Rouse-ing out the Legitimation Project: Scientific Practice and the Problem of Demarcation

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Abstract

This essay critically examines Joseph Rouse's arguments against, what he dubs, the "legitimation project", which are the attempts to delimit and justify the scientific enterprise by means of global, "a priori" principles. Stipulating that a more adequate picture of science can be obtained by viewing it as a continuously transforming pattern of situated activities, Rouse believes that only by refocusing attention upon the actual practice of science can philosophers begin to detach themselves from the irresolvable epistemological problems that have remained the primary byproduct of the traditional philosophical approach. On closer inspection, however, Rouse's project appears susceptible to the criticism that it is either too relativistic to do the work he envisioned for it, or that it participates in the very same legitimation venture that it was intended to replace (in addition to its own brand metaphysical/epistemological problems). This unexpected outcome, moreover, suggests that such legitimation projects may be an integral component of the very practice of science, contrary to Rouse estimate.

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(WORD COUNT: 5576)

In the burgeoning field of cultural studies of science, J. Rouse's project for developing the notion of a scientific "practice" deserves special merit, as well as attention from the larger philosophical community.[1] As utilized by Rouse, a scientific practice is a rather intricate idea, for it comprises the individual, social, political, material, and spatiotemporal aspects of the scientific enterprise: "practices are not just agents' activities but also the configuration of the world within which those activities are significant." (1996, 133)[2] The traditional approaches to science--such as, scientific realism, empiricism, rationalism, social constructivism--have all erred, on Rouse's estimation, in holding that the overriding concern of a philosophy of science is the issue of scientific knowledge; i.e., on how knowledge is acquired, represented, and legitimized. "What most fundamentally unites all the standard interpretive traditions is that the place of scientific knowledge in our culture is in *need* of global interpretive legitimation." (10) Scientific realists, for instance, legitimate the scientific process by invoking a world of theoretical entities that are independent of human categorizations, whereas social constructivists appeal to the larger social context to explicate particular scientific beliefs (8-9). What these interpretations of science have in common is a penchant for what Rouse calls "epistemic sovereignty", which "would permit a detached interpretation and assessment of conflicting claims and methods." (30-31) In its place, Rouse advocates an approach that would more effectively incorporate the actual processes and social dynamics of science, which he deems, cultural studies of science.

This essay contend, however, that Rouse's theory either (i) suffers from a crippling relativistic outlook which renders it, contrary to his stated intentions, powerless to demarcate cases of pseudo-scientific practices from legitimate scientific practices; or

(ii), in trying to meet the problems raised by (i), his practice concept unwittingly partakes of the same form of global legitimatory thinking that it was designed to reject. As just noted, this dilemma surfaces most conspicuously in his handling of the "demarcation" problem between (so-called) normal science and pseudo-science. Rather than reject global legitimation schemes, it will be argued that a more adequate formulation of a practice theory should include such legitimatory thinking within the very bounds of scientific practice. Moreover, in the final section, it will be argued (via the recent work of S. Turner's) that the very concept of a scientific practice offers little metaphysical and/or epistemological advantages over the standard legitimation schemes; thus calling into question the general usefulness of Rouse's project.

1. The Legitimation Project

In critiquing the standard views of science, Rouse draws much inspiration from A. Fine's "natural ontological attitude", or NOA. Like Rouse, Fine regards most foundational disputes in the philosophy of science as too far removed from the actual activity of science.[3] Questions related to truth, reality, justification, and explanation, to name the most important, are questions that science handles perfectly well at the local level of the practicing scientist, without the need for the philosopher's preconceived theories and methods, or detached standpoints, to justify the scientist's work. Put more gently, "what the adoption of NOA would require is that philosophical discussion engage the actual scientific use of [the above] concepts and respect the contextualized concerns that circumscribe such use." (74) Rouse ultimately concludes that the most consistent formulation of NOA should regard the theory as promoting a "Davidsonian" deflationary interpretation of truth, which advocates "unanalyzed, contextualized *uses* of (rather than interpretations of) concepts such as justification, explanation, and existence within the practice of science. . . ." (99)[4] As will be seen, Rouse's interest in a deflationary model of

truth owes much to his dissatisfaction with the standard philosophical interpretations of science; namely, the global legitimation project.

