fathers if William James is counted as the other (Boeree, 2000). Wundt "argued that conscious states could be scientifically studied through the systematic manipulation of antecedent variables" (Francher, 1979), a position that later became the sin qua non of

Skinner's behaviorism. Skinner first tested the "antecedent variable" theory on rats, discovering through this that he could reliable manipulate the rats behavior by rewarding them with food. He then extended this idea to humans, reasoning that humans, like all organisms, were motivated by the hope of rewards (positive stimulus) and by the desire to escape pain (negative stimulus):

> When a bit of behavior is followed by a certain kind of consequence, it is more likely to occur again, and a consequence having this effect is called a reinforcer. Food, for example, is a reinforcer to a hungry organism; anything the organism does that is followed by the receipt of food is more, likely to be done again whenever the organism is hungry. . . . Negative reinforcers are called aversive in the sense that they are the things organisms turn away from.
>
> (Skinner, 1971, P. 27.)

No one doubts that such consequences do affect behavior, but it is a large leap from that trivial observation to the conclusion that *all* behavior is merely the product of stimulus/response. Yet Skinner tried to extend stimulus/response theory even to very complex behavior. In 1957 he even tried to account for "verbal behavior" in a book by the same name, by stimulus/response theory. But, in one of the most famous public rebuttals in history—"Review Of Verbal Behaviour by B.F Skinner" (Chomsky 1959)—Chomsky, demonstrated that stimulus/response theory could not possibly account for the *infinite* range of sentences humans can produce. Chomsky acknowledged that Skinner's theory accounts well enough for behavior in rats and pigeons and presumably other lower organisms, but he argues there is no reason to assume it will work with a more complex organism. He then shows that Skinner fails to define the term *stimulus* when he applies the theory to human beings in real situations. In fact, Chomsky argues, Skinner cannot predict any but the most routine "verbal behavior" and instead resorts to characterizing the "stimulus" *after* the response is heard:

> A typical example of stimulus control for Skinner would be the response to a piece of music with the utterance Mozart or to a painting with the response Dutch. These responses are asserted to be "under the control of extremely subtle properties" of the
>
> physical object or event (108). Suppose instead of saying Dutch we had said Clashes with the wallpaper, I thought you liked abstract work, Never saw it before, Tilted, Hanging too low, Beautiful, Hideous, Remember our camping trip last summer?, or whatever else might come into our minds when looking at a picture (in Skinnerian translation, whatever other responses exist in sufficient strength). Skinner could only say that each of these responses is under the control of some other stimulus property of the physical object. If we look at a red chair and say red, the response is under the control of the stimulus redness; if we say chair, it is under the control of the collection of properties (for Skinner, the object) chairness (110), and similarly for any other response. This device is as simple as it is empty. Since properties are free for the asking (we have as many of them as we have nonsynonymous descriptive expressions in our language, whatever this means exactly), we can account for a wide class of responses in terms of Skinnerian functional analysis by identifying the controlling stimuli. But the word stimulus has lost all objectivity in this usage. Stimuli are no longer part of the outside physical world; they are driven back into the organism. We identify the stimulus when we hear the response. It is clear from such examples, which abound, that the talk of stimulus control simply disguises a complete retreat to mentalistic psychology. We cannot predict verbal behavior in terms of the stimuli in the speaker's environment, since we do not know what the current stimuli are until he responds. Furthermore, since we cannot control the property of a physical object to which an individual will respond, except in highly artificial cases, Skinner's claim that his system, as opposed to the traditional one, permits the practical control of verbal behavior is quite false.
>
> (Chomsky, 1959)

The gist of this criticism is that if one proposes a powerful enough mechanism, then, indeed, everything can be "explained." But "explanation" that lacks any predictive power is vacuous. Notice that the same criticism can often be applied to social constructionsist/ antifoundationalist "explanations" of responses to interlocutors or literature. For example, Stanley Fish, continually claims that all interpretations are "constrained" by the discourse community to the extent that no individual responses are

even possible. No matter what response a reader might have, Fish can always place that response in some relation to some discourse community such that he can claim "constraint." The problem with this is that it is trivially true; ultimately, it must be admitted that *whatever* anyone ever says must be a "reflection of his predispositions and biases" (Fish, 1989, p. 87), and if you have a loose enough definition of "discourse community"—say, everything a person has ever heard or read—then it's probably true that all responses are constrained by the discourse community; but unless the responses can be predicted and accounted for in some rule governed fashion, no useful explanatory principle has been demonstrated.

Chomsky's Epistemology

With this brief history behind us, we are now in a better position to understand the revolutionary aspects of Chomsky's work, and also to see that antifoundationalist attitudes toward language are to a significant degree still dominated by ideas that have long since ceased to hold sway among linguists. A detailed exposition of Chomsky's work would be far beyond the scope of this dissertation. Rather, in what follows, I will simply try to identify the leading principles of his thought about language, which principles I will later argue are essential for a minimal foundationalist perspective on literacy. Chomsky typically starts his books with a brief exegesis of the major problems he is concerned with, often tracing these problems back to their roots in classical philosophy and epistemology and also comparing them to linguistic systems that preceded generative grammar. Like Fodor, Chomsky is a rationalist/empiricist, so, although he is concerned to show how his work is related to previous work, the latter does not depend on the former in the historiographical sense that Rorty alleges is

fundamental for all work (1979, p. 178). This difference is important. From an antifoundationalist perspective, ideas are related *only* to other ideas, thorough discourse communities, not to the world itself. Thus, from that perspective the only way to assess theories is historiographically since the quality and importance of theories is *completely* a function of how they affect society. Rationalists, on the other hand, generally view the social effects of their work as irrelevant to the work itself, though they may nonetheless be intensely interested in those effects. But, for rationalists, the value of scientific work is wholly determined by how well it describes and explains aspects of the world. Thus, in principle, rational work in science should be culturally and historically neutral in the sense that it could theoretically be introduced anywhere, at any point in history, provided that the hypothetical society was willing to tolerate scientific exposition[85]. Chomsky demonstrates implicit agreement with this claim by starting from very basic, self evident assumptions and then building his theory up from there in as explicit a fashion as possible. Again like Fodor, Chomsky calls attention to the fact that he is developing a scientific theory, which is subject to falsification. I will trace one of his most extensive discussions of this kind below, which is found in *Knowledge of Language* (1986).

Chomsky approaches his subject from a broad historical and philosophical vantage point, presumably to establish a common ground of important concepts and arguments with the reader. From the first page, he is concerned to establish the point that people have long recognized cross-language similarities in grammar, which fact suggests

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[85] No doubt many constructivists would find this to be an unacceptably large assumption; they would point to periods of scientific intolerance like the middle ages and claim that scientific tolerance itself is just another cultural mode. Though rationalists would acknowledge that cultures can and do outlaw science, they would disagree that science was just another cultural mode. Rather, qua rationalists, they would argue that science is a fundamental expression of rationalism that is at least latent in all societies.

a universal, innate basis for language. Briefly tracing this nativist/universalist line of thought from Descartes to its hiatus during the Structuralist period, and then to its revival in the 1950s, Chomsky establishes generative grammar as a branch of cognitive psychology that is sharply distinguished from behaviorism:

> Put in the simplest of terms, to be elaborated below, the shift of focus was from the behavior or the products of behavior to states of the mind/brain that enter into behavior. If one chooses to focus attention on this latter topic, the central concern becomes knowledge of language: its nature, origins and use.
>
> The three basic questions that arise, then, are these: (1)
>
> (i) What constitutes knowledge of language?
>
> (ii) How is knowledge of language acquired?
>
> (iii) How is knowledge of language put to use?
>
> (1986, p.3)

Preparing to explore these problems, Chomsky then makes several quick appeals to common sense, to show that he is starting at bare beginnings with no special assumptions. In doing so, he implicitly assumes a rational foundation; however, he doesn't bother to exegete or defend that foundation, perhaps deciding that anyone who rejects rationalism is beyond argument. Speaking of the general program of generative grammar, as circumscribed by his three questions, Chomsky remarks:

> Some elements of this picture may appear to be more controversial than they really are. Consider, for example, the idea that there is a language faculty, a component of the mind/brain that yields knowledge of language given presented experience. It is not at issue that humans attain knowledge of English, Japanese, and so forth while rocks, birds, or apes do not under the same (or indeed any) conditions. There is then some property of the mind /brain that differentiates humans from rocks, birds, or apes.

(ibid, p. 4)

The reasoning and the examples are childishly simple, though logically irrefutable, thus challenging the reader to dispense with sophistries in favor of a straight-forward, rational/empirical approach to the problems outlined in (1), above. Moving quickly forward in this vein, Chomsky then argues for the fact of generative grammar:

> Generative grammar is sometimes referred to as a theory, advocated by this or that person. In fact, it is not a theory any more than chemistry is a theory. Generative grammar is a topic, which one may or may not choose to study. Of course, one can adopt a point of view from which chemistry disappears as a discipline (perhaps it is all done by angels with mirrors). In this sense, a decision to study chemistry does stake out a position on matters of fact. Similarly, one may argue that the topic of generative grammar does not exist, although it is hard to make this position minimally plausible.
>
> (ibid, p. 4)

Again, in these arguments, Chomsky throws down a rationalist gauntlet, saying, in effect, if you want to remain in this discussion, you have to accede to basic rationalist tenets, *and* widely held principles of science. With respect to chemistry, it is well known—though properly understood only by people with advanced knowledge of chemistry—that substances are composed of molecules and that these molecules have certain definite properties that allow them to combine, or not, in rule-governed ways, and that these properties are responsible for many of the effects that we observe in nature. Crucially, this sort of understanding is at least second order, in the sense that the molecular activity we know is responsible for chemical reactions is not directly observable. Yet, the central theories of chemistry, like valence, have been so extensively cross-verified that they have attained the status of "lawlike structures." Thus, to accept chemistry as a topic is to accept a general materialist cosmology, together with a great many specific premises of science, and, most importantly, the cardinal scientific premise that knowledge of the

world can be gained through careful analysis of data and extrapolation from data to probable models of reality. As Chomsky observes about chemistry, it is possible to come up with other explanations, but, clearly, in his view, these other explanations lack credibility because they are not supported by data or plausible models. In effect, in this allusion to the topic of chemistry, Chomsky establishes a set of minimal foundationalist assumptions about reality, in particular the Kantian notion that effects must have causes, and further, the materialist assumption, that complex systems, are ultimately explicable according to properties inherent in the components of the systems, together with certain general laws of whatever system one is looking at. By pairing generative grammar with chemistry in his analogy, Chomsky implies that grammar, like chemistry, must be a rule governed system, and that we therefore ought to look within human minds—the containing systems—for the components and properties of grammar. In this argument, Chomsky gets slightly ahead of himself, since, at this point in the discussion he has yet to provide evidence of the rules he refers to. However, he is writing against a backdrop of many years of published work in generative grammar, by literally thousands of linguists, and there is plenty of evidence in this work to support the claim that there are abstract rules behind human grammars. Moreover, a few pages later in the discussion, Chomsky provides several data sets as evidence for his claim that people have robust intuitions about sentences that are explicable only in terms of some internal rule system.

Chomsky's Empirical Evidence for I-language

Since Chomsky's central claim about language is that humans must have innate "knowledge[86]" of structure in order to create and parse the unlimited set of sentences that human grammars allow, he is particularly concerned to dispel the popular notion that knowledge of sentence meaning proceeds according to "analogy," i.e., that one simply learns a few "sentence patterns" and then interprets other sentences "by analogy" to the "master" sentences one already knows. I quote a few of these examples, along with some of Chomsky's analysis below[87]:

> Consider the following sentences:
>
> John is too clever to catch (10)
>
> John is too stubborn to expect anyone to talk to Bill (11)
>
> John is too stubborn to visit anyone who talked to Bill (12)
>
> Suppose we delete *Bill* from (11) and (12), yielding (13) and (14), respectively:
>
> John is too stubborn to expect anyone to talk to (13)
>
> John is too stubborn to visit anyone who talked to (14)
>
> Sentence (13 is structurally analogous to (10), and is understood in the same manner: it means that John is so stubborn that an arbitrary person would not expect anyone to talk to him (John). "By analogy," then, we would expect sentence (14) to mean that John is so stubborn that an arbitrary person would not visit anyone who talked to him (John). But it does not have that meaning; in fact, it is gibberish.
>
> (Chomsky, 1986, pps. 9-11)

This and other data and analysis Chomsky supplies convincingly illustrates that we have strong intuitions about sentences that must be derived by rules not contained in the

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[86] Chomsky often remarks that "knowledge" may be a misnomer for what is innate, since that term implies conscious access. Occasionally he has used the term "cognize" instead, but he usually stays with "knowledge," with the understanding that such knowledge is to be distinguished from consciously learned knowledge.

[87] Although the sequence of quoted sentences conforms to the original, for the sake of brevity, I have left out a page of intervening text between sentences (10) and (11).

sentences themselves. Crucially, these intuitions are shared by virtually all native speakers of the language, English in this case. Thus, in (13) virtually all native speakers of English will agree that the object of the preposition "to" can only be, and *is*, in fact, "John," and further, though inexplicably according to surface facts, that there is no object, not even an "understood object" for the corresponding sentence (14), and that (14) is therefore "gibberish; and, finally, that though an object can be supplied for (14), which will rectify the sentence, it cannot be "John," though any other name/person would be fine. The crucial point here is that these facts are neither derivable from the sentences themselves nor from common external sources, like dictionaries, yet they are known to everyone who is a native speaker of English, even though most English speakers have never encountered these sentences before. Moreover, if we continue working in this manner, i.e., continue examining native speakers' intuitions about language data, as Chomsky and many other linguists have done, we will find hundreds of similar facts, many of which can be subsumed under powerful general principles, which principles together with specific rules and idiosyncrasies, such as the particular lexicon of the language in question, form "generative grammars," i.e., grammars that generate all and only the licit strings in the language. An additional, astonishing fact is that all normal children develop these generative grammars at a very early age, without relevant instruction. Taken together, these facts can be used to form what is known as the "poverty of the stimulus argument":

1) All normal children acquire a robust knowledge of their native language, quickly and without relevant instruction.

2) The knowledge they acquire far exceeds the data they receive.

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3) Therefore, they *must* have some innate knowledge of language, though this knowledge must be at a very abstract level since it allows them to acquire any natural language

(paraphrased from Chomsky, 1968))

Since this argument and the other claims about the fact of generative grammars are very well known and almost universally accepted by linguists, on the basis of vast amounts of evidence, I will not take the space to defend them here, through more examples, but will simply stipulate them[88]. If they are accepted, however, there are far-reaching implications for literacy in general, and for our original question about the locus of meaning also. The fact that sentences require the addition of missing elements and structure for interpretation, categorically rules out the possibility of exhaustively "autonomous" or text-centered text. We can see this especially well with sentences that are syntactically ambiguous, though interpretable. Consider, for example, the old introductory linguistics chestnut :

1) The cop shot the robber with the rifle

This sentence has exactly two universal interpretations, where universal means "interpretations available to all normal, native speakers of English[89] without additional context." The sentence can mean either

2) The cop shot the robber who had the rifle.

or

3) The cop used the rifle to shoot the robber.

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[88] Though I am resorting to stipulation here, my action is not remotely comparable to the stipulations Rorty makes, and which I have complained about, above. The difference is that in my case, there is vast amounts of solid evidence to support my stipulations, and I cite sources for that evidence, whereas in his case there is none.

[89] Chomsky uses the notion of "ideal speaker/hearer"; the result is substantively the same.

This fact is standardly used by linguists to prove the existence of syntactic structure. Given that there are two, and only two, universal interpretations of sentence 1), we must conclude that English speakers rearrange the words in some rule governed way during interpretation. The standard analysis is that they either attach the prepositional phrase as a modifier to the noun phrase "the robber," yielding interpretation 2), or they attach it to the verb phrase "shoot," yielding interpretation 3). Extensive analysis of sentences and universal interpretations has shown that this sort of structural rearrangement of constituents is ubiquitous, occurring regularly in all languages; moreover, even in structurally unambiguous sentences the degree of structural rearrangement appears to be extensive, requiring the addition of abstract grammatical markers that are not present at all at the surface of the sentences (Chomsky, 1995). These facts also militate against the claim, favored by Rorty, that people think in natural languages. The fact that two, or sometimes more, quite different thoughts can be externally represented by one sentence, shows that these thoughts cannot be internally represented by the surface structures of those sentences, and that, in turn, strongly militates against the idea that thinking occurs in natural languages. Rorty's naivete about this, typical among non-linguists, shows why it is necessary to bring an empirical dimension into any discipline concerned with language. What linguists consistently find is that the real principles behind language are not guessable. The only way to discover how language really works is to study it scientifically, by building models of it and testing them against native speakers intuitions.

VII: TRUTH

Most linguists and virtually all laypeople make the commonsensical assumption that statements are true iff (if and only if) they correspond with the world. Thus "it's raining" is true iff it is raining. In truth-conditional semantics, also called truth-functional semantics (hereinafter TFS), this concept is formalized by reducing sentences to their putative "logical forms," and knowledge of meaning is defined as the ability to know precisely what real world conditions, or in some cases, what *possible world* conditions are entailed for a sentence to be true[90]. Thus, to know what "Rorty is a professional philosopher" means, is said to be equivalent to knowing what the world would have to be like for that statement to be true. I will argue in my penultimate chapter that we need much more than this to account for meaning; however, I will retain the notion of truth functions as nearly all linguists do, since these seem indispensable.

Antifoundationalism would appear to be in direct conflict with TFS, because TFS seems to require a correspondence theory, which in turn entails an objective reality and a range of fit between that reality and our beliefs and statements. I will take the position here that TFS does entail all these things, although this point is now disputed by one of the principle founders of TFS, Donald Davidson (e.g., 1990).

Antifoundationalists, particularly Rorty, have seized on Davidson's rejection of correspondence as support for their anti-realist position. Although Davidson has moved away from strong realism, I think his position is still far from antifoundationalist, and that Rorty egregiously misapplies Davidson's texts to his own antifoundationalist arguments,

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[90] Actually everyone now acknowledges that there is more to meaning that truth conditions, but, if certain other conditions are met, many theorists still regard TFS as a fairly complete theory of meaning.

much as he did with Fodor's. I will show some of this as we proceed, but it is not my main focus. Rather, what I seek to show here is that a correspondence definition of truth is essential to a sensible theory of meaning, and that the arguments against correspondence are fatally flawed.

In rejecting correspondence, antifoundationalists claim to be operating with fewer assumptions than correspondence adherents, because the latter posit both an objective reality and a relation to that reality, to which we stand; whereas, antifoundationalists supposedly operate with no assumptions. However, as we've seen in *The Mirror*, antifoundationalists begin from the axiom that all descriptions of reality are equidistant, and, that we stand in *no* relation to reality (see also Rorty 1998A, for repeated assertions of this position). Since these assumptions are nothing but the converse of correspondence assumptions, it is evident that antifoundationalists assume every bit as much as correspondence theorists, and, therefore, this objection is moot.

The main argument against correspondence is nothing other than the strong instrumentalist argument in a different form. Just as realist scientists attribute success in science to the increasing degree of fit between successive models and the world—whereas strong instrumentalists insist that better models are only better in that they are better at solving problems—so correspondence theorists explain truth as a relation of fit between statements and reality, whereas anti-correspondence theorists insist that truth is merely a matter of mutual coherence among propositions (Coherence), or, perhaps, only a name we use for propositions we "can defend against all comers" (Rorty, 1998A) or which we find "useful" (Pragmatics). As we've already seen, strong instrumentalism is underwritten by the same false assumption outlined above, i.e., by the assumption that it

is more conservative than realism; therefore, a good part of the argument against anti-correspondence is also the same as that outlined above, though there is also a substantial empirical dimension to the argument for correspondence, as we'll see.

I will take the position here that the notion of correspondence is analytically entailed in the concept "truth" and further that a correspondence theory of truth is a necessary though not sufficient condition for an adequate theory of meaning. Moreover, I argue that of the truth theories available—correspondence, coherence, pragmatic, redundancy, and semantic (Walker, 1997, p. 309), only correspondence fits minimal foundationalist assumptions. Since my task here is only to defend a minimal foundationalist framework, I will not discuss the other theories named above at length, since they are only a threat to correspondence if correspondence is shown to be deficient. But, in any case, it will turn out that defending correspondence entails rejecting these other theories, so most of the important points from them will be covered.

All of the objections to correspondence involve the charge that it is in one way or another unspecifiable. These objections break into two classes, linguistic and ontological/epistemological. There are three linguistic objections along these lines: 1) that correspondence is formally unspecifiable because one must translate the elementary terms of sentences into other terms, which in turn require translation, and hence infinite regress, or at best unspecified resolve; 2) that the terms can only be specified as "those which make a particular proposition true" (Walker, 1997, p. 310), which is ruled out because circular; and 3) that, technically, all true statements correspond to "the universe as a whole" and therefore correspondence is "trivialized" (Davidson, 1990, p. 303).

Wittgenstein is the most famous victim of the regress argument. In the *Tractatus*, Wittgenstein proposed a structurally isomorphic correspondence theory such that for every element in a proposition there would be a corresponding fact in the world (Walker, 1997, p.320) . The problem with this, according to Walker, is that

> it explains one correspondence (of the proposition with the fact) in term of others (names with objects, structure of a proposition with structure of fact) but since these are unexplained—according to Wittgenstein, inexplicable in principle—no real gain has been made.
>
> (ibid, p. 320)

While there is some truth to this, I'm not sure the objection has force, or if it does that it couldn't be made about any explanation. After all, every explanation must resolve somewhere, and wherever it does there is something "inexplicable" left[91]. Thus one can explain matter as an assemblage of molecules, molecules in terms of atoms, and atoms in terms of protons neutrons, electrons, and quarks, but that still leaves *these* entities "inexplicable." Perhaps Wittgenstein objected because the correspondence theory failed to break the corresponding terms in the world into smaller parts, thereby further explaining them, but if that is the objection, it seems to me to confuse explanation with analysis. Certainly we can imagine many cases where a non-analytical explanation would be completely satisfactory. The classic example is translation of a term from one language to another. Surely, to learn that *baño* means "bathroom" is a real gain just as it is to learn that a stick figure on a door corresponds with a bathroom. However, I will not pursue this defense of isomorphic correspondence because I find isomorphic correspondence untenable for other reasons—for one thing because words interact in

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[91] Ironically, Wittgenstein himself later made this same argument in *Philosophical Investigations*, although I think he applied it to things—linguistic behavior and intentions—that *are* further analyzable.

sentences thereby creating meanings that go beyond simple composition (Cruse, 1986; Levin, ?; Pustejovsky, 1995), and for another because the denotative meanings of sentences so often diverge from the speaker/writer's intent. I will return to this at length in the penultimate chapter.

The second linguistic problem is similar. Here the complaint is that correspondence is vacuous because a sentence is defined as true just in case it expresses propositions that match some state of affairs in the world, but that these states of affairs cannot be "specified except as those which make a particular proposition true," and such specification is to be rejected because circular (Walker, 1997, p. 310). Again, however, I think the objection lacks force. What it seems to show is just that there are limits to language. Language is a system of symbols, and symbols stand for things (where "thing" includes abstract concepts) but are not *equal* to them. It is impossible to improve on the definition of truth as correspondence, in its simple form because "correspondence with X" is what it *means* for a statement to be true, and, given this, circularity is analytically built into the concept "true." As we'll see, people have no trouble using the concept—something Davidson doesn't disagree with—nor do people generally have any trouble distinguishing true from false statements, at least not when those statements are about things that are within their competence to assess. Thus, regardless of whatever technical problems the concept may create for some system of logical semantics, it is evidently a perfectly clear I-language concept, and therefore one that has meaning.

The last linguistic objection is purely technical and for that reason the easiest to deflect. Endorsing an old line of logical analysis, Davidson argues that

> Frege's argument...can be formalized: starting from the assumptions that a true sentence cannot be made to correspond to something

> different by the substitution of coreferring singular terms, or by the substitution of logically equivalent sentences, it is easy to show that if true sentences correspond to anything, they all correspond to the same thing.
>
> (1990, 303)

Now, clearly, this is strictly a technical E-langauge problem that has nothing to do with the way people understand sentences. That alone should make anyone who claims to be working in semantics reevaluate his vaunted "logical system." But, in fact there is also a technical way around this problem, which I will explain very briefly. The problem arises because TFS is focussed on logical relations within and among propositions, and these are determined by truth tables (Larson and Segal, 1995). In logic, statements are said to be equivalent if they yield the same truth tables; the embarrassing consequence of this is that there is no way to block the "validity" of statements such as "Snow is white" iff *coal is black*, because the statements on either side of the biconditional (iff) are logically equivalent (Larson and Segal, 1995). But, since it is obvious that *people* do not consider such statements valid, the model must be adjusted, at least if the semanticist wants to make claims about natural languages. In fact, an easy adjustment is possible. We simply stipulate a rule for "substitution of equivalents" (SE) that says:

> we are allowed to substitute a formula β only if we have *proved* that it is equivalent to α as part of the derivation...The original production rule (SE) blocks [semantically unequivalent substitutions] because it does not allow substitution of arbitrary equivalents but only β*s* that have been proven equivalent to α as part of the derivation...Hence uninterpretive consequences like ["Snow is white" iff *coal is black*] are not derivable
>
> (Larson and Segal, 1995).

Thus, even within an E-language framework there is a way out of the absurd consequence that "true sentences all correspond to the same thing." But why should we worry about E-language in the first place? As Chomsky has argued on principle, and hundreds of

generative linguists have shown through analysis, natural languages are not explicable in E-language terms because the rules for natural languages are in speakers' heads, not in the data. Davidson seems thoroughly confused on this point. At times, as we'll see, he adopts a strong I-language stance, though he doesn't use that term, but, at other times, as in this example, he stubbornly clings to E-language "proofs," which he then applies to natural language analysis, with absurd results. As many people have argued, we can use TFS to discover logical properties of language, but where the logic of TFS contradicts native speaker intuitions, we should sacrifice the logic in favor of the intuitions if we want to understand language. I hasten to add that this is not to imply that natural language is "illogical." Language *is* logical, but the "code" is much more complex than the artificial logics people have developed. In my view—and that of hundreds of generative linguists—the only way to crack the code is by building models based on native speakers' intuitions

Actually, TFS can be naturalized[92] and made compatible with notions of I-language. Moreover, some of the traditional problems and solutions in TFS can be explained in I-language terms. For example, TFS, as developed especially by Davidson, based on Tarski's[93] logical system, solves the regress dilemma recounted above (the dilemma of an infinite regress of definitions) by inventing an "metalanguage" whose meanings are presumed. Thus, "snow is white" is true iff *snow is white*, is contentful because the meaning of the italicized object sentence is presumed, whereas the natural, or

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[92] For me, the "natural" and the "real," are not exclusive domains because minimal foundationalism allows the possibility of an intersection between the natural and the real (in a priori knowledge). Katz, in particular disagrees with this, arguing that naturalism excludes realism. I return to this issue at the end of this chapter.

"object" language sentence must be interpreted. Now TFS performs the translation of natural language sentences into metalanguage sentences by magic, as it were; the move is purely theoretical/hypothetical. That is, TFS just says that if we *could* assume that we know what such and such a sentence means, then we would be free to focus on its logical structure. Since we think we do know what sentences mean, the theoretical move is made on the assumption that semantics is largely a product of logical structure and, further, that logical structure becomes transparent when sentences are reduced to truth conditions. There are known problems with the reduction, but, nonetheless it is valuable to be able to abstract the logical structure of sentences, and TFS does do so. My suggestion is that a naturalized TFS can be justified, and can avoid some of the problems that E-language TFS runs into.

From an I-language perspective we can claim that TFS converts linguistic propositions into non-linguistic ones, just as E-language TFS does, but without resorting to magic. From a language of thought/I-language perspective (hereinafter "I-Lg. perspective"), there is nothing strange about non-linguistic propositions. To the contrary, according to that model propositions are formed first in the language of thought, and then in the I-language of the individual (the idiolect). At the public, or E-language level, most of the detail is stripped away leaving just a skeletal or "type" structure (Katz, 1990). In E-language TFS, an odd result of translation of natural language sentences into metalanguage sentences is that while TFS can theoretically justify metalanguages, it cannot get inside them, which is why truth functional semantics fails to explain anything beyond the logical structure of propositions, and even this is often compromised by

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[93] Tarski (1902-1983) was a world-renowned mathematician, logician and philosopher and the inventor of

lexical semantics. This creates problems for TFS because it must either ignore these conflicts in order to focus on its preconceived logic, or try, by whatever Procrustean means it can invent, to force natural language into the structure of formal logic. But a naturalized TFS welcomes logical problems because these problems tell us how to adjust the model. Since naturalized TFS is not wedded to a preconceived logic, but is, rather, only attempting to infer and model the system of logic that natural languages actually use, naturalized TFS never has to accept absurdities like universal equivalence of true statements. As we have already seen in the Wittgenstein *red spot, blue spot* example there are severe problems with the assumption that natural languages are reducible to formal logic, and Katz, in particular, argues that the entire project is fatally flawed because semantics is a different domain from logic (1990, 1998). I will follow Katz in claiming that semantics is a different domain from logic; however, I will argue that we still need to retain a naturalized TFS. Ironically, as we see above, TFS can be justified by I-language, but, in practice, TFS has often been pursued under E-language assumptions. Because of this, some TFS practitioners run into many seemingly intractable problems that could easily be resolved if they adopted an I-language perspective. I will argue here that Davidson's problem with correspondence is exactly this problem and that the solution to it is to naturalize the term "correspondence."

