"...in my essay [Against Method] I am addressing humans. I am addressing neither dogs nor logicians."

-Paul K. Feyerabend

1. Introduction: Philosophy of Science and Political Science

The relations between political science and the philosophy of science have always been one-sided. Behavioral political scientists have sought scientific legitimacy and methodological guidance and therefore turned to those who dispensed such guidance and proposed the standards of genuine scientific inquiry: the philosophers of science.

While few political scientists will dispute the great impact the philosophy of science has had upon recent scientific political studies, this one-way relationship has not occurred without some resistance on the part of political scientists. This is evident to any political methodologist who has heard the words: "What does all this philosophy of science have to do with political science?" from both colleagues and students alike. This dependency of political scientists upon the philosophy of science for methodological instruction is re-


Although Feyerabend does not indicate it in this book, he published a monograph (from which some pages are borrowed verbatim and included in Against Method with notation) with the same title in, Michael Radner and Stephen Winokur, eds., Analyses of Theories and Methods of Physics and Psychology, Minnesota Studies in the Philosophy of Science, Vol. IV (Minneapolis: University of Minnesota Press, 1970), pp. 17-130.

For purposes of convenience, references to Feyerabend's volume (which he informs the reader is actually a letter to the now deceased philosopher of mathematics and science, Imre Lakatos) will be noted as AM and the monograph as AMM and placed directly in the text.

While Feyerabend has written several significant articles and monographs within the philosophy of science, this discussion of his epistemology will focus solely upon the above two works. For most writers, this selectivity is questionable, however, it may be done safely concerning Feyerabend as he informs his readers: "When writing a paper I have usually forgotten what I wrote before the application of earlier arguments is done at the applicant's own risk" (AM: 114).
He criticizes what he deems the excessive reliance upon the philosophy of science by political scientists as follows:

The book argues that fundamental difficulties arise whenever any discipline attempts to construct a scientific identity and methodology from the literature of philosophy...In matters of methodology political scientists have become imitators of imitators and have committed themselves to doctrines elaborated by philosophy without rational consideration of their own enterprise...This is not to argue that political scientists should deny the relevance of philosophy but only that it is a mistake to assume which philosophy represents an Archimedean point to which they may automatically repair for answers. Political science must chart its own methodological route and the defense of that route cannot be achieved by invoking the authority of the philosophy of science-whatever school or set of ideas.

Given the obvious sociological, psychological, and economic factors involved, it is not surprising to find members of an academic discipline advocating their field's autonomy as we see in this statement by Gunnell.

With this as background, it is no wonder that Thomas Kuhn's *The Structure of Scientific Revolutions* took political science by storm. Here was a philosophy of science that used political terminology (such as "revolution") and emphasized modes of conflict and conflict resolution between natural scientists that were similar to the phenomena political scientists study. This type of philosophy of science was more accessible with its political, sociological and psychological explanations of social behavior (the scientific community) and more agreeable with its epistemological relativism to a political science audience that was: (1) tired of hearing how deficient its research was from a natural scientific viewpoint; (2) delighted to learn that disputes in the physical sciences and their

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3. Ibid., pp. v, 51 and 54.
5. See ibid., particularly chapter XII "The Resolution of Revolutions." Also note Kuhn's specific political references, for example: "like the choice between competing political institutions, that between competing paradigms proves to be a choice between incompatible modes of community life," p. 94; and, the statement by Kuhn that his account of "scientific development as a succession of tradition-bound periods punctuated by non-cumulative breaks" borrows from many other fields of history including the history of political development, see, ibid., p. 288.
manner of resolution were little different from what occurs in political science; and, (3) characterized by many of its members who had made an erroneous inference from cultural relativism to value relativism to value non-cognitivism.9

Political scientists now have Feyerabend's AM and AMM to consider although the reaction (thus far) to these works by political scientists seems slight compared to Kuhn's volume or the great controversy Feyerabend's views have caused in the philosophy of science.1

Not only does Feyerabend use political terms like "anarchism" and "law-and-order," he relies to a significant extent upon the works of such political theorists as Mill, Marx, Lenin and Trotsky. What is even more unusual for a philosopher of science, Feyerabend not only refers to political theory but actually prescribes the usefulness of politics and political thought to the philosophy of science!

We see here [referring to a statement by Lenin] very clearly how a few substitutions can turn a political lesson into a lesson for methodology. This is not at all surprising. Methodology and politics are both means for moving from one historical stage to another [compare this to Kuhn's remarks noted earlier in footnote 5]. We also see how an individual, such as Lenin, who is not intimidated by traditional boundaries and whose thought is not tied to the ideology of a profession can give useful advice to everyone, philosophers of science included...Basically there is hardly any difference between the process that leads to the announcement of a new scientific law and the process preceding passage of a new law in society.... (AM: 18, footnote 5, emphasis in original; 302).

Here we have a real switch. A philosopher of science recommending the instructive value of political thought and politics for the philosophy of science. One lesson we political scientists (should) have learned as a result of getting our science or philosophy of science...  


On the first page of the 1978 edition of AM Feyerabend notes his several replies to "the many people who have cursed my book in print...." For an example of the strident and extensive nature of the debate between Feyerabend and his critics see the three reviews of AM, Feyerabend's article-length reply, followed by further responses by two of the reviewers and Feyerabend again, all in Philosophy of the Social Sciences, Vol. 7 pp. 265-302, and Vol. 8 pp. 37-61 and 184-186.
science belatedly, second-hand, and muddled is that we must beware of possible abuses from intellectual borrowers.° Therefore, it seems appropriate that political scientists should consider Feyerabend's "anarchistic theory of knowledge" and, moreover, to examine whether he has, _inter alia_, his political theory straight. This is not the only reason why I think Feyerabend's epistemology is deserving of careful scrutiny by students of political inquiry. My main claim in this essay is that Feyerabend's attack upon standard philosophy of science (for example, Popper's critical rationalism in his _bête noire_) raises several important methodological issues that can prove helpful in the political scientists' task to "chart [their] own methodological route" independent of Feyerabend's position. If examining Feyerabend's epistemology will help clarify some serious methodological confusion among political scientists, then his theory of scientific inquiry will be of value to the latter whether or not his fellow philosophers of science accept his views.

The salutary effect of Feyerabend's discussions may be achieved by provoking political scientists to assess his recommended epistemology and thus derive by implication the proper methods° for political science (including its proper relationship to the philosophy of science), much in the same manner as Kuhn's work generated a multitude of articles and books on political methodology. (I refer here to the number and not necessarily to the overall quality of the Kuhn-inspired literature in political science.) For even if Feyerabend is mistaken (as I think he is) John Stuart Mill (whose theory of

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8. Gunnell writes: "In the past few years, the political scientist has become self-conscious about the implications for the discipline of current controversies in the philosophy of science. Often, however, he has a fuzzy conception of the character of these controversies and of the intellectual context in which they have arisen" _op. cit._, p. vi. For a discussion of the reliance of the social sciences, including political science, upon outdated theories of physics, see, Floyd W. Matson, _The Broken Image: Man, Science and Society_, Garden City: Doubleday, 1966.

