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Is there a logic of scientific discovery?

Is there a logic of scientific discovery? The approved answer to this is "No." Thus Popper argues: [1] "The initial stage, the act of conceiving or inventing a theory, seems to me neither to call for logical analysis nor to be susceptible of it." Again, "There is no such thing as a logical method of having new ideas, or a logical reconstruction of this process." Reichenbach writes that philosophy of science "cannot be concerned with [reasons for suggesting hypotheses], but only with [reasons for accepting hypotheses]." [2] Braithwaite elaborates: "The solution of these historical problems involves the individual psychology of thinking and the sociology of thought. None of these questions are our business here." [3]

Against this negative chorus, the 'Ayes' have *not* had it. Aristotle (*Prior Analytics* II, 25) and Peirce [4] hinted that in science there may be more problems for the logician than just analyzing the arguments supporting already invented hypotheses. But contemporary philosophers are unreceptive to this. Let us try once again to discuss the distinction F. C. S. Schiller made between the 'Logic of Proof' and the 'Logic of Discovery.' [5] We may be forced, with the majority, to

[1] Karl Popper, *The Logic of Scientific Discovery.* New York: Basic Books, 1959, pp. 31–32.

[2] Hans Reichenbach, *Experience and Prediction.* Chicago: Univ. of Chicago Press, 1938, p. 382.

[3] R. B. Braithwaite, *Scientific Explanation.* Cambridge: Cambridge Univ. Press, 1955, pp. 21–22.

[4] C. S. Peirce, *Collected Papers.* Cambridge (Mass.): Harvard Univ. Press, 1931, Vol. I, Sec. 188.

[5] F. C. S. Schiller, "Scientific Discovery and Logical Proof," Charles Singer, ed., *Studies in the History and the Methods of the Sciences.* Vol. I. Oxford: Clarendon Press, 1917.

conclude 'Nay.' But only after giving Aristotle and Peirce a sympathetic hearing. Is there *anything* in the idea of a 'logic of discovery' which merits the attention of a tough-minded, analytic logician?

It is unclear what a logic of discovery is a logic of. Schiller intended nothing more than "a logic of inductive inference." Doubtless his colleagues were so busy sectioning syllogisms that they ignored inferences which mattered in science. All the attention philosophers now give to inductive reasoning, probability, and the principles of theory construction would have pleased Schiller. But, for Peirce, the work of Popper, Reichenbach, and Braithwaite would read less like a *Logic of Discovery* than like a *Logic of the Finished Research Report.* Contemporary logicians of science have described how one sets out reasons in support of a hypothesis once proposed. They have said nothing about the conceptual context within which such a hypothesis is initially proposed. Both Aristotle and Peirce insisted that the proposal of a hypothesis can be a reasonable affair. One can have good reasons, or bad, for suggesting one kind of hypothesis initially, rather than some other kind. These reasons may differ in type from those which lead one to accept a hypothesis once suggested. This is not to deny that one's reasons for proposing a hypothesis initially may be identical with his reasons for later accepting it.

One thing must be stressed. When Popper, Reichenbach, and Braithwaite urge that there is no logical analysis appropriate to the psychological complex which attends the conceiving of a new idea, they are saying nothing which Aristotle or Peirce would reject. The latter did not think themselves to be writing manuals to help scientists make discoveries. There could be no such manual.[6] Apparently they felt that there is a *conceptual* inquiry, one properly called "a logic of discovery," which is *not* to be confounded with the psychology and sociology appropriate to understanding how some investigator stumbled on to an improbable idea in unusual circumstances. There are factual discussions such as these latter. Historians like Sarton and Clagett have undertaken such circumstantial inquiries. Others—for example, Hadamard and Poincaré—have dealt with the psychology of discovery. But these are not logical discussions. They do not even turn on conceptual distinctions. Aristotle and Peirce thought they were doing something other than psychology, sociology, or history of discovery; they purported to be concerned with a *logic* of discovery.

This suggests caution for those who reject wholesale any notion of a logic of discovery on the grounds that such an inquiry can *only* be psychology, sociology, or history. That Aristotle and Peirce deny just

[6] "There is no science which will enable a man to bethink himself of that which will suit his purpose," J. S. Mill, *A System of Logic,* III, Chap. I.

this has made no impression. Perhaps Aristotle and Peirce were wrong. Perhaps there is no room for logic between the psychological dawning of a discovery and the justification of that discovery *via* successful predictions. But this should come as the conclusion of a discussion, not as its preamble. If Peirce is correct, nothing written by Popper, Reichenbach, or Braithwaite cuts against him. Indeed, these authors do not discuss what Peirce wishes to discuss.

Let us begin this uphill argument by distinguishing

(1) reasons for accepting a hypothesis *H*, from

(2) reasons for suggesting *H* in the first place.

This distinction is in the spirit of Peirce's thesis. Despite his arguments, most philosophers deny any *logical* difference between these two. This must be faced. But let us shape the distinction before denting it with criticism.

What would be our reasons for accepting *H?* These will be those we might have for thinking *H* true. But the reasons for suggesting *H* originally, or for formulating *H* in one way rather than another, may not be those one requires before thinking *H* true. They are, rather, those reasons which make *H* a *plausible type of conjecture*. Now, no one will deny *some* differences between what is required to show *H* true, and what is required for deciding *H* constitutes a plausible kind of conjecture. The question is: Are these logical in nature, or should they more properly be called "psychological" or "sociological"?

Or one might urge, as does Professor Feigl, that the difference is just one of refinement, degree, and intensity. Feigl argues that considerations which settle whether *H* constitutes a plausible conjecture are of the *same type* as those which settle whether *H* is true. But since the initial proposal of a hypothesis is a groping affair, involving guesswork amongst sparse data, there *is* a distinction to be drawn; but this, Feigl urges, concerns two ends of a spectrum, ranging all the way from inadequate and badly selected data to that which is abundant, well diversified, and buttressed by a battery of established theories. The issue therefore remains: Is the difference between reasons for accepting *H* and reasons for suggesting it originally one of logical type, or one of degree, or of psychology, or of sociology?

Already a refinement is necessary if our original distinction is to survive. The distinction just drawn must be reset in the following, more guarded, language. Distinguish now

(1′) reasons for accepting a particular, minutely specified hypothesis *H*, from

(2′) reasons for suggesting that, whatever specific claim the successful *H* will make, it will, nonetheless, be a hypothesis of one *kind* rather than another.