Davidson On Correspondence

Interestingly, Davidson—the philosopher most responsible for the development of TFS—has come to realize, in a crisis reminiscent of Wittgenstein's *Tractatus* crisis—that TFS is completely inadequate as an explanation of meaning (Davidson, 1986). I

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truth functional semantics.

wholeheartedly agree with Davidson on that point, as most people do; however, I disagree sharply with Davidson on the implications of this fact for a theory of meaning. As we shall see, these differences are partly explicable as a consequence of differences I also have with Davidson over the correspondence theory, which differences are ontological/epistemological.

However, as with Wittgenstein, it is necessary to refer to the "early" and the "later" Davidson, for Davidson has radically changed his position on the status of truth in recent years. As late as 1986, he was strongly endorsing both correspondence and realism:

> In this paper I defend what may as well be called a coherence theory of truth and knowledge. The theory I defend is not in competition with a correspondence theory, but depends for its defence on an argument that purports to show that coherence yields correspondence.
>
> (1986/1990)

This is a perfectly rational position; however, the seeds of what I will refer to as the Davidsonian crisis (also mentioned above), were already germinating during this period:

> Setting aside aberrant cases, what brings truth and knowledge together is meaning. If meanings are given by objective truth conditions there is a question how we can know that the conditions are satisfied, for this would appear to require a confrontation[94] between what we believe and reality; and the idea of such a confrontation is absurd.
>
> (ibid, p. 120)

As we will see, Davidson's eventual rejection of correspondence is largely due to this alleged absurdity. However, at this stage, he has not yet abandoned realism:

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[94] Apparently, this is the source of Rorty's "[let us renounce] confrontation and constraint" slogan. Note that Rorty's use is substantially different from Davidson's; this is another example of Rorty's hallmark technique of twisting other people's statements to suit his "pragmatics."

> Given a correct epistemology, we can be realists in all departments. We can accept objective truth conditions as the key to meaning, a realist view of truth, and we can insist that knowledge is of an objective world independent of out thought or language.
>
> (ibid, p. 120-121)

This is strong realism; in fact, I will argue that it is too strong, and, ironically, that it is this inordinate strength that eventually causes Davidson's crisis. Again, the situation is oddly reminiscent of Wittgenstein's *Tractutus* crisis in that in both cases what eventually caused the break was that the old system was held too rigidly, and, further, that when the system became intolerable, analysis of natural language was abandoned instead of abandoning the logical system that was the real cause of the trouble. In any case, Davidson's apostasy is remarkable:

> There is thus a serious reason to regret having said that a Tarski-style truth theory was a form of correspondence theory...But I was still under the influence of the idea that there is something important in the realist conception of truth; the idea that truth and therefore reality, are (except for special cases) independent of what anyone believes or can know.
>
> (1990A, p. 304)

Davidson goes on to reinterpret his own realist writing so as to diminish its realist import; however, I view this as suspicious revisionism because, a) it requires us to take Davidson's word now for what he believed then, over what he clearly stated, and b) it is obviously not disinterested revision. In any case, I will argue that the early Davidson was largely correct about correspondence, though he stated the theory in realist terms that were too strong, and, worse, confused I-language and E-language correspondence. On the way to making those arguments I would like to bring Rorty back into the discussion

since much of what he—and by extension all antifoundationalists—hold to be true[95] about truth and meaning is related to what the later Davidson says.

Rorty seizes on these later Davidsonian statements as evidence that Davidson agrees with him about adopting a pragmatic stance toward truth, i.e., the stance that truth is purely a semantic concept and therefore irrelevant to the world. For example, in the introduction to a collection of his (Rorty's) essays (*Truth and Progress*, 1998) Rorty quotes a passage from the same essay I have quoted above, prefacing the quote with the remark that "[t]hese essays may be thought of as footnotes to Davidson's claim that"

> [w]e should not say that truth is correspondence, coherence, warranted assertability, ideally justified assertability, what is accepted in the conversation of the right people, what science will end up maintaining, what explains the convergence on single theories in science, or the success of our ordinary beliefs.
>
> (Davidson, 1990, p. 309; cited in Rorty 1998, p. 11)

However, Rorty's confidence is unjustified. First of all, as usual, Rorty carefully selects only the parts of Davidson's passage that seem to support his position. Let's look at the whole passage. Davidson's quote, above, continues directly as follows:

> To the extent that realism and antirealism depend on one or another of these views of truth we should refuse to endorse either. Realism with its insistence on radically nonepistemic correspondence, asks more of truth than we can understand; antirealism with its limitation of truth to what can be ascertained, deprives truth of is role as an intersubjective standard. We must find another way of viewing the matter.
>
> (Davidson, 1990, p. 309 p. 11)

This is a substantially different position than Rorty's, and it is therefore disingenuous of Rorty to have truncated the quote as he did. Davidson clearly wants to retain the notion of truth qua truth, though he gets into severe difficulty defining truth without

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[95] The irony of this has not escaped me.

correspondence. Davidson and Rorty both reject correspondence theories, but for different reasons. Rorty rejects correspondence because he rejects rational assumptions (that reason is veridical, that the universe is structured, law-governed, and relatively constant) or, to put it the other way, because he endorses the skeptical position that we can never know what reality is like, nor in what relation to it we stand[96]. But Davidson's rejection of correspondence is partly a result of technical difficulties with TFS, as we've seen above, and partly a result of a move toward instrumentalism. While antifoundationalists may endorse strong instrumentalism, they also reject all innate properties, including reason, but Davidson retains these notions, as we'll see.

The ontological/epistemological objection to correspondence, which Davidson among others has made in various forms, is just that correspondence requires us to extrapolate beyond things we clearly understand. Davidson is extremely conservative in his theorizing, probably as a result of his extensive work in logic, wherein everything must be rigorously defined. In any case, a hallmark of Davidson's work is his extreme discomfort with anything that can't be formally defined:

> The realist view of truth, if it has any content, must be based on the idea of correspondence, correspondence as applied to sentences or beliefs or utterances—entities that are propositional in character; and such correspondence cannot be made intelligible.
>
> (1990, p. 304)

Here, Davidson approaches the solution I'm suggesting (naturalized, I-language correspondence) when he includes beliefs in his set of possible correspondents, but he then flees from this good possibility because of the alleged unintelligibility of the idea.

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[96] Rorty often says he is not a skeptic, and in fact claims that his pragmatism is a bulwark against skepticism; however, he substantially alters the definitions of all the crucial terms in these arguments, which renders them irrelevant. I spare the reader these details.

Here, he seems to want to distance himself from strong realism, and he seems to think that he must abandon correspondence to do so. Presumably, strong realism—or more precisely, "reality"—is "unintelligible" because undefinable. However, Davidson never develops this point; rather, he just repeats his assertion in different words:

> I was concerned [in his 1986 article, *A Coherence Theory of Truth and Knowledge*] to reject the doctrine that either reality or truth depends directly on our epistemic powers. There is a point in such a rejection. But it is futile either to reject or accept the slogan that the real and the true are "independent" of our beliefs." The only evident sense we can make of this phrase, the only use that consorts with the intentions of those who prize it, derives from the idea of correspondence, and this is an idea without content.
>
> (ibid, p. 305)

Part of this argument is directly traceable to what has been called the "evil Wittgensteinian tradition in philosophy" of declaring all propositions that do not have clear, formally definable extensions "ideas without content."[97] But, at least in this case, the declaration is easily countered. If I tell you that it's raining when it isn't, I can probably make you believe it if, in your experience, I've generally been truthful; but your belief will be completely independent of the fact that it isn't raining. Anyone can prove this for him/herself. That's *content*. Again, it seems that Davidson is mixing two things here. The "slogan" he alludes to is a common one, shared by scientists, jurists, linguists, and most everyone else. It is certainly not exclusive to TFS; but the correspondence invoked in the last line is, apparently, the correspondence of E-language TFS, which is an exotic concept that is contentful but clearly substantially different from natural language correspondence.

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[97] I can't trace the source of this comment; possibly Fodor or Katz made it.

The odd thing about Davidson's position, the thing that makes it untenable, is that he wants to make declarations like the above and yet hang on to the core of what correspondence does for a theory of truth and meaning:

> The thesis that a theory of truth conditions gives an adequate account of what is needed for understanding the literal meanings of utterances, is of course much disputed, but since I have argued for it at length elsewhere, I will for the most part treat the thesis here as an assumption. A theory of truth, viewed as an empirical theory, is tested by its relevant consequences, and these are the T-sentences entailed by the theory. A T-sentence says of a particular speaker that, every time he utters a given sentence, the utterance will be true if certain conditions are satisfied.
>
> (ibid, p. 313)

Importantly, Davidson refers to T-sentences here not truth sentences, ostensibly because he wants to limit his claim to the formal theory. However, this leaves him open to problems like universal equivalence unless he builds in a condition like SE[98]. In this article, however, he doesn't address that problem, but he does give examples of applications that imply that, somehow, SE, or something like it, is respected and, further, that the theory is not to be considered merely a formal tool for analysis, but, rather, an explanatory theory for real linguistic communication:

> The possibility of understanding the speech or actions of an agent depends on the existence of a fundamentally rational pattern, a pattern that must, in general, be shared by all rational creatures.
>
> (ibid, p. 320)

It's hard to reconcile statements like this with Davidson's previous avowals of strict empiricism, and his denials of our ability to know what reality is like. As Fodor pointed out, "rationalists are nativists by definition," and a nativism that accepts rationality entails

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[98] In a footnote Davidson says "a theory of truth may make a distinction if there are circumstances which never arise but under which the truth conditions would be different," apparently as a partial explanation for

an epistemology that accepts something like the Kantian doctrine of *a priori* sources of knowledge. These are non-empirical positions, which should be disallowed by strict empiricists. Davidson continues:

> We have no choice then, but to project our own logic on to the language and beliefs of another...Here progress [in communication] depends on attending, not just to which sentences an agent holds true, but also to the events and objects in the world that cause him to hold the sentences true. The circumstances, observable by speaker and interpreter alike, under which an agent is caused to accept as true sentences like 'It is raining', That's a horse', or 'My foot hurts', provide the most obvious evidence for the interpretation of these sentences and the predicates in them. The interpreter, on noticing that the agent regularly accepts or rejects the sentence 'The coffee is ready' when the coffee is or is not ready will (however tentatively pending related results) try for a theory of truth that says that an utterance by the agent of the sentence 'The coffee is ready' is true if and only if the coffee can be observed by the agent to be ready at the time of the utterance.
>
> (ibid, p. 320)

Note that in this paragraph we are back to "truth sentences," as opposed to T-sentences. This is important because it commits Davidson to a naturalized TFS whether or not he acknowledges it. The last claim (last sentence) seems to have been hastily stated, and cannot be accepted as it stands. As stated, the sentence mixes the interpreter's truth theory with his theories about the verification strategies of the "agent." But why should the "agent's" verification strategies have anything at all to do with the interpreter's theories of truth? Probably, what Davidson meant to write was: "The interpreter...will try for a theory *about the agent* that says that an utterance...*will be held true by the agent* iff the coffee can be observed by the agent..." I think this is what Davidson must have meant; if not, i.e., if he means the statement as it stands, then I can make no sense of it at

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how universal equivalence could be handled. However, the explanation is far too restricted as it stands to handle all the possible cases.

all. But even if he means it as I have revised it, the theory is too strong since it is certain that few interpreters would limit their theory of what an agent holds true to cases that the agent personally verified. As I pointed out earlier, most agents will accept all sorts of secondary verification, such as statements from third parties. Clearly a theory of truth that limits verification to personal inspection will have vastly different consequences for theories of interpretation than truth theories that include secondary sources of verification. I'm sure Davidson would agree that agents regularly accept secondary sources of verification, so we can consider this to be a simple error.

(ibid, p, 320).

At this point, It think it should be clear that Davidson's objections to correspondence are technical rather than substantive. On any non-technical account, the examples above support a simple correspondence theory. Should the reader reflect on some of the discussions in the prior chapters, s/he may realize that there are ways to reconcile Davidson's position to a fully naturalized TFS. However, I will reserve my suggestions regarding those possibilities until the end of the chapter, so that we may first see how Rorty exploits Davidson's "rejection" of correspondence.

In what follows, I will demonstrate that Rorty's position deviates drastically from this central, natural correspondence theory, which, it is clear, Davidson still wants to retain. Since there could be no better spokesperson for the centrality of correspondence in a theory of truth, I will quote extensively from the "early" Davidson in this section, though I will not designate the quotes that way. After this exposition, I will return to the later Davidson's arguments and offer my suggestion for how the two positions can be reconciled.

Truth versus Rhetoric

In his essays, Rorty demonstrates the "usefulness" of rejecting standard notions of truth relations in language by substituting a complex, obfuscatory rhetorical style for logical argument and evidence. One of his rhetorical tricks is to use standard vocabulary in distinctly nonstandard ways that can only be understood from within his "hermeneutical" paradigm.[99]" He thus manages to sneak numerous doubtful premises into the discussion without ever establishing or defending them. Moreover, by imposing his bizarre definitions in this way, as if his definitions *were* standard, he implies that there isn't any controversy around these ideas, and thus attempts to bully the reader into a starting position that virtually guarantees agreement with the "philosophy" that ensues. For example, Rorty uses the term "language" as a rough substitute for "belief system." This is devious because his "language"—lets call it R-language—also includes the standard definitions of language. Thus, Rorty forces the notion that language and thought are one and the same without having to defend that discredited notion. He also uses "vocabulary" in the same way, so that he can speak of people using "old," "new," and "better" vocabularies" as a substitute for people learning new ideas; again, the rhetorical effect is to smuggle in the discredited notions that language creates thought, and that ideas are "acquired" behavioristically rather than built up intellectually.

Wittgenstein's Language Games

Another idea that Rorty simply assumes is that language use is a Wittgensteinian game, or "language game" as Wittgenstein famously called it. However, Rorty's use of

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[99] Indeed, his very use of "hermeneutics" to mean rejection of rational epistemology, is itself a good example of this practice.

this term is considerably shifted from Wittgenstein's, though not from the popular, antifoundationalist use of the term. In Rorty's usage—and throughout antifoundationalist texts—all uses of language are "games," in the hermetic sense, i.e., in the sense of being primarily related only to other "language games" rather than being correspondent to the world. However, this is not what Wittgenstein himself meant by the term, as we can see from his discussion of it:

> I shall in the future, again and again, call your attention to what I shall call language games. These are ways of using signs simpler than those in which we use the signs of our highly complicated everyday language. Language games are the forms of language with which a child begins to make use of words. The study of language games is the study of primitive forms of language or primitive languages. If we want to study the problems of truth and falsehood, of the agreement and disagreement of propositions with reality, of the nature of assertion, assumption, and question, we shall, with great advantage look at primitive forms of language in which these forms of thinking appear without the confusing background of highly complicated processes of thought.
>
> (1933/1958, p.17)

Here we see that Wittgenstein originally restricted the term "language game" to "simple" or "primitive" uses of language, which is no recommendation for Wittgenstein's ideas, but only points out that the use Rorty attributes to Wittgenstein is quite a bit different from Wittgenstein's. Later, Wittgenstein seems to abandon this restricted use, but, he still does not use the term the way Rorty does.

Regarding Wittgenstein's original concept, above, it seems to be a confused idea. There is nothing "primitive" in the "forms" that he selects, nor are there any such things as "primitive languages," though there may be something *basic* (or "primitive" in one sense) about the *functions* Wittgenstein selects, in the sense that most utterances can probably be attributed to one or a combination of these functions. Austin's speech act theory is somewhat similar in its functional approach, and, as Austin showed, language

use can be sensibly organized along these lines (Austin, 1962). But Wittgenstein's choice of the word "game" for this idea was unfortunate because it implies relations that are not easy to defend in ordinary language use. Plausibly, a game minimally entails a few things, including 1) that the player(s) recognize that they are playing a game, and 2) that the "moves" and goals of the game be internal to the game, although it is possible for the players to have outside motives for playing. To illustrate, chess is a game that is played by moving pieces on a checkered board. The object or goal of the game is to "checkmate" the opponent's king, and all the moves of the game can be related to that purpose. Players need to know the rules of the game, but these rules have nothing to do with anything outside the game. Moving a pawn to, say, "queen's bishop three," only has meaning and purpose within the game. A player who makes such a move, may be playing to win, and if s/he wins s/he may enjoy consequences outside the game, such as a prize. However, these exterior consequences are only remotely related to the game; moreover, it is entirely possible to play the game without making reference to anything outside it. I would argue that all games share these basic properties of self-containment and irrelevance, or at least distinctly indirect relevance, to the larger world. But language use is rarely like this. In ordinary situations, people using language do not believe that they are "playing a game" in the sense I've just outlined, nor are their utterances self-contained. To the contrary, ordinary language use is directly related to extra-linguistic purposes, chiefly communication of ideas about the world. Thus the ordinary concept of language as a tool, a direct means to non-linguistic ends, is a far better general description of language than Wittgenstein's game notion.

Actually, In *Philosophical Investigations* Wittgenstein also describes language as a tool, but only in three cases, whereas he uses the game metaphor at least sixty times. Nonetheless, it seems to me that what he meant by "game" was closer to the tool sense than to the "game" sense discussed above. In fact, in his first use of the tool metaphor, Wittgenstein seems to use the terms—"language game," and "tool" almost interchangeably. In this case, he is attempting to create a general functional outline for language use:

> But how many kinds of sentences are there? Say assertion, question, and command?—There are *countless* different kinds of use of what we call "symbols", "words", "sentences". And this multiplicity is not something fixed, given once and for all; but new types of language, new language-games, as we may say, come into experience, and others become obsolete and get forgotten...Here the term "language-*game*" is meant to bring into prominence the fact that the *speaking* of language is part an activity or of a form of life.
>
> Review the multiplicity of language games in the following examples, and in others:
>
> Giving orders, and obeying them—
> Describing the appearance of an object, or giving its measurements—
> Constructing an object from a description (a drawing)—
> Reporting an event—
> [eight more such examples]
>
> --It is interesting to compare the multiplicity of *tools* in language and of the ways they are used and of the ways they are used, the multiplicity of kinds of word and sentence, with what logicians have said about the structure of language (Including the author of the *Tractutas Logico-Philosophicus.*)
>
> (Wittgenstein, 1968, passage 23, p. 11; my italics on "tools")

Actually, it is not clear whether "tools" and "language games" are meant to be interchangeable here, i.e., that they both refer to the items on the list. Perhaps "tool" refers to grammatical rules used to construct the "games." This second sense is supported by Wittgenstein's second use of the tool metaphor. Wittgenstein is writing

about a complex, hypothetical example (passage 48) he had previously constructed in which colored squares are referred to by labels (R, G, W, B):

> Our language-game (48) has various possibilities; there is a variety of cases in which we should say that a sign in the game was the name of a square of such-and-such a color...if it were set down in writing, say in the form of a table, that *this element corresponded to this sign*...We can also imagine such a table's being a tool in the use of the language...if we call such a table the expression of a rule of the language-game, it can be said that what we call a rule of a language-game may have very different roles in the game may have very different roles in the game.
>
> (Wittgenstein, 1968, passage 53, p. 26; my italics)

OK, so "tool" is a rule of grammar for constructing the "games" (whether E-language or I-language is unclear), but the "language-games" themselves are clearly not hermetic; rather they are *routines* that have definite *correspondence* with things in the world. Again, I think Wittgenstein's choice of "game" ("sprachspiel" in the original German) was simply an unfortunate metaphor. I won't belabor the point, or bore the reader with numerous examples here, but if one peruses *Philosophical Investigations* I think s/he will see that a much better case can be made for "game" with the sense "routine" rather than the hermetic sense of "game" I developed above. This is important, for Rorty—and most antifoundationalists—lean very heavily on the hermetic sense of "game" in their use of the term "language game."

Language without Correspondence

With this background we are now in a position to understand the title of Rorty's essay, "The Contingency of Language,[100]" which essay develops the thesis that language is radically disconnected from reality, or, in our terms, not correspondent with it. In the essay, we also see that for Rorty language and classical philosophical questions are

completely intertwined, so that any discussion of one entails the other. Thus, the essay is about the "contingency of language," which aspect engages our current themes about truth, but also about Darwinian evolution (Rorty's very idiosyncratic version of course) and "intellectual history." As has been my strategy throughout this dissertation, I will address these substantive themes as well as Rorty's rhetorical strategies. In this case, the latter are particularly important since they have direct bearing on truth. As we know, Rorty claims that truth should not be defined in terms of statements' correspondence with reality, so it should not surprise us that Rorty's statements themselves frequently fail to correspond with reality. This state of affairs raises serious questions about the possibility of sustaining any sort of antifoundationalist program since one can never rely on statements made by people who explicitly reject truth as a goal of inquiry.

In the essay "Language" is R-language (see above), and the "contingency" refers to R-language's alleged total dependence on random blind forces or "contingencies" in the world. This is obviously a hopelessly paradoxical thesis since any argument one could make for it would be radically undercut by the thesis itself; i.e., if true, the thesis itself would be revealed to be nothing more than another "contingent language game." Rorty brazenly charges ahead anyway:

> The difficulty faced by a philosopher who, like myself, is sympathetic to this suggestion [that R-language empowers people to change, although it itself is contingent on random forces]—one who thinks of himself as auxiliary to the poet rather than to the physicist—is to avoid hinting that this suggestion gets something right, that my sort of philosophy corresponds to the way things really are. For this talk of correspondence brings back just the idea my sort of philosopher wants to get rid of, the idea that the world or the self has an intrinsic nature. From our point of view, explaining the success of science, or the desirability of political liberalism, by talk of "fitting the world" or

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[100] This is the same essay quoted above (p.112).

> "expressing human nature" is like explaining why opium makes you sleepy by talking about it's dormitive power[101]. To say that Freud's vocabulary gets at the truth about human nature, or Newton's at the truth about the heavens, is not an explanation of anything. It is just an empty compliment—one traditionally paid to writers whose novel jargon we have found useful. To say that there is no such thing as intrinsic nature is not to say that the intrinsic nature of reality has turned out, surprisingly enough, to be extrinsic. It is to say that the term *intrinsic nature* is one which it would pay us not to use, an expression which has caused more trouble than it has been worth. To say that we should drop the idea of truth out there waiting to be discovered is not to say that we have discovered that there is no truth. It is to say that our purposes would be better served by ceasing to see truth as a deep matter, as a topic of philosophical interest, or "true" as a term which repays "analysis."
>
> (1998A, p. 70)

Before addressing the particular claims in this passage, let me point out, once again, the extremely paradoxical and chauvinistic nature of these arguments. Rorty begins by exhorting philosophers "like [himself]" not to make claims about reality, i.e., not to be "epistemological" and then plunges into a set of dogmatic ontological and cosmological assertions. In the process he rules out evidentiary procedure by fiat, stipulating from the beginning that "talk of correspondence" between ideas and reality is naïve. This is like being told in a court of law that all evidence against the state's position is illegal. As for the substantive claims, *why* and *how* does it "pay us" to not use a term like *intrinsic nature*? Notice that Rorty doesn't say there isn't such a thing, although he implies that the very idea is naïve. But this is certainly false. There is overwhelming evidence that, in fact, there are many intrinsic aspects of nature, both human and otherwise. Wouldn't we be best off to understand these aspects of reality? If, to take just one example of

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[101] Actually Dr. Moliere's famous analogy suits Rorty's argument not the rationalists'. Dr. Moliere's point was that explanation requires analytical content; one must expose the mechanisms underlying the object of study, not merely recast it in alternate vocabulary. Science is dedicated to that kind of analysis, whereas antifoundationalism rejects the very idea of analysis.

literally thousands that easily spring to mind, schizophrenia is, at least to a significant degree, caused by some genetically inherited, i.e., "intrinsic" neuronal/chemical abnormality, as is now believed by most psychologists, doesn't it "pay us" to know that, rather than continuing to seek the cause in God, the Devil, or pathological human interactions, as was the case for hundreds of years? Rorty's implicit answer to such questions is dismissive avoidance: "The nature of truth is an unprofitable topic." But since the evidence overwhelmingly supports the opposite conclusion, and since Rorty supplies no evidence whatsoever to support his antifoundationalist claims, we are certainly justified to question them, and in fact, reason compels us to do so.

Rorty is aware of the weakness in these declarations, as is evident by the convoluted excuse he gives for them:

> On the view of philosophy I am offering, philosophers should not be asked for arguments against, for example, the correspondence theory of truth or the idea of "intrinsic nature of reality." The trouble with arguments against the use of a familiar and time honored vocabulary is that they are expected to be phrased in that very vocabulary. They are expected to show that central elements in that vocabulary are "inconsistent in their own terms" or that they "deconstruct themselves." But that can *never* be shown. Any argument to the effect that our familiar use of a familiar term is incoherent, or empty, or confused, or vague, or "merely metaphorical" is bound to be inconclusive and question-begging. For such use is, after all, the paradigm of coherent, meaningful, literal speech. Such arguments are always parasitic on, and abbreviations for, claims that a better vocabulary is available. Interesting philosophy is rarely an examination of the pros and cons of a thesis. Usually it is, implicitly or explicitly, a contest between an entrenched vocabulary and a half-formed vocabulary which vaguely promises great things.
>
> (Rorty, 1998, pps. 70-71)

Taking the last claims first, it is hardly the case that "interesting philosophy" fails to examine the pros and cons of theses. To the contrary, all the major philosophers through at least Heidegger, including, Plato, Aristotle, Descartes, Locke, Hume, and Kant; and literally hundreds of contemporary philosophers as well as writers who nominally do not qualify as philosophers since they do not belong to departments of philosophy, but who write on philosophical topics, invariably do treat the pros and cons of the theses they examine. In fact, it is only a rarified selection of philosophers, such as Wittgenstein, Quine, and Rorty himself, who prefer to focus on the disparities between "entrenched" and emerging vocabularies, and even these philosophers—including Rorty!—often debate the pros and cons of various theses—a minor point, no doubt, but one that exemplifies Rorty's loose relationship with facts. But notice the main sleight of pen in the above passage. Rorty argues that he doesn't have to provide arguments against theories he disagrees with, such as theories concerning the "intrinsic nature of reality" because such arguments reduce to arguing against a "time honored vocabulary." But in making this claim he dogmatically asserts the very thesis he purports to be explicating in the essay, i.e., why it is that we should prefer to assume that our perceptions are all contingent on "language" (R-language) as opposed to beginning from the common assumption that language serves thought and that thought itself is at least potentially grounded in reason and reason in reality. Crucially, this is not just a matter of arbitrarily picking one "vocabulary" (R-vocabulary, of course) over another. Rorty's "vocabulary" is currently unsupported by evidence, so we have a right to ask him to defend it rather than just assuming it. Rorty's preemptive statements are supposed to render common sense objections like this one "ineffective" since to make them is to "fall once again

under the sway of the time honored vocabulary." However, a moment's reflection on the history of science disproves his thesis. Time and time again, as many historians of science have shown, new arguments *are* offered in the "time honored vocabulary" of theory-backed-by-evidence, and it is simply false that these new arguments are never accepted. Now, of course if Rorty equates "vocabularies" with "ideas," as he often does, then, *by definition,* there can never be new ideas framed in old vocabularies, but this is just tautology, and can therefore be dismissed.