The demarcation problem, or the separation of science from "pseudo-science", has long been one of the most troublesome puzzles that the upholders of the legitimation project have strived to resolve. For example, how do well-confirmed scientific theories, like quantum mechanics or molecular biology, differ from their pseudo-scientific brethren, such as creationism and astrology. In a separate work, the specter of "relativism" apparently prompts Rouse to discuss this issue: "A post-sovereign epistemology would presumably offer no standpoint, outside the contested domain in which conflicting and heterogeneous knowledge claims circulate, from which to assess what one *ought* to believe . . . ."[5] In order to prove his point that the denial of a single privileged standpoint does not entail that all knowledge claims are equally valid, Rouse provides an example of a dispute between a creationist and a modern day biologist. Whereas a creationist will invariably fall back on the authority of the Bible in such a debate, the biologist will remain equally adamant about the necessity to rely solely upon the fossil record and other genetic/biological data. Consequently, toppled from our sovereign epistemological standpoint, must we conclude that "Scripture is just as good as data." (1996a, 413) Rouse responds:

> This conflict among competing standards will appear irresolvable only when one removes the conflict from any real setting, in which there are interested parties and something at stake. In any real conflict, there is a burden of proof, which is sustained by a strategical alignment of people and things that can be relied upon to support and enforce that burden. . . . In such real contexts, there are constraints upon which arguments and which evidence will count as relevant and persuasive, based upon the need for support from others and for reliability from things. (1996a, 413)

Thus, within the context of a *real* scientific debate, a theory has to satisfy or confront the "burden of proof": i.e., its standards of theoretical success need to earn vindication relative to the competing standards of other scientific disciplines and approaches. Rouse would seem to be assuming, of course, that this is a struggle the creationist must lose

since the debate is confined to a "real setting" where the established scientific standards will invariably prevail.

The first, and most obvious, objection to Rouse's strategy for overcoming the "relativity" problem (of the adequacy of competing scientific theories and standards) is to take him to task on his curiously robust appeal to a "burden of proof". In the above passage, Rouse seems to imply that there is only one burden of proof to be met--but whose burden of proof, the creationist's or the biologist's? (Alternatively, in a world entirely populated by creationists, would Scripture now satisfy the "burden of proof"?) Once again, the problem of relativism has reared its ugly head, only this time at the level of the competing standards of scientific practices, if not in the local problems and claims of the scientists themselves. Possibly in an attempt to forestall this very difficulty, Rouse may have specifically introduced his thesis that "knowledge is always contested", and that "where knowledge goes unchallenged, where a claim 'goes without saying,' there is little or no articulation or development." (1996a, 409) Since the veracity of the Genesis narrative is presumed true from the start, Rouse could thus maintain that the entire creationist hypothesis does not warrant epistemic justification (since it is never challenged).

Unfortunately, as anyone remotely familiar with the history of current, large-scale pseudo-sciences, such as creationism, can attest, the general features of Rouse's story (of competing standards) are often discernible *within* a single pseudo-scientific theory. The debates among creationists on the details of the biblical "flood" story, for instance, display a pattern of "contested" and "articulated" knowledge claims that is uncannily similar to the theoretical conflicts that take place in the so-called "real" sciences (e.g., physics, biology, etc.). In their numerous books and journals, and operating from their own research institutes, the creationists have subjected nearly all aspects of the flood to careful scrutiny: when, where, and how the flood occurred receives elaborate theoretical development alongside a concerted effort to furnish evidence, both biblical and natural, to

confirm these conflicting hypotheses.[6] Consequently, not only are most creationist doctrines hotly "contested" within their own domain, but the creationists could happily catalogue the numerous doctrines that "go without saying" within the modern scientific enterprise, especially (for their purposes) the rejection of supernatural influences on the course of material processes. The creationists' burden of proof, accordingly, lies in the attempt to procure a harmonious rendering of modern science and a literal interpretation of the book of Genesis. In fact, to contrast the creationist and modern scientific programs as a battle of "Scripture" versus "data", as Rouse seems to suggest, is rather misleading, for they both intend to explicate the "data" of the physical world. Rather, their chief difference seems to lie in the ultimate secular or non-secular basis of their respective investigations, which simultaneously furnishes their respective "burdens of proof".