9. It might appear to be the height of irony to defend an examination of Feyerabend's epistemology on the ground of the possible proper methods of (political) scientific inquiry when it seems on the surface that Feyerabend claims that "anything goes" is the "only one principle that can be defended under all circumstances...." (AMM: 26). To anticipate my later discussion, I will claim there is no irony because Feyerabend admits that his "principle" "anything goes" is a "joke" (AMM: 105, footnote 38) and that in fact Feyerabend does prescribe some "proper methods" for the conduct of scientific inquiry.
inquiry Feyerabend's own epistemology, including his principle of proliferation, is deeply indebted to) provides grounds for such a study when he writes: "He who knows only his own side of the case knows little of that" and that as a result of considering even an erroneous position we gain because of "the clearer perception and broader impression of truth, produced by its collision with error." 11

I do not intend by this prescription of the methodological value of Feyerabend's epistemology to argue (pace Gunnell, although I have reservations about his position as will be discussed shortly) for the predominance of the philosophy of science vis-a-vis political science or to deny Feyerabend's assertion regarding the relevance of political thought to the philosophy of science instead of vice versa. I only wish to claim that methodological matters should be considered necessarily philosophical and epistemological-and thereby normative as well-even political methodology, in the manner that any second-order or metalevel discourse is, 12 Feyerabend's anarchistic epistemology underscores the normative nature of metalevel inquiry, in particular, the philosophy of science (with applications to logic and epistemology too) and may aid political scientists' understanding of the nature and proper use of the philosophy of science.

While we must define our domain of inquiry clearly and stick within its boundaries we must be just as careful to recognize those extra-domain fields 13 that have profound implications for our own,
We should be less concerned about protecting our academic discipline’s autonomy (which, after all, is probably no more than a bureaucratic department within the university acting as credential gatekeeper, and, furthermore, if you believe Kuhn, a paradigm-socializer) and “charting our own methodological route” than making clear the problems we wish to address. Once this is accomplished we should then be prepared to use any of the relevant intellectual resources at hand no matter where their disciplinary home may be.

We are (should be) students of political problems first and political scientists second. If this problem-centered view sounds similar to Popper’s it is because I accept his emphasis upon problems as the center of rational thought. And, despite Feyerabend’s several attacks upon Popper I think the former would have to agree with Popper’s position here (which we will see is not the only issue where I believe Feyerabend is closer to Popper’s view than Feyerabend thinks himself!). After all, does not Feyerabend strongly criticize the evils of professionalism?

Perhaps this essay will constitute the beginning of an explanation of the appropriately unequal relationship between the philosophy of

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15. See, for example, AMM: 100-102, footnote 27; and AM: 48, footnote 2.

16. See Feyerabend’s ridicule of W.H. Master and V.E. Johnson’s *Human Sexual Response* as an example of the evils of professional expertise taken to an extreme, AMM: 95-99, footnote 13. Also see Feyerabend’s article replying to critical reviews, “From Incompetent Professionalism to Professionalized Incompetence—The Rise of a New Breed of Intellectuals,” *Philosophy of the Social Sciences*, Vol. 8, 37-54.

Rather than attacking rationalism, as many of Feyerabend’s critics read AMM and AM (and as Feyerabend claims to do himself), I think Feyerabend is vehemently expressing the harm (e.g. squelching creativity, the creation of jargonistic barriers to communication, etc.) produced by the separation of technical domains and the domain-wide standards imposed upon its members, representative of professionalism to him. Here again I do not think Popper would disagree.
science and political science; and, moreover, be a reply to Gunnell's (and others') misconceived critique of this relationship. Political scientists in fact do (and should) raise epistemological and philosophy of science questions about their mode of scientific political inquiry more than philosophers of science qua philosophers of science raise political issues. And this, of course, is how it should be in the relations between a first-order (political science) discourse and a second-order one (philosophy of science) with absolutely no implication for the former's autonomy. Do we question the autonomy of attorneys or mathematicians who consult the philosophy of law or the philosophy of mathematics, respectively? To chart a domain's own methodological route is not inconsistent with examining second-order issues and considering second-order positions as I hope to show in discussion Feyerabend's epistemology and philosophy of science.

II. Summary of Critical Assessment of Feyerabend's Empirical (sic) Epistemology and Philosophy of Science

In assessing AMM and AM, I would like to make the following points which will provide the agenda for the remaining sections of this essay.

(1) The most important issue that divides Feyerabend from his targets of criticism (like Popper) and the one I think is the most significant for scientific political inquiry concerns the cognitive status of the domains of logic, epistemology, and the philosophy of science. Feyerabend conceives of these fields, most explicitly, the philosophy of science, as empirical inquiries and therefore argues strongly for the necessary relevance of history, sociology, psychology ("and all the other factors that may influence the success of a certain procedure, or what he terms 'cosmology,'" se AM: 206, footnote 66) to scientific methodology.

Feyerabend's opponents within the mainstream of the philosophy of science (most importantly, Popper, although it will be necessary to modify this first impression of the opposition between Feyerabend and Popper) conceive of the philosophy of science as a second-order or meta-level discourse that is largely prescriptive. This view implies the irrelevance of much of Feyerabend's claimed empirical (mostly historical) AMM and AM to his opponents' intellectual goals (although how much of these two works is actually empirical is less
than what appears on the surface or what Feyerabend claims)."
This point about the normative versus empirical conceptions of
methodology and the philosophy of science (as well as logic and
epistemology) and its implications for political science will be the
main focus of what follows.

(2) Feyerabend's purported "theory of rationality" (which he
states is the "main topic" of his essay AM: 165) is actually nothing
more than an empirical theory of human creativity (which goes
mostly undefended probably because of Feyerabend's grander "pro-
pagandistic intentions"). Feyerabend's failure to recognize or his in-
tentional blurring of differences between the two questions, viz. the
nature of rationality and the methodological procedures to max-
imize creativity within science, seriously undermines the cogency of
his discussion. (Incidentally, once Feyerabend's position in AMM
and AM is so recognized for what it is: as a recom-
mandation—"anything should go"—in order not to inhibit scientific

17. For a similar analysis of the Kuhn-Popper dispute, turning on the same fun-
damental issue of the normative versus empirical conceptions of the philosophy of
science, see my, "A Comment on Girill's Dualistic View of Scientific Knowledge as a
149-154.

The parallel between the disagreements between Kuhn and Popper and those be-
tween Feyerabend and Popper should not be surprising given Kuhn's and Feyerabend's
similar empirical positions regarding the nature of the philosophy of science and its
methods of inquiry. For example, Kuhn describes his argument in The Structure of
Scientific Revolutions as "intrinsically sociological." see Kuhn's "Reflections on My
Critics," in Imre Lakatos and Alan Musgrave, (eds), Criticism and the Growth of

It should be mentioned here that Feyerabend contributed an article to the Lakatos
and Musgrave volume assessing Kuhn's philosophy of science. In fact one of Feyera-
bend's criticisms against Kuhn is quite pertinent to my own criticism of Feyerabend
himself. The latter writes: "Whenever I read K, I am troubled by the following
question: are we here presented with methodological prescriptions which tell the scien-
tist how to proceed; or are we given a description, void of any evaluative element, of
those activities which are generally called 'scientific'?" Paul Feyerabend, "Consola-
tions for the Specialist," in Lakatos and Musgrave, p. 198, emphasis in original.

