SCIENTIFIC DISCOVERY AND MAXWELL'S KINETIC THEORY*

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By reference to Maxwell's kinetic theory, one feature of hypothetico-deductivism is defended. A scientist need make no inference to a hypothesis when he first proposes it. He may have no reason at all for thinking it is true. Yet it may be worth considering. In developing his kinetic theory there were central assumptions Maxwell made (for example, that molecules are spherical, that they exert contact forces, and that their motion is linear) that he had no reason to believe true. In this paper I develop a position that explains why they were worth considering, and that rejects the retroductive position that a hypothesis is worth considering when, if true, it would explain the observed data.

"Physics is play." R. Feynman

"We must bear in mind that the scientific or science-producing value of the efforts made to answer these old standing questions is not to be measured by the prospect of ultimately obtaining a solution, but by their effect in stimulating men to a thorough investigation of nature." J. C. Maxwell

For the hypothetico-deductivist the initial proposing of a new hypothesis or theory in the "context of discovery", by contrast to its testing in the "context of justification", is a nonrational event. It involves a guess or conjecture, that may have a variety of causes, but not an inference subject to logical analysis. Since N. R. Hanson's revival in the 1950s of Peirce's account of retroduction, this h-d view has become much less popular than it once was. Using as an example James Clerk Maxwell's early kinetic theory, I want to argue that there is an important element of truth in what h-d theorists say about the context of discovery.

1. Maxwell's Early Kinetic Theory. In 1860 Maxwell (1965, vol. I, pp. 377–409) published "Illustrations of the Dynamical Theory of Gases", the first of his two great papers on kinetic theory. In it he proposes to work out a theory of gases "on strict mechanical principles" by dem-

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onstrating "the laws of motion of an indefinite number of small, hard, and perfectly elastic spheres acting on one another only during contact" (p. 377). At the beginning of the paper he sets down basic assumptions of the theory: that gases are composed of minute particles in rapid motion; that the velocity of the particles increases with the temperature of the gas; that the particles move with uniform velocity in straight lines striking against the sides of the container, producing pressure; that the particles are perfectly elastic spheres; that they act on each other only during impact; and that their motion is subject to mechanical principles of Newtonian mechanics.

These assumptions suggest to Maxwell a set of questions: What exactly is the motion of the particles after they collide? Are all directions of rebound equally likely? What is the distribution of velocities among the particles? What is the mean distance traveled by a particle before striking another? And so forth. Maxwell's project in this paper is to develop the kinetic theory so that it can answer these and other theoretical questions. The method he employs to carry out this task is to construct mathematical derivations from the basic assumptions to theorems that will contain answers to these questions. In the course of doing so Maxwell introduces further underived assumptions. (For example, in deriving his important distribution law he assumes that the x-, y-, and z-components of velocity are independent.)

With the exception of the first page, the paper is devoted entirely to the derivation of the theorems. Very little attention is given to the origin of the basic assumptions of the theory. How did Maxwell arrive at them? They are highly speculative, involving as they do the postulation of unobserved particles exhibiting unobserved motion. Maxwell does provide two clues concerning their origin. One is that other physicists—he mentions Bernoulli, Herapath, Joule, Kronig, and Clausius—also supposed that gases contain particles moving with uniform velocity in straight lines. (Hypothetico-deductivists, as we shall see, will make something of this.) The other is the claim that various observable properties of gases can be deduced and explained from the assumptions of kinetic theory postulating motions of particles. (Retroductivists, as we shall see, will make something of this.)

Let me begin with hypothetico-deductivism. On this view, Maxwell did not *infer* his basic assumptions from anything; he guessed them. The physicist Richard Feynman offers a succinct version of the h-d position:

> In general we look for a new law by the following process. First, we guess it. Then we compute the consequences of the guess to see what would be implied if this law that we guessed is right. Then we compare the result of the computation to nature, with experiment or

> experience, compare it directly with observation, to see if it works. If it disagrees with experiment it is wrong. In that simple statement is the key to science. (1965, p. 156)

The initial stage in the process—the guess—may occur as a result of various causal influences. As with Maxwell, these may include the fact that certain other physicists had proposed similar ideas. Maxwell was particularly influenced by reading a paper of Clausius entitled "The Nature of Motion which we call Heat", which was published in 1857, some three years before his own publication. But Maxwell does not draw an inference from the fact that Clausius and some others have proposed the basic assumptions to the assumptions themselves. He simply appropriates some of the assumptions as part of his own theory. He makes the same, or some of the same, guesses as Clausius does. And he proceeds to use these guesses, as well as new ones of his own, to develop the theory mathematically.