Hence, to return to our previous discussion, if Rouse's proposal is to act as a buffer against creationist-type programs, it must be *assumed* that the standards utilized by mainstream science met the burden of proof, relative to its goal of a secular construal of the world, than the set of criteria employed by creationists to accomplish their non-secular goals. And if one rejects this assumption, as the creationists will assuredly do, then we are confronted with the relativistic dilemma of choosing among competing scientific standards without recourse to any meaningful process of resolving the debate. Once again, we are confronted with the question: "Is Scripture as good as data?"

The tension that underlies Rouse's handling of the demarcation problem vividly demonstrates the explanatory impotence that can beset a philosophical thesis when deprived recourse to abstract epistemological and meta-theoretical considerations. If confined to the local questions of the practicing scientists, on what grounds can one claim that a particular research program is superior or inferior to a rival scheme? These issues are also tacitly at play in Rouse's comparison of creationism and feminism, where he surmises that "feminists have been rather more successful than have creationists, . . . [since] their arguments were more persuasive", and that "sexism and gender have also

shown themselves to be more resilient and readily manifest objects of inquiry than is biblical Creation." (1996a, 415) Yet, on what basis, were the feminist arguments shown to be "more persuasive" or that sexism and gender are "more resilient and readily manifest objects of inquiry"? Possibly implicit in these statements is Rouse's prior espousal of a primitive, unanalyzed concept of truth; that is, he might be arguing that sexism and gender are facts or truths about the world, whereas creationism (i.e., the literal truth of the Genesis story) is not. This interpretation of his strategy (for overcoming the pseudo-science problem) is ostensibly at work in his further contention that "the contested circulation of opposing knowledges, which cannot be consistently combined into a unitary framework of propositions, is a struggle for truth", and hence "Truth matters." (416) Yet, if Rouse is appealing to an unanalyzed notion of truth in this passage, it is important to note that it is being used as a *philosophical explanation*--truth is the explanation for, say, the success of geology and the failure of creationism. To employ truth in this manner, however, is to partake in a legitimation strategy that is suspiciously akin to a global, and not local, legitimation project, much like the scientific realist's program whose demarcation burden is carried by an objective "reality" of scientific entities. For both Rouse and his realist counterpart, there exists a general philosophical device--"truth" and "objective reality", respectively--which can potentially settle troubling demarcation problems.

Rouse may object, nevertheless, by insisting that his deflationary approach to truth askews the metaphysical speculation that characterizes most standard philosophical treatments of science: "Deflationary accounts of truth recognize that there is a well-established practice of ascribing or imputing 'truth' but see no need to ground or explain that practice by a theory that would characterize the underlying 'nature' of truth." (1996, 196) Arguably, this facet of a deflationary theory of truth may warrant merit, but its *function* or *role* in the demarcation debate is to legitimize normal science, thus it is ultimately concerned with the *relations between practices,* not with their local levels of

application. Needless to say, a likely rejoinder to these allegations would be to deem all legitimation projects as a form of local, contested knowledge claims. Specifically, despite their seemingly global and sovereign demeanor, any attempt to vindicate mainstream science (at the expense of pseudo-science) is just another aspect of the conflict for knowledge that occurs at the local level of scientific practice. Describing what his form of post-sovereign epistemology has to offer the legitimation of knowledge, Rouse explains:

> The crucial point is not that there is no legitimacy, but rather that questions about legitimation are on the same 'level' as any other epistemic conflict, and are part of a struggle for truth. In the circulation of contested, heterogeneous knowledges, disputes about legitimacy, and the criteria for legitimacy, are part of the dynamics of that circulation. (1996a, 412)

The problem with this approach to the role of scientific legitimation is that it is *too broad* to fulfill Rouse's intention of undermining the legitimation project: it would effectively open the door to all varieties of metaphysical speculation in the legitimation debates. Realists, rationalists, and social constructivists could now openly erect their philosophical structures under the guise of the "circulation of contested, heterogeneous knowledges," just as long as they simultaneously acknowledge that their disputes are "on the same level as any other epistemic conflict, and are part of the struggle for truth." In short, Rouse's analysis of legitimation seems perfectly compatible with the standard interpretations of science, since the abstract theorizing that distinguishes this kind of debate can be viewed as an integral part of scientific practice.