Neither Aristotle, nor Peirce, nor (if you will excuse the conjunction) myself in earlier writings,[7] sought this distinction on these grounds. The earlier notion was that it was some particular, minutely specified *H* which was being looked at in two ways: (1) what would count for the acceptance of that *H*, and (2) what would count in favor of suggesting that same *H* initially.

This latter way of putting it is objectionable. The issue is whether, *before* having hit a hypothesis which succeeds in its predictions, one can have good reasons for anticipating that the hypothesis will be one of some particular *kind*. Could Kepler, for example, have had good reasons, *before* his elliptical-orbit hypothesis was established, for supposing that the successful hypothesis concerning Mars' orbit would be of the noncircular kind? [8] He *could* have argued that, whatever path the planet *did* describe, it would be a closed, smoothly curving, plane geometrical figure. Only this *kind* of hypothesis could entail such observation-statements as that Mars' apparent velocities at 90 degrees and at 270 degrees of eccentric anomaly were greater than any circular-type *H* could explain. Other *kinds* of hypotheses were available to Kepler: for example, that Mars' *color* is responsible for its high velocities, or that the dispositions of Jupiter's moons are responsible. But these would not have struck Kepler as capable of explaining such surprising phenomena. Indeed, he would have thought it *unreasonable* to develop such hypotheses at all, and would have argued thus. [Braithwaite counters: "But exactly which hypothesis was to be rejected was a matter for the 'hunch' of the physicists." [9] However, which *type* of hypothesis Kepler chose to reject was not just a matter of 'hunch.']

I may still be challenged. Some will continue to berate my distinction between reasons for suggesting which type of hypothesis *H* will be, and reasons for accepting *H* ultimately.[10] There may indeed be "psychological" factors, the opposition concedes, which make certain types of hypotheses 'look' as if they might explain phenomena. Ptolemy knew, as well as did Aristarchus before him and Copernicus after him, that a kind of astronomy which displaced the earth would be theoretically simpler, and easier to manage, than the hypothesis of a geocentric, geostatic universe. *But*, philosophers challenge, for psy-

[7] Cf. *Patterns of Discovery*. Cambridge, Mass.: Harvard Univ. Press, 1958, pp. 85–92; "The Logic of Discovery," in *Journal of Philosophy*, **LV**, **25**, 1073–1089, 1958; More on "The Logic of Discovery," *op. cit.*, **LVII**, 6, 182–188, 1960.

[8] Cf. *De Motibus Stellae Martis*. Munich, pp. 250ff.

[9] *Op. cit.*, p. 20.

[10] Reichenbach writes that philosophy "cannot be concerned with the first, but only with the latter" (*op. cit.*, p. 382).

chological, sociological, or historical reasons, alternatives to geocentricism did not 'look' as if they could explain the absence of stellar parallax. This cannot be a matter of logic, since for Copernicus one such alternative *did* 'look' as if it could explain this. Insofar as scientists have *reasons* for formulating types of hypotheses (as opposed to hunches and intuitions), these are just the kinds of reasons which later show a particular H to be true. Thus, if the absence of stellar parallax constitutes more than a psychological reason for Ptolemy's resistance to alternatives to geocentricism, then it *is* his reason for rejecting such alternatives as *false*. Conversely, his reason for developing a geostatic type of hypothesis (again, absence of parallax) was his reason for taking some such hypothesis as *true*. And Kepler's reasons for rejecting Mars' color or Jupiter's moons as indicating the kinds of hypotheses responsible for Mars' accelerations were reasons which also served later in establishing some hypothesis of the noncircularity type.

So the objection to my distinction is: The only *logical* reason for proposing H will be of a certain type is that *data* incline us to think some *particular* H true. What Hanson advocates is psychological, sociological, or historical in nature; it has no logical import for the differences between proposing and establishing hypotheses.

Kepler again illustrates the objection. Every historian of science knows how the idea of uniform circular motion affected astronomers before 1600. Indeed, in 1591 Kepler abandoned a hypothesis because it entailed other-than-uniform circular orbits—something simply inconceivable for him. So psychological pressure against forming alternative types of hypotheses was great. But *logically* Kepler's reasons for entertaining a type of Martian motion other than uniformly circular were his reasons for accepting that as astronomical truth. He first encountered this type of hypothesis on perceiving that no simple adjustment of epicycle, deferent, and eccentric could square Mars' observed distances, velocities, and apsidal positions. These were also reasons which led him to assert that the planet's orbit is not the effect of circular motions, but of an elliptical path. Even after other inductive reasons confirmed the truth of the latter hypothesis, these early reasons were *still* reasons for accepting H as true. So they cannot have been reasons merely for proposing which type of hypothesis H would be, and nothing more.

This objection has been made strong. If the following cannot weaken it, then we shall have to accept it; we shall have to grant that there is *no* aspect of discovery which has to do with logical, or conceptual considerations.

When Kepler published *De Motibus Stellae Martis,* he had established that Mars' orbit was an ellipse, inclined to the ecliptic, and had the sun in one of the foci. Later (in the *Harmonices Mundi*) he generalized this for other planets. Consider the hypothesis H': *Jupiter's* orbit is of the noncircular type.

The reasons which led Kepler to formulate H' were many. But they included this: that H (the hypothesis that *Mars'* orbit is elliptical) is true. Since Eudoxos, Mars had been the typical planet. (*We* know why. Mars' retrogradations and its movement around the empty focus —all this we observe with clarity from earth because of earth's spatial relations with Mars.) Now, Mars' dynamical properties are usually found in the other planets. If its orbit is ellipsoidal, then it is reasonable to expect that, whatever the exact shape of the other orbits (for example, Jupiter's), they will all be of the noncircular type.

But such reasons would not *establish* H'. Because what makes it reasonable to anticipate that H' will be of a certain type is *analogical* in character. (Mars does x; Mars is a typical planet; so perhaps all planets do the same kind of thing as x.) Analogies cannot establish hypotheses, not even *kinds* of hypotheses. Only observations can do that. In this the hypothetico-deductive account (of Popper, Reichenbach, and Braithwaite) is correct. To establish H' requires plotting its successive positions on a smooth curve whose equations can be determined. It may then be possible to assert that Jupiter's orbit is an ellipse, an oviform, an epicycloid, or whatever. But it would not be reasonable to expect this when discussing only what type of hypothesis is likely to describe Jupiter's orbit. Nor is it right to characterize this difference between 'H-as-illustrative-of-a-type-of hypothesis' and 'H-as-empirically established' as a difference of psychology only. *Logically,* Kepler's analogical reasons for proposing that H' would be of a certain type were good reasons. But, logically, they would not then have been good reasons for asserting the truth of a specific value for H'—something which could be done only years later.