Given the fact that such ambiguity and worse exists in Feyerabend's AMM and AM,
he confuses descriptive claims with normative ones and, moreover, ignores the essen-
tial normative nature of methodology, as we shall see, we may thus include that this
article containing this critique of Kuhn was one of Feyerabend's "forgotten works" (see
footnote 1) or he, like Kuhn before him, according to Feyerabend's characterization of
Kuhn intended the ambiguity "to fully exploit its propagandistic potentialities" (p,
199). However, once attention is drawn to this confusion or intentional ambiguity it
weakens Feyerabend's argument.
imagination, it becomes clear that not only is it not in conflict with Popper's "bold conjectures" position but rather quite similar to it.

(3) Feyerabend misunderstands the nature of the objectivity of scientific knowledge as a consequence of overlooking entirely the distinguishing element within it; what I term "the epistemological accountability of assertion." Because of this omission Feyerabend's critique of "objective scientific knowledge is vitiated.

(4) Although their different definitions of "science" is a lesser important methodological disagreement (because purely verbal) between Feyerabend and his opponents like Popper, it is nonetheless especially significant to political scientists.

(5) Feyerabend's extensive use of history (in the form of historical case studies as his chosen example of Galileo's theory illustrates) and his strong recommendation of the history of science's importance to the study of scientific inquiry raise the general methodological issue of the proper role of history to the philosophy of science. Here once again we have an instructive discussion for political scientists concerning their relation to the philosophy of science and another instance where the similarities of Feyerabend's claims to Popper's appear greater than Feyerabend's harsh criticism of Popper would indicate.

(6) Given his heavy reliance upon the epistemology of John Stuart Mill, is Feyerabend's supposed Millian theory of knowledge compatible with its claimed source? I think not. Point (2) comes back to damage Feyerabend's discussion because of its false epistemological pretensions (intended or not) despite it only being an empirical theory of creativity (which is in fact borrowed from Mill). Therefore, Feyerabend disregards some truly important epistemological assertions in Mill.

III. Should the Philosophy of Science Be an Empirical or Normative Mode of Inquiry?

Feyerabend's assumption of the empirical (primarily historical) nature of the philosophy of science is clear right from the first sentence of Chapter 1 of AM (23): "The idea of a method that contains firm, unchanging, and absolutely binding principles for con-
ducting the business of science meets considerable difficulty when confronted with the results of historical research." This position is asserted again when Feyerabend writes: "...it is clear that the increasing separation of the history, the philosophy of science and science itself is a disadvantage and should be terminated in the interest of all these three disciplines" (AM: 48, footnote 2). Or, consider this claim by Feyerabend: "...historical data play a decisive role in the debate between rival methodologies" (AM: 183). And, finally, his more recent statement: "Methodologies can only be judged in connection with a practice...." 19

There is little doubt that Feyerabend intends to recommend to the philosophers of science that their field consist of empirical (really social scientific) inquiry: in particular, the historical, sociological, psychological, anthropological-and let us not forget our earlier discussion of the methodological value of politics-and thus political, analyses of scientists' thought and behavior for which his lengthy case study of Galileo (which constitutes several chapters of AM) is supposed to be an illustration. This methodological recommendation is in clear contradiction to Feyerabend's much-discussed principle of "Anything goes" which he not only admitted in AMM (105, footnote 38) was a "joke" but later added was "empty, useless and pretty ridiculous." 20 Therefore, we can put this alleged principle aside and not waste any more of our energies considering it except to note that Feyerabend's supposed anarchism or Dadaism (AM: 21, footnote 12) appears to be fundamentally inconsistent with the normative aspects of AMM and AM which are pervasive. Obviously, not "Anything goes" or not "Everything is permitted" (to use a nihilistic formulation) and Feyerabend is not a genuine Dadaist, despite his claims to the contrary, much less a "flippant one" (AM: 21, footnote 12). 21 However, we should note here that Feyerabend's prescriptions are just that, prescriptions, that are neither historical nor even empirical! Herein, Feyerabend's own criticism of Kuhn (cited above) boomerangs against himself and also reflects both the essential normative nature of methodology (epistemology) and Feyerabend's apparent unawareness of this

19. Feyerabend, "From Incompetent Professionalism..." op. cit., p. 46.
20. Ibid., p. 41.
21. This same point is recognized by one of Feyerabend's reviewers; see, J.N. Hattiangali, "Playphilosopher," Philosophy of the Social Sciences, Vol. 8, p. 60.
essential point. (Feyerabend admits later that AM and AMM are indeed normative but this admission does little to reconcile this conflict.)

It is because of this prescribed empirical approach to the philosophy of science that Feyerabend opposed vehemently the claimed significance of normative (i.e., non-historical) philosophy of science to science proper. Furthermore, according to Feyerabend, continued adherence to standard normative philosophy of science will seriously hinder scientific progress (see "law-and-order science" AM: 27).

Perhaps the greatest error Feyerabend commits in AMM and AM is his failure fully to appreciate the essential and ineliminable normative (prescriptive and evaluative) nature of second-order fields like the philosophy of science. It is significant regarding this point that when Feyerabend discussed different conceptions of the nature of logic (AM: 252) he omits entirely the normative one.

Stephen Toulmin addresses this methodological normative-empirical debate concerning the status of logic and clearly expresses the normative one when he writes: "He [the logician] is not simply a sociologist [or historian] of thought; he is rather a student of proper inferring-habits and of rational canons of inference." Or, as the author of a textbook in logic puts it: "The study of logic is the study of the methods and principles used in distinguishing correct from incorrect reasoning."

Logic, epistemology, and the philosophy of science (and the other second-order fields like the philosophies of, mathematics, art, law, etc.) should not be considered as modes of inquiry with the aim of informing us descriptively and explanatorily about particular knowledge claims (we have the empirical fields of the history, sociology, and psychology of ideas for that, where the last may be considered to address the problem of human creativity), but instead

22. That he is engaged in normative discourse in AMM and AM in finally explicitly stated by Feyerabend in his Philosophy of the Social Sciences article, op. cit., p. 40: "...in my case studies I not only try to show the failure of traditional methodologies. I also try to show what procedures aided the scientists and should therefore be used. I criticize some procedures, I defend and recommend others" (emphasis in original).


provide the evaluative criteria to assess these claims and methodological prescriptions on how to produce acceptable claims.

The meta-ethical philosopher Paul Taylor describes the second-order study of morality: "Concerning this subject matter [of meta-ethics] it asks three sorts of questions: questions about meaning, questions about truth and questions about method...Thus they [meta-ethical philosophers] are led to investigate the criteria of validity or the rules of valid inference which are tacitly assumed in systems of normative [first-order] ethics."  