To be sure, guessing is not incompatible with inferring. "Educated" guesses are inferences from somewhat meager data to conclusions deemed plausible. However, when h-d theorists speak of guessing they mean guessing that is not based on an inference from any data or facts. On this view Maxwell did not draw an inference, did not engage in any reasoning, to the fundamental assumptions of kinetic theory. In the context of discovery when Maxwell first arrived at the hypotheses he wished to consider further he had no data or facts that provided reasons for believing them.

By contrast, the retroductivist would say that Maxwell did not blindly guess the basic assumptions of his theory, or simply plagiarize them from others. He inferred them. He did have reasons for believing them before he constructed the derivations that constitute the bulk of his paper. More generally, according to the retroductivist, in the context of discovery a scientist does and should have some reasons to believe a hypothesis before considering it further. He does and should engage in a type of reasoning that does not *establish* a hypothesis but provides (at least some) basis for thinking it is true. Hanson proposes that it takes this form:

> Some surprising phenomenon P is observed
> P would be explicable as a matter of course if H were true
> Hence there is reason to think that H is true. (1958, p. 86)

Peirce suggests

> The surprising fact C is observed
> But if A were true, C would be a matter of course
> Hence there is reason to suspect that A is true. (1960, vol. 5, 5.189)

And Peirce makes it clear that he has in mind an explanatory relation between A and C.

In general, I take retroductivists to be claiming that in the context of discovery when a hypothesis is first proposed (and before conclusions from it—other than those that prompted it in the first place—are drawn and tested) there is an inference, the conclusion of which is that there is some reason to believe the hypothesis. The inference is based on the idea that the hypothesis if true would explain certain observed data.

Maxwell indeed notes at the beginning of his paper that various properties of gases can be explained by supposing that they are composed of minute parts in rapid motion. Thus, in the paper that preceded his, Clausius, by means of this assumption, offered qualitative explanations of the pressure exerted by gases, the work performed by gases when heated, and Gay-Lussac's law of combining volumes. And Maxwell, before he begins to derive consequences from the theory, explicitly notes that if gases are composed of minute particles in motion, then the pressure of the gas on the container is thereby (qualitatively) explained as being due to the impacts of the particles on the sides of the container. However, whether (as retroductivists would claim) Maxwell concluded from this and similar explanatory facts that there is a reason to think his kinetic theory hypotheses are true—whether he made a retroductive inference—is another matter, to which I will now turn.

2. Maxwell's Demand for "Independent Warrant". To begin with, Maxwell does not explicitly draw an inference of the retroductive sort described above. He does not say that the previous explanatory success of kinetic theory provides some reason to think that kinetic theory is true. His conclusion is much more guarded. It is that the previous explanatory success of a theory that assumes that gases have their minute parts in rapid motion makes "the precise nature of this motion . . . a subject of rational curiosity" (1965, vol. I, p. 377). The most that Maxwell concludes from the (preliminary) explanatory success of the theory is that it is reasonable to consider it further. (As I will argue below, it may be reasonable to consider a theory further without there being reasons to think that it is true.)

Secondly, in later writings (particularly in a paper published in 1875) Maxwell explicitly rejected reasoning similar in important respects to that described above. He notes that a method frequently used in getting from the observed to the unobserved

> is that of forming an hypothesis, and calculating what would happen if the hypothesis were true. If these results agree with the actual phenomena, the hypothesis is said to be verified, so long, at least, as

> some one else does not invent another hypothesis which agrees still better with the phenomena. (Maxwell 1965, vol. II, p. 377)

In accordance with this method, if the hypothesis entails and (thereby) explains known phenomena, then (in the absence of more successful hypotheses) it is verified or at least strongly supported. Maxwell objects to this method on the ground that if the hypotheses are not left vague and useless but their details are filled in, then there will be an "illegitimate use of the imagination". There will be insufficient empirical grounds to favor one hypothesis over a multitude of others that also entail and explain the known phenomena.