This claim that "vocabularies" are radically incommensurate depends entirely on the denial of correspondence, for correspondence with reality is what commensurates vocabularies. Rorty is fond of arguing that people from different periods in history could not even have had meaningful discussion because their vocabularies were incommensurate. Aristotle and Newton, for example, supposedly couldn't have discussed astronomy because all of their terms—sky, star, earth, etc.—would have referred to different things. Though we can't test this thesis, it seems extremely doubtful. As Maudlin pointed out (Sokal and Bricmont, p. 75) Aristotle would have perceived a moon rock as a rock, just as we would. Is there really much doubt, that likewise, if presented with modern astronomical evidence regarding, say, heliocentrism, he would not have adopted the new model, and that his doing so would be a result of the Copernican model corresponding better with the facts than the Ptolomeic? Rorty, is aware of the implausibility of a complete lack of correspondence between language and the world, which is demonstrated by such examples, so he tries to confuse the issue and then hedge his bets, first by dragging in allegations of Raving Platonism, and then by equivocating on correspondence:

> The suggestion that truth, as well as the world, is out there is a legacy of an age in which the world was seen as the creation of a being who had a language of its own...if one clings to the notion of self-subsistent facts, it is easy to start capitalizing the word *truth* and treating [sic] it as something identical either with God or with the world as God's project. Then one will say, for example, that Truth is great and will prevail.
>
> (ibid, p. 67)

This is all pure nonsense. The correspondence definition of truth merely says that true statements are those that correspond with the facts—no Platonic or other religious notions are entailed. I turn, then, to the more substantive claim—that there is a significant difference in correspondence between single claims and whole "vocabularies." The following block continues directly from the above quote:

> This conflation is facilitated by confining attention to single sentences as opposed to vocabularies. For we often let the world decide the competition between alternative sentences (e.g., between "Red wins" and "Black wins" or between "The butler did it" and the "The doctor did it"). In such cases it is easy to run together the fact that the world contains the causes of our being justified in holding a belief with the claim that some nonlinguistic state of the world is itself an example of truth, or that some state of the world is itself an example of truth, or that some such state "makes a belief true" by "corresponding" to it. But it is not so easy when we turn from individual sentences to vocabularies as wholes. When we consider examples of alternative language games—the vocabulary of ancient Athenian politics versus Jefferson's, the moral vocabulary of Saint Paul versus Freud's, the jargon of Newton versus that of Aristotle, the idiom of Blake versus that of Dryden—it is difficult to think of the world as making one of these better than the another, of the world as deciding between them.
>
> (ibid, p. 67-68)

As to the first part of this argument, if we can reliably "let" the world decide cases of fact; i.e., if independent observers can reliably reach the same conclusions—stated in natural language—based only on their observations of facts, then we have a strong basis for a correspondence theory. As Rorty implicitly admits here, such conclusions are ubiquitous, so he should accept correspondence. However, he clouds the issue by

claiming that holding a correspondence theory of truth is tantamount to believing that a state of the world [can] itself [be]an example of truth." But no one has alleged any such thing. Truth requires *correspondence* between propositions and some possible world because truth is a function that links statements to conditions; therefore, like an equation, it requires two sides, one propositional and one factual. Whatever one thinks about the theoretical status of truth, it is an empirical fact that the relation is universally recognized by humans, so this last Rortian argument can be dismissed.

The part about whole "vocabularies" not corresponding to reality is similarly irrelevant to correspondence definitions. No definition of correspondence alleges any such global correspondence, as Rorty knows, so this is just a red herring he is using to avoid direct discussion. However, while relations between whole "vocabularies" and the world are dubious, such "vocabularies" can be broken down into thousands of propositions, and these are no different in their potential relations to reality than the "single sentences" Rorty mentions. It would be easy enough to go through any of these "vocabularies" and pick out propositions to assess in terms of correspondence. But since Rorty leaves things in maximally vague terms, no analysis is possible.

Finally let us return to the claim that propositions about the world are nothing more than "language games". Put slightly differently the claim says that "only sentences can be true, and that human beings make truths by making languages" (ibid, p.71). Lets look at these claims carefully. First of all, notice that once again, we are exposed to extreme paradox: If truth is simply part of a "language game," something humans "make. . . by making languages," then the sentence *only sentences can be true* tells us nothing about the world, though it surely seems to. In fact, according to Rorty's statements

above, we are free to play any other language game, say the language game that says *sentences are true iff they correspond to the way things are in the world.* Of course playing that "game" gets us no closer to any objective truth than Rorty's game, but on Rorty's own account we can't get closer to truth anyway[102]. The problem for Rorty is that, by his own account, language games *cannot* say true things about the world, and he thus has no way to argue for one language game over another except to say that some games are "more useful" than others. But even this doesn't get him out of the paradox, for unless "usefulness" is defined in non-subjective ways, anything can be construed as "useful"; on the other hand, however, if "usefulness" *is* defined, that will entail saying things about the world, but unless those things are true, we will not be constrained to believe them. If taken seriously, then, Rorty's arguments spin into endless paradox. I think, however, that this is not the result Rorty seeks. Evidently, he seeks to persuade us of his view of the world, including his view of language, and he offers *reasons* for his position. He says for example:

> The temptation to look for criteria [for making decisions] is a species of the more general temptation to think of the world, or the human self, as possessing an intrinsic nature, an essence.
>
> (ibid, p. 69)

There is a very important claim about the world here, the claim[103] that neither the world nor human beings has an "intrinsic nature." Now, he can't have it both ways, either these sentences are just "language games," in which case they *cannot* say true things

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[102] By this time it should be superfluous to point out that the statement "we can't get closer to truth about the world" is yet another Epimenidean paradox (also know as a "liars paradox"), part of an endless chain of such paradoxes engendered by these kinds of arguments.

[103] The claim is not overt, but I think unavoidable given any reasonable reading.

about the world, or they are something more than language games, i.e., true (or false) claims. But if he wants us to interpret the sentences the second way, i.e., as claims about the world, then he is obligated to provide evidence for his claims, or at the very least, he is prohibited from preempting our presentation of counter-evidence. This is so because in making claims about the world, Rorty has entered an arena where evidence counts. His only way of excluding evidence is to fall back on the assertion that "truth is [just] a property of linguistic entities, of sentences" (ibid, p. 69), but then the arguments themselves are meaningless; *by defintion* they haven't said anything about the world.

While I find Rorty's arguments absurd, for the reasons given above, evidently they are persuasive to many people, so we might ask why. To begin with, I think that Rorty is preaching to the choir. I doubt that many confirmed rationalists, and that includes nearly all cognitive scientists and linguists, would find his arguments at all compelling, but for those who are already somewhat persuaded by social constructivism the passage is probably seductive. One reason for this is that the entities Rorty selects as examples—"the world" and "the self"—are both undefined and a bit mysterious. "The world" is a vague, potentially all encompassing term, and it is this infinite scope that might make it seem like a poor candidate for an intrinsic nature, particularly a nature that it "possesses." The implication is that to ascribe intrinsic qualities to the world is to take a very myopic, possibly animistic view of things, one that underestimates the complexity and ineffability of reality. The word "essence" also helps to discredit the notion of intrinsic nature because this word is strongly associated with ancient Greek mysticism and teleology, ideas such as the "harmony of the spheres" and Aristotle's "telic principle" of nature. Crucially, however, neither mysticism nor teleology is entailed by claims of

intrinsic nature. The cosmos as a whole and humans in particular could both be products of "blind contingent forces" and yet have intrinsic natures, if what we mean by this is just that they have qualities that are inherent, relatively stable, and not determined by our beliefs about them.

The other entity Rorty selects, "the self" is a completely mysterious term, one that everyone agrees is, at best, only experientially defined at present. Ascribing intrinsic qualities to such an entity thus seems premature and hypostatical, since how can we ascribe qualities to an undefined entity? Thus, by choosing undefined terms and implicitly linking them to discredited ideas, Rorty makes the notion of intrinsic natures seem anachronistic and naïve. But this is a purely rhetorical effect. In fact, he has given no evidence to support his "arguments" and he has avoided contemporary, informed notions about intrinsic nature, such as those from cognitive science and linguistics. If the discussion remained here, the impact would not be too strong. We could dismiss "the self" as poorly defined, and we would probably find that, logically, it is at least as compelling to ascribe intrinsic qualities to "the world," however we define that term, as to deny them. But Rorty doesn't linger here. Instead he makes a quick turn back into "language," forcing an unwarranted equivalence between thought and "language" and between "language" and theories:

> The temptation to look for criteria [for making decisions] is a species of the more general temptation to *think of the world*, or the human self, as possessing an intrinsic nature, an essence. That is, it is the result of the temptation *to privilege one among many languages* in which we habitually describe the world and ourselves.
>
> (repeated from above; emphasis mine)

Here it is asserted that thinking is nothing more than making "habitual" statements, a view that is entirely consonant with strong social constructivism, but which is

vehemently rejected by nearly all linguists and cognitive scientists because there is massive evidence against it (Pinker, 1994). And notice again his peculiar use of the word "language." As part of his strategy to equate language with thought, Rorty uses "language" instead of "idea" or "cultural belief system." Thus, later in this article he refers to the transition from "speaking Neanderthal to speaking postmodern" (ibid, p. 77). Again, there are strong claims about the world here, claims that are open to investigation; moreover, there is a large extant literature on these issues in psychology, linguistics, psycholinguistics, anthropology, and cognitive science, which could certainly shed light on them. But all of this is ignored in favor of pure rhetoric. Rorty continues:

> As long as we think that there is some relation called "fitting the world" or "expressing the real nature of the self" which can be possessed or lacked by vocabularies, we shall continue the traditional philosophical search for a criterion to tell us which vocabularies have this desirable feature.
>
> (ibid, p. 69)

This passage subtly shifts the reader's focus from the original question about whether or not things have intrinsic natures to the problem of describing these natures, and then, without answering either question, abruptly conflates the two problems. In doing so, another contentious claim, that language "constructs reality," is introduced and tacitly endorsed. These maneuvers make it seem as if evidence for verbal "construction" and against intrinsic nature have been offered when, actually, no evidence for or against either point has been produced.

The Early Davidson's View of Truth

Let's return now to the paradox created by the claim that "only sentences can be true." I have already shown that Rorty uses sentences to make claims about the world,

and this shows that antifoundationalists cannot maintain the "Wittgensteinian" doctrine of the hermeticism of sentences without falling into hopeless contradiction. Rorty has tried to confuse the issue by claiming a major difference between whole vocabularies and individual sentences. However, this strategy is undone by first rejecting global correspondence and then by reiterating the claim that individual propositions within "vocabularies" are indeed subject to truth conditional analysis. In fact, as I argued earlier in this chapter, truth conditional semantics—the system Rorty implicitly endorses when he names Davidson as his linguistic mentor[104]—is completely dependent on this point. Rorty avoids this difficulty by focussing exclusively on one half of the system. That is, Rorty emphasizes the (trivial) fact that "only sentences can be true"—extrapolating wrongly from there that sentences about the world are just hermetic language games — and avoids what that proposition entails, i.e., that to be true, sentences must be true *about something*. But Davidson is clear about this point:

> Language is an instrument of communication because of its semantic dimension, the potentiality for truth or falsehood of its sentences, or better, of its utterances and inscriptions. The study of what sentences are true is in general the work of the various sciences; but the study of truth conditions is the province of semantics. What we must attend to in language, if we want to bring into relief general features of the world, is what it is in general for a sentence in the language to be true. The suggestion is that if the truth conditions of sentences are placed in the context of a comprehensive theory, the linguistic structure that emerges will reflect large features of reality.
>
> (Davidson, 1984, p. 201)

In other words, while truth conditions may be semantical, there can be no such conditions without reference to a non-linguistic reality; true sentences are true *because* they jibe with the world. Note also Davidson's unequivocal delegation of fact finding to science.

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[104] In his article, Rorty explicitly names Davidson as the source for most of his ideas about language

Science tells us how the world is; linguistics tells us how language *represents* that knowledge. Davidson goes on to suggest how a formal theory based on these principles might work:

> Tarski's work on truth definitions for formalized languages serves as inspiration for the kind of theory of truth that is wanted for natural languages. The method works by enumerating the semantic properties of the items in a finite vocabulary, and on this basis recursively characterizes truth for each of the infinity of sentences. Truth is reached from the basis by the intervention of a subtle and powerful concept (satisfaction) which relates both sentences and non-sentential expressions to objects in the world.
>
> (ibid, p. 203)

Again, note the unequivocal dependence on exterior reality as a check on truth conditions, and also the reference to *non-sentential* expressions, which expressions are necessary in truth conditional semantics as an intermediate step between natural language and the world. That is, truth conditional semantics imposes nomological relationships among three naturally overlapping but uncoordinated domains: natural language, formal ("logical") language, and the world. As Davidson explains, this is done because

> [w]e know how to give a theory of truth for the formal language; so if we also knew how to transform the sentences of a natural language systematically into sentences of the formal language, we would have a theory of truth for the natural language.
>
> (ibid, p. 203)

For a small subset of simple natural language sentences such transformation is straight-forward. However, truth conditional semantics quickly encounters difficulties because natural language contains relations that do not appear in formal languages (Grice, 1989, p.22). The general approach to this difficulty is to try to break the natural language relations down into formal ones. Thus, for example, Davidson cites Frege's treatment of the natural language connective "before":

> we can formulate the truth conditions for the sentence 'Jack fell down before Jack broke his crown'[105] as follows: it is true if and only if there exists a time *t* and there exists a time *t'* such that Jack fell down at *t*, Jack broke his crown at *t'*, and *t* is before *t'*.
>
> (ibid, p. 211)

The translation of "before" to *t* and *t'*, imposes the necessary formality on the sentence for it to be handled by truth-conditional semantics, which is desired and expected, but it also brings in unwanted "ontological" aspects:

> So apparently we are committed to the existence of times if we accept any such sentence as true. And thinking of the holistic character of a truth definition, the discovery of hidden ontology in sentences containing "before" must carry over to other sentences: thus 'Jack fell down' is true if and only if there exists a time *t* such that Jack fell down at *t*.
>
> (ibid, p. 211)

Davidson's conclusion from all this, contra Rorty's language game position, is that truth conditional semantics is inextricably intertwined with propositions about the world. Taking as his final example, the "hidden ontology" behind sentences that refer to events, Davidson lays out the consequences of this position:

> What a theory of truth does is describe the pattern truth must make among the sentences, without telling us where the pattern falls. So, for example, I argue that a very large number of our ordinary claims about the world cannot be true unless there are events. But a theory of truth, even if it took the form I propose, would not specify which events exist, nor even that any do. However, if I am right about the logical form of sentences of very common kinds concerning change, then unless there are events there are no true sentences of very common kinds about change. And if there are no true sentences

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[105] Here Davidson ignores another problem, i.e., how to account for the regular use of "and" in natural language as a connective that also entails time relations. As we all know from nursery school, the correct quotation is "Jack fell down *and* broke his crown." The annoying problem for truth conditional semantics is how to translate this "and" properly without destroying the cardinal formal language tenet of strict compositionality. Work in lexical semantics by Cruse (1986), Levin (1993), Pustejovsky (1995), and others has cast extreme doubt on the viability of strict compositionality for natural language, which in turn casts doubt on the viability of truth conditional semantics as an economical way to explicate the rules of natural language semantics.

> about change, there are no true sentences about objects that change. A metaphysician who is willing to suppose no sentences like 'Vesuvius erupted in March 1944' or 'Caesar crossed the Rubicon' are true will not be forced by a theory of truth to admit the existence of events or even, perhaps, of people or mountains. But if he accepts that many such sentences are true (whichever they may be), then it is obvious that he must accept the existence of people and volcanoes; and if I am right, the existence of events like eruptions and crossings.
>
> (ibid, p.214)

We can extrapolate from this passage that for Davidson there can be no true sentences without corresponding entities and relationships in the world for those sentences to *refer* to[106], even though no particular entities or relationships are entailed by truth conditional semantics as a whole. Because truth relations are abstractions, no particular events or things are required by them. However, truth relations cannot be tested in the abstract; for that we need particular sentences, and particular sentences often do entail corresponding real objects and relations. Thus, Davidson's semantics reinforces the commonsensical idea that sentences represent actual or possible relations of things in the world, or in *some* "possible" world. It is clear, then, that Rorty overreaches when he attempts to link Davidson's semantics to his own campaign to separate truth relations from reality. In the abstract, the arguments seem abstruse, but the issue really isn't that difficult. It comes down to this: If the statement, *I have a loaf of bread,* and the actual fact *of having a loaf*

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[106] In another essay, *Reality Without Reference* (1984) Davidson argues that reference "plays no role in explaining the relation between language and reality" (p. 225). This would seem to be a direct contradiction of his statements above. However, it may not be if "reference" is construed in a technical way, that is as an exhaustive and exclusive relation between words and objects. Following Quine, Davidson denies that such exhaustive/exclusive relations could exist, and he therefore desires to rid truth conditional semantics of the troublesome concept "reference." Crucially, however, he applies this stricture only to *words* not *sentences*. Moreover, he wishes to retain "ontological implications of the usual sort" (ibid), which I take to mean that he wishes to retain the idea that sentences do refer to things in the world as he insists above. In these arguments we see some foreshadowing of the later Davidsonian crisis about the inadequacy of language, as expressed in his essay "A Nice Derangement of Epitaphs," the same essay Rorty draws from in his arguments for the "contingency of language."

of bread seem equivalent to you, then Rorty's arguments will also be compelling, and you can save a great deal on groceries too. On the other hand, if you prefer eating to talking about it, at least a few times a day, then you'll want to maintain a distinction between sentences and their referents. Ultimately, what's at stake here is whether or not humans have the capacity to reliably represent reality with language, as the cognitivist/rationalists claim, or whether, per Rorty and the antifoundationalists, language and thought are one and the same, and only arbitrarily related to reality.

Reconciling the Two Davidsons

A superficial inspection of some of the arguments made in the first Davidson essay quoted in this chapter (by the later Davidson) compared with these excerpts from earlier essays (by the early Davidson), could lead one to believe that Davidson had completely reversed himself on the issue of truth. However, recall that the later Davidson unequivocally endorsed his early position that "a theory of truth conditions gives an adequate account of what is needed for understanding the literal meaning of sentences" (1990A, p. 313), and he supported that statement with examples of how speakers use truth conditions in conversation. All of this is consistent with the correspondence view outlined above. Yet, we know that he now rejects correspondence theories. How can these positions be reconciled? I see one possibility, though I don't insist that Davidson would agree with me. Here, is a typical statement Davidson makes regarding his rejection of correspondence:

> I have myself argued in the past that theories of the sort Tarski showed how to produce were correspondence theories of a sort. I said this on the ground that there is no way to give such a theory without employing a concept like reference or satisfaction which relates the expressions to objects in the world. It now seems to me to have been a mistake to call such theories correspondence theories.

> Here is the reason I think it is a mistake. The usual complaint about correspondence theories is that it makes no sense to suggest that it is somehow possible to compare one's words or beliefs with the world, since the attempt must always end up simply with the acquisition of more beliefs...Rorty has repeatedly insisted, claiming sympathy with Dewey, that a correspondence view of truth makes the concept of truth useless. I have made much the same argument myself. This complaint against correspondence theories is not sound. One reason it is not sound is that it depends on assuming that some form of epistemic theory is correct; therefore, it would be a legitimate complaint only if truth were an epistemic concept. If this were the only reason for rejecting correspondence theories, the realist could simply reply that his position is untouched; he always maintained that truth was independent of our beliefs or our ability to learn the truth. The real objection to correspondence theories is simpler; it is that there is nothing interesting or instructive to which true sentences might correspond.
>
> (Davidson, 1990A, p. 303)

From here Davidson goes back into the technical argument we've already seen that "if true sentences correspond to anything at all it must be the universe as a whole." But this result can be avoided. The problem Davidson has is that he either conceives of reality in a transcendent sense, which then puts the correspondent entities beyond our reach, in fact beyond our ken, and thus renders them useless; or he reverts to the technical world of logic, which tells him that all true sentences have the same extensions. The cure for this is to naturalize correspondence under a Kantian[107] framework. We can then say, along the same lines we used to neutralize Raving Platonism in science, that statements humans make correspond to reality but only at the K-objective level. Thus, while it may be true that the universe "really" has ten dimensions—or a hundred—and that therefore, our descriptions of any part of it are inaccurate because woefully incomplete, yet in K-objective terms the correspondence is real. Importantly, recall that, per Kant, we do not

believe that our perceptions are *illusions,* just that they are given in terms of our sensory apparatus. Thus, assuming that the human apparatus and the universe are stable, our perceptions, though subjective, are nonetheless objectively related to—or *correspondent to*-- reality "as it really is." Since, as I've argued at length throughout this dissertation, such assumptions are minimal—no more substantial than their skeptical counterparts—there is no reason to reject this solution, and many good reasons to accept it.

Naturalizing correspondence theory in this way gives us all the benefits Davidson (as well as Grice, and many others) insists we need for a theory of meaning while avoiding the technical trap, as well as the transcendence objection that supports Rorty's allegations of Raving Platonism. Moreover, naturalized correspondence fits much better with an I-language perspective. Under an I-language perspective, linguistic intuitions are treated as primary data, which reflect "elements of the mind" (Chomsky, 1986). Since, as Davidson shows with the coffee example, (non-technical) correspondence theories of truth are ubiquitous among speakers, and very robust, ostensibly because they are derived from universal human rationality, correspondence should be central in any theory of meaning.

Naturalism versus Realism in Theories of Truth

The question now arises, whether "truth" is to be understood transcendentally, i.e., as a "real" property of the world, or only as a natural human function. Among generative linguists, all of whom assume I-language, both positions are represented. Jackendoff takes the first position arguing that

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[107] Walker says that a "plausible reading of Kant ascribes...a coherence theory to him" (1997, p.311). I think this is mistaken, however, for the reasons given above.

> ...truth as a part of objective reality is itself suspect. Consider that something is a *sentence* only if it has syntactic structure, a mentally imposed organization. This means that the entities that are to be characterized as true by Tarskian truth-conditions exist only by virtue of our construal of the world. Now if "true" is a predicate over entities of the world as construed, and if the conditions on truth also involve such entities, it is odd to assume that truth somehow at the same time has an objective status independent of mind. Put differently, if both the "symbols" and the "world of non-symbols" are constrained by the nature of the mind and its construal of reality, it is hard to see the relation between them as not being similarly constrained. Truth too must be regarded as a characteristic of the world as construed.
>
> (1987, p. 131)

This is an astonishing argument for a rationalist. Like Churchland and Sejnowski, Jackendoff apparently doesn't see here that if he is right, then his own argument is reduced to nothing more than a "mentally imposed construal." As I've argued at length, *a-priori* there is no more reason to believe this than to reject it. But if you believe it, then I cannot see the value of argument or science. On the other hand, it is completely reasonable to say that, somehow, humans developed a capacity for reason, and that reason is Really veridical. Reason of course is supported entirely by correspondence notions of truth. It is only because we are able to think, "if X, then Y," the universal hypothetical, which contains reference to truth, that we are able to reason.

Jackendoff also seems to have picked up Rorty's notion of truth (though not from Rorty I'm sure) as an *entity* rather than a function. The following continues directly from the above quote:

> Suppose one tried to avoid this argument by removing from truth any notion of mentality: truth is to be a property of the world as it is in reality, independent of how it is perceived or construed. Such a view would have a number of unpleasant consequences. First, sentences like (7.8a-g) could not have truth values, since they refer to entities of dubious real world status.

(7.8) a. I am/Fred is now vividly imagining a purple cow.

b. In my/Louise's dream, a unicorn bit Harry
c. I/Harry saw a menacing figure in the road, but it out just to be the shadow of a tree.
d. When you said "banana" just now, you said it with a "p" instead of a "b"
e. The subject of the previous sentence is "you."
f. The rhythm of the word "banana" is "da-DA-da."
g. David's semantic theory is untestable.

(ibid, p.132; data inserted from p. 130)

But this conclusion is only warranted if we do remove " from truth any notion of mentality," and we don't need to do that. Rather, we insist that truth is a *relationship* between propositions and the world, and that mental states are part of the world. Thus, following Fodor, we should have no problem holding that Fred's imagination, Louse's dream, Harry's perceptions, "your" statements and phonology, the subjects of sentences, rhythms of words, and David's semantic theories, *all* potentially have real status in the world as mental states supervening on brain states, and further, that propositions like (7.8 a-g) can be true or false depending on whether such mind/brain states do/did or do/did not exist. Jackendoff is a thoroughgoing mentalist, so, again, it is astonishing that he would suddenly reject mental entities as real. Unless you want to embrace dualism, mind/brain states must be accorded the same real status as trees, rocks, and chimpanzees. Perhaps Jackendoff is bothered by the fact that only humans can perceive these brain states in the ways described, but there is no reason to be bothered by that either. Human perceptions *of* brain states (which are actually perceived as psychological dispositions of others) are themselves supervenient on brain states, and so also real. Certainly we can easily imagine any of the statements in (7.8) being made falsely, in which case they would be just as false as statements about "real" solid objects can be false.

In any case, Jackendoff has other objections. Again, the quote below follows directly from the above:

> More damaging, truth would no longer be a potential property of *sentences*—it would pertain only to the world. Tarskian statements of the form
>
> (7.7) Sentence S (of language L) is true if and only if
>
> $C_1,\ldots C_n$ [where $C_1,\ldots C_n$ are truth conditions]
>
> would be part of a different theory, one of how sentences come to correspond to the Truth...In particular, one would still need a psychological theory of how humans grasp The Truth...
>
> One might attempt to meet this criticism by adopting the "promiscuous realism" of Barwise and Perry (1985), which admits into Reality anything one may happen to need, including sentences, states of mind, mental images, theories, and so forth. Even so, one must still ask what constrains and structures this menagerie, such that humans can mentally represent and organize it. And if states of mind are included, then what constrains and structures *their* possibilities.
>
> (ibid, p. 132)

Since "promiscuous" realism is the answer to Jackendoff's first objection, we need only defend the that notion, and actually I've already argued for it above. The only change to Jackendoff's presentation of it that I would make is to say that we don't posit what we *need* but only what we have evidence for. Again, I'm astonished that a mentalist would complain about the "promiscuousness" of positing real mental structures. As for what "constrains and structures the menagerie..." well, that is what we'd like to know! But clearly, something does, and as I've argued before, not knowing how a complex mechanism works is no argument against its existence.

Lurking behind these objections is the familiar confusion between justification and truth, and, I suspect, discomfort at the thought of allowing pure relationships, which arguably are *not* physical entities to exist. The confusion is easily addressed. As

Davidson noted, we just insist that truth is not an epistemic concept. We need to do this, for otherwise we will have no way to distinguish between justified and true beliefs, though clearly there is often a difference. I've already argued for this at length, so I'll leave this point here. The reality of pure relationships—"ghosts"—is tougher. There are various degrees of reality one can accept. A compromise Natural/Real solution is to say that the ghosts are real but only when instantiated somewhere. On this account the Pythagorean theorem is real, but only inside heads. Thus, if Rorty took over the schools and everyone forgot the theorem, it wouldn't exist even though it was stated in books, though if some intrepid student broke free of the antifoundationalist conversation long enough to read and understand the theorem it would instantly spring back into the natural world. Likewise on this account for the hieroglyphs in the Mayan jungles. For all those centuries after the end of the Mayan empire, Naturalists will say there were no ancient Mayan propositions, only scribbles. But, when the vines were cleared and the code cracked, the ancient propositions became naturally real again because they found harbor in some anthropologist's or linguist's head.

The Realist position, on the other hand, is to say that heads or no heads, at least some things are Real. Classically, mathematics has always had the strongest hold on Reality, and even staunch naturalists like Chomsky allow that "[i]n the case of arithmetic there is a certain initial plausibility to a Platonistic view insofar as the truths of arithmetic are what they are independent of any facts in individual psychology" (1986, p. 33). But surely this is a ghostly proposition, for *where* could these truths exist independently of individual mind/brains? If we want to maintain a materialist/naturalist stance, perhaps we could say that they exist only *potentially*, i.e., that the relationships are always there

but not as truths unless recognized as such. Full blown Realists, however, are not afraid of ghosts. Katz, for example, is perfectly comfortable to allow, not only arithmetic, but also logic, and even semantics to haunt the universe. It's not entirely clear to me how far minimal foundationalism can go in this direction. Minimal foundationalism necessarily posits the ghost Reason, which ghost is justified on the grounds that the (antifoundationalist) alternative is just another ghost, the ghost NoGhosts[108]. Moreover, as we've seen NoGhosts is a very weak ghost that cannot keep Reason at bay. Therefore, following Kant, I have argued—and I hope convincingly demonstrated—that since it seems to be the human fate to be haunted by *some* ghost, Reason is the best choice. I have also argued in this chapter that truth is part of reason, and therefore truth must be regarded as Real. The question that remains, is how many other Real entities must be admitted. In keeping with minimal foundationalism, we should admit only those entities that cannot be denied, though we may speculate freely about other entities. In the last chapter, we will see that these questions loom large in theories of meaning.

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[108] If you've followed this discussion, then you know that NoGhosts is a ghost because his doctrine cannot be derived empirically

VIII: TALKING OUR WAY OUT OF THE PRIMORDIAL MUCK

Arguments over the nature of mind must eventually deal with its etiology. On this point, there is wide agreement in the academic community that evolution is to blame. However, it's not clear how this perspective constrains theories of mind. Rorty, in particular, argues that Darwinian evolution entails an a-representational, pragmatic view, which would no doubt surprise the thousands of rationally minded scientists who see no conflict between evolution and representational theories of mind.

In what follows we will return to Rorty's essay *The Contingency of Language* to examine his argument. In the process, we will also consider the nature of metaphor, since Rorty considers metaphor to be the driving mechanism in the (Darwinian) development of language (R-language, of course). Finally, we will return to Davidson for his take on metaphor, partly because Rorty cites Davidson as his mentor on metaphor, and partly because it will turn out that Davidson's position on metaphor has interesting consequences for the theory of meaning I am attempting to build.