In this context second-order fields may be seen to consist of prescriptive theories of rationality proposed for a particular first-order domain and includes the methodological recommendations to conduct such first-order inquiry.

One might think that Feyerabend is either unaware of the normative nature of second-order discourse or rejects it implicitly by his repeated claims concerning the importance of empirical disciplines to the philosophy of science. But like Kuhn which Feyerabend himself noted-he wishes to exploit (in an unwarranted manner) the necessary normative force of second-order methodological inquiry while not making it clear that he is prescribing and thus not burdened with a defense of his prescriptions. Feyerabend obviously makes methodological recommendations (and relies on such positively normatively loaded terms like "progress," "advancement," "correct," and "rationality") which cannot come from historical, sociological, psychological, political, or anthropological studies totally. Consider the following statement and its essential normative character: "This liberal practice [of scientists not obeying methodological prescriptions, according to Feyerabend's historical studies of science] I repeat, is not just a fact of the history of science. It is both reasonable and absolutely necessary for the growth of knowledge" (AM: 23, emphasis in original). Other clear examples of Feyerabend doing more than merely describing scientists' behavior occurs when he says: "In order to progress, we must step back from the evidence, reduce the degree

25. Taylor, op. cit., pp. xiv-xv (emphasis in original) and xi. Compare these latter remarks of Taylor's to Toulmin's cited earlier regarding logic.

26. Although Feyerabend seems on the right track when he says the theory of rationality is the main topic of AM (see above) he either never actually pursues this topic or does indeed pursue it but proposes a much too narrow and question-begging (empirical) "theory of rationality."
of empirical adequacy (the empirical context) of our theories, abandon what we have already achieved, and start afresh" (AM: 113); and, "One would have thought that the philosopher of science would be most interested in picking out and analyzing in detail those moves which are necessary for the advancement of science. Such moves, I have tried to show, resist rational reconstruction" (AM: 155, footnote 7, emphasis in original).

These two sentences reflect Feyerabend's confusion over normative matters. I agree with the first of them but "advancement" is clearly a normative term as well as "rational reconstruction."27 (This confusion over empirical and normative modes of inquiry by Feyerabend is similarly quite pervasive in political science.) The unannounced and undefended shift from cosmological (historical) empirical analysis to prescriptive scientific methodology (which is often made to appear as Feyerabend's main object of criticism, represented by the likes of Popper) reveals several profound errors in Feyerabend's entire discussion.

First, Feyerabend's shifting from empirical claims about Galileo, for example, to methodological prescriptions seems vulnerable to the charge of committing the naturalistic fallacy. Why choose Galileo as opposed to Lysenko to offer as a case study? Such a choice implies a methodological prescription as all case studies do—a point which political scientists, particularly comparative political scientists must keep in mind—which cannot be defended wholly by historical (or other empirical) methods alone.

Second, Feyerabend's attacks on prescriptive philosophy of science for being remote, idealistic, a hindrance to "progress in

27. Feyerabend seems to have missed the usual meaning of "rational reconstruction" and its inherent normative elements. See Carl Hempel's influential discussion of rational reconstruction or "explication" as he puts it: Fundamentals of Concept Formation in Empirical Science, International Encyclopedia of Unified Science, Vol. 4. (Chicago University of Chicago Press, 1965), p. 1, where he writes: "The definitions proposed in these [explicative] theories are not arrived at simply by an analysis of customary meanings. To be sure the considerations leading to the precise definitions are guided initially by reference to customary scientific or conversational usage; but eventually the issues which call for clarification become so subtle that a study of prevailing usage can no longer shed any light upon them. Hence, the assignment of precise meanings to the terms under explication becomes a matter of judicious synthesis, of rational reconstruction, rather than of merely descriptive analysis: An Explication sentence does not simply exhibit the commonly accepted meaning of the expression under study but rather proposes a specified new and precise meaning for it" emphasis in original.
science" (e.g. AM: 166, and his attacks on Popper's scientific epistemology), and "reconstruction[s] of the instruments of reconstruction" appear similar to the typical political criticism of "utopianism" in politics. Yet Feyerabend proceeds to attempt to cover up the unavoidable normative element inherent in the notion of "progress" by saying (AM: 27) that it be understood in any way the reader wishes to! This move, I think, renders his entire argument incoherent when he attempts to justify his many prescriptions and evaluations by invoking the now variously defined goal of "scientific progress" (e.g. AM: 113). Which notion of "progress" is Feyerabend assuming? Why? He never addresses this important question. If the meaning of "progress" is completely open-ended then it would include something like Poppies "error elimination" view which Feyerabend rejects. It seems as if, like Kuhn, Feyerabend wishes to have it both ways-Dadaist and prescriptivist-at the same time but it will not work.

Third, at key points in his argument, Feyerabend repeatedly uses the expression "science as we know it" (e.g. AM: 65) without even raising let alone attempting to answer the question: assuming his historical claims are correct (which are always open to challenge given the complexity of the scientific community and scientists' behavior; see opposition to Feyerabend's historical account of Galileo's theory by P.K. Machamer et al; AM: 112-119), why should present scientific methods be maintained? The phrase "science as we know it" and its important role in Feyerabend's argument reveals some important normative question-begging on Feyerabend's part and is inconsistent with his empirical "philosophy" of science.

We need not discuss in detail the other errors implicit in Feyerabend's epistemological anarchism (or, to be more accurate, as I shall...

28. "From Incompetent Professionalism..." op. cit., p. 51. Compare this criticism to Gunnell's charge of political scientists being "imitators of imitations" (see above) in the philosophy of science. Both Feyerabend and Gunnell attack (erroneously, in my view) metalevel prescriptive discourse.

29. How would Feyerabend comment upon Popper's strong distinction between "change" and "progress" within a domain? See, Karl Popper, *Conjectures and Refutations*, (New York: Harper and Row, 1968), pp. 216-217, where Popper emphasizes the crucial importance of a metalevel theory of progress: "And it is this (meta-scientific) knowledge which makes it possible to speak of progress in science, and of a rational choice between theories" (p. 217). If Feyerabend rejects metalevel prescriptive philosophy of science (as he appears to but actually does not) he is precluded from using "progress" in the normative manner he does.
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claim shortly, his theory of scientific creativity, which rather than being new is actually similar to not only Mill to whom he acknowledges debt, but Peirce and Popper as well; for example, his erroneous conflation of causes with reasons (see Feyerabend’s attack on the limits of argument, AMM: 21–26). Feyerabend does not appreciate the normative status of epistemology and he misconceives its limits. Epistemology is not intended to produce happiness as he implies it should in one of is criticisms of Popper (because of the Tat-tor’s alleged puritanism, see AM: 48, footnote 2). To so conceive epistemology would completely change the nature of “science as we know it” from a way of knowing reliably to knowing only that which will make us happy.

Epistemology is not devoted to moving people beyond the epistemological force of our arguments. We, political scientists, are too well aware of the limits of rational persuasion in human affairs. What will usually move an actor and what should epistemologically move him/her can be (and, unfortunately, often is) different, but this is not ground to reduce without defense one to the other, or the philosophy of science to its history, sociology, psychology, politics or anthropology.