Moreover, although Maxwell does not explicitly mention this, there will be "crazy" hypotheses—ones that, given all of our background information, have a probability as close to zero as you like—that nevertheless (together with background information) entail observed data, and *if true,* would explain those data.[1] Yet this would constitute no reason for thinking them true. To take a nonscientific example, let the observed fact be that I am happy about the news I have just received. Let the hypothesis h be that I have just received the news that I have won the Nobel prize in literature. Let the background information include the fact that anyone who is awarded a Nobel prize is happy when he receives the news. Hypothesis h together with this background information entails that I am happy about the news I have just received; and if h were true it would correctly explain my happiness. But this fact provides no reason for thinking I have just received news I have won the Nobel prize.[2]

To avoid these illegitimate flights of fancy, but still allow certain hypotheses to be introduced, Maxwell in this paper of 1875 proposes what he calls a "method of physical speculation" (1965, vol. II, p. 420). Although he never formulates this method in a general or precise way, when

[1]Peirce (1960, 5.172–173) was aware that retroductive reasoning can yield such hypotheses, but he seems not to take this as a mark against retroduction. To the question "Why then do scientists make retroductions to the hypotheses they do—retroductions to the 'reasonable' rather than the 'unreasonable' ones?", his answer is that we possess a certain faculty of insight: "This Faculty is at the same time of the general nature of Instinct, resembling the instincts of the animals in its so far surpassing the general powers of our reason and for its directing us as if we were in possession of facts that are entirely beyond the reach of our senses." (5.173)

[2]In this example there is only one observed fact being explained. But the same problem arises even if the hypothesis inferred is required to explain and/or entail numerous, varied observations. Let $O_1, \ldots, O_n$ be a conjunction containing as many and varied observed facts as you like, including, for example, "the sky is blue", "grass is green", and "the sea is salty". Our hypothesis h is a conjunction of two propositions, the first of which postulates the existence of X, where X is anything you like, however implausible. The second conjunct in h is of the form "If X exists, then X causes it to be the case that $O_1, \ldots, O_n$", where the latter are the many and varied observation reports above. Hypothesis h entails these observation reports, and if true correctly explains them. Yet this fact provides no reason for thinking that h is true.

applying it to molecular hypotheses he begins by saying:

> Of all hypotheses as to the constitution of bodies, that is surely the most warrantable which assumes no more than that they are material systems, and proposes to deduce from the observed phenomena just as much information about the conditions and connections of the material system as these phenomena can legitimately furnish.
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> When examples of this method of physical speculation have been properly set forth and explained, we shall hear fewer complaints of the looseness of the reasoning of men of science, and the method of inductive philosophy will no longer be derided as mere guess-work. (Maxwell 1965, vol. II, p. 420)

Following this, Maxwell assumes that bodies are composed of unobservable particles, a hypothesis he regards as warranted by "experimental proof" (which he does not give). And he cites a version of Clausius' virial equation that relates the pressure and volume of a gas to the kinetic energy of the system of particles it contains and to the distance between particles and the forces between them. This is warranted because it is derivable from general Newtonian principles as applied to such a system of unobserved particles.

In general, Maxwell's method of physical speculation requires that a hypothesis have independent warrant for it to be believable. It is not sufficient that some observed phenomena be derived or explained via a hypothesis. However, Maxwell does derive and explain known phenomena from hypotheses, and there are occasions on which he seems to take such derivations or explanations as providing some positive support for the hypotheses. For example, after citing the virial equation he shows how this equation can be used in deriving and explaining Boyle's law as well as observed deviations from Boyle's law at low temperatures and high densities. And he takes this as providing support for ideas contained in the virial equation. From the fact that the virial equation entails and explains deviations from Boyle's law at high densities and the fact that such deviations are observed, Maxwell concludes that at high densities there are significant forces between molecules and that these are mainly attractive—in accordance with the ideas of the virial equation. But if the h-d and retroductive accounts are to be rejected, how can these facts provide such support? What Maxwell is assuming, I suggest, is that they can provided that there is independent warrant for the virial equation (which Maxwell thought there was).

More generally, I take Maxwell to be insisting on the existence of independent warrant for a hypothesis in order to take the fact that the hypothesis entails and/or explains known phenomena as constituting a reason to believe it true. Although Maxwell does not use this terminology, the requirement of independent warrant might be understood as demanding at least that the hypothesis have some significant probability in the light of all the observations and accepted background information. To draw a contrast with retroduction, I shall consider Maxwell's method of physical speculation insofar as it is applicable to explanations. Both methodologies can be understood as committed to views about when the fact that a hypothesis explains something can be taken as some reason to believe it true. Let me formulate these two views initially as follows:

Basic Retroduction. The fact that hypothesis h, if true, would correctly explain observed facts $O_1, \ldots, O_n$ constitutes at least some reason for thinking that h is true.