What are and are not good reasons for reaching a certain conclusion is a logical matter. No further observations are required to settle such issues, any more than we require experiments to decide, on the basis of one's bank statements, whether one is bankrupt. Similarly, whether or not Kepler's reasons for anticipating that H' will be of a certain kind are *good* reasons is a matter for logical inquiry.

Thus, the differences between reasons for expecting that some as yet-undiscovered H will be of a certain type and those that establish this H are greater than is conveyed by calling them "psychological," "sociological," or "historical."

Kepler reasoned initially by analogy. Other kinds of reasons which make it plausible to propose that an *H*, once discovered, will be of a certain type, might include, for example, the detection of a formal symmetry in sets of equations or arguments. At important junctures Clerk Maxwell and Einstein detected such structural symmetries. This allowed them to argue, before getting their final answers, that those answers would be of a clearly describable type.

In the late 1920's, before anyone had explained the "negative-energy" solutions in Dirac's electron theory, good analogical reasons could have been advanced for the claim that, whatever specific assertion the ultimately successful *H* assumed, it would be of the Lorentz-invariant type. It could have been conjectured that the as yet undiscovered *H* would be compatible with the Dirac explanation of Compton scattering and doublet atoms, and would fail to confirm Schrödinger's hunch that the phase waves within configuration space actually described observable physical phenomena. All this could have been said before Weyl, Oppenheimer, and Dirac formulated the "hole theory of the positive electron." Good analogical reasons for supposing that this *type* of *H* would succeed could have been and, as a matter of fact, were advanced. Indeed, Schrödinger's attempt to rewrite the Dirac theory so that the negative-energy solutions disappeared was *rejected* for failing to preserve Lorentz invariance.

Thus, reasoning from observations of *A*s as *B*s to the proposal "All *A*s are *B*s" is different in type from reasoning analogically from the fact that *C*s are *D*s to the proposal "The hypothesis relating *A*s and *B*s will be of the same type as that relating *C*s and *D*s." (Here it is the *way* *C*s are *D*s which seems analogous to the way *A*s are *B*s.) And both of these are typically different from reasoning involving the detection of symmetries in equations describing *A*s and *B*s.

Indeed, put this way, what *could* an objection to the foregoing consist of? Establishing a hypothesis and proposing by analogy that a hypothesis is likely to be of a particular type surely follow reasoning which is different in type. Moreover, both procedures have a fundamentally logical or conceptual interest.

An objection: "Analogical arguments, and those based on the recognition of formal symmetries, are used because of inductively established beliefs in the reliability of arguments of that type. So, the cash value of such appeals ultimately collapses into just those accounts given by *H-D* theorists."

Agreed. But we are not discussing the *genesis* of our faith in these types of arguments, only the *logic* of the arguments themselves. *Given* an analogical premise, or one based on symmetry considerations—or even on enumeration of particulars—one argues *from* these in logically

different ways. Consider what further moves are necessary to convince one who doubted such arguments. A challenge to "All *A*s are *B*s" when this is based on induction by enumeration could only be a challenge to justify induction, or at least to show that the particulars are being correctly described. This is inappropriate when the arguments rest on analogies or on the recognition of formal symmetries.

Another objection: "Analogical reasons, and those based on symmetry are *still* reasons for *H* even after it is (inductively) established. They are reasons *both* for proposing that *H* will be of a certain type and for accepting *H*."

Agreed, again. But, analogical and symmetry arguments could never *by themselves* establish particular *H*s. They can only make it plausible to suggest that *H* (when discovered) will be of a certain type. However, inductive arguments can, by themselves, establish particular hypotheses. So they must differ from arguments of the analogical or symmetrical sort.

H-D philosophers have been most articulate on these matters. So, let us draw out a related issue on which Popper, Reichenbach, and Braithwaite seem to me not to have said the last word.

J. S. Mill was wrong about Kepler (*A System of Logic,* III, 2–3). It is impossible to reconcile the delicate adjustment between theory, hypothesis, and observation recorded in *De Motibus Stellae Martis* with Mill's statement that Kepler's first law is but "a compendius expression for the one set of directly observed facts." Mill did not understand Kepler (as Peirce notes [*Collected Papers,* I, p. 31]). (It is equally questionable whether Reichenbach understood him: "Kepler's laws of the elliptic motion of celestial bodies were inductive generalizations of observed fact . . . [he] observed a series of . . . positions of the planet Mars and found that they may be connected by a mathematical relation . . .") [11] Mill's *Logic* is as misleading about scientific discovery as any account proceeding *via* what Bacon calls *"inductio per enumerationem simplicem ubi non reperitur instantia contradictoria."* (Indeed Reichenbach observes: "It is the great merit of John Stuart Mill to have pointed out that all empirical inferences are reducible to the *inductio per enumerationem simplicem. . . .*") [12] The accounts of *H-D* theorists are equally misleading.

An *H-D* account of Kepler's first law would treat it as a high-level hypothesis in an *H-D* system. (This is Braithwaite's language.) It is regarded as a quasi-axiom, from whose assumption observation-statements follow. If these are true—if, for example, they imply that Uranus' orbit is an ellipse and that its apparent velocity at 90 degrees

[11] Reichenbach, *op. cit.*, p. 371.
[12] *Ibid.*, p. 389.

is greater than at aphelion—then the first law is confirmed. (Thus Braithwaite writes: "A scientific system consists of a set of hypotheses which form a deductive system . . . arranged in such a way that from some of the hypotheses as premises all the other hypotheses logically follow . . . the establishment of a system as a set of true propositions depends upon the establishment of its lowest level hypotheses . . .") [13]

This describes physical theory more adequately than did pre-Baconian accounts in terms of simple enumeration, or even post-Millian accounts in terms of ostensibly not-so-simple enumerations. It tells us about the logic of laws, and what they do in finished arguments and explanations. *H-D* accounts do not, however, tell us anything about the context in which laws are proposed in the first place; nor, perhaps, were they even intended to do so.