Overall, Rouse regards the legitimation project as a sort of alien metaphysical intrusion into the self-sufficient realm of scientific practice--yet, a more accurate treatment of those scientific practices might include the legitimation project *within* their very domains (for it has played a significant role in the work of at least a few famous scientists). High-level metaphysical speculation of science's legitimation could thus be envisioned as one particular consequence of, and response to, the problems that beset local practitioners of science; e.g., Einstein's famous rejection of the Copenhagen

interpretation of Quantum Mechanics, which largely stemmed from his commitment to a scientific realist ontology (which he viewed as threatened by the Copenhagen doctrine). Would Rouse want to claim that Einstein was not *practicing* science, but rather, useless non-scientific metaphysics, when he formulated his EPR thought experiment (in an attempt to embarrass Bohr's version of Quantum Mechanics)?[7] Therefore, Rouse's practice concept may not have gone far enough in delineating the various facets of the scientific enterprise. He has mistakenly confined scientific practice to what takes place only on the laboratory bench, forgetting the wider philosophical milieu which drives the formulation and interpretation of those very experiments.

In fact, many recent rhetorical studies of science have advanced this very picture of scientific practice; namely, "that demarcation practices are both more complex than normally thought and central aspects of scientific practice."[8] As well-known instances of scientists engaged in the legitimation project, C. Taylor examined the history and dynamics of both the creation science and "cold fusion" controversies. In both cases, scientists (in addition to philosophers) have come forward with *detached, global demarcation schemes* in an attempt to undermine the scientific credibility of their perceived pseudo-scientific opponents. Taylor is lead to conclude: "Practicing scientists, consciously or otherwise, discursively construct working definitions of science that function, for example, to exclude various non- or pseudo-sciences so as to sustain their (perhaps well-earned) position of epistemic authority and to maintain a variety of professional resources." (5)[9]

In response, Rouse may once more insist that the legitimatory schemes of scientists engaged in such debates (e.g., creation science) are nothing more than a particular manifestation of local contested knowledge claims. One may grant Rouse this concession; but, as argued above, it then becomes hard to see what force his anti-legitimation stance retains.

To summarize, Rouse's project faces the following dilemma: How is it possible to reconcile his commitment to a localized, non-legitimatory notion of scientific practice with an account of truth that bears all the hallmarks of a global, sovereign, legitimation narrative (i.e, his espousal of a Davidsonian deflationary theory)? Can one effectively interweave these apparently divergent strands of thought into a consistent and harmonious whole? In an effort to meet this challenge, Rouse would likely reply that any attribution of a literal, straightforward version of Davidson's deflationary model to his theory grossly distorts his original intentions. The appeal to Davidson's model was based primarily on those features of the deflationary model, namely, its non-representational explanation of knowledge, which Rouse found useful in the construction of his scientific practice concept. Rouse is more concerned with the issues of power and dynamical change within practices, and not specifically with the epistemological problems that motivated Davidson's work, or with the details of deflationary theories as regards the traditional metaphysical-linguistic range of issues. Needless to say, the type of theory that Rouse is striving to formulate is rather hard to conceptualize in clear and unambiguous terms (if at all, for it is uncertain how it generally relates to a theory of truth), yet it is safe to say that he is opting for some interpretation of truth more congenial to the localized context of scientific practices. On one possible rendering of Rouse's theory, which may acquire support from various passages (195-204, 1996), such claims as "more resilient and readily manifest objects of inquiry" (1996a, 415) could stand for some notion of "objectivity" or "invariance" that is only manifest at the localized setting of a scientific practice: e.g., only within the cultural study of gender can the "truth" or "objectivity" of gender bias come to the forefront.[10] These "truths" cannot be assumed to exist prior to such investigations (as some sort of "a priori" elements), since they only emerge within, and as a result of, a scientific practice. Consequently, global legitimatory judgments on the relative epistemic worth of competing practices could also be seen as a result of those

very practices, and not a metaphysical, sovereign narrative imposed on the practices from above.