Maxwell's Method of Physical Speculation. Given the background information b, the fact that h, if true, would correctly explain observed facts $O_1, \ldots, O_n$ constitutes at least some reason for thinking that h is true provided that $p(h/O_1, \ldots, O_n \& b) > k$.

Basic retroduction has as its only condition that h if true correctly explains O. Further conditions can be added (for example, conditions on the explanation) to obtain more complex versions. But as I will understand retroduction, it will not require that there be independent warrant for h.[3] The second condition in Maxwell's method, which stipulates that

[3]Do retroductivists such as Hanson and Peirce have in mind additional conditions for retroduction? If so do these include independent warrant? Hanson (1958, pp. 87–88), after introducing the retroductive inference form cited earlier, indicates two conditions he attaches to the idea of "explicable as a matter of course". One involves the idea of providing what he calls a "pattern" in terms of which to understand the observed phenomena. The other stipulates that "if h is meant to explain P, then h cannot itself rest upon the features in P which require explanation" (p. 88). (His example is that you can't explain the green color of chlorine by appeal to green atoms.) Whatever these conditions amount to, they do not seem to require any significant probability for h given P. Just before Peirce introduces his retroductive schema he writes (5.189): "Long before I first classed abduction [retroduction] as an inference it was recognized by logicians that the operation of adopting an explanatory hypothesis—which is just what abduction is—was subject to certain conditions. Namely, the hypothesis cannot be admitted, even as a hypothesis, unless it be supposed that it would account for the facts or some of them." In the present passage the only condition on retroductive inference that Peirce suggests is the explanatory one. In other writings, however, Peirce mentions two additional conditions (7.220). One is that the hypothesis be empirically testable. The other is that it be "economical". Among the several considerations that Peirce includes under economy is the "expectation that a given hypothesis may be true", which may be based on "positive facts which render a given hypothesis objectively probable". However, Peirce does not demand such probability as a necessary condition for a retroductive inference. He writes: "Nothing has caused so much waste of time and means, in all sorts of researches, as inquirers becoming so wedded to certain likelihoods as to forget all the other factors of the economy of research; so that, unless it be very solidly grounded, likelihood is far better disregarded, or nearly so; and even when it seems solidly grounded, it should be proceeded upon with a cautious tread, with an eye to other considerations, and a recollection of the disasters it has caused." (7.220) Accordingly, while Peirce's position in the present passage is more complex than

the probability of h, given the observations and the background information, be greater than some acceptable threshold value k, represents the idea that independent warrant is required. My Nobel prize hypothesis, if true, would correctly explain why I am happy over the news I have received. But this fact constitutes no reason for thinking this hypothesis is true, since, given the relevant background information, its probability is as close to zero as you like. It has no independent warrant. I suggest that Maxwell was right in rejecting basic retroduction, and that his independent warrant condition, or something like it, is required in addition.[4]

Accordingly (to return to his first kinetic theory paper), for Maxwell to have reasonably concluded that the success of kinetic theory in explaining the pressure and other observed properties of gases constitutes some reason for believing true the assumptions of kinetic theory, there would need to be background information that together with observations of gases gave some significant probability to these assumptions. Did Maxwell have such independent warrant?

There are, I think, two plausible candidates for such warrant.

1. Argument From Observations of Heat. In his book *Theory of Heat,* first published in 1871, Maxwell offers independent support for certain basic ideas in kinetic theory from facts about heat (facts known to him and the physics community before the publication of his first kinetic theory paper), together with widely shared assumptions about motion. Heat is known to be transferable from a hotter to a colder body by radiation. Now, says Maxwell,

> Whatever theory we adopt about the kind of motion which constitutes radiation, it is manifest that radiation consists of motion of some kind, either the projection of the particles of a substance called caloric across the intervening space, or a wave-like motion propagated through a medium filling that space. In either case, during the interval between the time when the heat leaves the hot body and the time when it reaches the cold body, its energy exists in the intervening space in the form of motion of matter. (Maxwell 1875, p. 303)

Now this motion of matter in the intervening space can only be caused by motion in the body radiating the heat.