The induction-by-enumeration story *did* intend to do this. *It* sought to describe good reasons for initially proposing *H*. The *H-D* account must be silent on this point. Indeed, the two accounts are not strict alternatives. (As Braithwaite suggests they are when he remarks of a certain higher-level hypothesis that it "will not have been established by induction by simple enumeration; it will have been obtained by the hypothetico-deductive method. . . .") [14] They are thoroughly compatible. Acceptance of the second is no reason for rejecting the first. A law *might* have been inferred from just an enumeration of particulars (for example, Boyle's law in the seventeenth century, Bode's in the eighteenth, the laws of Ampere and Faraday in the nineteenth, and much of meson theory now). It could *then* be built into an *H-D* system as a higher order proposition. If there is anything wrong with the older view, *H-D* accounts do not reveal this.

There *is* something wrong. It is false. Scientists do not always discover every feature of a law by enumerating and summarizing observables. (Thus even Braithwaite [15] says: "Sophisticated generalizations (such as that about the proton-electron constitution of the hydrogen atom) . . . [were] certainly not derived by simple enumeration of instances . . .") But *this* does not strengthen the *H-D* account as against the inductive view. There is *no H-D* account of how "sophisticated generalizations" are *derived*. On his own principles, the *H-D* theorist's lips are sealed on this matter. But there are conceptual considerations which help us understand the *reasoning* that is sometimes successful in determining the type of an as-yet-undiscovered hypothesis.

Were the *H-D* account construed as a description of scientific prac-

[13] Braithwaite, *op. cit.*, pp. 12–13.
[14] *Ibid.*, p. 303.
[15] *Ibid.*, p. 11.

tice, it would be misleading. (Braithwaite's use of "derived" is thus misleading. So is his announcement [p. 11] that he is going to explain "how we *come to make* use of sophisticated generalizations.") Natural scientists do not "start from" hypotheses. They start from data. And even then not from commonplace data, but from surprising anomalies. (Thus Aristotle remarks [16] that knowledge begins in astonishment. Peirce makes perplexity the trigger of scientific inquiry.[17] And James and Dewey treat intelligence as the result of mastering problem situations.) [18]

By the time a law gets fixed into an *H-D* system, the *original* scientific thinking is over. The pedestrian process of deducing observation-statements begins only after the physicist is convinced that the proposed hypothesis is at least of the right type to explain the initially perplexing data. Kepler's assistant could work out the consequences of *H′* and check its validity by seeing whether Jupiter behaved as *H′* predicts. This was possible because of Kepler's argument that what *H* had done for Mars, *H′* might do for Jupiter. The *H-D* account is helpful here; it analyzes *the argument of a completed research report.* It helps us see how experimentalists elaborate a theoretician's hypotheses. And the *H-D* account illuminates yet another aspect of science, but its proponents have not stressed it. Scientists often dismiss explanations alternative to that which has won their provisional assent along lines that typify the *H-D* method. Examples are in Ptolemy's *Almagest,* when (on observational grounds) he rules out a moving earth, in Copernicus' *De Revolutionibus . . .* , when he rejects Ptolemy's lunar theory, in Kepler's *De Motibus Stellae Martis,* when he denies that the planes of the planetary orbits intersect in the center of the ecliptic, and in Newton's *Principia,* when he discounts the idea that the gravitational force law might be of an inverse cube nature. These mirror formal parts of Mill's *System of Logic* or Braithwaite's *Scientific Explanation.*

Still, the *H-D* analysis remains silent on reasoning which often conditions the discovery of laws—reasoning that determines which type of hypothesis is likely to be most fruitful to propose.

The induction-by-enumeration story views scientific inference as being from observations to the law, from particulars to the general. There is something true about this which the *H-D* account must ignore. Thus Newton wrote: "The main business of natural philosophy is to argue from phenomena. . . ." [19]

[16] Aristotle, *Metaphysics* 982b, 11ff.
[17] Peirce, *op. cit.*, Vol. II, Book III, Chap. 2, Part III.
[18] Cf. John Dewey, *How We Think.* London: Heath & Co., 1909, pp. 12f.
[19] Newton, Principia, Preface.

This inductive view, however, ignores what Newton never ignored: the inference is also from *explicanda* to an *explicans*. Why a beveled mirror shows spectra in sunlight is not explained by saying that all beveled mirrors do this. Why Mars moves more rapidly at 270 degrees and 90 degrees than could be expected of circular-uniform motions is not explained by saying that Mars (or even all planets) always move thus. On the induction view, these latter might count as laws. But only when it is explained why beveled mirrors show spectra and why planets apparently accelerate at 90 degrees will we have laws of the type suggested: Newton's laws of refraction and Kepler's first law. And even before such discoveries were made, arguments in favor of those *types* of laws were possible.

So the inductive view rightly suggests that laws are somehow related to inferences *from* data. It wrongly suggests that the resultant law is but a summary of these data, instead of being an explanation of these data. A logic of discovery, then, might consider the structure of arguments in favor of one *type* of possible explanation in a given context as opposed to other *types*.

H-D accounts all agree that laws explain data. (Thus Braithwaite says: "A hypothesis to be regarded as a natural law must be a general proposition which can be thought to explain its instances; if the reason for believing the general proposition is solely direct knowledge of the truth of its instances, it will be felt to be a poor sort of explanation of these instances . . ." [*op. cit.*, p. 302].) *H-D* theorists, however, obscure the initial connection between thinking about data and thinking about what kind of hypothesis will most likely lead to a law. They suggest that the fundamental inference in science is from higher-order hypotheses to observation-statements. This may characterize the setting out of one's reasons for making a prediction after *H* is formulated and provisionally established. It need not be a way of setting out reasons in favor of proposing originally of what type *H* is likely to be.

Yet the original suggestion of a hypothesis type is often a reasonable affair. It is not as dependent on intuition, hunches, and other imponderables as historians and philosophers suppose when they make it the province of genius but not of logic. If the establishment of *H* through its predictions has a logic, so has the initial suggestion that *H* is likely to be of one kind rather than another. To form the first specific idea of an elliptical planetary orbit, or of constant acceleration, or of universal gravitational attraction does indeed require genius—nothing less than a Kepler, a Galileo, or a Newton. But this does not entail that reflections leading to these ideas are nonrational. Perhaps *only* Kepler, Galileo, and Newton had intellects mighty enough to fashion these notions initially; but to concede this is not to concede

that their reasons for first entertaining concepts of such a type surpass rational inquiry.