Despite its persuasive appeal, a close inspection of the actual practice-centered efforts to identify and isolate such objective features of the world readily demonstrates the implausibility of this general approach to Rouse's theory. To return to the gender studies example once again: What are the "objective" features of sexual discrimination and bias, and how do these differ from the "objective" features of scientific creationism? If objective features must constitute an invariant across many different practices--so that, from the perspective of all (or even most) practices, the same "fact" or "truth" is uniquely identified (which is how invariants are defined in the physical sciences)--then it is difficult to foresee the elevation of any meaningful feature of the gender studies practice to the status of an "objective" fact or truth. Besides the fairly trivial truth that women are generally paid less than men (which is an invariant, by the way), almost no right-wing, conservative gender studies practice will concede "objective" rank to such important claims as "women are subject to sexual discrimination". Conservative gender researchers do not concede any evidence for these claims (remarkable as it may seem), hence they will reject outright the alleged objectivity of the feminists' contentions. Alternatively, any endeavor to circumvent this difficulty by admitting "objective" status to claims as deemed such from the perspective of only one practice, or a few practices, inexorably leads us back to Rouse's original anxiety over the charge of relativism--since the creationists holds that a 6,000 year old earth is as objective a feature of their investigations as sexual discrimination is for the feminists.

In retrospect, it can be seen that the philosophical waters which Rouse has decided to navigate are inherently dangerous for his notion of a scientific practice. Enchanted by the siren song of cultural studies, Rouse has neglected to notice that his journey towards a new understanding of a scientific practice passes through either the

Charybdis of relativism and the Scylla of sovereign legitimatory narratives--and like that other cultural voyager, Odysseus, he cannot escape without loss!

2. Science as Practice

As previously discussed, Rouse claims that his notion of a scientific practice avoids the pitfalls of the legitimation project by obviating the need for a representationalist construal of scientific knowledge. It is this move towards an intermediary between the world and the knower which generates the problem, according to Rouse, since this picture of knowledge "motivates the question of how representations could ever be appropriately connected to objects represented." (1996, 29) The legitimation of the correct, as opposed to incorrect, representation thus follows as a natural consequence of the representationalist view. Rouse is aware, however, that simply adopting a practice theory of science may not rule out the possibility of covertly employing a representationalist strategy:

> Many of the increasingly common philosophical and sociological appeals to "social practices" still reproduce representationalism by equating practices with rules, norms, or skills that knowers or agents embody or enact. Practices thereby become analogues to the knowing subject and are consequently distinct from the objects practiced with or on. As an alternative, I suggest conceiving of practices as meaningful situations or configurations of the world. Agents or knowers do not bestow meaning on practices but instead they count as agents and knowers only through their place in the ongoing patterns of practice.

While Rouse's goals are certainly laudable, but the real question is whether or not his project constitutes a viable alternative. Unfortunately, when judged by the demanding criteria of ontological and epistemological coherence, it will be demonstrated that Rouse's theory (via S. Turner's work) fairs little better than even the most otiose brands of representationalism.

In his book, *The Social Theory of Practices,* Turner methodically unveils many of the inherent weaknesses of the "practice" concept, particularly with respect to the important issues of transmission and change.[11] Rouse is not unaware of Turner's analysis,

but holds that the problems raised pertain only to those theories which promote a representationalist account of practice: "Although [Turner] does not talk specifically about 'representationalist' conceptions of practice, . . . it is clear that his principle targets are those views for which the term 'practice' is interchangeable with such terms as . . . 'paradigm, ideology, framework, and presupposition'. " (1996, 30n) While it is true that many of Turner's arguments are directed at representationalist versions of practice theory, such as Kuhn's "paradigm" or Polanyi's "tacit knowledge", it would be a mistake to regard Turner's approach as generally sympathetic to Rouse's program.