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that expressed in his own retroductive schema given above, it would be incorrect to equate it to Maxwell's method of physical speculation.

[4]For more discussion see my 1983, ch. 10, and my 1985, pp. 127–145.

> Every hot body, therefore, is in motion. We have next to enquire into the nature of this motion. It is evidently not a motion of the whole body in one direction, for however small we make the body by mechanical processes, each visible particle remains apparently in the same place, however hot it is. The motion which we call heat must therefore be a motion of parts too small to be observed separately. . . .
>
> We have now arrived at the conception of a body as consisting of a great many small parts, each of which is in motion. We shall call any one of these parts a molecule of the substance. . . . (Maxwell 1875, pp. 303–305)

Maxwell's argument from considerations of heat transfer depends on various assumptions about motion that he seems to regard as plausible, for example, that motion can only be caused by other motion, that if something is moved from *A* to *B* there is motion at *A*, and that if there is motion at *A* but this is unobservable, then there is unobserved matter in motion. The argument is written with a degree of certainty about the existence of molecules that was lacking years earlier when he published his first kinetic theory paper. But it is conceivable that the known facts about heat transfer, together with assumptions about motion, would have provided Maxwell with at least some independent warrant for (a) the existence of invisible particles as constituents of bodies, (b) the claim that these particles are in motion, and (c) the claim that the motion of these particles is responsible for the radiation of heat. In fact a somewhat similar argument had been given in 1847 by Joule (who discovered the mechanical equivalent of heat). (See Joule 1965, pp. 78–88.) And Maxwell was aware of Joule's work.

I will not here try to assess the claim that the above observations of heat radiation (in the light of assumptions about motion) provide at least some independent support for (a)–(c) above. It is plausible to suppose that Maxwell took them to do so. More generally, supporters of kinetic theory of this period cited such facts, together with Joule's discovery of the mechanical equivalent of heat and the problems besetting the caloric theory, as providing some support for the idea that heat is molecular motion.

2. *Argument From the Previous Success of Dynamical Theories.* In 1856 in his Inaugural Lecture at Aberdeen, as well as in 1860 in his Inaugural Lecture at King's College, London, Maxwell stressed the success of *dynamical* theories—those that describe systems containing parts in motion between which forces that obey Newtonian laws are acting. He notes the success of dynamical theories in astronomy and he takes this

success as providing at least some reason, though by no means a conclusive one, to think that dynamical principles are applicable to any physical system composed of moving parts, whether or not these parts and their motion are observable. In 1875 in his paper "On the Dynamical Evidence of the Molecular Constitution of Bodies" Maxwell reiterates the success of dynamical theories in other domains—this time adding "electrical science" to astronomy—and he again takes this as some reason to suppose that Newtonian principles are applicable to unobservable parts of bodies.

In sum, then, we have the following two ideas:

(i) Considerations of heat transfer (together with widely held assumptions about motion, and also, let us say, with Joule's determination of the mechanical equivalent of heat and the difficulties of caloric theory) lend some support to the claim that bodies contain unobservable parts, that these parts are in motion, and that this motion is responsible for heat.

(ii) Considerations of the success of Newtonian dynamical principles in other domains suggest that if bodies are composed of unobservable parts in motion, then this motion too is subject to Newtonian dynamical principles.

Combining (i) and (ii) we get some independent warrant for basic ideas of kinetic theory. I shall assume that Maxwell did have both (i) and (ii) on his mind when he proposed his fundamental assumptions of kinetic theory. If so, it looks as if retroductivists are right on at least one important point. Maxwell did not simply guess these assumptions. He *inferred* them from certain facts. Where those who support the basic retroductive position go awry—at least from the Maxwellian point of view—is in supposing that the fact that kinetic theory, if true, would correctly explain certain observed properties of gases suffices to justify an inference to that theory. As required by Maxwell's own method of physical speculation, such an inference is reasonable only if there is at least some independent warrant for the assumptions of kinetic theory. On the present considerations such an inference is reasonable, since there was independent warrant.

3. Is Hypothetico-Deductivism Refuted? If Maxwell inferred the basic assumptions of kinetic theory, and if his inference was reasonable, is hypothetico-deductivism refuted? I want to challenge this conclusion by challenging the premises. Although Maxwell was in a position to reasonably infer some of his assumptions in kinetic theory—and for the sake of the argument let us suppose he did so—he was not in a position to reasonably infer the entire set. From the fact that $h_1, \ldots, h_n$ if true would correctly explain $O_1, \ldots, O_n$ we are permitted to infer that (there is some reason to think that) $h_1, \ldots, h_n$ are true only if there is independent warrant for $h_1, \ldots, h_n$. But from the fact that there is independent warrant for some members of this conjunction, it does not follow that there is independent warrant for the entire conjunction.