Rortian Darwinism

The passage that follows occurs some ten pages after the passages on truth, quoted in the last chapter. In the spirit of *The Mirror,* Rorty has been building a case against both mind and language as distinct entities, and, after quoting extensively from Davidson's essay *A Nice Derangement of Epitaphs* (1986), which essay we will look at in the next chapter, Rorty offers the following summary of his position:

> To see the history of language, and thus of the arts, the sciences, and the moral sense, as the history of metaphor is to drop the picture of the human mind, or human languages, becoming better and better suited to the purposes for which God or Nature designed them, for example, able to express more and more meanings or to represent more and more facts. The idea that language has a purpose goes

> once the idea of language as medium goes. A culture that renounced both of these ideas would be the triumph of those tendencies in modern thought that began two hundred years ago, the tendencies common to German idealism, Romantic poetry, and utopian politics.
>
> (Rorty, 1998A, p. 78)

Much of this is familiar from *The Mirror*. Rorty is employing a variant of the Raving Platonism argument here, insisting that either we sign up with Jerry Falwell, or we renounce representationalism. A particularly devious phrase is "the idea that language has a purpose," since, as Rorty surely knows, the phrase is ambiguous, meaning either a *creator's* purpose, or the *language user's* purpose. But of course the latter does not entail the former, so the argument against teleology is not an argument against representationalism, though it masquerades as one.

Having "established" this tough-minded non-teleological orientation, Rorty then waxes "Darwinian." The following passage picks up directly from the block above:

> A non-teleological view of intellectual history, including the history of science, does for the theory of culture what the Mendellian, mechanistic account of natural selection did for evolutionary theory. Mendel let us see mind as something that just happened rather than as something that is the point of the whole process. Davidson lets us think of the history of language, thus of culture, as Darwin taught us to think of the history of a coral reef. Old metaphors are constantly dying off into literalness, and then serving as a platform and foil for new metaphors. This analogy lets us think of "our language"—that is of the science and culture of twentieth century Europe—as something that took shape as a result of a great number of sheer contingencies. Our language and our culture are as much a contingency, as much a result of thousands of small mutations finding niches (and millions of others finding no niches), as are the orchids and the anthropoids.
>
> We must follow Mary Hesse in thinking of scientific revolutions as "metaphoric redescriptions" of nature rather than insights into the intrinsic nature of nature. Further, we must resist the temptation to think that the redescriptions of reality by contemporary physical or biological science are somehow closer to the "the things themselves," less mind-dependent than the redescriptions of history offered by contemporary cultural criticism.

> We need to see the constellations of causal forces that produced talk of the Big Bang as of a piece with the causal forces that produced talk of "secularization" or of "late capitalization." These various constellations are the random factors that have made some things subjects of conversation for us and others not, have made some projects and not others possible and important.
>
> (1998, p. 78)

Rorty is arguing here that intellectual history (by which I assume he means "development"; see comments below) is a Darwinian evolutionary process, but, in the same paragraph, he asserts that science itself is part of that process, and in the following paragraph he exhorts us to think of "scientific revolutions as '"metaphoric redescriptions"' of nature rather than insights into the intrinsic nature of nature." Now, he cannot have it both ways; he cannot allude to Darwinian evolution as a scientific fact about the world, and at the same time claim that there are no scientific facts. Darwin's theory of evolution certainly qualifies as a "scientific revolution," but, then, on Rorty's own account, it is just a "metaphoric redescription" not a real insight into nature. But if it's not a real insight, then Rorty's analogy has no force—it's just a comparison between metaphoric redescriptions, neither of which say anything about the real world, that is, neither of which say anything at all.

And even if we forgive these absurdities—i.e., allow Rorty to have science and trash it too—there are intractable problems with his argument. Rorty attempts to draw an analogy between Darwinian evolution and intellectual history, but in doing so, he strains both terms of comparison, "evolution" and "intellectual history," as well as the principles of analogy. Before we get started on this major point, however, let's clarify a minor one. By comparing "intellectual history" with "evolution," Rorty implicitly declares the two terms to be compatible as objects of comparison, but they are not. Evolution is a *process* while intellectual history is the *record of a process or processes*.

Note that we don't say that the *history of evolution* works according to the principles of natural selection, but, rather, that *evolution* does. Therefore, in the interest of focussing on Rorty's major claim, suppose we take the small liberty of replacing the incompatible term "intellectual history" with the compatible term "intellectual activity over time," or for short, "intellectual development." We shall see a bit later that there may be more to Rorty's terminological confusion than carelessness, but for our purposes right now, I think the switch makes Rorty's comparison clear without substantively changing his major claims. So let's proceed.

Analogy works by comparing systems or processes that are essentially unlike yet similar in some crucial respect. Thus, for example, when Descartes compares Archimedes' fulcrum[109] to a fixed point from which to reason, he draws our attention to two very different systems, each of which is composed of two "objects" in a systematic relationship, such that the relationships within one system, A:B, are in some way the same as those in the other system, C:D. The analogy splits the systems into two sets of compared objects, A to C, and B to D—in Descartes' case a fulcrum to a known point, and a lever to reason. Descartes' analogy is powerful because of its symmetry and surprising juxtaposition of physical and mental processes, which, though different in nearly every aspect, share to a remarkable degree this one aspect of dependence on fixed points. But Rorty's "analogy" collapses upon inspection. Evolution is classically understood to be a *mindless*, *goal-less* process, which, nonetheless, achieves success by

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[109] "Archimedes, that he might displace the whole earth, required only that there might be some one point, fixed and immovable, to serve in leverage; so likewise I shall be entitled to entertain high hopes if I am fortunate enough to find some one thing that is certain and indubitable" (Descartes, 1637/1960, p.33).

dint of natural selection and beneficial mutations. Intellectual development[110], on the other hand, is, by definition, mindful, purposeful, human activity. Note, by the way, that this was Darwin's great insight, and has always been the sticking point for people who reject his theory. The revolutionary idea behind evolution is that you can get ingenious organizational complexity without a genius, in fact without a mind of any kind, just by the workings of natural selection over vast periods of time. The whole reason it is a revolutionary idea is that it defies what people "know" about the world, that is, that highly organized systems require a conscious organizer. In these basic aspects, then, we can say that evolution and intellectual development are nearly perfect opposites. This in itself is not a problem, since, recall, the essential trick of analogy, like metaphor, is the judicious comparison of essentially dissimilar things that, nonetheless, are similar in some crucial aspect; however, if it is this dissimilarity—mindlessness versus mindfulness—that supports Rorty's analogy, he cannot later deny that dissimilarity. Alternatively, if he does wish to deny that dissimilarity, there must be some *other* dissimilarity to support the analogy.

Now, as to process, the usual focus of similarity in analogy, it is true that mindless evolution and intellectual development both achieve "success" in the sense that both seem to lead to improved lives, or at least, improved chances for survival and procreation, for successive generations, and both seem to do so incrementally, at least most of the time. So then, if analogy is possible between evolution and intellectual development, it seems it should focus on these shared aspects, success and incrementalism, or perhaps more elegantly, success through incrementalism. In fact,

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[110] Importantly in Rorty's essay the term "intellectual history" refers to recorded history, a period far too

such analogies are frequently drawn; we hear, for example, of the "evolution of the automobile," which analogy is intended to draw our attention to the remarkable cumulative effects of thousands of minor improvements on a basic design over time[111]. But it is here that Rorty's "analogy" breaks down, for his point is not that the essentially unlike processes of intellectual development and evolution achieve similar success through incrementalism, but, rather, that intellectual development, including the progress of science, *is* nothing more than mindless, purposeless evolution: "this analogy lets us think of 'our language'—that is of the science and culture of the twentieth century Europe—as something that took shape as a result of a great number of sheer contingencies." It is as if Descartes should intend by his analogy that we understand reason to be an essentially mechanical process, rather than that reason and leverage both require fixed points. But if your point is that A is like B in nearly all its aspects, then analogy is the wrong device to express your position. We don't draw "analogies" between dogs and wolves, lions and tigers, harpsichords and pianos; we simply compare them overtly ("x is like y except..."). Likewise, if Rorty wants to claim that intellectual development is actually mindless, he should just say so, though of course, he would have to dispense with the term "intellect" since, there can be no mindless intellects; In short, the "evolution" of intellectual pursuits, such as science, is either an oxymoron or a metaphor. But if it's a metaphor it is only coherent in the toothless sense of "incrementalism," outlined above, in which case it is neither a novel nor an

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short for Darwinian evolution to have had any effect at all.

[111] Unfortunately, this analogy is often applied in reverse, much to the consternation of evolutionary theorists, such that people impose purposefulness on evolution.

antifoundationalist claim. On the other hand, if it is an oxymoron, well, that needs no further comment.

A possible objection to this line of criticism is that Rorty is not extending his analogy to individuals, but, rather, to larger social constituents—"cultures"—which, it might be argued, *are* mindless, susceptible to the vagaries of "sheer contingencies." My first answer to this is that though Rorty does frequently invoke larger social constituents, he also regularly conflates individuals and "culture," and that, therefore, claims about culture are for him tantamount to claims about individuals. Moreover, it should be clear that this is the only position he could take, given his strong stance against "intrinsic nature" and his enthusiastic endorsement of such antifoundationalist, tail-chasing maxims as "*languages* are made rather than found" and "the human self is created by the use of vocabulary" (1998, p.69)[112]. But we need not merely infer his position, for he is quite direct about it:

> I can develop the contrast between the idea that the history of culture has a *telos*—such as the discovery of truth, or the emancipation of humanity—and the Nietzchean and Davidsonian picture which I am sketching by noting that it is compatible with a bleakly mechanical description of the relation between human beings and the rest of the universe. For genuine novelty can, after all, occur in a world of blind, contingent, mechanical forces. Think of novelty as the sort of thing that happens when, for example, a cosmic ray scrambles the atoms in a DNA molecule, thus sending things off in the direction of the orchids or the anthropoids. The orchids, when their time came, were no less novel or marvelous for the sheer contingency of this necessary condition of their existence. Analogously, for all we know, or should care, Aristotle's metaphorical use of *ousia*, Saint Paul's metaphorical use of *agape*, and Newton's metaphorical use of *gravitas* were the results of cosmic rays scrambling the fine structure of some crucial neurons in their respective brains. Or, more plausibly, they were the result of

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[112] These sentences occur a few lines apart in the original passage, but there is nothing in that passage that ameliorates the circularity in them.

> some odd episodes in infancy—some obsessional kinks left in these brains by idiosyncratic traumata.
>
> (ibid, p. 79)

Cosmic rays? Idiosyncratic traumata? Perhaps Chomsky's chemistry angels would also be in order here. Incredibly, however, I don't think Rorty is joking—he is seriously proposing that *individual* intellectual development as well as intellectual development at the cultural level is just the result of "blind contingent forces" which are qualitatively superior in their power and effects on humans, to deliberate intellectual work. Thus, according to his "Nietchean and Davidsonian picture" the fact, for example, that Newton devoted his life to physics, and spent most of his time reading, discussing, and researching science, and the fact that he "stood on the shoulders of giants" who preceded him, such as Galileo, Tychoe Brae, and Keppler—scientists who provided much of the data and beginning theory that led him to his own conclusions—has no more to do with his discovery of the principles of gravity (i.e., his "metaphorical use of *gravitus*") than some errant cosmic ray. But, taking Chomsky's lead, shouldn't we at least ask why these blind contingent cosmic rays only seem to affect humans in this way, and moreover, nearly always only humans who just "happen" to be engaged in the appropriate intellectual work? Why don't rocks, birds and chimpanzees write treatises on physics? The tiresome "answer" to this question is that rocks, birds and chimpanzees, weren't struck with the same cosmic rays, and thus they remained as they were, i.e., didn't turn into humans first, and then do science later. But that's the whole point. The obvious fact is that being human is itself a powerful "contingency" for intellectual achievement; in fact, and again quite obviously, it is overwhelmingly the most important

"contingency," overshadowing other factors such as cosmic rays to such an extent as to make these other factors generally unworthy of serious consideration.

Returning to our original, reasonable objection, i.e, whether evolutionary mechanisms could operate at the cultural level even if they don't at the individual level, note, again, that Rorty consistently conflates individual, cultural, and historical levels, a fact that is responsible for a lot of the confusion in his writing. Thus, for example, he "contrasts" "the ideal that the *history of culture* has a *telos,* with the '"bleak universe picture."' But this is an inane contrast. Does anyone suppose that the *history* of anything has or could have a *telos*? What would that mean? I am quite certain, for example, that *I* have been purposeful in my life, but I can't make any sense out of the notion that *my history* has!

But there is far more behind this "contrast" than idiomatic infelicity. This is another example of Rorty charging rationalists with "Raving Platonism," and it's totally unwarranted. Rorty continually tries to force the idea that to assume that individuals are conscious, intellectually alive, and purposeful somehow entails the "teleological" idea that the universe itself has these qualities, or is controlled by someone who does. Despite his fondness for Darwinism, Rorty seems to have missed Darwin's main point: extraordinary complexity and organization can evolve from a "bleak universe." So, it is perfectly in line with Darwin that people should have minds with innate abilities, including reason, a capacity to learn and use languages in ways that correspond with reality, consciousness, and the curiosity and will to use these capacities for discovery.

None of this is meant to deny the workings of serendipity, or blind contingent forces at the cultural level. The interaction of billions of complex mechanisms of *any*

kind would be bound to create "blind contingent" effects, and if the mechanisms in question are conscious, purposeful individuals, then, by all means we should expect a lot of unlooked for structure as well as serendipitous opportunities which are later exploited purposefully. Again, though, it should be obvious that the effects of blind contingent forces on conscious mechanisms will be—and are—vastly greater than the effects of the same forces on unconscious mechanisms, and still less on inanimate mechanisms. Thus, as far as we know, in recorded history there have been no changes at all at the systemic level for such organized systems as the solar system or the weather, and very few major changes in the behavior of animals, or plants in recorded history, though during that same time, humans have reorganized themselves in thousands of new ways. A natural hierarchy of change is suggested by this: inanimate mechanisms interact slowly and predictably according to fixed laws; animate, unconscious mechanisms react with only slightly more variability; while conscious mechanisms react swiftly, creatively, and in unpredictable ways. All of this is so obvious it shouldn't need elaboration. Moreover, it should cast great doubt on the comparison of Darwinian evolution to intellectual development.

Representations at the Cultural Level

The point is, even though "blind contingent forces" operate within cultures, just as they do at the macro-level of evolution, the effects are vastly different because the affected mechanisms are different. Thus, culture should be understood as the result of an interaction between blind contingent forces—many of which didn't begin as such—and conscious individuals who manipulate information in specific ways as determined by the contents of their minds. Since what makes culture interestingly different than other

systems—e.g., the solar system, the weather, animal societies—is the human mind, it again seems obvious that the mind should be the focus of cultural study.

In his book, *Explaining Culture*, Dan Sperber makes this argument, and, in so doing, invokes the familiar term "representation" as a central concept that unifies his theory of culture:

> Let us start as simple as possible. Cultural things are, in part, made of bodily movements of individuals and of environmental changes resulting from these movements. For instance, people are beating drums, or erecting a building, or slaughtering an animal. The material character of these phenomena is so far unproblematic. But we must go further. Is it a musical exercise, a drummed message, or a ritual? Is it a house, a shop, or a temple? Is it butchery or sacrifice? In order to answer one must, one way or another, take into account the representations involved in these behaviors. Whatever one's theoretical or methodological framework, representations play an essential role in defining cultural phenomena.
>
> (Sperber, 1996, p. 24)

Sperber's point is that when words are actually spoken or deeds done, the actions are performed by individuals, and therefore, we have to understand how individuals represent to themselves what they are doing. Even casual analysis of such "cultural actions" as those above easily reveals a great range in the meanings and importance people ascribe to them, which problematizes simplistic conceptions of cultural values or "discourse community" interpretations. Cultures are defined by the representations that are shared, and this is perfectly reasonable for theoretical purposes; but the term "cultural representation" refers to an abstraction that doesn't actually exist anywhere. Ultimately, representations only exist in individuals' heads.

As far as the evolution question goes, it is again obvious that people get ideas from other people —and culture may be regarded as the cumulative shared ideas of a group (Sperber, 1996). Thus, cosmic rays or not, Islamic ideas only flourish where lots

of Muslims live and recede where they are sparsely represented. This is practically a tautology, of course, but not if you regard culture as somehow separate from individuals. The point is, if you're going to look at changes in a culture, or changes in some aspect of it, like its intellectual development, you're going to have to look to individuals as the precipitating causes of change, not cosmic rays. And, if you do that, you're going to run into *purpose*; moreover, if you take a look at history, it seems that the stated purposes of thousands of individuals have very often come to pass, though usually at the expense of other individuals and their purposes. Thus, millions of Europeans *decided* to immigrate to the Americas and take all the land away from the indigenous people. Regarding science, it is also very clear that the stated purpose of thousands of scientists over the last four hundred years or so, i.e., the purpose to "read the book of nature and discover its secrets[113]" has been followed assiduously and with great success.

This raises another major flaw in Rorty's analogy. Hidden by his comparison is the vast differences in time between the compared processes. Evolution is literally glacially slow, and it takes all sorts of odd turns, most of which we know nothing about. In contrast, consider any strand of intellectual development, say our previous example, the "evolution" of the automobile. In a mere hundred years or so, automobiles "evolved" from buggies to the technical marvels we have today, and they did so with very few fruitless junctures. Now compare this to the evolution of any known organism. By telescoping the organism's (hypothesized) progress over millions of years, into a compact chart or book, we can create an illusion of similarity to the progress of machinery, but that's really all it is. Intellectual progress over time within a cultures is

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[113] I'm not implying that nature really is a text, just that people purposely set out to understand how nature

qualitatively different from blind evolutionary processes, both in its mechanisms and in its effects, though there are intriguing superficial similarities. To understand either case, we need to isolate the driving mechanism. Thus, once Darwin identified natural selection as the mechanism, evolutionary theory progressed rapidly by matching the mechanism to archeological data. In our case, we seek to understand language, so we ought to look within human minds for the answers, not to blind contingencies, even if somewhere down the line blind contingencies factor in.

Hesse, Metaphors, and Science

Before we leave Rorty let's take a brief look at the "must be followed" thinking of Mary Hesse, i.e., the claim that "scientific revolutions [should be thought of] as 'metaphoric redescriptions' of nature rather than insights into the intrinsic nature of nature" (ibid, 78). We've already dealt with Rorty's categorical denial of humans' capacity for genuine discovery, so I'll ignore that claim and instead focus briefly on the comments about metaphor and science. First, I'm curious as to how a scientific revolution could be metaphorical (although of course the *term* "scientific revolution" is a metaphor; but this is irrelevant). If the term "scientific revolution" is meant in its broad, Kuhnian sense, i.e., as the collective process through which thousands of individuals gradually change their thinking about some aspect of science, then the statement that "scientific revolutions are metaphors" is completely opaque, no more sensible to me, at least, than saying that WWI is a metaphor. Of course in a novel, either term could be a metaphor, but novels have authors who get to control all the thoughts and actions of the characters and arrange all the events and settings to suit whatever semiotic or artistic

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works.

purposes they have in mind. Unless Hesse is suggesting something similar regarding scientific revolutions, wherein God is the author—which is too wild even for Rorty—there must be another explanation.

There is. In fact, Hesse doesn't say anything about "scientific revolutions" in her article. The term never even appears, so the suggestion dropped by Rorty, that she like him sees "vocabularies" operating semi-autonomously on culture as a whole, is something he created out of thin air. When we look at Hesse's article we find that, she refers only to particular scientific descriptions.

But this too is immediately problematic, for, arguably, metaphoric usage requires *intention* on the part of the author to be metaphorical. This is unequivocally Davidson's position as we shall see, and it is also Max Black's position (though Black and Davidson disagree on many other points), and Black is the author Hesse cites as her source for the theory of metaphor she applies to science—the same article Rorty cites. Black puts it this way:

> When a statement is metaphorical, the sentence used, part only of which consists of a word or words used metaphorically, is not *intended* to be taken literally.
>
> (Black, 1978, p. 185; my italics)

and:

> One may indeed agree with Davidson that awareness of the "ordinary" [literal] meaning" is necessary if the metaphor is to be understood. The question to be considered, then, is...whether the metaphor maker is *attaching* an altered sense to the words he is using in context.
>
> (Black, 1978)

There is broad agreement about this, i.e., that a metaphor must be intended as such to qualify as a metaphor. But there is no evidence that the major scientific claims that have been called "revolutions" were intended metaphorically, at least not in this,

traditional sense of metaphor. Without doubt, for example, Copernicus and all the other astronomers who took up his revolutionary line meant their statements about the earth revolving around the sun to be taken literally. Thus, at best, Hesse is ignoring the principle element of metaphor—the author's intent to be metaphorical.

In her article, Hesse never mentions this element, but she seems to think that scientific claims are almost never meant to be taken literally. She claims, for example, that the description of a gasses as "collections of randomly moving massive particles" is a "metaphor" (Hesse, 1980, p.115). Yet, this description, or descriptions very much like it, can be found in current textbooks on chemistry, and there is no indication that the authors of such texts intend these descriptions non-literally. Per Black, what "word or words" in this description are to be taken metaphorically? It seems that Hesse is using the term "metaphor" in a very loose sense, i.e., in the sense of "tentative" or "partial." Thus, all she is saying is that, over time, descriptions of nature change as scientists' observations and theories change. As in Wittgenstein's choice of the term "language game" for language routine, it seems to me that Hesse has simply chosen an unfortunate term for what she is trying to say. Actually, we already have a perfectly good term—"model"—that suits the sense of her article. In fact, Hesse later uses the terms "model" and "metaphor" interchangeably, which shows that she sees no important difference between them. But there's a world of difference. Models are best approximations, which are meant to directly represent their correlates. Moreover, models can never be too accurate; to the contrary, the closer to the real thing they are the better. Metaphors, on the other hand, rely on essential differences between the compared terms for their impact, and, in fact, fail if the terms are too close. The fact that metaphoric use is universal and

clearly distinct from literal use says something important about how people convey meaning—although just what it says is debated—and to ignore this is misleading.

Hesse blurs the distinction between metaphors and models because she wants to stress the point that metaphoric and literal usage affect each other—a position she calls the "interaction view"—so that it is impossible to draw a sharp line between literal and metaphoric use. This is a variant of the argument that all observation statements are ineluctably "theory laden." But in making this argument for a "metaphoric" use of language in scientific descriptions, Hesse relies on E-language evidence, and that, I would argue, is her error. Because she looks at language data instead of language users' internal rules and intentions, she finds a continuum between the literal and the metaphoric:

> An important general consequence of the interaction view is that it is not possible to make a distinction between literal and metaphoric descriptions merely by asserting that literal use consists in the following of linguistic rules. Intelligible metaphor also implies the existence of rules of metaphoric use, and since in the interaction view literal meanings are shifted by their association with metaphors, it follows that the rules of literal usage and of metaphor, though they are not identical, are nevertheless not independent...The interaction view sees language as dynamic: an expression initially metaphoric may become literal (a 'dead' metaphor), and what is at one time literal may become metaphoric (for example the Homeric 'he breathed forth is life', originally literal, is now a metaphor for death). What is important is not to try to draw a line between the metaphorical and the literal, but rather to trace out the various mechanisms and their interactions.
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> (Hesse, 1980, p.117).

Historically, no doubt, Hesse is correct—language changes over time, in all sorts of ways, including the sort of semantic drifting of terms between literal and metaphoric realms that Hesse describes. But it is a great mistake to conflate diachronic and synchronic perspectives on language. To make the case that scientific redescriptions are

metaphorical, one would have to show that the original speakers/writers of those descriptions meant them metaphorically and Hesse has not provided any evidence of this. In irresponsible hands, this kind of conflation can also be used to remove the mind from intellectual development, as Rorty has done above; but this is an illusion created by ignoring speaker's intentions. As we will see, the ability to infer intentional states, such as the intention to be metaphorical is paramount in theories of meaning, which is why I have placed so much emphasis on this point..

Davidson's Iconoclastic Theory of Metaphor

The traditional view of metaphor outlined above by Black is maintained by nearly everyone except, maybe, Donald Davidson. I say maybe because although Davidson appears to reject the traditional view he also provides a technical way to save it. We have seen this kind of maneuvering with correspondence theory already, and we will see it again when we look at Davidson's theory of meaning. But let's get the metaphor story first.

The traditional account is the same one I outlined for analogy: metaphors directly compare two unlike things that are alike in some crucial aspect. The meaning of the metaphor, then, is the abstracted crucial aspect, plus whatever special impact is created by making the statement in metaphorical terms. Thus, to call someone a pig is to refer to their slovenliness or greediness or whatever pig like quality one might select. But Davidson says this is all wrong. In a gravity defying declaration, Davidson says flatly that

> ...metaphors mean what the words, in their most literal interpretations mean, and nothing more. Since this thesis flies in the face of contemporary views with which I am familiar, much of what I have to say is critical...The central mistake against which I will be inveighing is the idea that a metaphor has, in addition to its literal sense or meaning, another sense or meaning. This idea is common

> to many who have written about metaphor: it is found in the works of literary critics like Richards, Empson, and Winters; philosophers from Aristotle to Max Black; psychologists from Freud and earlier, to Skinner and later; and linguists from Plato to Uriel Weinreich and George Lakoff.
>
> (1980, p. 30)

In making this declaration, in which he pits himself against the entire Western tradition on metaphor, Davidson sets us up for some extraordinary thesis, but the account he gives is largely deflationary, in a Wittgensteinian sense. After acknowledging, in a seeming contradiction to his initial remarks, that he agrees with the above authors about what metaphor *accomplishes*, He says:

> My disagreement is with the explanation of how metaphor works its wonders. To anticipate: I depend on the distinction between what words mean and what the are used to do. I think metaphor belongs exclusively to the domain of use. It is something brought off by the imaginative employment of words and sentences and depends entirely on the ordinary meaning of those words, and hence on the ordinary meanings of the sentences they comprise.
>
> (ibid, p. 31)

So, then, it appears that the only difference between Davidson and everyone else is the way he carves up the communicative space. For him, there is a narrow technical domain of "meaning," which, not surprisingly, considering his background, is delimited by literal meanings of words and the truth conditions of sentences; and then there is a nebulous world of "use," which determines intentions and which also guides us toward artistic and affective dimensions of language, including but not limited to those of metaphor. But one could argue that, like his position on correspondence, this is just a technical difference. In fact I think that's pretty much true. But, before I make the argument, I would note that others have taken Davidson's declarations at face value. In particular, Black, the target of much of Davidson's criticism in *What Metaphor's Mean* (the article

quoted above), argues against Davidson's contentions point by point in his *Reply to Donald Davidson* (Black, 1978).

This is not to say that Black is unaware of the technical escape; in fact he cites the most telling remark of this kind Davidson makes, viz, that "What makes the difference between a lie and a metaphor is not a difference in the words used or what they mean (in any strict sense of meaning) but in how the words are used" (Davidson, 1978, p. 259). Yet, after correctly observing that "much of this vigorous polemic is beside the point," since no theorist claims that metaphors change the meanings of words in Davidson's "strict" sense, Black returns to the quixotic project of showing that metaphors *do* have secondary meanings. I will take a different tack.

Metaphors and Meaning

It seems to me that the real issue here is not what metaphors mean (in the *not* strict sense), but, rather, the different implications various accounts of metaphor have for a theory of meaning. Davidson makes two stipulations in his paper, without defending them, that are crucial. First, there is this idea of "strict" meanings, by which we can assume he means denotative meanings, which are ostensibly built up from the word level, and then determined by truth conditions. Now, what Black should see (and does see, but for some reason doesn't stress) is that if you define meaning that way, then, by definition metaphors only have literal meanings. It is bootless to present evidence of other meanings, as Black does, since, on Davidson's strict definition, metaphors can't have other meanings, and that's it. The other stipulation is that intentions are determined by use, as we've seen from the quotes above.

Together these stipulations create a strange picture of language in which words and sentences are treated as E-language tokens, whose "strict" meanings are entirely predictable, and then yoked to this other vast, undefined space called "use" that can apparently do anything, though never according to any rules:

> What metaphor adds to the ordinary is an achievement that uses no semantic resources beyond the resources on which the ordinary depends. There are no instructions for devising metaphors; there is no manual for determining what a metaphor 'means' or 'says'; there is no test for metaphor that does not call for taste.
>
> (ibid, p. 245)

And:

> No theory of metaphorical meaning or metaphorical truth can help explain how metaphor works...What distinguishes metaphor is not meaning but use—in this it is like assertion, hinting, lying, promising, criticizing.
>
> (ibid, p. 251).

Now, some might be tempted to write this whole issue off as a squabble over terminology. Davidson wants "meaning" to be restricted to the denotative level; fine, let him have that point and let's just get another word for "meaning" in the everyday sense. But there's a lot more at stake here than that. First of all, we should vigorously oppose Davidson's facile assignment of meanings to E-language, and intentions to use. Even if we were disposed to agree that intentions *are* only *revealed* by use, it is, nonetheless, inane to insist that there is some natural class called "use" that *determines* intentions. Surely, intentions have to be in speakers' heads *before* use occurs. To insist otherwise is to deny the entire psycholinguistic realm. We've been down this behavioristic road before, and we know it leads nowhere, so I won't belabor the point. But it is worthwhile to examine the sorts of maneuvers Davidson is forced into to support this doctrine.