One of Turner's primary targets is the tendency to portray practices as a sort of causal, "quasi-object" shared among its practitioners. It is not enough for the practice theorist to merely accept a similarity of outward individual behavior (or, "external" manifestation of the practice), such as a group of children playing a game. What is also required, reasons Turner, is an "internal" similarity; e.g., all the children playing the game guided either by the same (representational) rules or presuppositions, or by comprising individual instances of some non-individual shared object (viz., a practice). In the latter case, the practice must possess some essential property or identity criterion so that it can be properly demarcated from other practices. Consequently, when construed as either a shared presumption or shared quasi-object, the practice theorist must provide a story on how practices are individuated -- and it is on this crucial issue (and the closely related problems of transmission and change) that Turner launches his attack. In essence, if a practice's external behavior is all that can be observed, then the identity of that practice (i.e., which practice is operative) is radically underdetermined by the evidence. To return to our example, any number of shared presuppositions or quasi-objects are compatible with the same outward behavior of the children: Is the game, say, volleyball, a unique practice, or is it a member of a larger practice whose members differ in various ways, e.g., games played with a ball and net, which also includes tennis, handball, etc.. There are numerous possibilities for identifying the children's practice, most notably those

linked to such biologically-influenced traits as: community cooperation, aggression, motor-skill development, self-esteem, etc., or some/all of the above. Which practice gets ascribed to the external behavior, moreover, will largely depend on the conceptual framework adopted by the theorist.[12] Turner adds that practice theorists strive to objectify practices, "but they cannot: practices are objects of a peculiar kind, dependent on a cultural perspective." (24) Hence, which practice the children in our example will get grouped under will vary accordingly to the primary interests of the researchers.

The delicate problem of providing an identity criterion for practices is quite evident in Rouse's work. While accepting a normative aspect for practices (i.e., some guideline as to what is to count as correct/incorrect performances of the practice), Rouse nonetheless specifies that practices are open-ended, and that they are never fixed by their past instances: "These [past] instances are, of course, relevant to the identification and continuation of the practice, but they cannot be decisive in settling whether new cases exemplify the practice; the new cases may, after all, constitute a reinterpretation of their predecessors." (1996, 141) The move towards an open-ended account of practices may mark an advance over more simplistic, closed versions of the theory, but it also leaves Rouse's project open to the charge of incoherence. With respect to such versions of the practice theory, Turner comments: "if the notion of open-endedness is pushed too far, the notion of limitation is threatened, and with it the notion of system, for if there are no limits there is no system." (89) Yet, the situation is even more complex for Rouse, for he insists that practices are mutually interdependent (156-157 -- but how can this be if their boundaries are ill-defined?), and that the identity of the individual practitioners are largely constituted by their respective practices: "agents do not have an identity fully separate from the practices in which they participate, such that who or what the agents are is partially constituted and/or transformed by those practices." (143) Needless to say, the metaphysical burden that Rouse has placed upon his concept of practice would seem to have strained it to the breaking point: on the one hand, a practice is sufficiently weak to

allow its very identity to be underdetermined and contested by its practitioners; yet, on the other hand, the same practice is sufficiently robust to comprise the main part of the practitioner's identity. ("The agents who engage in practices thus belong to the practice, rather than the reverse." (143))

Faced with such insuperable difficulties, Turner is lead to abandon the concept of a practice altogether. Instead, he opts for a minimalist hypothesis that interprets practices as merely individual, private "habits." (57-60) Forsaking the internalist requirement for a shared presupposition or quasi-object, Turner explicates practices in terms of the individual's external performance, rather than the reverse. (Rouse, on the contrary, favors the latter strategy: "The normative configuration of the world which emerge in practices must be understood as prior to any distinction between agents and their environment, the social and the natural, or the human and the nonhuman." (146))