Look again at the two independent warrant considerations. What they make plausible are certain assumptions about matter generally, not just about gases. Considerations of heat radiation—whether from gases, liquids, or solids—suggest that there is motion of unobservable parts in the matter that is heated. And considerations from the success of dynamical theories suggest that the motion of these parts obeys Newtonian dynamics. But these considerations have nothing to say about:

(a) The Paths of the Molecules in a Gas. Maxwell assumes that the particles travel in straight lines only. But considerations from heat transfer and from the success of dynamical theories in astronomy and electrical science do not preclude stationary molecules that exhibit rotational motion. (A rotational theory had been suggested in 1847 by Joule (1965, p. 86).) And if we don't assume that the forces acting between the particles are contact forces, the independent warrant considerations permit nonlinear translatory motion. (Indeed, in his second paper on kinetic theory Maxwell abandons contact forces in favor of a law according to which the force between two molecules varies as the inverse fifth power of the distance between them.)

(b) The Particular Force Law Governing Molecules in a Gas. Maxwell assumes in this paper that the only forces are contact forces. Newtonian principles require that the force, whatever it is, satisfy $F = ma$. But they don't require contact forces. As in astronomy and electrical theory, molecules may exert forces at a distance. That is, what is known about other forces in nature does not provide more support for contact than for noncontact forces.

(c) The Shape of Molecules. Although Maxwell assumes they are spherical, this is not required by Newtonian dynamics or by considerations from heat transfer. In his second kinetic theory paper Maxwell abandons this assumption.[5]

[5]"In the present paper I propose to consider the molecules of a gas, not as elastic spheres of definite radius, but as small bodies or groups of smaller molecules repelling one another with a force whose direction always passes very nearly through the centres of gravity of the molecules, and whose magnitude is represented very nearly by some function of the distance of the centres of gravity." (Maxwell, 1965, vol. II, p. 29)

(d) The Relationship Between Components of Molecular Velocity. Although it is not one of the assumptions given at the outset of his paper, in order to derive a velocity distribution law Maxwell assumes (without any argument) that the different spatial components of velocity are independent. This allows him to suppose that the probability that a molecule has an x-component of velocity between x and $x + dx$ is independent of the probability that it has a y-component between y and $y + dy$, and of the probability that it has a z-component between z and $z + dz$. This assumption is not required by Newtonian dynamics or by heat transfer considerations. In his second kinetic theory paper he abandons this assumption and offers an alternative derivation of his distribution law.

At most, the two independent warrant considerations provided the basis for an inference to assumptions that Maxwell made that gases are composed of unobservable particles, that these particles are in motion which is also unobservable, that this motion is responsible for heat (so that the temperature of the gas increases with the velocity of the particles), and that the motion of the particles satisfies Newtonian dynamics. But the independent warrant considerations do not provide a basis for an inference to a number of very central assumptions that Maxwell made pertaining to the paths of molecules, the forces between them, their shapes, and the relationships between components of their velocity. At best, the retroductivist is right in claiming that there were fundamental assumptions in kinetic theory to which Maxwell made, or was in a position to make, a reasonable inference at the outset when he proposed them. But unless we can find more independent warrant than we have so far, there were other fundamental hypotheses to which Maxwell was not in a position to make a reasonable inference at the outset. Can we conclude with the hypothetico-deductivist that Maxwell made no inference to these hypotheses but simply guessed them?

4. Simplicity and Analogy. Before drawing this conclusion we ought to note two factors frequently mentioned in connection with "independent warrant". The first is simplicity. Doesn't the fact that Maxwell's basic assumptions were simple ones constitute some independent warrant for them? That depends on the source of their simplicity.