H-D accounts begin with the hypothesis as given, as cooking recipes begin with the trout. Recipes, however, sometimes suggest, "First catch your trout." The *H-D* account is a recipe physicists often use after catching hypotheses. However, the conceptual boldness which marks the history of physics shows more in the ways in which scientists *caught* their hypotheses than in the ways in which they elaborated these once caught.

To study only the verification of hypotheses leaves a vital part of the story untold—namely, the reasons Kepler, Galileo, and Newton had for thinking their hypotheses would be of one kind rather than another. In a letter to Fabricius, Kepler underlines this:

> Prague, July 4, 1603
>
> Dear Fabricius,
>
> . . . You believe that I start with imagining some pleasant hypothesis and please myself in embellishing it, examining it only later by observations. In this you are very much mistaken. The truth is that after having built up an hypothesis on the ground of observations and given it proper foundations, I feel a peculiar desire to investigate whether I might discover some natural, satisfying combination between the two . . .

Had any *H-D* theorist ever sought to give an account of the way in which hypotheses in science *are discovered*, Kepler's words are for him. Doubtless *H-D* philosophers have tried to give just such an account. Thus, Braithwaite[20] writes: "Every science *proceeds* . . . by thinking of general hypotheses . . . from which particular consequences are deduced which can be tested by observation . . . ," and again, "Galileo's deductive system was . . . presented as deducible from . . . Newton's laws of motion and . . . his law of universal gravitation . . ."

How would an *H-D* theorist analyze the law of gravitation?

(1) First, the hypothesis *H*: that between any two particles in the universe exists an attracting force varying inversely as the square of the distance between them ($F = \lambda Mm/r^2$).

(2) Deduce from this (in accordance with the *Principia*)
  (a) *Kepler's* Laws, and
  (b) *Galileo's* Laws.

(3) But particular instances of (a) and (b) square with what is observed.

(4) Therefore *H* is, to this extent, confirmed.

The *H-D* account says nothing about how *H* was first puzzled out.

[20] *Op. cit.*, pp. xv, xi, 18.

But now consider why, here, the *H-D* account is *prima-facie* plausible.

Historians remark that Newton's reflections on this problem began in 1680 when Halley asked: "If between a planet and the sun there exists an attraction varying inversely as the square of their distance, what then would be the path of the planet?" Halley was astonished by the immediate answer: "An ellipse." The astonishment arose not because Newton *knew* the path of a planet, but because he had apparently deduced this from the hypothesis of universal gravitation. Halley begged for the proof; but it was lost in the chaos of Newton's room. Sir Isaac's promise to work it out anew terminated in the writing of the *Principia* itself. Thus the story unfolds as an *H-D* plot: (1) from the suggestion of a hypothesis (whose genesis is a matter of logical indifference—that is, psychology, sociology, or history) to (2) the deduction of observation statements (the laws of Kepler and Galileo), which turn out true, thus (3) establishing the hypothesis.

Indeed, the entire *Principia* unfolds as the plot requires—from propositions of high generality through those of restricted generality, terminating in observation-statements. Thus Braithwaite [21] observes: "Newton's *Principia* [was] modelled on the Euclidean analogy and professed to prove [its] later propositions—those which were confirmed by confrontation with experience—by deducing them from original first principles . . ."

Despite this, the orthodox account is suspicious. The answer Newton gave Halley is not unique. He could have said "a circle" or "a parabola," and have been equally correct. The general answer is: "A conic section." The greatest mathematician of his time is not likely to have dealt with so mathematical a question as that concerning the possibility of a formal demonstration with an answer which is but a single value of the correct answer.

Yet the reverse inference, the retroduction, *is* unique. Given that the planetary orbits are ellipses, and allowing Huygen's law of centripetal force and Kepler's rule (that the square of a planet's period of revolution is proportional to the cube of its distance from the sun), the *type* of the law of gravitation can be inferred. Thus the question, "If the planetary orbits are ellipses, what form will the gravitational force law take?" invites the unique answer, "an inverse square type of law."

Given the datum that Mars moves in an ellipse, one can (by way of Huygen's law and Kepler's third law) explain this uniquely by suggesting how it might follow from a law of the inverse square type, such as the law of universal gravitation was later discovered to be.

The rough idea behind all this is: Given an ellipsoidal eggshell, imagine a tiny pearl moving inside it along the maximum elliptical orbit. What *kind* of force must the eggshell exert on the pearl to keep the latter in this path? Huygen's weights, when whirled on strings, required a force in the string, and in Huygen's arm, of $F_{(k)} \propto r/T^2$ (where r signifies distance, T time, and k is a constant of proportionality). This restraining force kept the weights from flying away like stones from David's sling. And something like this force would be expected in the eggshell. Kepler's third law gives $T^2 \propto r^3$. Hence, $F_{(k)} \propto r/r^3 \propto 1/r^2$. The force the shell exerts on the pearl will be of a kind which varies inversely as the square of the distance of the pearl from that focus of the ellipsoidal eggshell where the force may be supposed to be centered. This is not yet the law of gravitation. But it certainly is an argument which suggests that the law is likely to be of an inverse square type. This follows by what Peirce called 'retroductive reasoning.' But what *is* this retroductive reasoning whose superiority over the *H-D* account has been so darkly hinted at?

[21] *Op. cit.*, p. 352.

Schematically, it can be set out thus:

(1) Some surprising, astonishing phenomena p_1, p_2, p_3 . . . are encountered.[22]

(2) But p_1, p_2, p_3 . . . would not be surprising were a hypothesis of *H*'s type to obtain. They would follow as a matter of course from something like *H* and would be explained by it.

(3) Therefore there is good reason for elaborating a hypothesis of the type of *H;* for proposing it as a possible hypothesis from whose assumption p_1, p_2, p_3 . . . might be explained.[23]

How, then, would the discovery of universal gravitation fit this account?

(1) The astonishing discovery that all planetary orbits are elliptical was made by Kepler.

(2) But such an orbit would not be surprising if, in addition to other familiar laws, a law of 'gravitation,' of the inverse

[22] The astonishment may consist in the fact that p is at variance with accepted *theories*—for example, the discovery of discontinuous emission of radiation by hot black bodies, or the photoelectric effect, the Compton effect, and the continuous β-ray spectrum, or the orbital aberrations of Mercury, the refrangibility of white light, and the high velocities of Mars at 90 degrees. What is important here is *that* the phenomena are encountered as anomalous, not *why* they are so regarded.