Feyerabend also erroneously conflates questions of origin with questions of epistemological acceptability. The historian, sociologist, or psychologist of ideas (Popper would say “beliefs”) here can tell us why (causally) a particular person or group of people make or accept a claim to knowledge, but they cannot tell us why (epistemologically) one (or they) should have accepted the claim.

Finally, Feyerabend’s argument does contain a genuine disagree-

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31. It should be mentioned here that Feyerabend does briefly discuss his rejection of the well-known distinction between the contexts of discovery and justification (AM: 165–167). This discussion should have been a central portion of his argument, as I see it, since it embodies the confilation of the two modes of inquiry: empirical (discovery) and normative (justification) and such a treatment might have prevented Feyerabend from committing some of the errors here cited. Instead, we find an extremely brief argument that culminates with the following: “Indeed, science as we know it today could not exist without a frequent overruling of the context of justification (AM: 167). The underlying normative element within the phrase “science as we know it today” is overlooked by Feyerabend.

Also the idea that scientific practice “overrules” prescriptive scientific methodology
ment with Popper over the latter's third world view. Without going into Popper's position too deeply, his distinction between the production process (of knowledge) and its result or product is helpful. Feyerabend is always forward-looking (what we would expect from a theory of creativity) anxious to stimulate future creativity but rarely if ever "retrospectively justificatory." 

Feyerabend does not address at all the central question that prescriptive philosophers of science are preoccupied with: theory-choice and its grounds, or, determining the epistemological quality of what we have created.

While Feyerabend does not treat all scientific knowledge claims as if they were of equal value (perhaps as a genuine epistemological Dadaist should)-he does distinguish between Galileo's and Lysenko's ideas-he provides no method of how we may assess or hold accountable the products of our creative efforts except to refer, approvingly, to the "political maneuvering," "brainwashing," (AM: 25) or "psychological tricks" (AM: 81) of Galileo! Despite the fact that political theorists thought that we had dismissed the "might makes right" doctrine long ago we have with Feyerabend's epistemology a position that comes very close to "might makes epistemological right." (Here the similarity with Kuhn's non-epistemological political and sociological bases of scientific change is striking.)

In summarizing this section, I would like to reiterate appears identical to the long-standing controversy within political theory regarding the conflict between the recommendation of participatory democracy and actual political practice within so-called "democracies" (including the United States, of course). Very much like Feyerabend, the "democratic" pluralists have argued that where such a conflict exists between the ideal (participatory democracy or philosophy of science, respectively) and the real (pluralist democracy or historical scientific practice) instead of admitting the discrepancy and attempting to improve the practice they argue that the ideal should be changed to fit the practice because of prior commitments to the practice: in the pluralist case, commitment to the United States' political system and in Feyerabend's, commitment to (some) scientists thought and behavior. For this conflict within democratic theory, see, Henry S. Kariel, (ed.), Frontiers of Democratic Theory. (New York: Random House, 1970).

34. Toulmin, op. cit., p. 6. It is to this aim that logic is (should be) addressed according to Toulmin.
Feyerabend’s inadequately defended empirical view of the philosophy of science, and, furthermore, his shifting from empirical to normative claims without the proper evidence or reconciling this shift with his previous empirical assertions.

In brief, what I see as Feyerabend’s most serious error is the "modal fallacy," that is, the confusing of two different modes of discourse, wherein each discourse contains different problems, cognitive aims, methods, and criteria of assessment. These two modes of discourse are the empirical mode of studying science (social scientifically according to Feyerabend—interestingly, he makes several comments indicating his aversion to the economics of science, the grants game, etc., but we can only guess why if we accept his social scientific view entirely) and the second-order normative methodological mode of making prescriptions and evaluations regarding scientific inquiry. Once the normative nature of second-order methodological discourse is clearly understood most of Feyerabend’s historical account is irrelevant to it except where he unwittingly slips into normative pretentions.

The proper reaction to the inevitable gaps between prescribed ideals and actual practice is to either attempt to improve the practice according to the ideas, for example, the discrepancies between moral prescriptions and moral practice (is Feyerabend a moral Dadaist too?) or change the ideas to better fit the practice when the practice is judged superior according to another set of defensible ideals which Feyerabend (and I might add the "democratic" pluralists as well) fail to do!

If political scientists can appreciate this point about the normative nature of second-order inquiry it will be an important step to their better comprehension of the relationship between political science (first-order) and the philosophy of science (second-order) which does not threaten the autonomy of political science.

IV. Feyerabend’s Theory of Creativity

Under the Guise of a Theory of Rationality

When reading AMM and AM one is struck by Feyerabend’s passionate rejection and fear of the stultifying consequences of conformity. Concerning this point, he does, in fact, follow the same driv-

ing force as Mill in the latter's *On Liberty* (and we may add of Peirce with his major methodological principle of "Do not block the way of inquiry and Popper, who wrote: "...it is part of our critical and fallibilist approach; that every source [of ideas] is welcome....")

Feyerabend parts company with these epistemologists when he stops his discussion just at the point of proposing criteria of assessment of the epistemological value of offered knowledge claims. This produces an anarchistic or Dadaist theory of creativity alone and not a genuine theory of knowledge despite his pretensions to the contrary. Here are some of Feyerabend's statements:

a) This, I think, is the most decisive argument against any method that encourages uniformity...It enforces an unenlightened conformism, and speaks of truth; it leads to a deterioration of intellectual capabilities, of the power of imagination, and speaks of deep insight; it destroys the most precious gift of the young—their tremendous power of imagination, and speaks of education (AM: 45).

b) It is possible to retain what one might call the freedom of artistic creation and use it to the full, not just as a road of escape but as a necessary means for discovering and perhaps even changing the features of the world we live in (AM: 52).

c) Counterinduction is an essential part of such a process of discovery (AMM: 52).

d) Extreme positions are of extreme value. They induce the reader to think along different lines. They break his conformist habits (AMM: 111, footnote, 51).

When Feyerabend's argument is read as theory of creativity calling for the maximum stimuli and opportunities of expression of human imagination, thus the thwarting of any excessive conformity, there is no difference between his position and Popper's of "bold conjectures" and "preferring the more improbable theory." And what else could Feyerabend mean when he says "extreme [improbable] positions are of extreme value" except to say extreme positions stimulate or provoke our creative imaginations and certainly not that they are of extreme epistemological value!

To show how far Feyerabend is willing to go, he comes close to recommending violence for its creative benefits ("it [violence]

releases one's energies and makes one realize the powers at one's disposal" AM: 187). One may inquire here if Mill would endorse this extreme(ly creative) position of Feyerabend's knowing his concern to carefully distinguish thought and discussion from action."