Consider Maxwell's assumption about the paths of molecules. Straight line motion is simpler than rotational motion or than translation along an elliptical path (say). The source of this simplicity is mathematical. Linear equations of the form $Ax + By + C = 0$ are mathematically simpler than those for conic sections (which include ellipses) of the form $Ax^2 + By^2 + Cxy + Dx + Ey + F = 0$. But does the fact that the equation for a straight line is simpler than that for an ellipse make the hypothesis that molecules travel in straight lines more probable than that they travel in elliptical orbits? (Keep in mind that we are assuming nothing about the motion of particles except that it obeys Newton's laws; in particular we are making no assumption about the type of force between molecules.) Suppose you take the same walk each night, but I know nothing about the path you take. Does the fact that the equation for a straight line is simpler than that for an ellipse make it more likely that your path is linear than elliptical? Such a conclusion seems very dubious.

Or take Maxwell's assumption about the forces between molecules. A force law of the form $F = 0$ (except when the distance between the centers of two molecules equals the sum of their radii) is mathematically simpler than one of the form $F \propto 1/r^n$. But does this fact make the hypothesis that molecules exert no forces on each other except at contact more probable than that they exert a noncontact force that varies inversely as some power of the distance between them? Again the reasoning is not persuasive.

I am not here making the more general claim that simplicity is always irrelevant for probability. I assume only that where one physical hypothesis is simpler than another solely because an equation in one is mathematically simpler than an equation in the other we cannot on this basis alone conclude that one is more probable than the other. Since the source of simplicity in the hypotheses about the paths of molecules and the forces between them derives (as I see it) entirely from such mathematical simplicity, we do not yet have independent warrant for such hypotheses. This does not mean that the mathematical simplicity of the equations is irrelevant in determining whether to *consider* a certain hypothesis (more of this later). What I am disputing is only that it necessarily enhances the probability of the hypothesis.[6]

It has been argued that analogies can provide independent warrant for basic assumptions of a theory.[7] Maxwell himself emphasized the use of analogies in developing theories, particularly in his early work in electromagnetism. In the present paper on kinetic theory he also mentions at two points that he is seeking to draw a "physical analogy" between a gas and a system of unobservable particles of the sort he describes in his basic assumptions. Unfortunately, Maxwell does not spell out his analogy idea in kinetic theory the way he does in electromagnetism. But he may have had in mind something like this.

We have an "original" system—in this case a gas—that has certain known properties and satisfies known laws. For example, a gas exerts pressure on the walls of its container; it has a certain density and tem-

[6] Popper, as is well known, has defended the view that the greater the mathematical simplicity of the hypothesis, the lower its probability.

[7] See Mary Hesse (1974), and Robert MacLaughlin (1982, pp. 69–100).

perature; it exhibits viscosity; and its satisfies Boyle's law. We now describe a second system, the "analogue" system, in terms of a set of assumptions. For example, we describe a dynamical system containing an enormous number of unobservable particles satisfying all of the assumptions that Maxwell makes at the beginning of his paper, including the assumption that molecules are spherical, that they travel in straight lines, that they are subject only to contact forces, and so forth. We then show that (and how) this analogue system has (some or all of) the same properties and satisfies (some or all of) the same laws as the original system. Thus we show that (and how) the dynamical system of particles exerts pressure on the walls of its container; that it has density, temperature, and viscosity; and that it satisfies Boyle's law.

Could this analogy provide some independent warrant for those assumptions of kinetic theory for which we found no independent warrant in section 3? If so, presumably there would be a reasonable argument with a form such as this:

> Gases have properties $P_1, \ldots, P_n$
>
> The analogue system of particles also has properties $P_1, \ldots, P_n$, and it does so because it satisfies molecular assumptions $h_1, \ldots, h_n$.
>
> Hence there is reason to suppose that gases also satisfy molecular assumptions $h_1, \ldots, h_n$ (or assumptions similar to these).

But such an argument is legitimate only if there is some reason to preclude the description of analogue systems in which properties $P_1, \ldots, P_n$ are produced by quite different causes. If no reason is given to think that a system satisfying molecular assumptions $h_1, \ldots, h_n$ is the only one capable of manifesting properties $P_1, \ldots, P_n$, or, if not the only one capable, is more likely to exist than the others, then the argument above carries no force.

Now from our earlier discussion let us grant that there is some reason to believe that there exists a set of unobservable particles that satisfy Newtonian laws and are responsible for heat transfer. But (so far at least) we have no support for the assumption that in addition those particles are spherical, move in straight lines, exert contact forces, and have independent components of velocity. We have no support for the claim that there exists a set of particles of just the kind postulated by Maxwell in his kinetic theory. Nor do we have support for the claim that only such a system of particles can satisfy the properties and laws of gases, or that if various systems can, this one is the most likely to exist. The fact that we can imagine such particles and show that as a consequence of all of Maxwell's assumptions a set of such particles has certain properties (for example, pressure and viscosity) and satisfies certain laws (for example, Boyle's law) identical to those of gases does not by itself provide a reason for thinking that gases are such systems.