[23] This is a free development of remarks in Aristotle (*Prior Analytics*, II, 25) and Peirce (*op. cit.*). Peirce amplifies: "It must be remembered that retroduction, although it is very little hampered by logical rules, nevertheless, is logical inference, asserting its conclusion only problematically, or conjecturally, it is true, but nevertheless having a perfectly definite logical form" (*op. cit.*, Vol. I, p. 188).

square type obtained. Kepler's first law would follow as a matter of course; indeed that kind of hypothesis might even explain why (since the sun is in but one of the foci) the orbits are ellipses on which the planets travel with non-uniform velocity.

(3) Therefore there is good reason for further elaborating hypotheses of this kind.

This says something about the rational context within which a hypothesis of *H*'s type might come to be "caught" in the first place. It begins where all physics begins—with problematic phenomena requiring explanation. It suggests what might be done to particular hypotheses once proposed—namely, the *H-D* elaboration. And it points up how much philosophers have yet to learn about the kinds of reasons scientists might have for thinking that one kind of hypothesis may explain initial perplexities; why, for example, an inverse square type of hypothesis may be preferred over others, *if* it throws the initially perplexing data into patterns within which determinate modes of connection can be perceived. At least it appears that the ways in which scientists sometimes reason their way *towards* hypotheses, by eliminating those which are certifiably of the wrong type, may be as legitimate an area for conceptual inquiry as are the ways in which they reason their way *from* hypotheses.

Recently, in the Lord Portsmouth collection in the Cambridge University Library, a document was discovered which bears on our discussion. There, in "Additional manuscripts 3968, No. 41, bundle 2," is the following draft in Newton's own hand:

> And in the same year [1665, twenty years before the *Principia*] I began to think of gravity extending to ye orb of the Moon, and (having found out how to estimate the force with which a globe revolving within a sphere presses the surface of the sphere), from Kepler's rule . . . I deduced that the forces which keep the planets in their Orbs must be reciprocally as the squares of their distances from the centres about which they revolve . . .

This manuscript corroborates our argument. ("Deduce," in this passage, is used as when Newton speaks of deducing laws from phenomena—which is just what Aristotle and Peirce would call "retroduce.") Newton *knew* how to estimate the force of a small globe on the inner surface of a sphere. (To compare this with Halley's question and our pearl-within-eggshell reconstruction, note that a sphere can be regarded as a degenerate ellipsoid—that is, where the foci superimpose.) From this and from Kepler's rule, $T^2 \propto r^3$, Newton determined that, whatever the final form of the law of gravitation, it would

very probably be of the inverse-square type. These were the reasons which led Newton to think further about the details of universal gravitation. The reasons for accepting one such hypothesis of this type *as a law* are powerfully set out later in the *Principia* itself; and they are much more comprehensive than anything which occurred to him at this early age. But without such preliminary reasoning Newton might have had no more grounds than Hooke or Wren for thinking the gravitation law to be of an inverse-square type.

The morals of all this for our understanding of contemporary science are clear. With such a rich profusion of data and technique as we have, the arguments necessary for *eliminating* hypotheses of the wrong type become a central research inquiry. Such arguments are not always of the *H-D* type; but if for that reason alone we refuse to scrutinize the conceptual content of the reasoning which precedes the actual proposal of definite hypotheses, we will have a poorer understanding of scientific thought in our time. For our own sakes, we must attend as much to how scientific hypotheses are caught, as to how they are cooked.

Paul K. Feyerabend

Comments on Hanson's "Is There a Logic of Scientific Discovery?"

Hanson's main thesis seems to consist in the following two statements: (1) The invention of a new physical theory need not be a completely irrational process because there may exist arguments to the effect that the theory must be of a certain kind; (2) the hypothetico-deductive model (*H-D*-model) of explanation is incomplete because it cannot give an account of these arguments.

My reply is that Hanson is correct when asserting (1), but that he is wrong when asserting (2).

In order to substantiate my reply let me first describe the main features of the *H-D* model. In this model it is assumed that the purpose of science is explanation. Explanation always starts from a situation to be explained, or from an explanandum. The *explanandum* need not refer directly to observables, but it must at least be plausible that it is true. According to the *H-D* model there are three conditions which an *explanans* must satisfy in order to be satisfactory. They are: (1) it must be *relevant*—that is, it must enable us at least to obtain a sentence describing the situation *S* to be explained; (2) it must *not* be *ad hoc*—that is, it must yield more than just *S;* and (3) it must be *testable*—that is, it must be possible to describe an (observational) situation which is such that the hypothesis will be withdrawn as soon as this situation occurs. Let me add here a little comment on condition (2). Professor Hanson has pointed out, and I think with some justification, that it is not sufficient for the hypothesis to give *S* plus something else *in a simple additive manner:* that *all* stones fall to the ground when released would seem to be a very poor explanation of the fact that *this* stone falls to the ground when released, although it satisfies (1), (2), (3) above. For quite clearly my question why *this* stone behaves as it does will not be answered by the hint that the behavior of *any* stone will give rise to the same question. This indicates that simple generalization will not do as an explanatory device. Without going into too much detail I think that a necessary condition of a satisfactory explanation will be (2′) that the explaining hypothesis contain terms which do not occur in the explanandum, but which are essential for the derivation of the explanandum from it. It is this necessary condition which forces us to introduce terms that initially do not possess a direct observational significance (although they may become observable as time goes on and people become accustomed to expressing even observational affairs in these new terms) and which thereby forces us to leave the observational domain. Popper has expressed this consequence by saying that explanation is always explanation of what is known in terms of what is not known. Unfortunately most of the highly sophisticated contemporary studies in the logic of explanation and confirmation deal with explanations that do not satisfy (2′).

