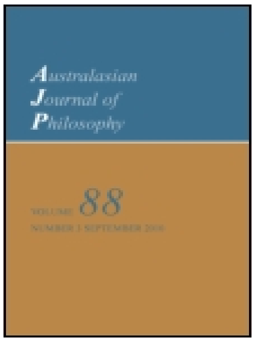

Australasian Journal of Philosophy

ISSN: 0004-8402 (Print) 1471-6828 (Online) Journal homepage: https://www.tandfonline.com/loi/rajp20

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To cite this article: Norwood Russell Hanson (1960) Is there a logic of scientific discovery?, Australasian Journal of Philosophy, 38:2, 91-106, DOI: 10.1080/00048406085200111

To link to this article: https://doi.org/10.1080/00048406085200111

Published online: 28 Jun 2007.

The Australasian Journal of Philosophy

VOL. 38 AUGUST, 1960 No. 2

IS THERE A LOGIC OF SCIENTIFIC DISCOVERY?

By NORWOOD RUSSELL HANSON

The approved answer to this is "No". Thus Popper argues (*The Logic of Scientific Discovery*) "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." (p. 31.) Again, ". . . there is no such thing as a logical method of having new ideas, or a logical reconstruction of this process." (p. 32.) Reichenbach writes that philosophy of science ". . . cannot be concerned with [reasons for suggesting hypotheses], but only with [reasons for accepting hypotheses]." (*Experience and Prediction,* p. 382.) 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." (*Scientific Explanation,* pp. 20, 21.)

Against this negative chorus, the 'Ayes' have not had it. Aristotle (*Prior Analytics,* II, 25), and Peirce (*Collected Papers,* I, Sec. 188) 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'. (Cf. *Studies in the History and the Methods of the Sciences,* ed. Charles Singer.) We may be forced, with the majority, to 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 an hypothesis once proposed. They have said nothing about the conceptual context within which such an hypothesis is initially proposed. Both Aristotle and Peirce insisted that the proposal of an 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 an hypothesis once suggested. (This is not to deny that one's reasons for proposing an 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. ("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, Chapter I.) 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, e.g., 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 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 an 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 that H constitutes a plausible kind of conjecture. The question is: are these logical in nature, or more properly 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 an 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 re-set 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 an hypothesis of one *kind* rather than another.

Neither Aristotle, nor Peirce, nor (if you will excuse the conjunction) myself in earlier papers, 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 favour of suggesting that same H initially.

This way of putting it is objectionable. The issue is, whether (*before* having hit on an 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, e.g., have had good reasons (*before* his elliptical orbit hypothesis was established) for supposing that the successful hypothesis concerning Mars' orbit would be of the non-circular kind?[1] 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° and at 270° (of excentric anomaly) were greater than any circular-type H could explain. Other *kinds* of hypotheses were available to Kepler: e.g., that Mars' *colour* 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 *un*reasonable 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" (*Scientific Explanation,* p. 20). 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.[2] There may indeed be "psychological" factors, the opposition concedes, which make certain types of hypothesis '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 psychological, 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. In so far 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

[1] Cf. *De Motibus Stellae Martis* (Munich, pp. 250 ff).

[2] Reichenbach writes that philosophy "cannot be concerned with the first, but only with the latter" (*Experience and Prediction,* p. 382).

stellar parallax constitutes more than a psychological reason for Ptolemy's resistance to alternatives to geocentricism, then, in so far, 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*. Again, Kepler's reasons for rejecting Mars' colour 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 non-circularity type.

So the objection to my distinction is: the only *logical* reason for proposing that 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 abandons an hypothesis because it entails other-than-uniform circular orbits—simply inconceivable for him. So psychological pressure against forming alternative types of hypothesis 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 excentric 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, 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 non-circular 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 Earth observes with clarity because of our 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 (e.g., Jupiter's) they will all be of the non-circular 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, e.g., 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, this is a logical inquiry.

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

Kepler reasoned initially by analogy. Another kind of reason which makes it plausible to propose that an H, once discovered, will be of a certain type, could be 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 ultimate 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 the *type* of H which would succeed would be along these lines could have been, and 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 As as Bs to the proposal "all As are Bs" is different in type from reasoning analogically from the fact that Cs are Ds to the proposal "the hypothesis relating As and Bs will be the same type as that relating Cs and Ds". (Here it is the *way* Cs are Ds which seems analogous to the way As are Bs.) And both of these are typically different from reasoning involving the detection of symmetries in equations describing As and Bs.