Consider, for example, his otherwise intelligent analysis of how truth operates in metaphors[114]:

> Patent falsity is the usual case with metaphor, but on occasion patent truth will do as well. 'Business is business' is a too obvious in its literal meaning to be taken as having been uttered to convey information so we look for another use.
>
> (ibid, p. 258)

The last word, "use," can easily be exchanged for "meaning" here, and in fact, it seems pretty clear that that's what it means (in the ordinary sense of "meaning"). Moreover, it is only because he is hanging on to the use doctrine that Davidson is forced into such awkward phrasing. If not for that he could simply have said "'Business is business' is too obvious in its literal meaning *so we look for another meaning.*" Davidson's insistence on the use doctrine also forces him into the very questionable and completely undefended assertion that only literal uses can convey information ("*another* use"). But it's easy to concoct situations in which the sentence could be used non-literally and yet convey information. In fact, when Coolidge famously said "The business of America is business"—a sentence that is roughly equivalent to Davidson's example—he *did* convey information: he conveyed his opinion that the first priority of government was to create a thriving business climate. Now Davidson might not agree with that interpretation, but it's a standard interpretation that thousands of native speakers get, and its clearly informative, so at least this part of the use doctrine is falsified. Moreover, it is easy to multiply such examples, and this evidence, combined with the absurdity of maintaining

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[114] Actually, I'm not sure the 'Business is business' example qualifies as metaphor—I rather think not. But it's not important for the point I want to make.

that use could somehow determine meaning rather than the other way around, should be enough to dispense with this doctrine.

Of course, Davidson published this article in 1978, and as we've seen, he is not consistent over the years. In fact, in the 1990 article, quoted in the previous chapter, he says a number of things that don't square at all with these assertions about metaphor. For example, in the later article, he says:

> An utterance has certain truth conditions only if the speaker intends it to be interpreted as having those truth conditions...A malapropism or slip of the tongue, if it means anything, means what its promulgator intends it to mean. There are those who are pleased to hold that the meanings of words are magically independent of the speaker's intentions; for example, that they depend on how the majority, or the best-informed, or the best-born, or the community in which the speaker lives speak, or perhaps how they would speak if they took enough care...For the purpose of the present enterprise, that of understanding truth and meaning, we should I think, stick as closely as possible to what is made directly available to an audience by a speaker, and this is the relevant state of speaker's minds. What matters to successful linguistic communication is the intention of the speaker to be interpreted in a certain way, on the one hand, and the actual interpretation of the speaker's words along the intended lines through the interpreter's recognition of the speaker's intentions, on the other.
>
> (Davidson, 1990, p. 310-311).

Here, Davidson seems to have completely abandoned his technical definition of meaning in favor of a "communicative intentions" definition, a definition that is surely more congruent with most people's sense. If we apply this sense of meaning to his metaphor article, I think all his arguments in that article collapse. Whatever, he may think about metaphors, it is undeniable that millions of speakers use them with the intention of conveying various non-literal meanings (as well as affective and artistic impressions), and that millions of interpreters get these other meanings. The contradiction only gets worse as we continue. On the very next page, he says:

> A theory of truth for a speaker is a theory of meaning in this sense, that explicit knowledge of the theory would suffice for understanding the utterances of the speaker. It accomplishes this by describing the critical core of the speaker's potential and actual linguistic behavior, in effect, how the speaker intends his utterances to be interpreted. The sort of understanding involved is restricted to what we may as well call *the literal meaning of the words, by which I mean, roughly, the meaning the speaker intends the interpreter to grasp, whatever further force or significance the speaker wants the interpreter to fathom.*
>
> (1990, p. 312; my italics)

But if the "literal meaning of the words" is whatever the speaker intends, then how in the world does one ever get a figurative meaning? This kind of fancy footwork is just beyond the pale. In my last sentence, for example I intended you to understand "fancy footwork" as "intellectual maneuvering," and "beyond the pale," as beyond reason and intellectual decency. Now if you did get that meaning, or something close to it, then surely Davidson's original argument that the meaning of a metaphor is just the "literal meaning of the words" is contradicted, unless of course, we use the above definition of "literal"; but if we do that, the original argument falls apart for a different reason. According to the original argument, meaning is "strict meaning," and it's clear from reading the article, that "strict meaning" is something close to "denotative meaning" or dictionary definition. Moreover, it is only because Davidson holds to that point that he is able to make the argument for "use" determining intentions. But, in the above, use is nowhere to be found; instead, we are now told that *intention* determines meaning. Incredibly, Davidson sees no contradictions here—or at least doesn't admit to any—for he cites his *Metaphor* article approvingly in a footnote in the later article.

I've gone through this exercise not so much to show that Davidson is inconsistent, but, rather, to establish the centrality of intentions in a coherent theory of meaning, and to lay the groundwork for the argument that intentionality is a natural class that shouldn't be

cavalierly divided for the sake of theoretical integrity elsewhere, as is so often done in semantics. Davidson works himself into all sorts of contradictions because he is unclear about the position of intentionality. In his more technical TFS moods intentionality is relegated to "use," and truth conditions are determined by "strict meanings"; but then, in more liberal moods, he wants to let intentionality rule over the whole TFS machine. I would probably prefer the latter position to the former, but I think neither is coherent. Truth conditions are important in a theory of meaning, but they can't be made to do the whole job. Davidson sees this, but instead of looking about calmly for more theory, he tends to throw up his hands and declare the whole situation to be ineluctably mysterious:

> ...a speaker must be able to tell whether an utterance is intended as a joke, an assertion, an order, a question, and so forth. I do not believe there are rules or conventions that govern this aspect of language. It is something language users can convey to hearers and hearers can, often enough, detect; but this does not show that these abilities can be regimented. I think there are sound reasons for thinking nothing like a serious theory is possible concerning this dimension of language. Still less are there conventions or rules for creating or understanding metaphors, irony, humor, etc.
>
> (1990, p. 313)

One is reminded of David Hume's assertions that the "mystery "of how bread nourishes the body would "forever" be beyond our understanding—all we could do is observe that it does seem to do so, and hope that tomorrow it still would. But if, pace Hume—and apparently Davidson—effects *do* necessarily have causes, then "serious theories" are *always* possible. No doubt, the problem of how speakers' convey and recover intentions is a very hard problem, but it's not so hard that we can't at least begin to formulate a theory. Or so I will argue.

Before turning to these issues, in the next chapter, I want to briefly raise another troubling conflict in Davidson's arguments, above, regarding intentions and meaning.

Although his approach here is thoroughly I-language, in the sense that he is locating meaning within speakers, not linguistic tokens, he nonetheless continues to insist that we "stick as closely as possible to what is *made directly available to an audience* by a speaker, and this is the relevant state of speaker's minds" (repeated from quote above), and later that "meaning is entirely determined by observable behavior, even readily observable behavior" (ibid, p. 314). Now, from an I-language perspective this can't be right. As Chomsky points out, when we hear such sentences as

1) John is easy to please

versus 2) John is eager to please

and we "automatically" understand "John" as the subject of the lower clause in 1) but the object of the lower clause in 2), it cannot be because of anything on the surface of the sentences. Rather, facts like these can only be explained by a theory that posits internal rule systems. Surely the same is true for understanding intentions. That we do understand intentions with remarkable accuracy, is not at issue, so if we believe that effects have causes, we will look for an explanation. But there is no reason to limit the explanation to "observable behavior." In a broad sense, of course, it is true that we must be cueing off something observable, when we infer intentions, just as we are reacting to E-language tokens above. But this is sort of like saying that radios "only" reflect things that are in the air. True, but uninformative. In the case of radios, a substantive explanation will involve transistors, capacitors, amplifiers, etc. In the case of language the explanation must involve internal rule systems, sorting algorithms, and so forth. In fact, as we shall see, in the next chapter, Davidson makes a good attempt in that

direction—only to abandon it in a fit of Davidsonian crisis. But we will see if we can rescue him.

IX: A WORKING THEORY OF MEANING

I have called this penultimate chapter a "*working* theory of meaning" because I want to immediately dispel any great expectations. I will have little to add to existing theories, other than synthesis and commentary, and, moreover, that will be brief. However, I think the right combination of existing theories goes quite a ways toward explaining what meaning is and how it is transmitted and recovered, though it also leaves some recondite mysteries. But, recall, the current project is only to establish a minimal foundation for literacy studies, and to do that we need only establish that there are principles "more firm and stable than mere belief or unexamined practice." Mysteries notwithstanding, I think we can accomplish that.

Let's take stock of what has been minimally established so far. Following Kant, I argued first that reason itself could be accepted as a veridical, *a priori* function. In making this argument I conceded that global skepticism couldn't be indubitably refuted; however, I also argued that reason was an inevitable mode of thought for humans, and that that fact plus the impossibly paradoxical position created by refusing reason was more than enough to establish a minimal foundation for knowledge. Over the course of several chapters I also considered antifoundationalist arguments for ineluctable "situatedness." I showed through analysis of literature in the philosophy of science that that position is untenable given progress in science, and I briefly presented the theory of the virtuous circle as an explanation for why progress is possible despite the problem of "situatedness." This move is crucial because it allows us to establish further theory on the basis of evidence, and because it insulates us from the antifoundationalist argument that all theories are ineluctably theory laden.

Concurrent with my arguments for virtuous rather than vicious circles in science, I presented numerous arguments from cognitive science and linguistics that support nativist theories of mind, and I showed that antifoundationalist arguments against nativism were weak to incoherent. Regarding the innate content of mind, I presented evidence from linguistics that knowledge of language must, at least in part, be a result of innate principles, and I also showed that the main competing theories for language, most of which are forms of behaviorism, are untenable because they are unfalsifiable and without predictive power.

In the last several chapters I have argued that correspondence is an intrinsic part of human beings' understanding of truth, and that, in turn, the concept of truth itself is part of the function of reason. I then showed through a presentation of Davidson's early work, that linguistic communication would be impossible without a correspondence theory of truth, instantiated within individuals, and I argued that Davidson's final rejection of correspondence was a technical, not a substantive, rejection. In another section, I also briefly presented Grice's theory of Cooperation, which strongly overlaps with the correspondence theory; in fact, Grice's maxim of Quality entails correspondence. Both theories posit an innate orientation toward truth in linguistic communication, and both are supported by extensive evidence from the field; moreover, both theories are easily verified by analysis of everyday conversations. Finally, I have argued that intentions are central to meaning, and I presented some evidence from the literature that supports this.

No doubt, some of these points are better established than others; however, since we are only required to establish minimal foundations, and since preponderance of the

evidence is good enough for that, I would argue that all of these points are at least minimally established and that it is therefore proper to extrapolate from them. On the other hand, I should be candid about the mysterious aspects of meaning that are not addressed by any of these points. First and foremost there is the problem of sorting and integrating information. Close inspection of interpretation indicates that interlocutors—including readers—somehow sort and analyze text very rapidly, adjusting their interpretations accordingly. We will see that no one has a complete explanation for how this happens, though I will hazard a few tentative guesses as to how this occurs.

Since Davidson is our immediate link to Rorty—and hence to antifoundationalism—I will once again begin with him as I develop our working theory. Conveniently, it also so happens that Davidson has one of the most lucid accounts of authentic linguistic communication I know of, and he provides what I regard to be the best concise, general account of what occurs (with one glaring though easily rectified mistake), though, ironically, he winds up rejecting the implications of his own account. Rorty, in carrion fashion, swoops in on this rejection, using it to support his antifoundationalist agenda. Thus, as is in the peculiar examples that we've already seen, viz, correspondence theory and metaphor, Davidson provides excellent rational analysis of complex linguistic phenomena, but then, inexplicably, undercuts that analysis with bizarre conclusions that are not supported by the analysis, but which provide fodder for antifoundationalists.

The Deranged View of Meaning

The Davidson essay that generates this considerable sound and fury, is *A Nice Derangement of Epitaphs* (1986). Rorty draws heavily on this essay in his own essay *The*

Contingency of Language (the same essay we looked at on metaphor), as he argues that language is just a random contingency of "blind forces." I won't rehearse Rorty's arguments, since we know those quite well by now; however, as usual, I'll make a few comments about Rorty's (mis)use of Davidson's work.

Davidson's 1986 essay, henceforth referred to as *Derangement,* expounds on the point that meanings are determined by intention, which point was also touched on in Davidson's 1990 essay, *The Structure and Content of Truth* (henceforth referred to as *Structure*). You may recall that in *Structure,* Davidson claimed that malapropisms should be interpreted as the speaker intended them, not according to their denotations; no doubt, this was a reference to *Derangement,* while the title of the latter essay is itself an allusion to a particularly memorable phrase uttered by Sheridan's character, Mrs. Malaprop. Davidson's central point in *Derangement* is that even though "phenomena [such as malapropism] is ubiquitous" in normal conversation, "the hearer has no trouble understanding the speaker in the way the speaker intends," which fact, according to Davidson, is inexplicable according to current models of language (ibid, p. 433, 434). Unfortunately, Davidson supplies no empirical evidence for his claims about the prevalence of error in ordinary conversation, which is a serious lapse given that some researchers, notably Halliday (1976), have concluded that serious error is in fact surprisingly rare in casual conversation, as well as the easily self-verified fact that error does frequently cause misunderstanding, sometimes severe misunderstanding. Nonetheless, it is certainly true that interlocutors get by quite well with input that is degraded in various ways, including by speaker error, so Davidson's main point stands, even if it may be exaggerated.

Davidson's strategy in *Derangement* is to first present the best formal linguistic theory he knows of for recovery of meaning, particularly "first meaning," and to then radically problematize that theory. Davidson glosses "first meaning" as "the first meaning of an utterance...found by consulting a dictionary based on actual usage" (p. 435). Concurrently with this gloss, Davidson identifies "literal meaning" as synonymous with "first meaning," and he stresses that "nothing should be allowed to blur the distinction between the speaker's meaning and the literal meaning" (ibid, p. 434). Yet, later in the essay, Davidson himself "blurs the distinction" when he abandons this definition arguing that "every deviation from ordinary usage, as long as it is agreed on for the moment' [is] the "literal" meaning (p. 442). Note that this is a similar though still significantly different definition for "literal meaning" than what he will later give in *Structure*. In that essay he said that "literal meaning" was whatever "meaning the speaker intends the interpreter to grasp," which is a looser definition than this one because in this case he requires there to be agreement, or at least possible agreement, between interlocutors. This is a crucial point, especially when it comes to adjudicating between contested interpretations of texts.

Davidson creates considerable confusion by mixing these definitions this way; while I agree with him that we need terms for what speakers intend, as well as for what interlocutors momentarily agree on, I do not agree that we should abandon the notion of denotative meaning; we need all three concepts. I will argue that Davidson's general (erroneous) conclusions are a result of this abandonment and extensions of it. First, however, let's see how Davidson pleads his case.

Davidson lays out three "plausible principles concerning first meaning" that he says are required to build a formal theory:

> (1) *First meaning is systematic*. A competent speaker or interpreter is able to interpret utterances, his own or those of others, on the basis of the semantic properties of the parts, or words, in the utterance, and the structure of the utterance. For this to be possible, there must be systematic relations between the meanings of utterances.
>
> (2) *First meanings are shared.* For speaker and interpreter to communicate successfully and regularly, they must share a method of interpretation of the sort described in (1)
>
> (3) *First meanings are governed by learned conventions.* The systematic knowledge or competence of the speaker or interpreter is learned in advance of occasions of interpretation and is conventional in character.
>
> (1986, p. 436).

As Davidson observes, there are known problems with these principles even for formal theories, including the existence of ambiguous words (at the denotative level), as well as ambiguities in syntax; however, at least in the first part of the paper, Davidson argues (correctly) that these principles are part of what is necessary to explain successful communication. But, Davidson moves quickly to show that such principles cannot possibly explain a lot of what occurs regularly in conversation. In making this argument he alludes to Grice's principles of implicature, again correctly observing that "Grice has done more than anyone else to bring these problems [of extra or changed meanings conveyed by implicature] to our attention and to help sort them out" (p. 437). Strangely, however, he later seems to forget this, for he winds up arguing that there are no rules at all that can explain linguistic communication. As with his iconoclastic conclusions on metaphors, Davidson's final argument in *Derangement* can be read as a technical rather than a substantive position; yet, as in the metaphor case, it is very unclear whether this is

right. Some critics have taken Davidson more or less at face value (e.g., Hacking, 1986; Dummett, 1986), and, so far as I know, he has not disabused them of this notion. I myself have wavered on the issue, sometimes convincing myself that the theoretical crisis Davidson presents is merely technical, hinging only on where one decides to draw the line between "language" and "communication," while at other times deciding that Davidson is, in fact, making a substantive claim. I will present both alternatives here.

The slide toward this "Davidsonian Crisis" begins with Davidson's analysis of malapropisms. Davidson argues (correctly) that principles 1-3 above, together with other well known bits of linguistic machinery, including TFS, simply cannot explain how interpreters (which term is used for both speakers and hearers by Davidson) arrive at appropriate interpretations of such words. He then begins a perspicacious discussion of intentions, arguing that we must carefully distinguish what words and sentences say at the denotative level, from what the speaker intends them to mean. To illustrate this point, Davidson presents Donnellan's "famous" example of the difference between "referential" and "attributive" uses of the sentence, "Smith's murderer is insane." In the first case, the statement is made by Jones, who *intends to refer* to a man that "he (Jones) takes to have murdered Smith" (ibid, p. 439) Unfortunately, it is unclear in the example just how this reference is accomplished, but perhaps we could imagine Jones pointing at some man while he utters the sentence. According to Davidson,

> Donnellan says that even if the man that Jones believes to have murdered Smith did not murder Smith, Jones has referred to the man he had in mind; and if that man is insane, Jones has said something true.
>
> (ibid, p. 439).

The key to the analysis is the stipulation that the use was *referential.* The clearest referential use I can imagine is the one I suggested above, wherein Jones is pointing at

the man (say in a courtroom). Davidson doesn't extrapolate on this point, but I think it is critical here that both Jones *and* his interlocutor recognize the referential intentions, which means that they must both have the same man in mind (whether or not the man is in fact the murderer of Smith). If this condition is fulfilled, I would agree with Donnellan; however, I would *not* agree with the analysis in the case where Jones *alone* has some (wrong) man in mind, but his interlocutor does not and could not reasonably infer the same man; for in that case, neither Jones' words, nor the interlocutors' shared understanding of those words, link up with anyone in the world. For convenience, let's call this latter case the *Jones' intentions* case; we will see that my analysis converges with analysis Davidson makes of another case. Perhaps Donnellan meant to cover all cases by making the distinction between *referential* and *attributive* uses, for the facts around the attributive case overlap with those of the *Jones' intentions* case ; however, neither Donnelan nor Davidson notice this. At any rate, the attributive case would be the one wherein Jones has no particular man in mind—yet believes that there is a man who is insane and who murdered Smith, and says as much with the sentence, "Smith's murderer is insane." In that case, according to Donnellan (per Davidson), "the speaker does not say something true if no one murdered Smith, nor has the speaker referred to anyone." This seems fine to me. The principle that is emerging from this—which Davidson strongly agrees with—is that denotative meanings are secondary (in importance) to intentions. I agree as well. However, as we'll see, Davidson seems prepared to abandon denotative meanings altogether, whereas I hold that it is only *through* denotative meanings that intentions can be reached. Oddly, most of what Davidson says should lead him to the same conclusion, yet, inexplicably, it doesn't.

As Davidson continues to make his case, he analyzes a discussion between Donnellan and MacKay regarding the Smith murder example. Mackay apparently objected to Donnellan's analysis, calling it the "Humpty Dumpty theory of meaning," which is an allusion to the Humpty Dumpty of *Alice in Wonderland,* who told Alice that words meant what ever he wanted them to mean. Resting on this assertion, Humpty Dumpty tells Alice that 'glory' means 'a nice knockdown argument.' This will be important in a minute. Coming to the crux of this discussion Davidson observes that

> MacKay says you cannot change what words mean (and so their reference if that is relevant) merely by intending to; the answer is that this is true, but you can change the meaning provided you believe (and perhaps are justified in believing) that the interpreter has adequate clues for the new interpretation.
>
> (ibid, p. 439).

To drive this point home, Davidson observes that Donnellan could close his reply to MacKay, with the words "there's glory for you," and be confident that these words would be taken to mean "there's a nice knock-down argument for you." Generalizing from this, Davidson stresses that what is needed in analysis of meaning and interpretation is a "firm sense of the difference between what *words* mean or refer to and what *speakers* mean or refer to" (ibid, p. 439). Moreover, in accord with what I said above about the *Jones' intentions* case, Davidson goes on to argue that words can only change meaning when *both* interlocutors have access to the new meaning. Thus Humpty Dumpty is not allowed to claim that "there's glory for you" means "there's a nice knock-down argument" because he didn't give Alice any way to correctly infer the altered meaning. I agree with all of this, provided that the phrase "what words mean" refers to denotative meaning.

Crucially, Davidson does not argue that the interlocutors must *actually* agree on the altered meaning—only, rather, that they *could* do so, given the clues. Thus, if

MacKay were to forget his own allusion to Humpty Dumpty, he could fail to interpret Donnellan's (hypothetical) "there's glory for you" as "there's a nice knock-down argument," and might wonder what "glory" had to do with anything. But in that case, the correct analysis would be that MacKay had failed to interpret the words *correctly* since, clearly, the clues and context amply supported the altered meaning. Note by the way, that this position totally contradicts Davidson's *Metaphor* position. In fact, in the MacKay/Dummett case "glory" can be considered a sort of metaphor (certainly as much as the 'business' example), yet contra his *Metaphor* position, Davidson argues here that "glory" has a special meaning. I of course agree.

As Davidson continues to build this theory (which he will later deny *is* a theory, or at any rate a viable theory), he develops some terminology to help explain the stages of what he thinks occurs in conversation. The first stage is ruled by what Davidson refers to as the interpreter's *theory* (where, recall, "interpreter" refers to all interlocutors). I'll call this "I-theory" to distinguish it from the common term. I-theory is a formal theory possessed somehow by the interpreter, though certainly not consciously. This is a familiar I-language position that is accepted by virtually all generative linguists, as Davidson points out. Davidson says this I-theory (or alternatively "system") can be thought of

> as a machine which, when fed an arbitrary utterance (and certain parameters provided by the circumstances of the utterance), produces an interpretation. One model for such a machine is a theory of truth, more or less along the lines of a Tarski truth definition. It provides a recursive characterization of the truth conditions of all possible utterances of the speaker, and it does this in terms of sentences made up from the finite vocabulary and the finite stock of modes of composition. I have frequently argued that command of such a theory would suffice for interpretation. Here however there is no reason to be concerned with the details of the theory that can adequately model the ability of the interpreter. All that matters in the present discussion is that the theory has a finite

> base and is recursive, and these are features on which most philosophers and linguists agree.
>
> (ibid, p. 337-338)

But, Davidson argues (again correctly) that I-theory isn't an adequate explanation for what interpreters accomplish since we cannot explain with such theories how interpreters arrive at correct interpretations in situations that are linguistically devious in some way, as so many situations are. Therefore, Davidson roughs out a new theory for what actually happens in conversation (and implicitly for writing; Davidson doesn't see a crucial qualitative difference between these modes). But it is at this juncture that confusion occurs.

Prior and Passing Theories of Meaning

Davidson proposes that interpreters employ two theories, a *prior theory* and a *passing theory* in the process of interpretation. The first problem with this is that I-theory seems to have disappeared[115]. I will argue that this is the main problem with the entire model. In any case, let's see how it works:

> Here is a highly simplified and idealized proposal about what goes on. An interpreter has, at any moment of a speech transaction, what I persist in calling a theory. (I call it a theory, as remarked before, only because a description of the interpreter's competence requires a recursive account.) I assume that the interpreter's theory has been adjusted to the evidence so far available to him: knowledge of the character, dress, role, sex, of the speaker, and whatever else has been gained by observing the speaker's behavior, linguistic or otherwise. As the speaker speaks his piece the interpreter alters his theory, entering hypotheses about new names, altering the interpretation of familiar predicates, and revising past interpretations of particular utterances in the light of new evidence.
>
> (ibid, p. 441)

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[115] Both Hacking and Dummett seem confused about this as well (though for somewhat different reasons).

This is plausible, but, it's unclear what happened to I-theory. Later we discover that Davidson refers to what I have been calling I-theory, as "competence," but it takes an eagle eye to see this. In the original description, I-theory (designated by Davidson as "the system") works like a "machine," returning interpretations according to formal algorithms. Though not stated explicitly, the implication is that I-theory is limited to formal rules and that the interpretations it returns are *denotative*. As Davidson surely knows, this is a common assumption in generative linguistics: the standard picture is that there is a "machine" side to competence (restricted to "grammar"), which generates well formed denotative structures, and then a "pragmatic" side that steps in and makes whatever adjustments are necessary, on the fly (Green, 1996; Larson and Segal, 1995). But Davidson seems to be abandoning the "machine" in favor of a theory that relies exclusively on pragmatics. There's nothing wrong with integrating pragmatics into the model from the outset—in fact this is a common practice in psycholinguistics (see, especially, MacDonald, Perlmutter and Seidenberg. 1994), but it is inconceivable that pragmatics could operate successfully without I-language competence. Nonetheless, let's give Davidson room to swing his sledge hammer.

What is important about the theory Davidson is proposing is that it is infinitely flexible, adjusting to each speaker and then, further, to each moment in conversation. Davidson shows how this might work:

> The speaker wants to be understood, so he utters words he believes can and will be interpreted in a certain way. In order to judge how he will be interpreted, he forms, or uses, a picture of the interpreter's readiness to interpret along certain lines. Central to this picture is what the speaker believes is the starting theory of interpretation the interpreter has for him. The speaker does not necessarily speak in such a way as to prompt the interpreter to apply this prior theory; he may deliberately dispose the interpreter to modify his prior theory.

> But the speaker's view of the interpreter's prior theory is not irrelevant to what he says, nor to what he means by his words; it is an important part of what he has to go on if he wants to be understood.
>
> (ibid, p. 442)

All of this is more than plausible and completely in accord with intuition (as long as I-theory is assumed to be operating underneath the prior theory). One can catch the workings of such a "prior theory" easily in one's own conversations. Moreover, it also fits nicely with virtuous hermeneutics, which is handy since that allows us to unify at least some of our account of linguistic communication with our account of how science proceeds. In both cases, what seems to occur is that we start with some sort of a model (a "prior theory") which we then adjust—both from the top down and from the bottom up—as information comes in.

Davidson now adds a little detail. The quote below directly follows from the one above:

> I have distinguished what I have been calling the *prior theory* from what I shall henceforth call the *passing* theory. For the hearer, the prior theory expresses how he is prepared in advance to interpret an utterance of the speaker, while the passing theory is how he *does* interpret the utterance. For the speaker, the prior theory is what he *believes* the interpreter's prior theory to be, while his passing theory is the theory he *intends* the interpreter to use.
>
> (ibid, p. 442)

This is elegant, and again at least plausible, though I am not sure about the direct identification of prior and passing theories from speaker to interpreter. But from here Davidson makes an unwarranted leap. Again, the quote follows directly:

> I am now in a position to state a problem that arises if we accept the distinction between the prior and the passing theory and also accept the account of linguistic competence given by principles (1)—(2)

> According to that account, each interpreter (and this includes speakers, since speakers must be interpreters) comes to a successful linguistic exchange prepared with a 'theory' which constitutes his basic linguistic competence, and which he shares with those with whom he communicates. Because each party has such a shared theory and knows that others share his theory, and knows that others know he knows (etc.). Some would say that the knowledge or abilities that constitute the theory may be called conventions.
>
> (ibid, p. 442)

Davidsonian Crisis

In this passage, and what follows, Davidson begins building a case against "competence" by claiming that competence must either do all the work or none:

> I think that the distinction between the prior and the passing theory, if taken seriously, undermines the commonly accepted account of linguistic competence and communication. Here is why. What must be shared for communication to succeed is the passing theory. For the passing theory is the one the interpreter actually uses to interpret an utterance, and it is the theory the speaker intends the interpreter to use. Only if these coincide is understanding complete. (Of course, there are degrees of success in communication; much may be right although some is wrong. This matter of degree is irrelevant to my argument.)
>
> The passing theory is where, accident aside, agreement is greatest. As speaker and interpreter talk, their prior theories become more alike; so do their passing theories. The asymptote of agreement and understanding is reached when passing theories coincide. But the passing theory cannot in general correspond to an interpreter's linguistic competence. Not only does it have its changing lists of proper names and gerrymandered vocabulary, but it includes every successful—i.e., correctly interpreted use of any other word or phrase, no matter how far out of the ordinary. Every deviation from ordinary usage, as long as it is agreed on for the moment (knowingly deviant or not on one or both sides) is in the passing theory as a feature of what the words mean on that occasion. *Such meanings, transient though they may be, are literal; they are what I have been calling first meanings.* A passing theory is not a theory of what anyone (except perhaps a philosopher) would call an actual language. 'Mastery' of such a language would be useless, since knowing a passing theory is only knowing how to interpret a particular utterance on a particular occasion. Nor could such a language, if we want to call it that, be said to have been learned, or to be governed by conventions.