Conditions (1) through (3), with (2) modified into (2′), are *restrictive* conditions. They do not tell us what hypothesis to choose because they do not uniquely determine the explanans. Indeed, once (2′) has been introduced, it is very difficult to see how a unique determination could be possible. Now if I understand Professor Hanson correctly, he says that in actual scientific practice the choice of proper explanations is further restricted by conditions which are different from all the conditions mentioned so far and which may even make the choice of the explanans *unique*. I suggest that the condition which Hanson has in mind can be stated in the following terms:

Condition (4): the explanans, apart from satisfying (1) through (3), must not be inconsistent with certain more general points of view, theories, or metaphysical principles which are held either by the inventor, or by the (scientific) community in which he lives. I think that in spite of its negative form (which I have chosen quite deliberately) this condition covers what Professor Hanson wants to add to the *H-D* model. Contemporary physicists reject Bohm's general hypothesis about the nature of microscopic particles (the hypothesis developed, for example, in his *Cause and Chance in Modern Physics,* which is very different from the model of 1951), not because they think it violates one of the conditions (1) through (3), but because it is inconsistent with some further assumptions which are thought to be confirmed to such a high degree that it would be foolish to give them up. At a certain stage of its development the hypothesis of the shortest length was criticized, not because it was considered *ad hoc,* or empirically false, but because it was difficult to see how it could be made relativistically invariant. Ptolemaios rejected the hypothesis of Aristarchos, not because it could not give a satisfactory account of the facts, but because it was inconsistent with the Aristotelian dynamics. It is very important, by the way, to realize that the principles or laws with which a new hypothesis is supposed to be consistent need not always be of the empirical kind. Copernicus' acceptance of Aristarchos' model was prompted by empirical considerations as well as by his desire to make astronomy consistent with Neoplatonism. That esthetic considerations sometimes play an equally strong role may be seen from the reasons which made Galileo reject Kepler's ellipses.

Now all these examples, while showing that rule (4) indeed expresses what Professor Hanson wants to say, seem also to show that physicists, when considering a new theory, do not only pay attention to conditions (1) through (3), but also to a background of partly empirical, partly metaphysical beliefs. Does this fact prove that the *H-D* account is incomplete?

In order to show that it does not, we split condition (4) into two parts, (4a) and (4b). According to (4a) the total set *S* of hypotheses made about the world, be they now empirical, or metaphysical, must be *consistent*. According to (4b) a certain subset, *S′*, of *S* has already been accepted. (4a) and (4b) taken together demand that any newly introduced hypothesis *H,* apart from satisfying (1), (2), and (3), must also be compatible with *S′*. I contend that this demand cannot be interpreted as a new methodological rule. For when enquiring about the reasons why we accept *S′*, we shall meet two: the empirical adequacy of some of its elements (which can be accounted for by the *H-D* model), and the (as yet untested) belief in others. Let the set of the latter elements be *S″*. Quite clearly *S″* cannot be justified, apart from its agreement with (1), (2), and (3), by the additional demand raised with respect to *H,* for this demand presupposes that *S′* and, therefore, *S″* are already given. It follows that the acceptance, *in advance of the carrying out of tests,* of *S″* must be regarded as irrational to the extent to which it is not restricted by (1), (2), or (3). But if that is the case, then the acceptance, on the basis of (4), of the hypothesis *H* is likewise irrational, for the rationality of a procedure is not increased by showing that it is consistent with other irrational procedures. And as is well known, the demand of consistency—namely, (4a)—is not a new rule, but is inherent in the *H-D* model.

To sum up: The new rule which Hanson wants to add to the *H-D* model is dispensable because part of it is already contained in this model and the rest—namely, (4b)—is not a new methodological demand: it is the (sociopsychological) fact that certain beliefs are being held.

Let me finally comment on Hanson's belief that his rule (4) may, under certain circumstances, lead to a unique determination of the hypothesis required for the explanation of a given set of facts. This is not further surprising. After all, if the hypotheses included in the set with which the new hypothesis H is to be compatible are rich enough, they will of course allow for the derivation, on the basis of the facts, of assumptions which could not have been derived with the help of the observational statements alone. Now, if the elements of the set are only implicitly used, then the impression will arise that a new kind of 'logic,' or new ways of 'reasoning,' have been discovered, whereas all that is going on is a sloppy kind of talking where premises are neither explicitly stated nor consistently used. (In the sense, of course, in which sloppy thinking is a special kind of thinking, it is indeed true that we are here confronted with "new ways of thinking.")

Considerations "by analogy" are a good example of what has just been said. Consider the following argument: "Mars moves in an ellipse: Mars is a planet (I think the word "typical" which Professor Hanson seems to think is important, can be safely omitted); therefore Jupiter (which is a planet) will also move in an ellipse." As it stands, this argument is quite clearly not deductive. Does this show, however, that we are here presented with a new and special type of reasoning, which has not been sufficiently taken into account by formal logicians, and which therefore ought to be an object of special study? It seems to me that such a conclusion, although in agreement with the contemporary fashion to replace criticism according to old and precise standards by the assertion that what one is dealing with requires the recognition of new standards, is utterly unwarranted. After all, the assumption that the planets all obey the same laws of motion is such a basic idea of the planetary astronomy from Eudoxos up to Copernicus that an astronomer could safely omit *stating* this assumption without thereby leaving the domain of deductive reasoning, or running danger of being misunderstood by his colleagues as an innovator of logic. While reading an argument like the one quoted above, they all would know what else

was being presupposed, and they would therefore be perfectly able *to read this argument* in the way it was intended—namely, *as an incompletely stated deductive argument.* It was left to Professor Hanson and some other philosophers of science to imply that these astronomers knew in fact much less and were therefore in need of new types of argument. I think that a little more historical research would convince Professor Hanson that none of the examples he discusses can be interpreted in this way and that the *logic of discovery* for which he is looking so eagerly simply does not exist.

How suppression of premises by those who regard them as obvious and well known can cultivate in some of their less well-oriented readers the belief in new ways of reasoning is excellently shown by the case which Professor Hanson himself has chosen to discuss, namely, the alleged 'derivation,' or 'retroduction,' from Kepler's laws of the law of universal gravitation. What are the premises of this argument, and what is its conclusion? The premises are the elliptical motion, in accordance with Kepler's laws, of a single mass point around a center which is *at rest* in one of the foci. The conclusion is that the acceleration, with respect to this center, of the circling mass point is of the $1/r^2$ type. This quite obviously has nothing whatever to do with Newton's law of universal gravitation and with the planets. For in the case of the planets, the *premise* of the argument is not valid because there is not a *single* mass point circling around the center, but many. Also the center is not at rest (with respect so some inertial frame). And, secondly, the law of gravitation is pronounced quite generally which shows also that the *conclusion* of the above deduction has nothing to do with gravitation. Hence, this deduction by itself can by no means be regarded as a unique determination, on the basis of experience, of the law of universal gravitation. Only if we add to it certain assumptions about the magnitude of disturbances as well as a more general hypothesis about the similarity, with respect to their dynamical properties, of all particles of matter, is it possible to make the conclusion a unique consequence of the thus extended premises. But since the new set of premises already makes use of some very decisive properties of the law of gravitation, this is not any more the great achievement it seemed to be in the beginning. The real achievement lies somewhere else: it lies in the conception of a universal, attractive, central force. This *act* of conception, itself, is not justifiable, either by "retroduction," or by arguments of analogy; for it is an irrational, inventive act, the *result* of which is justified only *after* it has been shown to conform with (1), (2), and (3) above and has had predictive success. A "logic of invention" which helps us to produce such a law, simply does not exist.