Indeed, put in this way, what could an objection to the foregoing consist in? Establishing an hypothesis, and proposing by analogy that an hypothesis is likely to be of a particular type: surely these 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 As are Bs", 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—these 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 Hs. 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 compendious 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 planets Mars and found that they may be connected by a mathematical relation . . ." (*Experience and Prediction,* p. 371). 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* . . ." (*op. cit.,* p. 389).) 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, e.g., they imply that Uranus' orbit is an ellipse and that its apparent velocity at 90° is greater than at aphelion—then in so far is the First Law 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 . . ." (*Scientific Explanation*, pp. 12-13).)

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.

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 . . ." (*op. cit.,* p. 303).) 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 (e.g., Boyle's law in the 17th century, Bode's in the 18th, the laws of Ampère and Faraday in the 19th, 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 says: "Sophisticated generalizations (such as that about the proton-electron constitution of the hydrogen atom) . . . [were] certainly not derived by simple enumeration of instances . . ." (*op. cit.,* p. 11).) 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* which is sometimes successful in determining the type of an as-yet-undiscovered hypothesis.

Were the H-D account construed as a description of scientific practice, 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 (*Metaphysics* 982 b 11 ff.) that knowledge begins in astonishment. Peirce makes perplexity the trigger of scientific inquiry. (*Collected Papers,* II, Book III, ch. 2, Part III.) And James and Dewey treat intelligence as the result of mastering problem situations. (Dewey, *How We Think,* pp. 12 ff.)

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 centre 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 which determines which type of hypothesis it 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. . . ." (*Principia,* Preface.)

This inductive view ignores what Newton never did: the inference is also from *explicanda* to an *explicans.* Why a bevelled mirror shows spectra in sunlight is not explained by saying that all bevelled mirrors do this. Why Mars moves more rapidly at 270° and 90° 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 bevelled mirrors show spectra and why planets apparently accelerate at 90° 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 favour of those *types* of law 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 favour 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).) However, they obscure the initial connection between thinking about data and thinking about what kind of hypothesis will be most likely to 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 favour of proposing originally what type H is likely to be.

Yet the original suggestion of an 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 non-rational. Perhaps only Kepler, Galileo, and Newton had intellects mighty enough to fashion these notions initially; 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. However, recipes 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, what were 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. . . .

If any H-D theorist has 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 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 . . ." (*Op. cit.,* pp. xv, xi, 18.)

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 ($F = \gamma Mm/r^2$).

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, in so far, confirmed.

The H-D account says nothing about how H was first puzzled out. But 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 an hypothesis (whose genesis is a matter of logical indifference, i.e., 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 observes: "Newton's *Principia* [was] modelled on the Euclidian analogy and professed to prove [its] later propositions—those which were confirmed by confrontation with experience—by deducing them from original first principles . . .". (*Scientific Explanation,* p. 352.)

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 concerning whether a formal demonstration is possible with an answer which is but a single value of the correct answer.

Yet the reverse inference, the *retro*duction, *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)—from this the *type* of the law of gravitation can be inferred. Thus the question "If the planetary orbits are ellipses what form will the 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 egg-shell, imagine a pearl moving inside it along the maximum elliptical orbit. What *kind* of force must the egg-shell 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 egg-shell. 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 egg-shell where the force may be supposed to be centred. 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 hinted at?

Schematically, it can be set out thus:

1. Some surprising, astonishing phenomena p_1, p_2, p_3 . . . are encountered.[3]

2. But p_1, p_2, p_3 . . . would not be surprising were an 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 an hypothesis of type H—for proposing it as a possible hypothesis from whose assumption p_1, p_2, p_3 . . . might be explained.

This is a free development of remarks in Aristotle (*Prior Analytics,* II, 25) and Peirce. (*Collected Papers,* Vol. I, 188. 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.")

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

[3] The astonishment may consist in the fact that *p* is at variance with accepted *theories*—as, e.g., 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°. What is important here is *that* the phenomena are encountered as anomalous, not *why* they are so regarded.

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 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 elaborating hypotheses of this kind further.

This says something about the rational context within which an 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, viz., 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, e.g., an inverse square type of hypothesis may be preferred over others, *if* it throws 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 MS 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, i.e., 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 arguments which precede 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.

Indiana University.