Feyerabend's error regarding the normative nature of second-order inquiry is compounded here with his misconception of the problems addressed by such inquiry pertaining to science. Philosophers of science, like Peirce and Popper, encourage total freedom or spontaneity concerning the proposing of new scientific ideas (no scientific claim is absolutely certain, all scientific claims are tentative, as a result of their fallibilist epistemology). But they proceed, unlike Feyerabend, to address the quintessential epistemological issue of critically assessing which claims are more rationally preferable and why. They would reject the position which seems implied by Feyerabend's sole emphasis upon creative spontaneity and freedom of thought; applied to not only proposing ideas (context of discovery - which Peirce and Popper do accept) but, in addition, to the bases of accepting some (context of justification, which Peirce and Popper reject by their proposing specific grounds for acceptance in their epistemologies). This creativity applied to the acceptability of knowledge claims might permit some people to use the scientist's astrological sign, others his/her place of birth, still others his/her age or who know what, as criteria for the acceptance of scientific claims.

In standard prescriptive philosophy of science there is no conformity with regard to proposing ideas only a recommended set of criteria regarding the acceptability of some of the ideas. This apparent conformity (Is claiming the truth of "2+2=4" or the second law of thermodynamics conforming? Is it stifling of human creativi-

39. While Mill is clear and strong about his prescription for absolute freedom of inquiry and discussion he does drop this absolute claim when he discusses freedom of action. He notes that, "no one pretends that actions should be free as opinions. On the contrary, even opinions lose their immunity when the circumstances in which they are expressed are such as to constitute their expressing a positive instigation to some mischievous act," *On Liberty*, op. cit., p. 80.

A critic of Mill might say here that what Mill gives with one hand he takes away with the other regarding the freedom of speech since the precise determination of which circumstances deem the expression of opinions "to constitute a positive instigation to some mischievous act" is notoriously fraught with difficulties.
ty?) to Feyerabend should not squelch any scientist's creativity as long as we distinguish the formative process of producing scientific ideas (which is the appropriate domain for a theory of creativity) and evaluating what is produced (which is the appropriate domain for a normative second-order inquiry).

V. Feyerabend's Omission of the Epistemological Accountability Element with the Concept of "Scientific Objectivity"

One of the most important concepts in all of scientific epistemology is the concept of "objectivity" and is probably the most misunderstood by political scientists as well. Feyerabend attacks the aim of "objective" scientific knowledge (he calls it a "craving for intellectual security" AM: 27-28). His most definite statement regarding objectivity is the following: "Variety of opinion is the necessary for objective knowledge (AM: 46). Here we have another example of the pervasive concern of Feyerabend with unhindered creativity. His attack on objectivity in science seems to follow from his desire to maximize the creative freedom for new scientific ideas and his identifying scientific objectivity with conformity and fixed methodological rules ("mental rigor mortis" AM: 182).

Scientific objectivity does not mean a kind of deadening of the creative spirit as a result of some methodological conformity to fixed rules. Once again Feyerabend confuses the proposing of scientific claims which should be completely open with the grounds for judging them (although Mill's limiting condition of speech and presumably inquiry as well, constituting a "positive instigation to some mischievous act" might seem wise when we consider something like a racial theory of intelligence). Applied to this productive process of new claims, philosophers of science would probably have no difficulty in going along with Feyerabend's joke of "anything goes." Throughout his many epistemological writings, Popper has attacked the idea of any epistemological authority which attempts to rule autocratically.

Objectivity in science refers to the evaluation of scientific claims- the epistemological accountability of scientific assertions. If there truly is one scientific methodological principle, rather than Feyerabend's misleading joke, it is: "All scientific claims must be held accountable." In Feyerabend's AMM, AM, and his remarks in Philosophy of the Social Sciences I cannot find any reference to this
idea at all. Even when he characterizes his "rationalist" opponents
he never includes this crucial idea.

According to Feyerabend's own source on epistemology Mill, it is
the openness to criticism which is central to his "collision theory of
truth." Without such a possibility of epistemological accountability,
the full critical discussion which Mill sought so fervently to create
and maintain would not be possible. (This is just one example of
Feyerabend diverging from Mill, but see below.) Because Mill (and
Peirce and Popper to name two others who stress accountability)
took seriously men's fallibility, and ineliminable possibility of being
wrong, the manner in which this determination was made was
crucial to their epistemologies. A political methodologist has put it
succinctly: "It is accountability, not neutrality, that marks the prin-
ciple of objectivity.

Feyerabend's confusion regarding the accountability element
within scientific objectivity is revealed in his shockingly (for they are
clearly intended to shock his "rationalist" readers) favorable com-
ments upon "myth, religion, magic [and] witchcraft...." (AM: 298).
He views the philosophy of science's dismissal of these claimed ways
of knowing as a "taboo reaction" founded on pure power (AM: 299).
What disturbs philosophers of science about these purported
epistemological methods is that they reportedly lack public ac-
countability of their claims. (I say "reportedly" because Feyerabend
has challenged a critic to provide evidence of his investigations in
such "fields" and I have done little.)

If the likes of Carlos Castaneda and Rabbi Akiba (AM: 190) seek
and provide causal explanations of empirical phenomena open to
public accountability as to their adequacy, philosophers of science
should not rule their claims out of bounds no matter what they used
to stimulate their imaginations. It should be remembered, however,
that scientific epistemology is for scientific knowledge only and
should not presume to address the issue of the possibility of other
forms of human knowledge and methods to achieve these.

Dogmatism, or not being open to epistemological accountability,
is what philosophers of science reject. When Feyerabend says, "At-
tacking the basic ideas of a scientific opponent evokes taboo reac-
tions which are not weaker than are the taboo reactions in so-called

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Feyerabend's Against Method

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40. Landau, op. cit., p. 54.
41. "From Incompetent Professionalism..." op. cit., p. 47.
primitive societies. Basic beliefs are protected by the reaction as well as by secondary elaborations, as we have seen, and whatever fails to fill into the established category system or is said to be incompatible with this system, is either viewed as something quite horrifying, or, more frequently, *it is simply declared to be non-existent*. The massive dogmatism I have described is not just a fact, it has also a most important function. *Science would be impossible without it.*" (AM: 298, emphasis in original), he is confusing once again an empirical with an epistemological point. To be sure, openness to criticism and dogmatism can be viewed psychologically. And, probably no doubt dogmatism may be true of individual scientists or groups of scientists during particular historical periods who have been unwilling to consider their ideas fallible and thus accountable. But these psychological and historical considerations have no bearing upon what they should have been doing epistemologically. As Peirce emphasized, science is a community activity and advances within it occur on the social level (on which the accountability process occurs). This point (and others we have discussed previously) raises the general problem of the proper relations between the empirical sciences which study human behavior (the social sciences) and epistemology. More will be said on this subject in a later section on the role of history in the philosophy of science, but I hope by now the reader is convinced that Feyerabend's discussions raise essential questions for all social scientists including political scientists.

**VI. Feyerabend's Conception of Science**

Whenever scholars communicate with each other the nature of the problem must be clearly defined to make sure that they are in fact addressing the same problem (failure to do so only produces the extremely wasteful cross-purpose debate that political scientists are so familiar with).