(ibid, p. 442-443; my italics)

These conclusions far exceed the (hypothetical) data. I think we can all agree with Davidson that grammatical competence in the Chomskyan sense (what Davidson calls simply "competence") is insufficient for linguistic communication, except perhaps under the most controlled, artificial circumstances. But "not sufficient" does not entail "not necessary," or "irrelevant." To adequately defend his conclusion, Davidson would have to show both of those conditions, but he doesn't so much as gesture in that direction. Let's take a look, then ourselves, starting with "necessary." Does anyone doubt, even for a minute, that for successful (where success is defined as full, detailed, and free ranging) linguistic communication to occur it is *necessary* that the interlocutors speak a common language? The point is so obvious that we don't even need carefully defined terms. Any layperson knows that a speaker who only speaks, say, Mandarin Chinese, will have very little success speaking to an interlocutor who speaks only German. This is a simple fact that can be stipulated. Given the fact, it is impossible to conclude that "prior" or "passing" theories could somehow be independent of grammatical competence. Moreover, why can't we have both? Why not say we have an I-theory (or in Chomsky's terms an I-language "knowledge of language") *and* a prior theory that includes the I-theory? For some reason Davidson never even considers this possibility, though it is fairly standard in linguistics. Now, Davidson is no dummy, so he must realize this. Thus, our real problem here is to arrive at a reasonable "passing theory" for his radical statements.

I think the key to the enigma may be in the italicized sentence, wherein Davidson claims that special meanings become "literal." It seems that Davidson wants to eliminate the distinction between denotative and intentional meaning, even though, as I noted

above, he earlier stressed the importance of maintaining "a firm sense of difference between what *words* mean or refer to and what *speakers* mean." By blurring this distinction, Davidson can allow competence to be *subsumed* by prior theory rather than replaced. But he is clearly still relying heavily on competence (our I-theory):

> An interpreter's prior theory has a better chance [than a formal theory] of describing what we might think of as a natural language, particularly a prior theory brought to a first conversation. The less we know about the speaker, *assuming we know he belongs to our language community*, the more nearly our prior theory will simply be the theory we expect someone who hears our unguarded speech to use.
>
> (ibid, p. 443; my italics)

Davidson minimizes the italicized assumption, but surely that's the most important assumption of all. At any rate, this passage shows that he isn't really discarding the importance of competence; rather, he is arguing that we can't consider competence separately. The passage below follows directly from the one above:

> If we ask for a cup of coffee, direct a taxi driver, or order a crate of lemons, we may know so little about out intended interpreter that we can do no better that to assume that he will interpret our speech along what we take to be standard lines. But all this is relative. In fact we always have the interpreter in mind; there's no such thing as how we expect, in the abstract, to be interpreted. We inhibit our higher vocabulary, or encourage it, depending on the most general considerations, and we fail to have premonitions as to which of the proper names we know are apt to be correctly understood.
>
> In any case, my point is this: most of the time prior theories will not be shared, and there is no reason why they should be.
>
> (ibid, p. 443)

Again, the conclusions do not seem to follow from the premises. If there are such things as "language communities" and "what we take to be standard lines," and of course there are, then how can it be the case that "most of the time prior theories will not be shared"? To the contrary, the passage supports the conclusion that most of the time *most* of prior

theories *are* shared. But degree of fit is only part of the issue. Although I think Davidson does exaggerate the gap between speakers, and likewise minimizes the overlap provided by common language, these are matters of degree that could be adjusted. The real issue for Davidson is the fact that there is no formal theory to explain how aspects of communication *not* covered by any formal theory are to be explained. He's right to call attention to this problem, but I think wrong to discard formal theories in seeking an explanation. In fact, he considers an integrated model, but he won't let the formal theory do the work it is supposed to do:

> First, any general framework, whether conceived as a grammar for English, or a rule for accepting grammars, or a basic grammar plus rules for modifying or extending it—any such general framework, by virtue of the features that make it general, will by itself be insufficient for interpreting particular utterances.
>
> (ibid, p. 444)

This is perfectly true, but also not a problem. No one is claiming that the general framework does the whole job; what *is* claimed—quite reasonably in my opinion—is that you need a general framework to get you close enough to your interlocutor's prior and passing theories to be able to make the leap. For some strange reason, Davidson won't consider this:

> Second, the framework theory must be expected to be different for different speakers.
>
> (ibid, p. 444)

This is the big mistake. To the contrary, the framework theory must be as stable as possible for it to be effective with divergent speakers in divergent circumstances. That's why it's a framework. Davidson's error here is to mix one genuine problem—the problem of explaining how speakers creatively extrapolate from the denotative level to the contextualized level, or in other terms, from the type level to the token level—with

the non-problem of explaining why formal grammars don't work. The latter is a non-problem for everyone except the early Wittgenstein and the later Davidson, because, with those two exceptions (I exaggerate; surely there are others), no one *expects* formal theories to do the whole job. Davidson slides into this error because he wants to define language in functional—or behavioristic—terms:

> Perhaps we can give content to the idea of two people 'having the same language' by saying that they tend to converge on passing theories; degree or relative frequency of convergence would then be a measure of similarity of language. What use can we find, however, for the concept of language? We could hold that any theory on which a speaker and interpreter converge is a language; but then there would be a new language for every unexpected turn in the conversation, and languages could not be learned and no one would want to master most of them.
>
> (ibid, p. 445)

But the same sort of complaint can be made about *every* theory in every realm of experience. We can have no general theory of chairs (as Wittgenstein "proved") because, at the token level every chair is unique; ditto for people, cats, sunny days, and everything else you can think of. This is ludicrous. The obvious answer to Davidson's question about the use of the term "language," is that it is a term for a system that operates at the *type* level not the *token* level. That's all it does (but that's a lot). *We*, creative, meaning makers that we are, tweak the types by whatever means necessary, and the degree to which this tweaking is correctly interpreted, is the degree to which the intended representation is inferred. Davidson agrees with the creative aspects of this story, but for some reason he can't tolerate a model in which creative extrapolation relies on formal mechanisms for its beginning position, nor the idea that "creativity" itself may be a rule governed process. These biases lead him to his unwarranted conclusion:

> For we have discovered no learnable common core of consistent behavior, no shared grammar or rules, no portable interpreting

> machine set to grind out the meaning of an arbitrary utterance. We may say that linguistic ability is the ability to converge on a passing theory form time to time—this is what I have suggested, and I have no better proposal. But if we do say this, then we should realize that we have abandoned not only the ordinary notion of language, but we have erased the boundary between knowing a language and knowing our way around the world generally. For there are no rules for arriving at passing theories, no rules in any strict sense, as opposed to rough maxims and methodological generalities...In linguistic communication nothing corresponds to a linguistic competence as often described: that is, as summarized by principles (1)—(3). The solution is to give up the principles...I conclude that there is no such thing as a language, not if a language is anything like what many philosophers and linguists have supposed.
>
> (ibid, p. 446)

To the contrary, the solution is to not *think* of language as the early Wittgenstein did, but, rather the way Chomsky does, or perhaps as Katz does. The formal principles get us denotative meaning, and from there we extrapolate to intentions. The proof that formal theory is useful is the fact that we can indeed predict a great deal about how interpreters will interpret strings with no knowledge of these interpreters *at all*, a fact which is also overwhelmingly supported by the common observation that communication depends on shared language. If we order "a crate of lemons" from an English speaking grocer, by God, that's what we get, damn near every time, but if we try it with a monolingual Chinese speaking grocer we get nothing but unintelligible "gibberish." How anyone can overlook this is beyond me.

Rortian Language

Of course Rorty goes to town with Davidson's incredible pronouncements, declaring that Davidson is "saying" (along with Ryle):

> Think of the term *mind* or *language* not as the name of a medium between self and reality but simply as a flag which signals the desirability of using certain vocabulary when trying to cope with certain kinds of organisms. To say that a given organism—or, for that matter, a given machine—has a mind is just to say that, for

> some purposes, it will pay us to think of it as having beliefs and desires. To say that it is a language user is just to say that pairing off the marks and noises it makes with those we make will prove a useful tactic in predicting and controlling its future behavior.
>
> This Wittgenstienian attitude, developed by Ryle and Dennett for minds and by Davidson for languages, naturalizes mind and language by making all questions about the relation of either to the rest of the universe *causal* questions, as opposed to questions about adequacy of representation or expression.
>
> (Rorty, 1998, p. 77).

I don't see that Davidson is saying any of this in *Derangement*. To the contrary, it seems to me that Davidson is very much concerned with how people represent ideas through language. His iconoclasm consists in rejecting formal theories of language for deriving representations of meaning, not in rejecting the principle of representation itself. In *Derangement,* Davidson asks us to look more carefully at the gap between formal theories and recovered interpretations, and this would not make any sense if he were arguing for a "causal theory of language," nor would any of his ideas about prior and passing theories. Rorty, as usual, just has it wrong (and he probably knows it and doesn't care). But I promised to be brief in my comments on Rorty, and I will be. To borrow a phrase from my opponent, analysis of Rortian arguments is an "unprofitable topic." Therefore, let's turn to something more fruitful.

The issue Davidson left us with was how to account for unique, yet highly successful uses of language. What bothers him, quite rightly, is that he can't see how a formal theory, which, by definition, can only operate on predefined constituents, can derive appropriate inferences from malapropisms and other devious tokens. This is essentially the same worry Fodor has about how a computational mind could possibly generate "abductive" inferences. We have seen some of this already in Fodor's discussions of visual illusions, but he also extends the discussion into linguistics.

The Frame Problem

Fodor considers how a computational theory of mind (CTM) might explain the relative complexity of propositions, arguing that the proposition, "the cat is on the television" should be simpler than proposition, "the cat is asleep on the television," because the second proposition contains more information. So far so good—plausibly the second proposition *is* more complex conceptually, and since it is also more complex syntactically, the CTM idea that the complexity of a "thought supervenes on its syntax" is supported (p. 26). But it is easy to see this can't be the whole, or even the usual story. To show this, Fodor examines different cases in which the proposition *no wind tomorrow* is heard, or thought (2000, p.26). The problem is that, according to computational theories, strings that have the same syntax should always have the same causal impact on thoughts, regardless of context, but we know this isn't true. If, as Fodor notes, you are planning on sailing tomorrow, the proposition *no wind tomorrow* is a complicating factor; whereas, if you are doing something else, it may not have any impact at all on your thoughts. The troubling generalization is that

> the complexity of a thought is not intrinsic; it depends on the context. But the syntax of a representation is one of its essential properties and so doesn't change when the representation is transported from one context to another. *So how could the simplicity of a thought supervene on its syntax*? as, please recall, CTM requires it to do [because computations *must* treat constituents categorically, according to predetermined algorithms]
>
> (ibid, p. 26-27).

According to Fodor this is a devastating problem for computational theories of mind because it shows that some "mental representations [are] *not* determined by [their] individuating properties; hence. . . not determined by [their] syntax" (ibid, p. 28; italics in original).

At first blush this might not seem like such a difficult problem. Suppose we treat the tokens of an (identical) proposition equally at the syntactic and even semantic levels, but allow the propositional tokens to be embedded in a larger constituent whose properties are determined by context. This way, even though the "local" syntax of the proposition is the same, at the higher level, it could have a differential impact. This is a line Fodor tries (ibid pps. 27-30) but rejects because it seems to entail implausibly long computations. Here's why. If we look at the mind as a computer that represents to itself the totality of things it is concerned with at any given moment (there are other horrible problems here about what this "self" could be, and how to avoid regress, etc., etc, that we will ignore for right now), then a new constituent like *no wind tomorrow* would change the total configuration, or "theory" (something like Davidson's passing theory), but the thinker wouldn't be able to know just how that configuration had changed without running the whole "theory" of representations, and this seems wrong to Fodor:

> For it is enormously plausible, in the typical case, that the representations over which mental processes are actually defined are *much shorter* than whole theories. Or, to put it slightly differently, it's just *got to be* possible to determine, with reasonable accuracy, the impact of adopting a new belief on one's prior epistemic commitments without having to survey those commitments in their totality. *Whole theories* can't be the units of computation any more than they can be the units of confirmation, or of assertion, or of semantic evaluation. The totality of one's epistemic commitments is vastly too large a space to search if all one is trying to figure out is whether, since there are clouds, it would be wise to carry an umbrella. In fact the totality of one's epistemic space is vastly too large a space to have to search *whatever* one is trying to figure out.
>
> (ibid, p. 31)

If this is right, and it surely sounds right, it means that the classical computational model is seriously inadequate as a complete explanation of mind, though CTM may still figure prominently as a partial explanation. The "obvious" solution to the total search problem

is to limit the search somehow to the relevant areas. Intuitively, this certainly sounds right; it seems to us—though of course what seems and what is may differ—that we only consider certain things, not all things, as we solve the thousands of mundane problems, including interpretive problems, that confront us every day. However, no one has been able to come up with a process or algorithm for limiting the search space appropriately. As Fodor wryly remarks:

> [T]he failure of artificial intelligence to produce successful simulations of routine commonsense cognitive competences is notorious, not to say scandalous. We still don't have the fabled machine that can make breakfast without burning down the house; or the one that can translate everyday English into every day Italian; or the one that can summarize texts; or even the one that can learn anything much except statistical generalizations.
>
> ...It does seem to me that there's a pattern to the failures. Because of the context sensitivity of many parameters of quotidian abductive inferences, there is typically no way to delimit a priori the considerations that may be relevant to assessing them. In fact, there's a familiar dilemma: Reliable abduction may require, in the limit, that the whole background of epistemic commitments be somehow brought to bear in planning and belief fixation. But feasible abduction requires, in practice, that not more than a small subset of even the relevant background beliefs is actually consulted.
>
> (ibid, p. 37)

This dilemma, which is known in artificial intelligence research (AI) as the "frame problem," is in Fodor's opinion one of the most difficult problems for theories of mind. Fodor works through various proposed solutions to the problem, including the "heuristic" solution and the connectionist solution, but finds serious fault with every proposal. Since working through this discussion would take us far afield, and since, in any case, Fodor concludes his fascinating book with the words:

> In fact, what our cognitive science has done so far is mostly to throw some light on how much dark there is. So far, what our cognitive science has found out about the mind is mostly that we don't know how it works.

(ibid, p. 100),

I am going to take a shortcut, and make some quick suggestions for how rationally minded literacy theorists can work within this highly indeterminate theoretical space.

How to Work in Literacy Studies Despite the Frame Problem

A general insight that is well supported from the history of science is that it is possible to make good progress within a local domain even when seemingly intractable theoretical problems remain at higher levels. One of the best examples of this is the history of agriculture. Until very recently almost nothing was known about such basic processes as photosynthesis, fertilization, pollination, or plant genetics. Nonetheless, farmers the world over acquired considerable "local" (conceptually local not geographically) knowledge about plants, more than enough to farm successfully. Moreover, even though they may often have been harboring completely false ideas about why things worked—attributing the benefits of fertilizer, for example, to the appeasement of the gods, rather than to fertilizer's chemical composition—their empirically derived insights helped guide later scientists toward the right explanations. It seems to me that everyone who works in fields related to cognition is in a similar position. The fact is, the mind as a whole is a black box that we currently cannot model in even a crude fashion. The major questions—What is consciousness and how is it possible? What is the self and why doesn't self awareness induce infinite regress? How is free will possible given assumptions of cause and effect?—seem beyond all of our theories at present. Some philosophers, notably Colin Mcginn, have concluded that such problems are irresolubly "mysterious," because our minds are simply not equipped to grasp the principles involved (Mcginn, 1993). Mcginn may turn out to be right, but his extreme pessimism seems

unwarranted to me, given that cognitive science is a very young field. Hume's certainty that we would never understand nutrition suggests to me that today's intractable mysteries may turn out to be tomorrow's complex, yet, tractable puzzles.

Philosophers like Mcginn like to point out that the problems mentioned above have been central to philosophy since the dawn of time, and that in all that time we appear to have made no progress toward solutions for any of these problems. But I'm not sure they are considering all the facts. It seems to me that these problems have only recently been approached from a rational/empirical standpoint. Thus, just as natural science made almost no progress under the pure rationalism (and superstition) of the Aristotelian model, but then leaped forward dramatically under Baconian methods, so cognitive science, might finally begin to make progress vis-à-vis these philosophical problems of mind, under the still quite new nativist/empiricist[116] paradigms that are currently being employed with great success in linguistics and in some limited areas of cognitive science such as vision. Crucially, I am not suggesting, as Churchland and Sejnowski have, that we can have a pure ontology without epistemology. But I think we can work under a very minimal epistemology, as outlined here, and a rich ontology, along the lines of Fodor's work. It also seems to me that there are two positions from which to work. One can approach the major problems head-on, as Fodor has been doing, or, one can select a local area of interest that is related to the larger issues, as, for example, Chomsky has done, and just try to understand the local problems in the hopes that local solutions will shed light on global issues. I think we need people working from both directions, and there are theoretical/logistical benefits to splitting the work this way. If

one chooses to work from the top down (e.g. Fodor), one need not master the intricacies of local theories—though good understanding is necessary—but can, rather, use the insights from local theories as evidence for what sorts of comprehensive solutions are most likely. On the other hand, if one chooses to work at a local level, one can suspend the larger "mysteries" and work at the local problems under a set of "working assumptions" about the larger issues. Notice that this way of dividing the work manifests a hermeneutical circle at the macro level.

In addition to a minimal foundationalist epistemology, everyone working on cognitive problems at the local level, probably needs to assume that, somehow, a compromise between CTM processing and holistic processing is possible. I think we need to assume something like the following—although we should bear in mind that we are assuming the "inexplicable"—for the purpose of getting on with our work: The mind is able to *glance* at how new constituents affect its total configuration of representations *without* running through them—glances are just glances, not searches. Based on these glances, the mind is able to assign each new constituent to a certain domain or "frame" where it does run the affected program or "theory" thoroughly. Probably this process is iterative so that we start with a glance from the top, move to a smaller domain where another glance is taken and so on, until, for some reason we settle in a domain and do a real search. Crucially, this isn't a theory—it's just a loose framework licensed by the decision to exercise a certain judiciously chosen suspension of disbelief. It's up to the global theorists to give us a better framework whenever they think they have one.

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[116] Katz would insist that we add Realist to this compound. I at least "minimally" agree with him, as we'll

As far as literacy theory goes, this framework is compelling since we have good evidence for this kind of guided search in language processing. Consider what happens when you hear an unexpected voice speaking in English from some hidden quarter, say the next room. The distal stimulus is just some sound waves, which your auditory processors turn into noise. Now, if the dumb CTM theory were correct, you would have to search your entire memory to decide what sort of noise you were hearing, but no one thinks this happens. Rather, some dedicated "module" recognizes the noise as linguistic and then rapidly recognizes it as English, and so on. We can represent this search process linearly like this: sound waves →noise → linguistic noise → English → particular phones → categorial representation of these phones as particular phonemes → hypothesis and confirmation of particular morphological units → hypothesis and confirmation of particular syntactic constituents → recovery of denotative meaning. This is as far as we've come so far, though, note, Davidson rejects this story. We will see though that his objections can be handled under fairly standard linguisitic assumptions. Our goal is to dispense with his objections and then to work on the abduction probem, or, in Davidson's terminology, the prior/passing theory problem. Literacy is a particularly good area to do research on abduction because 1) the domain is vastly simplified, reduced to the text on the page, the reader, and the context of the reading, and 2) because we can look at exactly the same texts again and again with different readers in different circumstances, and finally because 3) we have the luxury of time; we can allow subjects to study the text as long as we please.

---

see.

Returning to the model, at every juncture, there are many other nodes, each leading to choices that are rejected, so the real map would look like a tree rather than just a linear series of arrows. Now, if this story is roughly correct, what it does for us is explain—at least locally—how the mind narrows the search space to a point where correct inferences about meaning are possible. One of the most important aspects of this model is that most of the work culminating in the retrieval of denotative meaning is done mechanically—i.e., by the "machine" in Davidson's model. This is good, because we know that mechanical processes are *fast*, and we need speed in our model because we know that people recover meaning almost instantaneously in many situations. Now, it is also possible and entirely likely that global inferences contribute to this process. Psycholinguists have found plenty of evidence of such contributions, so we should assume they occur. Yet, contra Davidson, the use of global inferencing strategies is in no way incompatible with mechanical aspects of processing. Rather, it is entirely plausible that mechanical processes proceed in tandem with global inferences. In one such model MacDonald, Perlmutter, and Seidenberg (1994) have proposed that

> comprehension is the process of concurrently deriving a number of linked representations of different types for a given input sentence or sequence of sentences. Sentences are represented at three major levels: (a) lexical...(b) syntactic; and (d) discourse. Lexical processing involves activating different types of information associated with a word form and then using this information to compute representations at the other levels.
>
> (p. 682)

In this type of model, which is also favored by Jackendoff among many others, there are feedback loops running between all representations, and some "executive" controller that "decides" which bits of information are relevant to which other bits in the process of recovering meaning. Again, hermeneutics is strongly suggested by such models. And,

at a higher level, the level of model building itself, we should be hermeneutical as well. In the instant case, we have a general theory that makes pretty good sense, though it requires us to allocate some very important roles to black boxes. Here, for example, we can model the individual processors for phonetics, phonology, morphology, lexicon, syntax, and even some aspects of discourse, but we cannot model the "executive" controller, at least not in any way that satisfies the sorts of objections that Fodor has raised, or that Davidson has raised in a very different way. Nonetheless, given that it is a fact that we recover structure at all of these levels, in very reliable ways, a fact that has been amply supported by psycholinguistic tests of thousands of individuals, it is far more reasonable for us to accept a model that explains a lot of what goes on, even though it includes some black boxes, than to chalk the whole process up to one big black box like "situatedness," or to throw up our hands as Davidson does and declare the end of language. Thus, we come to a familiar minimal foundationalist turn: there is no certainty anywhere, and many recondite problems, yet some models are better supported than others. In our case, we have a pretty good story for recovery of denotative meaning, and though there are mysteries we must accept along with the story, there are no objections to it that don't ask us to accept even worse mysteries. Davidson's route, for example, asks us to throw out explanations we do understand and can model, and which get us at least part of the way to a unified theory of interpretation/meaning, in favor of a non-model that is completely mysterious at every juncture. The choice to continue then with the flawed model we have should be easy.

Interestingly, antifoundationalists don't generally quibble about the fact of lower level processing, though if they thought about it, they should realize that these lower level

processes strongly contradict antifoundationalist assumptions about mind, since such processes reliably compute information independently from contexts. Things like syntactic ambiguity, for example, are virtually impervious to discourse communities or contexts. No matter who or where you are, if you are a native English speaker, you recover two specific structures for sentences like *The cop shot the robber with the rifle.* Effects such as these are very robust and there are thousands of them—actually an infinite number since there are an infinite number of such sentences. However, antifoundationalists might be quick to point out that meaning is rarely or never so simple. They might impatiently wave all this lower level stuff aside and argue that the real construction of meaning occurs at higher levels and that these levels are constrained only by the discourse community, which in turn rests on nothing but blind contingencies, etc. But that's an empirical question. If it can be shown that final inferences are directly tied in rule governed ways to denotative meanings, then at least that part of the antifoundationalist argument is effectively countered. In the following section, we will look briefly at a model (Katz/Dowling) that ties "second order" meaning to denotative, or first order meaning. As we will see, I have a few quibbles with the position Dowling takes, and maybe with Katz as well, though, at this point, I am undecided about Katz'major claim about the Reality of language. But, it seems to me that we can use Katz' system without committing ourselves to all of his metaphysical claims[117]. Regardless of the Real status of intensions, if it can be shown that denotative meaning

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[117] Again, I think Katz would disagree, though he himself once thought semantics could be naturalized. I agree with Katz that at some point we have to accept Reality; for me that point is the defense of reason and truth. Katz' argument, if I understand him, is that intensions *necessarily* overlap with logical relations in such a way that they too must be accepted as Real. Again, I agree with the overlap, but I'm still not sure that accepting that overlap as Real commits us to accepting *all* of semantics as Real.

can be reliably recovered, and that from there one can work in principled ways at recovering the author's intentions, without undue reliance on, or constraint from, one's discourse community, then I will have provided a minimal foundation for literacy studies, which is all I set out to do.

The alert reader may have noticed that, without fanfare, I have located meaning in the author's intentions, though I am also claiming that these can be reliably recovered from the text—hence I am endorsing an author-text position on meaning. This has come about naturally as a result of accepting I-language, and as a consequence of accepting many of Davidson's arguments. It seems to be the case, that our principle opponents—Rorty and Fish—do not oppose my position on intentions, although they do argue that such intentions are usually out of reach (see for e.g., Fish, 1989A; Rorty, 1998A) However, they regard texts as being if not arbitrarily, then, at best mysteriously related to intentions. Thus, according to them, although intentions do originate with authors (albeit "situated" authors), interpretation of texts depends entirely on discourse communities, and, therefore, intentions are lost. Their position regarding intentions, then, is like the strong instrumentalist's regarding scientific discovery. Just as the strong instrumentalist denies access to reality as it really is, the antifoundationalist literary critic denies access to authorial intentions. And the parallel continues as we consider the minimal foundationalist's counter argument. Just as we argued regarding scientific discoveries that better models are best explained by assuming them to be closer to reality than models that explain less, so we will take the position that interpretations that best explain a text—including its internal relationships, its relationships to other contemporary texts, and to

historical and linguistic facts—are probably closer to the author's intentions than interpretations that explain less. In both cases, the antifoundationalist can argue that ultimate proof—of the shape of reality in science, or the author's intentions in literacy studies—is unavailable. We concede the point. However, we also argue that strong evidence is "more firm and stable than mere belief or unexamined practice," and that therefore, whenever such can be provided, antifoundationalism is countered. Happily, it turns out that with the right tools, it is usually not hard to provide strong evidence for reasonable interpretations. Or so I will argue. Let us look, then, at the Katz/Dowling model.

The Katz/Dowling Model of Interpretation

In his recent book *The Senses Of The Text* (1999) William Dowling has taken Katz' New Intensionalism, which Katz has been arguing for at least since 1981, and applied it to literary interpretation. Much of this Katz/Dowling system is relevant to the problems of interpretation raised by Davidson, and the system can also be made compatible with minimal foundationalism, though it must be altered in some important ways. Katz is not concerned with literacy in his work, so a path must be developed from that work to literacy studies. Since Dowling has already done a fine job of explicating and applying Katz' system to literacy studies, I will draw often from his text, rather than developing my own path from Katz, since I think I would come to many of the same conclusions Dowling has. And I will be brief since the reader can access the original texts easily enough. My purpose here, then, is not to thoroughly explore either Katz' system or Dowling's application of it, but rather to connect this system to minimal

foundationalism, pointing out along the way places where a minimal foundationalist might choose to alter the system, and to then show just enough of how it works to apply it to a couple of texts.

The heart of the system is the claim that words, and larger constituents, have *senses*, or intensions, which are complex, precise, and available to all native speakers just as a result of knowing the language. Katz' most controversial claim is that these intensions are Real, in the Platonic sense. While I find this idea intriguing, I would say from the outset that it seems fairly extravagant, far beyond "minimal" in our terms. It will be helpful here to recall the minimal foundationalist attitude toward reason. The most minimal position we can take vis-à-vis reason is simply to say that it is an inevitable human function, without committing ourselves to any doctrines regarding reason's Reality or etiology. However, to avoid paradox, we also need to accept reason as a veridical function, and it is at this point that minimal foundationalism is connected to Reality. I have argued that there is no sense claiming, as Jackendoff does, that truth is merely a human function, since if that step is taken, argument becomes irrelevant; there is no reason to accept argument A) over argument B) based on the fact that A) is more "reasonable," if reason itself is merely a human function. So, reason is connected to Reality at its intersection with truth. Following Kant, I also endorsed the idea of *a priori*, necessary truths, such as those of arithmetic. To be clear about this, what makes something Real, is its independence from our sensory apparatus. We need truth to be Real to avoid paradox in reasoning, and therefore, accepting truth as Real is a minimalist move.

Now, where Katz' system overlaps with this is in its focus on entailments in semantics. Drawing from examples that go back at least to Kant, such as *bachelor entails unmarried man*, Katz argues that intensions are Real because they reflect *necessary* entailments; there simply is no way to understand *bachelor* without accepting the entailment that every *bachelor* is an *unmarried man.* As far as such entailment goes, I would agree that intensions are Real, in the sense that they are connected to logic, and logic to necessary truths. However, it seems that Katz has a lot more than this in mind. He apparently wants to include not only the entailments, but the words and concepts themselves in Reality, and I'm not sure this is necessary. Once we have such things as men, women, and institutions of marriage, each of which has entailed properties, we can talk about possible and necessary relations among them, and these relations, in turn, overlap with logic. But that's just a property of logic in the abstract, one that is only coincidentally related to the things themselves, or at least so it seems to me at this point. Moreover, there are important aspects of senses which can be explained perfectly well within a Natural framework, and the latter is more minimal than a Realist framework because it assumes less. Again, following Kant, if our perceptions are subjective, then it seems that most aspects of intensions should be explained as reflections of our apparatus rather than direct hooks into some Reality. Thus, our descriptive terms for sensory experience—color, shape, feel, taste, sound, etc.—are undeniably reflective of our sensory apparatus, but whether they are Real or not, is something we can't answer, and therefore shouldn't assume, unless it can be shown that it is logically necessary to do so. At this point I don't see that it is, so I prefer a natural account. But Katz may yet convince me.