The above discussion was not intended to be an exhaustive survey of the numerous issues touched upon in Turner's book. Our limited goal, rather, has been to demonstrate that the move away from the standard interpretations of scientific knowledge, with their attendant legitimation projects, may not be as persuasive as it at first had seemed. In fact, the onus metaphysical and epistemological difficulties associated with Rouse's concept of a scientific practice may stem directly from the fact that it essentially constitutes another global, "a priori" resolution of the problem of scientific knowledge. Unlike his predecessors, however, Rouse is not advancing an epistemological theory that would provide a list of rational criteria for solving the demarcation problem (and which may be linked to an underlying ontology, as in the case of scientific realism). Instead, he is postulating an all-encompassing ontological entity--namely, a practice--which is intended to dissolve the issue entirely. Unfortunately, as discussed in section 1, Rouse's use of an unanalyzed notion of truth would seem to either tacitly reinvoke the legitimation project or fall into a debilitating relativism; while, in

section 2, the troubling issues surrounding the coherence of Rouse's practice concept (notably, identity) raise serious doubts concerning the viability of his overall project.

ENDNOTES

1 In this essay, we will mainly focus upon Rouse's recent work; namely, *Engaging Science: How to Understand its Practices Philosophically* (Ithaca: Cornell University Press, 1996), as well as an important separate article. Rouse's initial views are presented in, *Knowledge and Power* (Ithaca: Cornell University Press, 1987).

2 This conception of practice is not new, of course, and Rouse devotes an entire chapter of his new book (chapter 5) specifying the manner in which his use of the notion differs from that of other philosophers, especially Dreyfus and Brandom.

3 A. Fine, *The Shaky Game: Einstein, Realism, and the Quantum Theory* (Chicago: University of Chicago Press, 1986), chapter 7.

4 In order to meet the challenge of its various critics. See, for example, R. H. Schlagel, "Fine's 'Shaky Game' (And Why NOA is No Ark for Science)", *Philosophy of Science* , 58(2), 1991.

5 J. Rouse, "Beyond Epistemic Sovereignty", in P. Galison and D. J. Stump, eds., *The Disunity of Science: Boundaries, Contexts, and Power* (Stanford: Stanford University Press, 1996a) 412.

6 Some creationist hypotheses, for example, take the Genesis narrative as literally involving the entire world, while others allow a certain degree of "filling in the gaps" in order to accommodate modern scientific notions or disconfirming evidence. As an instance of the latter strategy, some interpret the biblical flood as having been only a local, small scale occurrence since this harmonizes more effectively with current geological findings. See, for example, R. L. Numbers, *The Creationists* (New York: Alfred A. Knopf, 1992), especially chapter 10.

7 There numerous accounts of Einstein's philosophical presuppositions, and how they affected his scientific theorizing. Ironically, given Rouse's project, one of the most relevant is; Fine 1986.

8 C. A. Taylor, *Defining Science: A Rhetoric of Demarcation* (Madison: U. of Wisconsin Press, 1996), 224.

9 The line that separates the "standard" legitimation projects (of the rationalists, realists, constructivists, etc.) from the new breed of rhetoric/practice theorists would appear to be quite thin. In the work of both L. Prelli and M. Pera, for example, a rhetorical analysis of the practice of science leads them to conclude that scientific discourse is normally guided by a set of (more or less) constant methodological criteria, such as the problem-solving capacity of the contested hypothesis. These recent studies, it would seem, constitute more of a continuation and elaboration of the standard methodological discussions, rather than their outright rejection. The differences, if any, appear more stylistic than content driven, which would render Rouse's position on the legitimation project somewhat intangible. See, M. Pera, *The Discourses of Science,* trans. by C. Botsford (Chicago: U. of Chicago Press, 1994), chap. 6 & 7; and, L. Prelli, *A Rhetoric of Science: Inventing Scientific Discourse* (Columbia: U. of South Carolina Press, 1989), chap. 8. Both acknowledge that methodological standards are never fixed and/or followed in every debate, but this observation is commonplace among most of the "old school" philosophers of science, such as Lakatos or Lauden.

10 I owe the basic idea for this interpretation of Rouse's theory to an anonymous referee from *Ratio.*

11 S. Turner, *The Social Theory of Practices* (Chicago: University of Chicago Press, 1994).

12 Additionally, there is the problem that the children may all perform the same external act, but as a result of different internalized practices. (Turner 1994, chap. 2)