Norwood Russell Hanson

Rejoinder to Feyerabend

In a rejoinder, one rejoins. It is a pleasure to join, or rejoin, issue with my friend Professor Feyerabend—especially when he leaves undefended, indeed indefensible, so many flanks in his attack.

After absorbing Feyerabend's stern moralizing on the virtues of "a little more historical research" and the vices of "sloppy thinking," it gives me some satisfaction to be able to point out that he cannot really have *read* my essay at all. Or, if he did, his scrutiny was sloppy. For he credits me with views I am explicitly at pains to reject.

Thus, on page 37 Feyerabend writes as if my intention had been to propound a 'new methodological rule.' It was the purpose of my sixth footnote to deny the very possibility of such a "rule." He regards as *irrational* the acceptance of untested beliefs *in advance of the carrying out of tests*—and announces this as a hit against me. But this is precisely the point I seek to make on page 25 of my essay, when I write "But such reasons [involving untested beliefs] would not *establish* [that is, lead to the acceptance of] *H′*." Never once did I suggest that it would be rational to *accept* untested beliefs.

I *did* suggest that it often is rational to *entertain* certain types of untested beliefs, with an eye to exploring them further experimentally. By this I mean that reasons could be given, before actual testing, which would show up the plausibility, or the unplausibility, of exploring further some particular *kind* of hypothesis, rather than others. Indeed, Reichenbach, in his *Philosophic Foundation of Quantum Mechanics,* gives a splendid illustration of this point. After setting out the time-independent Schrödinger equation for waves corresponding to particles in a field of force (p. 71), Reichenbach writes:

> We certainly do not disparage Schrödinger's work when we do not consider the derivation given, or the more complicated derivations originally given by Schrödinger, as a proof of the validity of the resulting wave equation. Such a derivation—and Schrödinger never meant anything else—can be used to make the wave equation *plausible,* and therefore represents an excellent guide within the context of discovery . . .

Feyerabend then goes on to say that I am opting for a "new methodological demand." I can find in my paper no linguistic basis for

this reaction. He then speaks of "Hanson's belief that his rule [1] may, under certain circumstances, lead to a *unique determination of the hypothesis* required for the explanation of a given set of facts." (My italics.) Apparently I need not have bothered to set out the analyses of pages 21 and 22—and especially entry (2′). For the "unique determination of the hypothesis" *in advance of experimentation* is precisely what I deny can ever be had. At most, an argument making it plausible to explore one *kind* of hypothesis, rather than others, can be entertained before experiment. This is *all* I argue for, and all I want to argue for.

But it takes two people to argue; one to speak (or write), and one to listen (or read). If Feyerabend will not read what I say, his "Comments" on what I say must be expected to suffer somewhat.

> . . . the assumption that the planets all obey the same laws of motion is such a basic idea of the planetary astronomy from Eudoxos up to Copernicus, that an astronomer could safely omit *stating* this assumption without leaving the domain of deductive reasoning . . . (Feyerabend, p. 38).[2]

Poppycock! No astronomer could ever safely omit stating such an assumption, particularly when orbital noncircularity appeared to be precisely what was going uniquely to *distinguish* Mars from the other planets! Nor, as a matter of historical fact [which Feyerabend chides us for neglecting] was the inference from Mars' elliptical orbit to Jupiter's actually taken as *deductive.* In *De Motibus Stellae Martis* (1609) Kepler settles the issue for Mars. Not until the *Harmonices Mundi,* ten years later, is Jupiter brought into the elliptical orbit generalization, *after observations* on that planet. But why should Kepler have *observed* Jupiter during the interim at all? What reasons had he for examining Jupiter's orbit more closely, rather than just accepting extant sixteenth century studies? *This* it was the function of my essay to explore—a fact Feyerabend seems not to appreciate.

Incidentally, it is as a *result* of "historical research" in seventeenth-century scientific texts that my interest in "plausibility arguments" for as-yet-untested hypotheses was kindled. I am not "looking so eagerly" for a *logic of discovery* as Professor Feyerabend supposes. (Cf., the second paragraph of my paper.) If I misconstrue the texts, this should

[1] Incidentally, this is Feyerabend's rule, not mine. He hangs it on me and then fires with a rhetorical scatter-gun that certainly hits "the rule." But *my* essay was supposed to have been the target, not Feyerabend's shooting-gallery rewrite of it.

[2] My example is drawn straight out of Kepler. What, then, is the relevance of referring to planetary astronomy "from Eudoxos up to Copernicus?"

be easy enough to disclose. Feyerabend may yet find time to do this for me, between his own bouts of "historical research" and "rigorous [that is, nonsloppy] thinking." But, alas, not a word in his "Comments" illuminates the arguments Kepler *actually used.*

The last paragraph of Feyerabend's "Comments" is quixotic in the extreme. Here, I can no longer identify his opponent. Discovering the acceleration of a particle in elliptical motion about a center to be of the $1/r^2$ type—to say that this *"has nothing whatever to do with Newton's law of universal gravitation and with the planets"* is almost irresponsible. On page 34 I set out Newton's own original reflections on this problem. *He* felt the $1/r^2$ type of law involved in estimating "the force with which a globe revolving within a sphere presses the surface of the sphere" *certainly* had something to do with "the forces which keep the planets in their Orbs." *Of course,* this early demonstration does not yield the law of universal gravitation as a conclusion. But it certainly *does* make it plausible to suppose that such a gravitational law, when discovered, would be of a type which describes forces acting on the planets "reciprocally as the squares of their distances from the centres about which they revolve . . ."

Professor Feyerabend's parting shot, namely,

> A "logic of invention" which helps us to produce such a law, simply does not exist,

is lofted as if it hit my thesis dead center. But how can this be so, since I agree with this very contention; indeed I stress the point on pages 21 and 25? This fact may stimulate Feyerabend's curiosity. He may even go back and read what I say.

Never mind, someone *could* have said the things he says I say. And then his "Comments" would have been devastating.

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