When Feyerabend does clarify what he means by "science" (which is not all that often) he delineates two general traits: a) that science aims "to change and to influence" the material world (AMM: 80) and change it toward the particular goals of freedom, happiness and humanitarian concern for others (AM: 48, 188); and, b) that science is a "complex and heterogeneous historical process" (AM: 146, emphasis in original). "We want to know...quantum theory as actually practised by physicists...For it is the work of the
physicists and not the work of the reconstructionists we want to examine" (AM: 253).

It is clear that Feyerabend considers science to be the actual behavior and thought of a group of people called "scientists" (a group presumably defined by those attaining a credential like a Ph.D.—note who is emphasizing professionalism here!) aimed toward changing the world to maximize certain values (it is not clear whether these values are in fact those defined as those of the scientists or Feyerabend’s)

This should not surprise the reader of AMM and AM. This conception of science is certainly compatible with Feyerabend’s emphasis upon the empirical social sciences, such as history and anthropology. However, while most philosophers of science will grant the possibility and sometimes the desirability of the empirical approach to science (and scientists), they would reply that it is irrelevant to their normative view of science as a method of knowing simpliciter and not a method of achieving happiness, freedom, etc. and certainly not a historical process, but a prescribed manner of attaining reliable, publicly defensible knowledge claims.

Now, such a debate should seem familiar to political scientists. We have had raging in our discipline the same problem of whether scientific political science should be solely a method of obtaining political knowledge or to use the knowledge produced to change the world. We have two questions here: a) What is the nature of the philosophy of science: empirical social science (history, sociology, psychology, etc.) as opposed to prescriptive epistemology? and, b) What is its aim: to change the world or not?

Feyerabend claims the first alternative of each question and philosophers of science choose the second. Philosophers of science can, consistent with their inquiry avoid (although not completely, e.g., history) Feyerabend’s conceptions and aims and usually do. Similarly, while Feyerabend might wish, given his empirical approach, to avoid normative claims (although why he would want merely to describe the behavior of scientists unless there were some underlying values implied escapes me, although it explains his lack of hesitation to examine magicians, witches, etc.), he does not. Like Kuhn, Feyerabend makes many normative claims. This is what agitates his critics so much. It is not that he chooses different questions, but that he wishes to use his different methods and aims to cast aspersions upon their questions and methods.
As social scientists, Kuhn and Feyerabend may be excellent (which leads to the prediction that like Kuhn’s, once Feyerabend’s work filters down to political scientists, given time and overcoming the intellectual lag between the disciplines, his work will become quite popularly accepted). As pretenders to the mantle of philosophers of science they are merely wrongheaded.

The history of science, for example, addresses important empirical questions (about what scientists did and thought) but we need to distinguish these from what the scientists should have done and thought (the domain of the philosophy of science).

To claim simultaneously that the former is the latter, and that the latter mode of inquiry is characterized by all kinds of negative traits such as restricting man’s freedom, creativity, etc. is quite a feat. No wonder Feyerabend’s critics in the philosophy of science have reacted so strongly.

I believe that there is a proper role for both approaches to science but that we must keep them distinct and not-to use a favorite word of Feyerabend’s—“contaminate” them both.

VII. The Proper Role of History within the Philosophy of Science

Among the social sciences, Feyerabend prescribes history for the philosophy of science. From his assertions on the importance of history, we may infer its possible application to political science. Feyerabend writes: a) ”It is this reluctance [of philosophers and scientists to adopt their general views to actual activities] this psychological resistance which makes it necessary to combine abstract argument with the sledge-hammer of history...History, however, is necessary also, at least in the present state of philosophy, because it gives our arguments force” (AM: 159); b) ”...we must ... make sure that our prescriptions have a point of attack in the historical material...” (AM: 167); and, c) ”...historical data play a decisive role in the debate between rival methodologies” (AM: 183). It is difficult to consider Feyerabend’s position persuasive. How can the history of actual scientific thought relevantly bear upon what scientific thought should have been? Would (should) we expect to learn about moral prescriptions from observing actual moral conduct? How can history play a "decisive" role deciding between competing (prescriptive) methodologies? Why accept Galileo as a (prescribed) historical case study as opposed to some other actual scientists? Is not a methodological prescription necessarily embed-
ded in all case studies? And, moreover, should not this prescription be defended? If Galileo did in fact act and think in the manner Feyerabend claims, why should other scientists emulate him? Feyerabend says because he was "on the right tract" (AMM: 25) but if so, does not this conclusion beg essential normative methodological questions?

All these points apply not only to Feyerabend but to political scientists who utilize historical case studies. Unless we are intrinsically interested in the particular case (e.g. Feyerabend's six chapters on Galileo), we wish to know the general methodological value of the specific case chosen. From a methodological point of view, case studies are only of instrumental value insofar as their claimed lessons are in fact generalizable; Feyerabend does not show this despite several such claims about the history of science. Should all scientists rely on tricks and political maneuvers? Why? How has Feyerabend answered this essential epistemological question of defending his prescribed methodology, other than claiming it will maximize creativity (which he does not support)?

I do not know what kind of "sledge-hammer" history is or what kind of "force" it possesses, but it cannot be epistemological. Prescriptive philosophy of science can be ahistorical, as any prescriptive field may be, in contrast to a derivative practical application of the general prescriptions which must consider specific historical circumstances. We do not oppose lying, political torture, or even prescriptive philosophy of science simply from observing history. These are matters of normative principles and the rationality of their respective discourses.

Feyerabend argues throughout AM that because his historical research regarding Galileo shows violations of some philosophy of science prescriptions (primarily Popper's) that not only are these particular prescriptions wrong but that the whole enterprise of prescribing scientific methodology is doomed. This seems like a straightforward commission of the naturalist fallacy (and very much like the pluralists' changing-the-ideals-to-fit-the-practice move).

I also am unclear when Feyerabend talks about a historical "point of attack." Certainly the history of science has a role in the empirical study of science. It can help us understand the meaning of scientific theories by studying their intellectual background. But what will examining case studies do for philosophy? For essential normative questions? Feyerabend does not tell us.
As in other places in our discussion (except I think both are wrong here) Popper's and Feyerabend's positions are quite similar. Not only does Popper use the same scientist, Galileo, to illustrate his "problem situation" view and to criticize a Feyerabend-like explanation of Galileo's methods such as; "ambition, jealousy, or aggressiveness, or the wish to create a stir...." Popper clearly seems to reject Feyerabend's empirical social scientific approach to the philosophy of science when he says:

> We should constantly be aware of the distinction between problems connected with our personal contributions to the production of scientific knowledge on the one hand, and problems connected with the structure of the various products, such as scientific theories or scientific arguments, on the other... We can learn more about the heuristics and the methodology and even about the psychology of research by studying theories, and the arguments offered for or against them, than by any direct behavioral or psychological or sociological approach. In general, we may learn a great deal about behavior and psychology from the study of products."