Fortunately, however, it seems to me that the Realist claims in Katz system can be minimized, as I suggested above, by accepting the logical aspects of senses as Real, without committing ourselves to such things as Platonic bachelorhood. Making this move allows us to retain the many powerful explanatory features of Katz' system without exposing us to extravagant, possibly indefensible claims (Raving Platonism). The crucial point in Katz' system is that words—and, by composition, larger constituents—are complex conceptual units that reflect regularly occurring logical relations such as super/subordination. As Dowling explains:

> one never simply grasps an isolated concept as such, but always and simultaneously a descending and ascending order of relations:
> Object → Physical → Artifact → furniture → Table →coffee table → mahogany coffee-table-with-Uncle Leonard's-cigarette-burn (etc).

Thus, any use of the word "furniture" entails the facts that all pieces of furniture are artifacts, and all tables[118] are furniture. There are a limited number of such entailment relations, and they are universal. Thus, virtually all lexical items (as opposed to functional) are embedded in sub/superordinate relations, as the reader can easily verify for him/herself. Other intrinsic logical/semantic relations that are universal, and which perhaps are closer to consciousness for most people are "synonymy, antonymy, ambiguity, redundancy, similarity in meaning, meaningfulness, and meaninglessness" (Katz, 1990, p. 30). Katz cites this list as an example of "folk semantics," arguing, in a Chomskyan vein, that the science of langauge can and should proceed from an examination of "the notion of meaning in ordinary disuptes" (ibid, p. 32). Importantly, though nonlinguists may not have conscious knowledge of such semantic relationships, it

is easy to show, from their use of language, and by simple psycholinguistic tests, that they have procedural (or in Chomsky's terms, I-language) knowledge of them.

Katz' main task is to argue for a Realistic framework; though this is a fascinating topic, we will not pursue it here. Rather, I want to show how his system can be applied to interpretation, which is also Dowling's concern.

There are two things that are crucial for the system to work: 1) that we can establish that native speakers share intensions to a strong degree, and 2) that there be a regular, rule governed way, or at least a rationally constrained way to get from the denotative level to the secondary level of implied, or implicated, or metaphorical (or however changed) intentions that interpreters (in the broad Davidsonian sense, including writers) often attach to text. As for the reality (small r) of the denotative level, I would like to stipulate this, and certainly I could do so among linguists. Any reasonable person must admit that native speakers agree to an overwhelming degree on denotative meanings for most common words, including their entailment relations (check *bachelor* and *furniture* for example). This doesn't mean that there is never trouble—there is—but on the whole there is vast agreement. As far as I know, no linguist disagrees with this. However, antifoundationalists, specifically Fish, argue that even such facts as these can be explained as examples of "situatedness." I will present as much reasonable argument as space allows against this position, but, eventually, it is probably the sort of thing that must simply be accepted on the preponderance of evidence, or not.

There is a great deal of interesting detail about intensions that I will not cover here, in the interest of space, but I stress that intensional space is extremely rich and

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[118] Except, of course, tables of text or figures, or water, maybe others. But homonymy is irrelevant to the

widely shared across native speakers. The richness of intensional space is particularly important in the light of Davidson's and Fodor's doubts about how the mind can "abductively" arrive at correct inferences, because it provides a way to mechanically restrict the search space rather drastically. We still have to accept a large "creative" component of interpretation, but if denotative meaning is as rich as Katz claims, and mechanically derivable as a result of knowing a language, then perhaps the creative leaps that interpreters make are smaller than they appear to be to people like Davidson. Moreover, if Grice is correct about universal principles of cooperation, and people have built in correspondence notions of truth, then these principles in conjunction with a restricted search space greatly reduce the contribution we have to attribute to abductive reasoning.

Types and Tokens

In Katz' system "abduction" is a constrained process that begins after recovery of denotative meaning. Katz explains this process as a rule-governed extrapolation from the "type" level to the "token" level, which terms he borrowed from Charles Pierce. Katz explains the importance of the distinction:

> On their face, facts about sense involve no reference to speaker, utterances, or inscriptions.
>
> There is a reason for this difference. Facts about the sense of expressions of a language are facts about linguistic *types*, whereas facts about use, being facts about what people do with their articulatory organs or hands at particular time and place, are facts about lingustic tokens. Charles Sanders Peirce drew the distinction between types and tokens this way:
>
> "there will ordinarily be about twenty "the's" on a page, and of course they count as twenty words. In another sense of the word "word," however, there is but one "the" in the English language;

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point at hand.

> ...it is impossible that this word should lie visibly on a page or be heard in any voice."
>
> (Katz, 1990, p. 39).

In Katz' system the type level is identified with, but not identical to, "literal" meaning and the token level is identified with "speaker" meaning, which helps explain how denotative meaning (which is the same as type meaning) constrains "abduction." Katz explains the difference between literal and nonliteral use as the difference between tokens that converge with the type level and tokens that don't:

> In a case of literal use, the speaker grasps the meaning of an expression type and intends to get the audience to recognize his or her intention to have them assign that meaning (which, since it is objective, they can also grasp) to the token of the expression type that he or she has produced. In a case of nonliteral use, the speaker grasps two meanings: the meaning of the expression a token of which will be used in the communication, and the meaning to be communicated. These meanings are related by a series of pragmatic inferences on the basis of which the audience can start with the former meaning and come to recognize the speaker's intention to have them assign the latter meaning to the token.
>
> (ibid, p. 153)

Before we look at how pragmatic inferences are achieved, there are some interesting psycholinguistic ramifications of this type/token model that bear on our understanding of meaning. The type level of words and larger linguistic constituents is an abstract level, rich with intension, but devoid of detail. Thus in the sentence *The cop shot the robber with the rifle* "cop," "shot," "robber," and "rifle," all come with entailment relations, as discussed above, but no particular attributes. The cop can be short, tall, old, young—can, in fact vary an infinite number of ways, and likewise for the other lexical items. This fact reveals an interesting aspect of linguistic communication that is not often noticed. Interlocutors convert tokens to types and vice versa without even realizing it. If, to use

the *cop/robber* sentence as an example, interlocutor A) witnesses the drama, for him, the the scene and all the players in it are experienced and mentally represented at the token level: this *particular* cop, robber, shooting, and rifle. Yet, when he narrates the tale to interlocutor B), most or perhaps all of the detail is removed; that is, what he communicates is a representation at the *type* level, even though his own representation is at the *token* level. Because we do this effortlessly, we don't notice anything special about it, but it is remarkable. In comprehending the scene, interlocutor B) perhaps creates a token level scene, but it isn't necessary for him to do so to understand A)'s utterance, nor, if he does do so, will he confuse that created token with the real thing. An important point about all this is that the common view of meaning, wherein interlocutor A) has a thought which he then converts into words which interlocutor B) hears and reconverts to the intended meaning, i.e., A)'s original thought, can't be right. Rather, interlocutor A) not only converts his thoughts into words, but, at least in many cases, converts representations he holds at the token level into representations at the type level, and this is all interlocutor B) receives.

Pragmatic Scatter

Because token meaning is derived from type meaning in Katz' system, and type meaning is automatically given to native speakers, the system offers us another way to reduce the search space rapidly, which is what we need to explain abduction. I will speculate here that it may very well also be the case that extrapolation from type to non-literal token occurs in a hierarchically rule governed way. That is, it seems likely that upon encountering a text (anything from a single word or an extended discourse) whose type meaning(s) does not seem appropriate, the interpreter begins extrapolating from the

most important pieces of text first, and then moves down the line until something fits. However, this would only be in the case of "all things being equal," and that case is probably rare to non-existant. This doesn't render the generality vapid, but it does make it very hard to investigate. In actual cases, there are always clues—which, however, are "abductively" apprehended—that tell the interpreter where to focus.

This process of moving from type to non-literal token is referred to by Katz, and then by Dowling, as "pragmatic scatter." Katz has taken this idea almost directly from Grice, so it is a familiar idea to us. Recall that the core of Grice's system is the Cooperative Principle. Grice argues that if we assume this principle we can explain how speakers arrive at correct non-literal interpretations of utterances:

> ...what is implicated is what it is required that one assume a speaker to think in order to preserve the assumption that he is observing the Cooperative principle.
>
> (Grice, 1989)

Again, I am going to scant the details here since to do justice to them would require a lengthy summary. But I think the principle is intuitively obvious. We all use it whenever we speak ironically, or sarcastically, tell jokes, or make allusions. What Katz has added to this explanation is his expanded account of intensions. Together, these principles provide more than enough theory to refute antifoundationalist claims that interpretation is nothing more than the application of current "interpretive community" norms and assumptions. Perhaps the best way to show this is to reanalyze some of the claims that antifoundationalists have made regarding specific texts, so, in the last chapter, I will briefly do that, starting with Fish's famous essay, *Is there a text in this class?*

X: RESURRECTING THE TEXT

The minimal foundationalist position we have come to regarding meaning is that meaning originates in authors, probably in a language of thought, and is then conveyed through natural language and pragmatic cues to interlocutors. Importantly, in this conception, there is no felt divide between linguistic and pragmatic aspects of meaning, though we have good reasons for supposing that there is such a divide at the computational level. Thus, as Olson observed, interlocutors focus on what the *speaker* means, not what the words mean; yet, speakers rely on the relative stability of intensions in language to convey their thoughts. As Grice, Katz, and many others have stressed, it is precisely *because* language is stable that we can exploit it pragmatically to achieve non-denotative meanings. The antifoundationalist position happily accepts the pragmatic part of this story, but it tries to deny that there is any denotative level of language that is not ruled by pragmatics, which is the same as saying there is no denotative level of language.

A Fish Story

Stanley Fish's essay, *Is there a text in this class,* is a classic exposition of this position. In that essay, Fish relates this story:

> On the first day of the new semester a colleague at Johns Hopkins University was approached by a student who, as it turned out had just taken a course from me. She put to him what I think you would agree is a perfectly straightforward question: "Is there a text in this class?" Responding with a confidence so perfect that he was unaware of it (although in telling the story, he refers to this moment as "walking into the trap"), my colleague said, "Yes, it's the *Norton Anthology of Literature,"* whereupon the trap (set not by the student but by the infinite capacity of language for being appropriated) was sprung: "No no," she said, "I mean in this class do we believe in poems and things, or is it just us?"
>
> (Fish, 1980, p. 305)

The interesting thing about this narrative is that it perfectly illustrates the workings of pragmatic scatter and therefore strongly supports a minimal foundationalist position; yet, it is being offered by Fish, confidently, as a "glorious" argument (in Humpty Dumpty's sense) for antifoundationalism. The reason for this difference is that Fish does not understand that the denotative level is not only not incompatible with the radical shifts of meaning he revels in, but that, in fact, it is the denotative level that makes these shifts possible. Fish thinks that one can either believe in stable denotative meanings *or* believe, as he does, in the "infinite capacity of language for being appropriated," but not both. Fish thinks this because, like the later Wittgenstein, he is heavily oriented toward a "use" perspective of language, which convinces him that language can never occur outside some particular context. In Katz' terms, Fish believes in tokens, but absolutely disbelieves in types. Because he thinks this way, he thinks that people who argue for denotative meaning are arguing for decontextualized meanings at the *token* level (because that's the only level he can imagine), and, since it is obvious to him that there are no decontextualized tokens (correct), he concludes that denotative meanings are foundationalist fantasies and that *all* meaning is determined, or in his words "predetermined" by the discourse "situation." My strategy here will be first to show that Fish really does see things this way, and then to show why even his analysis supports Katz' model, or something like it, better than his own.

Prepatory to his argument, Fish lines out what he thinks is the opposing position:

> Of the charges levied against what Meyer Abrams has recently called the New Readers (Derrida, Bloom, Fish) the most persistent is that these apostles of indeterminacy and undecidebility ignore, even as they rely upon, the "norms and possibilities" embedded in the language, the "linguistic meanings" words undeniably have, and thereby abandon "our ordinary realm of experience in speaking,

> hearing, reading and understanding" for a world in which "no text can mean anything in particular" and where "we can never say just what anyone means by anything he writes" The charge is that literal or normative meanings are overriden by the actions of willful interpreters.
>
> (ibid, p. 305; internal quote attributed to Abrahms, 1977)

Notice, again the dichotomy Fish is setting up: either we have stable meanings *or* we have indeterminate ones. Fish continues to stress this point. The quote follows directly:

> Suppose we examine this indictment in the context of the present example. What, exactly, is the normative or literal or linguistic meaning of "Is there a text in this class?"
>
> Within the framework of contemporary critical debate (as it is reflected in the pages, say, of *Critical Inquiry*) there would seem to be only two ways of answering the question: either there *is* a literal meaning of the utterance and we should be able to say what it is, or there are as many meanings as there are readers and no one of them is literal. But the answer suggested by my little story is that the utterance has *two* literal meanings: within the circumstances assumed by my colleague (I don't mean that he took the step of assuming them, but that he was already stepping within them) the utterance is obviously a question about whether or not there is a required textbook in this particular course; but within the circumstances to which he was alerted by his student's corrective response, the utterance is just as obviously a question about the instructor's position (within the range of positions available in contemporary literary theory on the status of text. (ibid, p. 306)

Here, Fish's definition of "literal" is virtually identical to Davidson's in the latter part of *Derangement*. Davidson's "deranged" definition was that "literal" was whatever the interlocutors agreed on at the moment. Fish, puts the definition in different terms, but his definition seems to be substantively the same as Davidson's "deranged" definition. Fish ties "literal" to what the current circumstance *allows*, but by including the interlocutors themselves in his construal of "circumstance," he limits his definition to what the interlocutors agree on, or in his terms, what is "available to them." In any case, as with Davidson's, Fish's definition effectively eviscerates a useful term. If "literal" is equated

with interlocutor meaning, then we have no way to differentiate between, say, metaphorical and literal uses of a word, but we clearly need to be able to do so. In less "deranged" moments, Davidson sees this, as when in *Metaphor* he argues that

> a woman who believed in witches but did not think her neighbor a witch might say, 'She's a witch', meaning it metaphorically; the same woman, still believing the same of witches and her neighbor but intending to deceive, might use the same words to very different effect. Since sentence and meaning are the same in both cases, it is sometimes hard to prove which intention lay behind the saying of it...
>
> (Davidson, 1984, p. 258).

Davidson's point is that it is a psycholinguistic fact that people can use the same words *literally* or metaphorically, and that speaker meaning will change accordingly. Fish doesn't deny that the same words can be used to express different meanings, but he wants to deny that there is any special "base" meaning of words that we can call "literal." This is a crucial difference between Fish and our minimal foundationalist position; in fact, everything hinges on it. Everyone agrees that words can express many different meanings, but linguists argue that some of the meanings are special, in the sense that they are "given," whereas Fish maintains that no meaning is ever given, but rather, that all meanings are equally dependent on context. Now, Fish bases this claim on the assertion that words are always apprehended in contexts. There are two things wrong with this. First, it isn't true that all words are always apprehended in contexts, at least not in communicative contexts. Pierce's citation of "the" as a type, is not construed in any particular context. To the contrary, in making his point about types versus tokens, Pierce depends on our ability to consider words *out of context*, and it does seem that we can do so. Second, even if it were true that all words were always encountered in contexts, it wouldn't necessarily follow from that that all of our *senses* about those words were

derived from contexts. Kant's subtle argument against pure empiricism applies well to the linguistic situation: "But though all knowledge begins with experience, it does not follow that all knowledge arises from experience." And, of course, this is Chomsky's argument for UG, and Katz' argument for Real intensions. Chomsky's evidence for a priori linguistic knowledge is the fact that people can comprehend and construct an *infinite* set of syntactically well-formed sentences, and that, therefore, they *must* be constructing them according to an internal rule system. Transferred to semantics, the argument is that people can construe and construct and infinite set of semantically well-formed sentences, and that, therefore, they *must* be using some internal set of intensions and rules for combining them. Both the syntactic and semantic arguments are further bolstered by the fact that people also have very strong syntactic and semantic intuitions about sentences, regardless of their background, and that most of these intuitions are manifested at a very young age.

Nativism in Category Recognition

In fact, during lexical acquisition children *universally* demonstrate knowledge of strong semantic principles such as sub/superordinate relationships. Brown (1958) first called attention to this in his paper, "How Shall a Thing Be Called" (Lakoff, 1987, p. 31). In that paper and subsequent work, Brown observed that there seems to be a universally recognized "middle" category that children immediately grasp. Thus, when children are presented with objects, which are named for them, they easily generalize to a middle category. As Lakoff says:

> The picture Brown gives is that categorization, for a child, begins "at the level of distinctive action," the level of flowers, and cats, and dimes, and then proceeds upward to superordinate categories (like *plant,* and *animal*) and downward to subordinate categories (like

> *jonquil* and *Siamese*) by "achievements of the imagination." "For the latter categories there seem to be no characterizing actions" (Brown 1965, p. 32). this "first level of categorization was seen by Brown as having the following converging properties:
>
> --It is the level of distinctive actions.
>
> --It is the level which is learned earliest and at which things are first named
>
> --It is the level at which names are shortest and used most frequently
>
> --It is a natural level of categorization as opposed to a level created by "achievements of the imagination."
>
> (Lakoff, 1987, p. 56)

Plausibly, without such principles to guide them, children could *never* learn to use a natural language. Lakoff cites eleven sources who, though divergent in many respects, all support nativism in intensions. Importantly, Lakoff is a Naturalist with little or no sympathy for Realism, and a good bit of sympathy with social constructivism; thus, he cannot be faulted for having a Realist axe to grind. Nonetheless, the research he cites unambiguously supports a world of intensions that is Given, though only in the Kantian subjective sense. Included in this world are the following concepts, each associated with a different philosopher or researcher, as I've indicated in the bracketed insertions:[119]:

> *Family resemblances*: the idea that members of a category may be related to one another without all members having any properties in common that define that category. [Wittgenstein]
>
> *Centrality:* The idea that some members of a category may be better members of that category than others. [Brent Berlin and Paul Kay]
>
> *Polsyemy as categorization:* The idea that related meaning of words form categories and that the meanings bear family resemblances to one another. [J. L. Austin]

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[119] The scholars cited are identified by Lakoff, later in his text.

Generativity as a prototype phenomenon This idea concerns categories that are defined by a generator (a particular member or subcategory) plus rules (or a general similarity). In such cases the generator has the status of a central, or "prototypical," category member. [Floyd Lounsbury]

Membership gradience: The idea that at least some categories have degrees of membership and no clear boundaries. [Lotfi Zadeh]

Conceptual embodiment: The idea the properties of certain categories are a consequence of the nature of human biological capacities and of the experience of functioning in a physical and social environment. It is contrasted with the idea that concepts exist independent of the bodily nature of any thinking beings and independent of their experience. [Paul Ekman]

Functional embodiment: The idea that certain concepts are not merely understood intellectually; rather; they are used automatically, unconsciously, and without noticeable effort as part of normal functioning. Concepts used in this way have a different, and more important, psychological status than those that are only thought about consciously. [Kay and McDaniel; Berlin; Ekman]

Basic-level categorization: The idea that categories are not merely organized in a hierarchy from the most general to the most specific, but are also organized so that the categories that are cognitively basic are "in the middle" of a general-to-specific hierarchy. Generalization proceeds "upward" from the basic level and specialization proceeds "downward." [Roger Brown; Eleanor Rosch]

Basic-level primacy: The idea that basic-level categories are functionally and epistemologically primary with respect to the following factors: gestalt perception, image formation, motor movement, knowledge organization, ease of cognitive processing (learning, recognition, memory, etc.), and ease of linguistic expression. [Ekman; Brown; Rosch; Berlin]

Reference-point, or "metonymic," reasoning: The idea that that a part of a category (that is, a member or subcategory) can stand for the whole category in certain reasoning processes. [Rosch]

(Lakoff, 1987, p. 12-13)

This broad sampling of research strongly supports the idea of *intrinsic* semantic relationships that are represented in natural languages, largely by the way concepts are

distributed across the lexicon. Because these categories and semantic relationships are basic, they impose strong constraints on the lexicon, which is good since people need reasonable stability in the lexicon to be able to communicate. All of this strongly supports a denotative level of meaning since, according to this view, word meanings are not so much invented as *assigned.* What this says is that any arbitrary human population will have words for things like body parts, flora and fauna, time, space, etc., and these words will, at least at the denotative level, reflect the basic Given categories and semantic relations that are common to all humans. Crucially, though, there are no claims to "correctness" of definitions in the sense that the words reflect reality as it really is. To the contrary, most of the above research is strongly Kantian, supporting the view that essential categories are tied to the human apparatus, not reality as it really is. Of course, Katz strongly disagrees; yet as, argued above, his system works under a Natural construal, so we will leave the problem of Reality out of the discussion for now.

Another crucial point here is that there are no claims being made about absolute stability of denotative meanings. What is claimed is that most semantic relations and categories themselves are stable and universal. However, it is well known that there is semantic drift in natural languages such that words change meaning over time. There is no contradiction here. Given the Saussurian principle of arbitrary sign/symbol relationships, and the fact of I-language development, one individual at a time, both of which are well established in linguistics, we should expect semantic drift since each new language learner assigns essential categories to words under unique circumstances. Thus, we get both broadening and narrowing effects in the lexicon. Famous examples of broadening are extensions of brand names, e.g., coke (coca cola) for any cola drink,

kleenex, for any facial tissue, etc. Cases of narrowing include *meat* which once referred to any food; *girl* which once referred to children of both sexes, and *corn* which was formerly used for all grains (Wolfram, 1998, p. 59). This sort of language change occurs regularly in all languages. However, semantic drift is fairly slow, affecting only a few lexical items at a time. Moreover, written language and academic standards strongly retard drift. Thus, most words retain strong denotative identities that are widespread in the population.

Fishy Argumentation

Against this evidence, Fish has only assertions and very weak arguments. In fact Fish's main argument doesn't detract from the denotative view at all, though he thinks it does. Fish thinks that the fact that interpreters almost instantly grasp meanings, and that these meanings can change as a result of further input from the interlocutors is evidence against a denotative level of language, because he assumes denotation is incompatible with pragmatic dominance. The following quote continues from the above:

> Notice that we do not have here a case of indeterminacy or undecidability but of a determinacy and decidability that do not always have the same shape and that can, and in this instance do, change. My colleague was not hesitating between two (or more) possible meanings of the utterance; rather, he immediately apprehended what seemed to him to be an inescapable meaning, given his prestructured understanding of the situation, and then he immediately apprehended another inescapable meaning when that understanding was altered. Neither meaning was imposed (a favorite word in the anti-new-reader polemics) on a more normal one by a private, idiosyncratic interpretive act; both interpretations were a function of precisely the public and constituting norms (of language and understanding) invoked by Abrams. It is just that these norms are not embedded in the language (where they may be read out by anyone with sufficiently clear, that is, unbiased eyes) but inhere in an institutional structure within which one hears utterances as already organized with reference to certain assumed purposes and goals. Because both my colleague and his student are situated in

> that institution, their interpretive activities are not free, but what constrains them are the understood practices and assumptions of the institution and not the rules and fixed meanings of a language system.
>
> (Fish, 1980, p. 306)[120]

What makes this analysis somewhat convincing is the fact of the change in meaning, and Fish's (correct) attribution of that change to assumptions in the discourse community—in our terms, to "pragmatic scatter." Where he is wrong is in his assertion that none of the interpretation relies on "rules and fixed meanings in a language system." Note also that in making this assertion, he is departing from antifoundationalism because he is asserting something definite about language. Fish is claiming dogmatically here that there is no level of denotative meaning, but he has no evidence whatsoever to support that position. Moreover, without denotative meaning, his story becomes untenable. He has no account for *how* people interpret utterances. All he can say is that they do so according to community norms. But try to imagine how this could work without a denotative level of language. Under such assumptions, when you hear a word, how do you assign meaning to it? According to Fish's story, you will have to sort through all the instances where you've heard that word before, and decide which of all the many ways that people have used that word is appropriate. What could possibly guide such a process? Now of course Fish says interpretation is not a matter of "decision making," but rather, an automatic reaction, guaranteed by the context and the assumptions of the interlocutors. But this only begs the questions. *How* does the context interact with the assumptions and the linguistic stimulus? Fish has no psycholinguistic account of any kind. He may as well say it's done by magic. Note also, that under his system we cannot account for

phonological, morphological, or syntactic recovery either, for according to him there are no "rules" in the language system. Thus, there are no phonemes, no morphemes, and no syntactic structures. Utterances are whole chunks that are just magically "understood" according to community norms. Perhaps Fish would want to deny this; perhaps he would like to accept the lower level processes that allow recovery of words and sentences, and just restrict meaning to pragmatics. But surely we should ask why. On what grounds should we accept lower level, rule governed processes, all of which are essential to recovery of meaning, and then turn around and reject such processes for the "higher" level of construing meaning? This story is fatally inconsistent as anyone should be able to see.

A Linguistic Take on the Fish Story

On the other hand, we (minimal foundationalists and linguists) have a compelling, elegant account that is supported by massive amounts of evidence from linguistics and psycholinguistics. Intensional space is tightly constrained by innate categories and semantic relations, and the lexicon reflects these constraints, and, furthermore, is *limited.* Thus, although there is an infinite set of possible sentences, reflecting an infinite set of meanings, these are formed by iterations of a limited set of lexical items according to a highly constrained internal grammar that is shared by native speakers. These conditions allow almost instantaneous construction of denotative representations, precisely because such can be accomplished mechanically. In conjunction with this "machine," however, is a creative capacity that "scatters" the representations as necessary for the infinite range of communicative purposes humans

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[120] This argument is similar To Gadamer's *Horizontverschmezung* in *Wahrheit und Methode.* For

come up with. It's a wonderful system that marries "machine" to "abduction" and thus gives us the best of both.

Let's look at how this account explains Fish's anecdote. For ease of presentation, I will speak as if there were a fixed temporal order such that denotative meaning was recovered first and then pragmatic scatter allowed to operate, but, crucially, we do not believe this. The psycholinguistic evidence, as cited above, shows that these processes occur in parallel and also interact.

O.K., so Fish's colleague, let's call him FC, hears the utterance "Is there a text in this class?" under the circumstances Fish describes. On our account, this sentence has one very strong denotative meaning, and perhaps two or three other very weak ones, because the lexical items "text" and "class" are both somewhat ambiguous. By itself, "text" can mean either printed words, or, in some cases oral speech. But the indefinite article "a" eliminates these possibilities because the indefinite article only attaches to count nouns, whereas the former possibilities are mass nouns. For most contemporary native speakers this leaves only one choice, $text_1$: a textbook. The other noun "class" is also potentially ambiguous, between $class_1$: an academic course; $class_2$: a social or academic class; $class_3$: a set or collection containing members of the same category; $class_4$: elegance of style or taste; perhaps others. This is where it is convenient that pragmatics is allowed to influence lexical selection. No doubt, Fish is right that FC is predisposed to hear something related to classes ($class_1$), and, since this selection is strongly supported by affiliation with the textbook definition of text, he will be guided to

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commentary on Gadamer, see Hirsch, 1967.

that interpretation for "class," while "text" as "textbook" will, reciprocally, be reinforced by $class_1$. Note that, in this case, pragmatics operates to constrain competing denotative representations; at this point there is no scatter involved. Note also that although we have arrived at precisely the same place Fish did in our account of FC's first interpretation of the student's utterance, we have a much better account than Fish does. We can explain in detail *how* the interpretation came about, and in doing so we make falsifiable claims. For example, we claim that the definition of "text" is constrained syntactically by the indefinite article. This is empirically testable, through psycholinguistic tests. As it happens, there is already plenty of evidence to support such effects, but should anyone doubt our account, they have means to check it. But there is no way at all to falsify Fish's account.