However, throughout his recent collection of epistemological writings Popper appears to be making prescriptive epistemological assertions. Popper does so in quite empirical-sounding language, leaving himself open to an attack by an empiricist like Feyerabend that he (Popper) is incorrect empirically by the use of historical studies. For example, Popper says: "In all sciences, the ordinary approach is from the effect to the causes." "Our conjectural reconstruction of the situation may be a real historical discovery." We must note the underlying normative element in rational reconstruction (see above). (Popper seems to confuse empirical and normative discourse as does Feyerabend.) "I suggest that it is the aim of science to find satisfactory explanations...." Again, like Feyerabend, Popper puts normative claims into empirical language making the philosopher's theory vulnerable to historical criticism which could be avoided if the confusion in language, or meaning, or methodological understanding, did not occur.

42. Objective Knowledge, op. cit., p. 174.
43. Ibid.
44. Ibid., p. 114.
45. Ibid., p. 115, my emphasis.
46. Ibid., p. 189.
47. Ibid., p. 191, emphasis in original.
The lesson here is plain: methodologists, including philosophers of science and political methodologists, must make their prescriptions explicitly clear as such—must not say "is" when what is meant is "should" or "ought," for example. This is necessary if both the empirical and normative fields are to assume their proper domain and have them so recognized by researchers. Methodology ought to be prescriptive and its language should reflect this clearly. Not to do so, and to slide back into empirical language is exploitative as Feyerabend himself noted about Kuhn!

VIII. Feyerabend's Incomplete Reading of Mill's On Liberty

In Chapter II of On Liberty, "On the Liberty of Thought and Discussion," John Stuart Mill presents his epistemology. In it we find his "collision theory of complex and many-sided truth." As Feyerabend's significant error in his interpretation of Mill is to read Mill's epistemology solely as a theory of creativity, (This is the very error Feyerabend repeats in his own epistemology. We have here a substantive mistake taking the same form as an exegetical one.)

While Mill did believe truth was "complex and many-sided" he believed nonetheless that it existed and that man could know it. Even if we could not know that we knew it we could still assume "its [knowledge claim] truth for purposes of action...". It is this very heart of an epistemology (the claim to knowledge) which Feyerabend pointedly omits.

The reason Mill emphasizes the absolute freedom of critical discussion is because he claims it to be the best human method of achieving knowledge of the truth: by combining partial truths. Here we must stress Mill's belief in the corrigibility of man's errors. As a result of this view regarding the essential importance of public discussion of propositions and the corrigibility of man's errors, Mill opposes strongly any effort to limit public discussion

49. On Liberty, op. cit., p. 28.
50. See ibid., p. 81.
51. Ibid., p. 28. Compare to Popper’s fallibilist epistemology.
which assumes infallibility and constitutes dogmatism.

But Feyerabend pace Mill seems to endorse just such a dogmatism, indeed he appears to prescribe dogmatism and deflection of criticism that is in flagrant violation of the main thrust of Mill's epistemology (e.g. AM: 298). Feyerabend may be empirically correct about the important function he discerns and claims about dogmatism in the history of science but Mill would say it was undesirable epistemologically.

Mill emphasizes the importance of actual criticism on one's claims, especially for "a real understanding of moral and human subjects," to such an extent that if opponents do not exist, it is essential to construct the strongest possible opposing argument so that one may have a better understanding of his own position and its defense. Mill clinches this point when he says: "He who knows only his side of the case, knows little of that." This contrast between Feyerabend and Mill regarding the role of criticism and dogmatism in epistemology could not be clearer or more important.

Mill does not prescribe the proliferation of ideas for their spontaneity value alone (as does Feyerabend) but for the purposes of increasing man's knowledge through "reconciling and combining of opposite [opinions]." Feyerabend makes two serious errors in his reading of Mill: 1) he overlooks Mill's epistemology and his belief that man can make progress in his knowledge; critical comparisons and choices are possible and are not just exercises in creativity; and, 2) Mill would be opposed fundamentally to any kind of dogmatism (that is, the conviction that one's knowledge claims are not vulnerable to criticism which therefore implies the limitation or elimination of such criticism) which Feyerabend appears to accept and indeed even prescribe to scientists!

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52. See for example, ibid., p. 30. This absolutist view does appear inconsistent with Mill's "instigation of some mischievous act" (see footnote 39) condition to free speech. Perhaps this is why On Liberty has the reputation of, in the words of a sympathetic commentator of "lack[ing] logical congency." See Isaiah Berlin, "John Stuart Mill and the Ends of Life," in his Four Essays on Liberty, (New York: Oxford University Press, 1969, p. 174. But this reputed inconsistency in Mill's work may be the result of underestimating his "combining of competing partial truths" method.

53. See ibid., p. 53.

54. Ibid., p. 52.

55. Ibid., p. 104.
IX. Conclusion: Lessons for Political Inquiry from Our Examination of Feyerabend's Epistemology

I hope that in this examination of Feyerabend's epistemology, the following implications for political inquiry are clear, especially concerning their relations to the second-order discourse of the philosophy of science.

1) All logical, epistemological and philosophy of science discussions should be understood as distinctively normative. While the history, psychology, sociology, politics, and anthropology of science are legitimate and valuable empirical modes of inquiry they should not be considered by themselves to provide methodological prescriptions on the conduct of scientific (political) studies.

2) Scientific objectivity is not value-neutrality but the central fallibilist epistemological requirement of the accountability of assertion. It is for this goal of epistemological accountability that maximum public scientific discussion derives its epistemological value and defense.

3) Science should not be considered merely an historical process but as a method of knowing, a particular and distinctive method, addressed to its range of empirical causal questions (which does not thus preclude other ways of knowing for other forms of human experience, e.g. religion).

4) The value of history is quite limited to normative epistemology and philosophy of science. It is less with what researchers did than why they should do what they should, that these fields should be concerned. And, where a discrepancy occurs between these two questions (empirical vs. normative), what must be changed is not the prescribed methods (if defensible) but the actual practices.

5) Feyerabend should reread Mill (and Popper as well) and note more carefully their epistemology and their arguments for the epistemological value of criticism.

The last two sentences of AM reveal Feyerabend's confusion or his trying to have it both ways (exploitively) regarding the double entendre (both normative and empirical) of the word "rationality": "Science uses the method of ballot, discussion, vote, though without a clear grasp of its mechanism, and in a heavily biased way. But the rationality [commitment to continued practices or to some ideas] of our beliefs will certainly be considerably increased" (AM: 309).

The rationality (in the normative sense) of our scientific beliefs
will only increase by recognizing the need for epistemological accountability and full public critical discussion. Feyerabend has appreciated part of this message (the need for the freedom and enhancement of the conditions of creativity) but only in part. Like political anarchism, which raises the central question of politics (legitimacy of political rule versus political authority), Feyerabend's epistemological anarchism raises the central question of epistemology (the accountability of assertion through criticism). Just as political anarchism erroneously rejects political authority, Feyerabend's epistemological anarchism erroneously rejects the accountability of assertion through criticism. To this error, Feyerabend adds his confused and unwarranted proposal that the empirical social sciences constitute the philosophy of science.

From a Millian point of view, if political scientists have a better understanding (a "clearer perception and livelier impression") of their own mode of inquiry, then this consideration of Feyerabend's misguided and misleading anarchistic theory of knowledge has not been for nought. Creativity yes, (for who can oppose creativity?), but also criticism and growth of knowledge.

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