We're now at the point of scatter. The student replies to FC, "No, no, I mean in this class do we believe in poems and things or is it just us?" Now, per Grice, FC will reason internally[121] as follows: "This student is seeking information about my class, information that is somehow related to the word *text;* however, she doesn't mean 'textbook.' Whatever could she mean?" At that point, plausibly, FC's linguistic processor is rapidly computing all the other denotative representations, eliminating them one by one as they fail to match the context. At the same time, FC is ruminating on the student's corrective comments. His interpretation of "class" as $class_1$ has been vindicated, but there seems to be something wrong with "text" since the student is talking about "poems and things." At this point the word "text" becomes highly suspect, and, since denotative alternatives have all been found wanting, the process of scatter is

unleashed. Somehow, "text" has to be made to fit the student's second question. Once again, there are some hints from syntax. The student has referred to belief in "*this* class" perhaps with a little added stress on "this," and has also used the conjunction "or" ; This syntax and stress pattern implies comparison. That interpretation is also supported by pragmatic conjecture since, plausibly, the student has taken other classes. Moreover, the preposition "in" in the original utterance was always odd. Had the utterance been about a "textbook" the preferred preposition would have been "for"—"Is there a text *for* this class?" So all of these clues lead to "text" in the pragmatically awakened "belief in stable texts" sense. At this point, our analysis converges with Fish's, though, note, we have a plausible, detailed account of how we got here, whereas he has nothing. In any case, we agree with Fish at this point, at least about the pragmatic influences, though we disagree that denotative meanings have dropped out of the calculation. To the contrary, as FC considers other instructors' positions, the word "text" remains suspect number one precisely *because* the denotative alternatives don't work. Without a denotative base, FC wouldn't be so focussed on that word. But, because FC is so focussed, he picks up on the syntactic clues, and these together with his acquaintance with Fish, prompt the pragmatic alternative definition for text, "a belief in stable texts." He tries that on for size, and, voila! comprehension. Crucially, note that on both our account and Fish's there was never any more than *one* speaker meaning, though Fish confused that point by referring to *two* "literal" meanings. Again, our account elegantly accords with the facts, while his does not. Under our account, meaning is psychologically unitary, though computationally synthetic. That is, inasmuch as meaning is produced consciously by

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[121] I assume that most of this reasoning is below the level of consciousness.

individuals, an assumption we need in order to explain the psychological facts (i.e., of felt intentions to communicate), we should consider meaning to be unitary. On the other hand, we have good theoretical reasons, as outlined here, for believing that there are dedicated, somewhat independent modules and processes at work in producing and recovering meaning. Thus, re Olson, we consider meaning to be a communicative intention and there is thus, *one* speaker/author meaning or unified set of meanings per utterance/passage, at least in the usual case. Texts, oral and written, on the other hand, are typically multiply ambiguous, even at the denotative level. However, through the sort of analysis done above, most ambiguities can be resolved to a high degree of certainty. In oral communication, interlocutors can directly verify their interpretations. With written text, the situation may be less secure, though not always. Skilled writers employ a range of devices for constraining meaning, including careful use of vocabulary and syntax (to constrain denotative meanings), reiteration of important points, sylistic devices for creating emphasis, and artistic, tightly woven patterns for guiding scatter. Though "absolutely guaranteed interpretations" are never possible, skilled readers can assemble evidence for their interpretations, and when that evidence is strong, they can argue for one interpretation over others from an objective base.

It is at this point that our analysis clashes most significantly with Fish's. We tie the text to the author's intentions through the processes of denotative recovery and pragmatic scatter. Thus, we have basis for formally analyzing texts and a means for deciding among competing interpretations. Under Fish's model, on the other hand, interpretation is tied exclusively to the interpretive situation, so there is no way to adjudicate among competing interpretations. Fish is well aware of this charge against his

model, and he purports to answer it, but his answer is devious. Here's his synopsis of the argument against his position:

> ... the problem of communication, as someone like Abrams poses it, is a problem only because he assumes a distance between one's receiving of an utterance and the determination of its meaning—a kind of dead space when one has only the words and then faces the task of construing them. If there were such a space, a moment before interpretation began, then it would be necessary to have recourse to some mechanical algorithms procedure by means of which meanings could be calculated and in relation to which one could recognize mistakes. What I have been arguing is that meanings come already calculated, not because of norms embedded in the language but because language is always perceived, from the first, with a structure of norms. That structure, however, is not abstract and independent but social; and therefore it is not a single structure with a privileged relationship to the process of communication as it occurs in any situation but a structure that changes when one situation, with its assumed background of practices, purposes, and goals, has given way to another. In other words, the shared basis of agreement sought by Abrams and others is never not already found, although it is not always the same one.
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> (ibid, p. 318)

I don't see why it matters whether or not there is a "dead space" "between receiving of an utterance [or written text] and determination of its meaning." In fact we agree that there isn't any such space, though we also argue that the reason for that is that we have dedicated linguistic machinery—as well as pragmatic inclinations—that go to work immediately on linguistic input. And as for "meanings come[ing] already calculated, well, we don't care about that either. What is important to us is that we get to the *speaker's* (or author's) meaning. And that, by the way is what we mean by "shared"—that the interpreter shares the *speaker's* meaning. Since we also think that in general there is only *one* speaker meaning, Fish's last phrase "although it is not always the same one," just does violence to the concept "shared."

Relativism Relativized

Fish is aware of these objections, but he thinks he can appropriate them to his own purposes. The quote follows directly from the above block:

> Many will find in this last sentence, and in the argument to which it is a conclusion, nothing more than a sophisticated version of the relativism they fear. It will do no good, they say, to speak of norms and standards that are context specific, because this is merely to authorize an infinite plurarlity of norms and standards, and we are still left without any way of adjudicating between them...
>
> (p. 318)

Indeed this is the problem. When interpreters disagree we should like to have some way of deciding among their interpretations. But at this point, Fish perverts the discussion. Again, the quote follows directly:

> On one level this counterargument is unassailable, but on another level it is finally beside the point. It is unassailable as a general and theoretical conclusion: the positing of context or institution specific norms surely rules out the possibility of a norm whose validity would be recognized by everyone, no matter what his situation.
>
> (ibid, p. 319)

But it is only Fish's straw man who posits such things. We are interested in the speaker's meaning, and we know that specific norms alone are not enough to get us there, though they are necessary to recover denotations. But since we draw those norms—or try to—from the speaker/author's discourse community, and *not* from our own discourse community, in principle, there is no reason that everyone couldn't agree on the norms. Here comes the big canard (once again directly from the above):

> But it is beside the point for any particular individual, for since everyone is situated somewhere, there is no one for whom the absence of an assituational norm would be of any practical consequence, in the sense that his performance or his confidence in his ability to perform would be impaired.
>
> (ibid, p. 319)

No, what's beside the point is this argument. Let's concede the point: everyone is "situated somewhere." How does that help to recover the speaker/writer's meaning? In

the "Is there a text in this class example?" Fish cited people who didn't get the second meaning (Fish's second "literal meaning), which we agreed was also the speaker's meaning. As Fish puts it:

> ...neither reading of the question—which we might for convenience sake label as "Is there a text in this class?"$_1$ and "Is there a text in this class?"$_2$ –would be immediately available to any native speaker of the language.
>
> (ibid, p. 307)

Exactly. And that's why we need a way to figure things out. Now, if Fish wants to argue against this, he'll either have to claim that there wasn't a speaker meaning for "Is there a text in this class?" which is untenable since clearly there was, or he'll have to say that getting the speaker's meaning isn't important. But if it isn't important then what was the point of the speaker's correction of FC's interpretation? *She* thought it was important, and of course we all think it is important to be understood. I think Fish must see this, but he brazenly ignores it. The quote again follows from the last block cited on p. 319:

> So that while it is generally true that to have many standards is to have none at all, it is not true for anyone in particular (for there is no one in a position to speak "generally") and therefore it is a truth of which one can say "it doesn't matter." In other words, while relativism is a position one can entertain, it is not a position one can occupy. No one can *be* a relativist, because no one can achieve the distance from his own beliefs and assumptions which would result in their being no more authoratative *for him* than the beliefs and assumptions held by others, or, for that matter, the beliefs and assumptions he himself used to hold. The fear that in a world of indifferently authorized norms and values the individual is without a basis for action, is groundless because no one is indifferent to the norms and values that enable his consciousness.
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> (ibid, p. 319)

But all of this is irrelevant to the problem. No doubt Hitler was fully situated when he "read" history as a mandate to murder the Jewish people, just as the Rhenquist court was situated when it interpreted state and federal laws in a way that justified its decision to

appoint the loser in the 2000 election to the presidency. But that's no comfort. Rationalists don't despise relativism because it might rob individuals of their situatedness, we despise it because it licenses any and all "value systems." In the case of interpretation, we despise it because it renders criticism worthless. As I've argued throughout this dissertation, literacy studies can't be worth much if doesn't help us solve the central problems of the field: 1) explaining in detail how interpreters convey and recover meaning, and 2) providing a critical methodology for controlling and determining meaning. The first issue is theoretical, with strong connections to issues in linguistics and cognitive science. The second issue concerns literary critics, writers, professional speakers, and teachers. I have devoted most of this dissertation to the theoretical side of literacy since my task has been to provide a foundation for rationalism in literacy studies, rather than a primer for how to apply theory, but in this last section I will make some comments on pedagogy since it is in that realm that literacy studies has most impact.

Application of the Theory

Literacy remains the central skill in academe, and probably always will because language is the preeminent vehicle humans have for conveying thought. People who are naïve about language may imagine a time soon to come when video will overtake text, but this is extremely unlikely. Abstract concepts are best and most accurately represented with language, and written text is the most compact, conceptually refined, and accessible form of language for large communities that need to share ideas. However, for most people[122] literacy is not a naturally acquired linguistic skill like speech, although it makes

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[122] It's an open question whether literacy could be naturally acquired. People are innately oriented toward oral/aural channels for linguistic expression, but if they were deprived of those modes and presented with a community of uni-channel literates, it could be that they would acquire literacy naturally, just as deaf

good use of much of the same innately supported linguistic knowledge, i.e., I-language. This is not the place for extended discussion of processing skills in reading, but the theory presented here strongly supports an integrated approach in which "bottom up" decoding skills including sound/symbol relationships, morphology, and syntax are made explicitly available to students through linguistically informed instruction, while "top down" skills are taught through close analysis of texts. We, experts in literacy, are responsible for providing teachers with a coherent theory of literacy that justifies such pedagogical practices, and which students can also appeal to as they work on their own skills. Most college students are reasonably proficient at decoding, but as a whole they are naïve and unpracticed readers, prone to misinterpret or simply not comprehend many of the texts they are presented with in college; and, since writing is in many ways the mirror image skill to reading, these deficient readers tend to write simplistic, uninspired prose that is inadequate for the kinds of analysis and creative expression academic and professional work requires. In my view, after having taught literacy in one form or another for nearly twenty years, the main problem is lack of practice combined with extreme naivete about the nature of written texts. People who are unpracticed readers tend to assume that written text is just a paper and ink version of speech, which leads them to miss literary tropes and allusions, and further, to interpret from their own local perspective. The theory we've developed here helps explain why; infrequent readers learn how to handle the vocabulary and pragmatic scatter that is common to oral conversation, but they are unprepared for the sorts of complex rhetorical devices

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children acquire sign language. Plausibly, children who grow up around lots of text do acquire some their literacy skills naturally, i.e., literacy becomes a sort of second native language for them.

employed in writing, and, in particular, they are unaware of the need to actively work at recovering authors' intentions.

These pedagogical conclusions, like our theoretical conclusions, are completely at variance with antifoundationalist doctrines. In particular, we are diametrically opposed on the issue of "situatedness." While we agree that all readers (and writers) come to the task as "situated" members of discourse communities, we vehemently disagree with the antifoundationalist doctrine that they are stuck there, susceptible to "contingencies" in the discourse community that can change their "situations," but incapable of securing a critical vantage point. Of course these differences have immediate and substantial implications for the classroom. In the following section I will briefly recount an experience I had as a writing teacher teaching a Twain text, that illustrates this difference, and which clearly shows the need for rational interpretative practices. I will give only a cursory analysis, since as we've seen, detailed analysis of pragmatic scatter can take pages per line of analyzed text. But, by now, if the reader has followed the argument, s/he can easily construe the rest of the analysis.

Fishin' with Mark Twain

Fish's "Interpretive Communities" critical approach, presented in such essays as *Is There a Text in This Class,* and *What Makes Interpretation Possible* (1980) among many others, has been hailed as a radical new approach to interpretation and criticism. But, in fact it is nothing new under the sun. Mark Twain was wise to this idea more than a hundred years ago. However, whereas Fish celebrates interpretive communities as inherently infallible creators of meaning, Twain mercilessly exposes the foolishness of

interpretive communities that are dominated by superstitions and nonsense and which therefore misinterpret not only texts but also human behavior and natural phenomena—all with comic but ultimately tragic results.

Twain's treatment of these themes is thoroughly social constructionist, in that he clearly blames social constructs such as "codes of [southern] honor and codes of morals,"[123] in addition to general ignorance, for the absurd misinterpretations and convictions he lampoons. However, in sharp contrast to "strong" social constructionists, such as Fish, Twain never implies that *all* interpretations are equally "constructed," and therefore equally valid (or invalid), or that all communities are equally in thrall (or "possessed by" in Fish's terminology) to socially constructed beliefs. To the contrary, Twain's humorous, though subtly serious exposé of received provincial foolishness is juxtaposed to an educated, and therefore, *liberated* perspective that is capable of correct judgement about all things, including texts. Twain, then, may be regarded as a *reasonable* constructionist, not a strong constructionist[124], since he clearly holds that beliefs are one thing, reality another. Moreover, though Twain was in general sardonic, and hardly optimistic about humanities prospects as a whole, he consistently presents education and knowledge as the antidote to socially constructed nonsense, although he also maintains

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[123] Just two items of a long list of injurious social constructs Twain cites in "Is Shakespeare Dead?" (1909/1963)

[124] I borrow these terms from Kukla (2000), who defines *reasonable constructivism* as the "hypothesis that each society can construct facts only about itself" [facts about other things are independent] (p. 95). Strong constructivism, on the other hand, is the thesis that "all the facts we can ever possess are constructed" (p. 25). Twain's characters are often depicted as under the sway of false beliefs, regarding race, class, religion, natural causation, etc, but we--privileged readers—are always provided an objective vantage point from which to survey the foibles of these hapless victims of socially constructed myths. In his most dour moments, Twain was a rigid determinist (e.g., "What is Man" (1906/1963)), but many of his other works are reflect a far less desperate view.

that it usually takes generations of work to kill any particular socially embedded dogma (1909/1963, p. 448).

The hallmark of provincialism is ignorance, but not just ignorance about the world, but, viciously, ignorance about ones ignorance. The provincial fool is a fool not so much because he doesn't know things but because he thinks he's fine with the drastically limited view of the world he has. There is a parallel here to the debate between antifoundationalism and rationalism and also to literacy theory. Rationalists think there is an accessible world "out there," an attitude that naturally translates in literary/literacy studies into a theory of texts as traces of real authorial intentions. Because these intentions are tied to I-language, rationalist literary critics hold that the *author's* context is privileged, since for them, texts are interpreted by recovering the *author's* denotations, and then using "scatter" to access divergent authorial intention. Antifoundationalists, on the other hand are dedicated provincialists, holding that the interpretive community is a law unto itself, and that there is no way to argue for the supremacy of one interpretation over another. So, we have a substantive difference worth investigating. Apropos of our questions, the text we will look at, *Pudd'nhead Wilson,* treats the theme of situatedness, among many others, as it pertains to literary, social, and natural events. A full exegesis of the novel is far beyond my scope here; however, the work has a marvelous fractal quality about it such that nearly every substantial passage in it treats in miniature the themes of the whole novel. Therefore, it will be sufficient for our purposes here to look at just one such passage, Twain's enigmatic *Whisper to the Reader*, which serves as a preface to the novel. This singular introduction poses interpretive problems of delicious intricacy which, I argue, can *only* be

unraveled by constant attention to the text *and* to authorial intent, as revealed by Gricean analysis, along the lines presented above. Because the text itself implicitly begs the question of how texts should be interpreted, critical analysis of it assumes a metalogical[125] quality, which, no doubt, Twain was well aware of.

It so happens that I once used this text while teaching a class in American Thought and Language (ATL), so we therefore have available not only my interpretation but that of my students. The latter unanimously agreed with each other in their interpretation of this short text, and are thus an excellent example of a homogeneous "interpretive community" in Fish's sense, "made up of those who share interpretive strategies. . . that determine the shape of what is read" (1980, p. 171). But I sharply *disagreed* with my students, a fact which raised then, and raises now, the issue Fish claims is unimportant, viz. How should we should choose among conflicting interpretations?

I will take up this question, using Twain's text as an example, in the following way. First I present the entire text. I then present my students' interpretation of it, and finally my own, noting as I do so, why my interpretation should be preferred over my students'. Along the way I will comment on what an antifoundationalist, *qua* antifoundationalist, might say about the problem of these divergent interpretations. I suppose my interpretation is fairly orthodox, though I haven't bothered to compare it with published criticism. However, although the text is in some senses deliciously intricate, creating a nested set of interpretive frames that the reader must negotiate, its structure and

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[125] I refer to Gregory Bateson's concept of the *metalogue* which is "a conversation about some problematic subject. This conversation should be such that not only do the participants discuss the problem but the structure of the conversation as a whole is also relevant to the same subject.

Twain's purpose with it are easily apparent to any practiced reader and obvious to anyone thoroughly familiar with Twain. I will demonstrate, through a brief analysis of this subtly wrought though "obvious" text, that correct interpretation—of this text anyway—depends on accurate type to token analysis, i.e, accurate recovery of denotative meaning combined with appropriate inferences about what Twain is up to. Here, then is Twain's "Whisper":

A WHISPER TO THE READER

There is no character, howsoever good and fine, but it can be destroyed by ridicule, howsoever poor and witless. Observe the ass, for instance: his character is about perfect, he is the choicest spirit among all the humbler animals, yet see what ridicule has brought him to . Instead of feeling complimented when we are called an ass, we are left in doubt.—*Pudd'nhead Wilson's Calendar.*

A PERSON who is ignorant of legal matters is always liable to make mistakes when he tries to photograph a court scene with his pen; and I was not willing to let the law chapters in this book go to press without first subjecting them to rigid and exhausting revision and correction by a trained barrister—if that's what they are called. These chapters are right now in every detail, for they were rewritten under the immediate eye of William Hicks, who studied law part of a while in southwest Missouri thirty-five years ago and then came over here to Florence for his health and is still helping for exercise and board in Macaroni Vermicelli's horse-feed shed which is up the back alley as you turn around the corner of the Piazza del Duomo just beyond the house where that stone that Dante used to sit on six hundred years ago is let into the wall when he let on to be watching them build Giotto's campanile and yet always got tired looking as soon as Beatrice passed along on her way to get a chunk of chestnut cake to defend herself with in case of a Ghibelline outbreak before she got to school, at the same old stand where they sell the same old cake to this day and it is just as light and good as it was then, too, and this is not flattery, far from it. He was little rusty on his law, but he rubbed up for this book, and those two or three legal chapters are right and straight now. He told me so himself.

Given under my hand this second day of January, 1893, at the Villa Viviani, Village of Settignano, three miles back of Florence, on the hills—the same certainly affording the most charming veiw to be found on this planet, and with it the most dreamlike and enchanting sunsets to be found in any planet or solar system—and given, too, in the swell room of the house, with the busts of Cerretani senators and other grandees of this line looking approvingly down upon me as they used to look down upon Dante, ad mutely asking me to adopt them into my family, and mutely asking me to adopt them into my family, which I do with pleasure, for my remotest ancestors are but spring

chickens compared with these robed and stately antiquities and it will be a great and satisfying lift for me, that six hundred years will.

Mark Twain.

(1999/1894)

I asked my students to carefully read this text for homework, and to be prepared to discuss it. Then, in class, I gave them another ten minutes to read it. I then began fielding opinions about it, starting with the question, "What do you suppose is the purpose of this note?" I had anticipated that some of the students might not get all the jokes, but I hadn't considered the possibility than none of them would get *any* of them, or that they wouldn't realize that the whole "Whisper" was an ironic aside. Yet, student after student told me that Twain was trying to show us (the readers) that he had diligently researched the legal and historical facts connected with his story. I considered whether they might not all be pulling my leg, but it was clear they couldn't have been, since they wouldn't have known what I would ask and couldn't have feigned such a unanimously innocent reading. I then called their attention to a few especially salient clues, such as the name "Macaroni Vermicelli." But I soon discovered that this was not a salient clue for them because they did not recognize "vermicelli" as a type of pasta. Likewise, they had either no recognition of Dante, or only the faintest knowledge that he was some famous guy (maybe Italian, maybe a writer), no recognition of Beatrice, the Ghibellines, or the Cerretani senators. So I started with the pasta joke. Once they realized the extreme unliklihood of someone bearing such a name, let alone someone bearing such a name quartering a hick lawyer named William Hicks in a horse-feed shed—that is once they began to pragmatically scatter the text—global comprehension of the irony was not far

off. I remarked to them that they had provided me with a perfect demonstration of the implied audience Twain was lampooning, i.e., the typical Dawson's Landing reader, at which point a very sincere young man, the light of growing realization shining on his face, exclaimed, "Wow it's a whole different text now, huh!?"

Now, it seems clear that the problem here was the students' general ignorance combined with their unsophisticated approach to reading. They were not expecting irony, and that, combined with their lexical gaps, made my reading of the text "unavailable" to them, in Fish's terms. On this point at least, I think Fish and I would agree. But where do we go from here? On a rationalist, linguistic account, the students didn't just read the text *differently,* they read it *incorrectly.* But the notion of "incorrect" reading is anathema to Fish and to the entire antifoundationalist camp. This is where the rubber hits the road. Fish is a sophisticated professor of literature and I have no doubt that his reading of Twain's *Whisper* would accord with mine. But he has no grounds to claim that our reading is better than my students'. All he can say is that we (he and I) are in some way part of the same interpretive community (the literati), and that, therefore, that reading is available to us, whereas, the students, not being part of that community can't get it, or at least not without help. To some extent, again, we are in agreement, though I would put less emphasis on "communities" and more on skills. But, the crucial questions remain, and are in fact exacerbated by the antifoundationalist perspective. If all readings are equal, then why should we bother trying to get the students to read as we do? Moreover, if there are no reliable paths from text to speaker/author meaning, then what exactly are we supposed to teach? Fish's antifoundationalist answer is that we teach whatever interpretive strategies we think are best, but only because whatever strategy we prefer is

"presently recognized," not because we have evidence that it leads us to the author's intentions. This cynicism is supported by the claim that there is no objective text, because the "text is always a function of interpretation" (1980A, p. 343). But, recall, this claim itself rests on the claim that there is no denotative level of language. If on the other hand, there is a denotative level of language and a means to extrapolate from that level to authorial intention, then the antifoundationalist spell is broken. Of course in order to establish denotation and a means of extrapolation, one must accept linguistic evidence, which in turn requires a commitment to rationalism. In the end, then, literacy studies, like all fields, rests on some set of epistemological assumptions—there is no avoiding this. I have tried to show here that rational assumptions are every bit as minimal as antifoundationalist ones, and, further, that they license a far more fruitful and coherent enterprise.

Final Comments

Although the influence of antifoundationalism on literacy studies has come almost exclusively from philosophers and literary and literacy scholars—and they therefore are primarily to blame for the deleterious effects of this doctrine—certain rationalists whose professional concerns intersect with those of literacy studies are also indirectly responsible by their failure to address issues in literacy. In particular, most linguists have largely ignored literacy, or in some cases, actively denigrated the study as "unscientific" or at best so difficult (as a science) as to be beyond scientific methodology[126]. Chomsky, in particular, often comments that although "we learn much more of human interest about

how people think and feel and act by reading novels or by studying history," such modes of "comprehending the world" are fundamentally different from the ones we use to achieve "theoretical understanding of the world" (2000, p. 77). He may very well be right that *interpretive* modes of thought are different from scientific modes, but it seems to me that there is no essential difference when it comes to analysis. Following Einstein—as Chomsky also does[127]—we can say that analysis of all sorts is just a refinement of everyday thinking. That being the case, it would seem that we should abandon the notion of a fundamental split between literary analysis and science. As for the other objection—that facts about interpretation in real circumstances are just "too hard" to contemplate—that objection fails for two reasons. First of all, pace Mcginn, we just don't know yet that such problems are too hard. But even if they are so hard that we will never be very good at solving them, perhaps nowhere near as good as we are at solving problems in chemistry and physics, it doesn't follow from that that we should abandon the application of rational analysis to such problems. For we have no other tools. Chomsky argues—obtusely in my view—that

> There is also a further problem, which we can formulate in vague terms but which cannot be studied in practice: namely to construct an "interpreter" which includes the parser as a component along with all other capacities of the mind—whatever they may be—and accepts nonlinguistic as well as linguistic inputs. This interpreter, presented with an utterance and a situation, assigns some interpretation to what is being said by a person in this situation. The study of communication in the actual world of experience is the

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[126] Notable exceptions to this are Wallace Chafe (e.g., 1994) and M.K. Haliday (1993), along with many other "discourse analysts." However, in most cases these authors ignore generative theory. I argue here that we need to integrate generative theory with discourse theory and literary theory.

[127] In Chomsky 1993 (p. 28) Chomsky writes, "suppose we claim further that science is just refined common sense," a statement that is virtually identical to Einstein's and which, in fact, may have come from the same Einstein essay I quoted (whether or not Chomsky is aware of it).

> study of the interpreter, but this is not a topic for empirical inquiry, for the usual reasons: there is no such topic as the study of everything. Similarly, science does not investigate other phenomena of the world as presented to us in everyday experience. The interpreter[128]—as Davidson correctly observes—includes everything that people are capable of doing, which is why it is not an object of empirical inquiry, and why nothing sensible can be said about it.
>
> (Chomsky, 2000, p. 69)

But this criticism exaggerates the extent of the domain under analysis while simultaneously declaring that it is an impossible area of study. I think there's room to quibble on both points. First of all, while any particular interpretive moment is indeed extremely rich, it does not approach the opulence of "everything." Moreover, there are principled ways to drastically reduce the domain during empirical study. As I've already suggested, written exchanges are simpler than oral ones in many ways because they are far less reliant on context. Thus, we can study authentic communication in context poor situations—i.e., reading and writing—which reduces the theoretical difficulties. Also, since written text is stable, we can gather multiple examples of the same communicative exchange, which makes it possible to formulate falsifiable hypotheses about what's going on. For example, my supposition that my students missed Twain's pasta joke in the text above—and thus his ironic purpose in that passage—because they lacked knowledge of the word "vermicelli" could be tested by glossing the word ahead of time for one group of students or by replacing it with a more common term like "spaghetti." If we found significant differences between the groups of students in understanding the passage, it would be reasonable to attribute the difference to knowledge of that lexical item. In fact,

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[128] I think Davidson says the *act of interpretation* includes "everything" not the "interpreter." Even if the word interpreter is replaced with "parser," the sentence is still odd, and not equivalent to Davidson's claim. Crucially, in *Derrangement* (the same essay Chomsky is referring to here) Davidson cited the interactional exchange as well as the cognitive machinery (read "parser") as contributing to the interpretation.

experiments of this type are common in psycholinguistics, and they seem to lead to real knowledge, however limited in scope.

I hasten to add here that I am not suggesting that literacy studies should be reduced to nothing more than such narrow, carefully controlled studies, though I do think that such studies should be a part of what we do. In my view, literacy studies should draw from philosophy, linguistics, cognitive science, and literary theory—and whatever else might prove useful—in an attempt to answer the central problems of interpretation that all of the main sources in this dissertation—Rorty, Fish, Chomsky, Fodor, Davidson, Katz, and Dowling—are concerned with. Apparently such a field is far too broad for Chomsky's taste, however, for he argues further that

> Davidson's "passing theory" is not a useful notion; about that he is surely correct. The interpreter will construct all sorts of "passing theories" (though, crucially, not *any* sort), changing moment to moment, because the interpreter as Davidson conceives it includes everything available to human intelligence; it makes no sense however, to call its transient states "theories" or to consider them a subject of direct inquiry.
>
> (ibid, p. 70)

Again, however, this is obtuse criticism. First, if the notion of passing theories is "not useful," why do all of the authors listed above—including Chomsky himself—find it necessary to refer to some such concept (i.e., Fodor's "abduction," Rorty and Fish's "situatedness," Grice's Cooperative Principle, Katz and Dowling's "pragmatic scatter")? Although Chomsky is correct that in general terms, the notion is extremely vague, that is because, at present, we just don't know how such gestalt processes work. But it seems that everyone agrees that they are central to interpretation, and, therefore, the notion *is* useful as a beginning point of inquiry, or at least so it seems to me. As for whether the "transient states" of interpreters can be dignified with the appellation "theories,"

Chomsky is unfairly misconstruing Davidson here. In *Derangement* Davidson is pointedly not using the term "theory" in a technical sense, but, rather, in the colloquial sense of "best guess"; moreover, he is restricting the term to interlocutors, whereas Chomsky, in his criticism, has extended it to Davidson's *description* of the act of interpretation. As Davidson describes them, I would agree with Chomsky that the notion of passing theories is just a notion—not fleshed out enough to be called a theory—but Davidson would probably agree as well. On the other hand, Grice's pragmatic principles and Katz and Dowling's notions of pragmatic scatter do seem detailed enough to be called "theories," and, moreover, as we've seen, they provide a good point of departure for literacy theorists who would like to integrate generative linguistics and pragmatic theories of interpretation. I, for one, find this approach useful not only for literary interpretation and pedagogy, but perhaps also the most promising avenue we have for exploring the "mysterious" aspects of mind that, more than any other, define what it is to be human.

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