In fact the situation here is similar to the earlier one involving basic retroduction. The fact that we can describe a hypothetical system that, if it existed (as described), would explain some phenomenon cannot by itself, according to Maxwell, be taken as a reason to suppose that system exists. Similarly, the fact that a certain system, if it existed (as described), would have many of the same properties as gases cannot by itself be taken as a reason to suppose that such a system exists, or to conclude by analogy that gases have many properties identical or similar to the ones attributed to the hypothetical system. Indeed, in his early work on electromagnetism Maxwell makes it clear that his use of analogies avoids any commitment to hypotheses about unobservables. In his paper "On Faraday's Lines of Force" Maxwell constructs a physical analogy between the electromagnetic field and an incompressible fluid flowing through tubes of varying section. But from the fact that the fluid he describes has certain properties and satisfies certain laws analogous to those of the electromagnetic field, he does not conclude that there is any reason to suppose that such a fluid exists or that the electromagnetic field has analogous microproperties.[8]

5. What Makes a Hypothesis Worth Considering? Let us grant that Maxwell inferred some of his basic assumptions, and that he did so on the basis of considerations from heat transfer and the success of dynamical theories in other domains. This still leaves important assumptions for which these considerations provide no independent warrant. To be sure, these assumptions (plus the others) if true would correctly explain a range of gaseous properties. But unless there was some independent warrant for them, Maxwell was not in a position to infer from this that there is a reason to think that these assumptions are true. He (rightly) rejected the basic retroductive account.

Here I propose to agree with the hypothetico-deductivists. Maxwell did not make any inference to a number of his central postulates when he first proposed them for consideration. *He had no reason at all to think they were true*. Although Maxwell had some reason to think that gases are composed of unobservable particles in motion satisfying Newtonian laws, he had no reason at all to think that such particles are spherical in

[8]Concerning this fluid Maxwell (1965, vol. I, p. 160) writes: "The substance here treated . . . is not even a hypothetical fluid which is introduced to explain actual phenomena. It is merely a collection of imaginary properties which may be employed for establishing certain theorems in pure mathematics in a way more intelligible to many minds than that in which algebraic symbols alone are used."

shape, or that they exert only contact forces, or that their motion is linear rather than nonlinear, or that their velocity components are independent.

Does this mean that h-d theorists would be correct in saying that Maxwell simply guessed these hypotheses? Suppose that a detective has ruled out all but ten suspects as perpetrators of the crime. The rest have airtight alibis. He then chooses one at a time to investigate. In choosing a particular one of these to investigate first the detective is not, or need not be, guessing that this person is guilty. That would be too strong an epistemic commitment for him to have. Rather, the detective is investigating the possibility that the person in question is guilty. Similarly, although Maxwell did not infer that molecules are spherical from any considerations, neither did he guess that this is so. In proposing this as a basic assumption he was simply considering that possibility. In doing so he was exhibiting no epistemic commitment to the truth or probability of this assumption.

But if Maxwell had no reason at all to think that molecules are spherical how could he have rationally proposed to consider that possibility? Such a hypothesis was *worth considering* even if it had no independent warrant, even if there was no reason to think it was true. How can this be so?

For the retroductivist, as we have described his position so far, it cannot be so. A hypothesis is worth considering only if there is some reason to think it is true. However, we might alter retroductivism by dropping this requirement and substituting

(1) Given observations O, hypothesis h is worth considering if and only if h if true would correctly explain O.

We would then be construing retroductivism as a position about when a hypothesis is worth considering, and not about when there is reason to think it true. Indeed, although Hanson does formulate retroductive reasoning in such a way that it has "therefore there is reason to think that h is true" as the conclusion, on occasion he also formulates it so that the conclusion is "therefore h is worth considering" or "therefore there is good reason for elaborating h".[9] If we do not construe this as implying that there is reason to think that h is true, we allow for the possibility that h may be worth considering even if there is no reason to think it is true.

Unfortunately, the alternative retroductive thesis (1) provides a condition that is neither necessary nor sufficient. That it is not necessary will be shown by an example below used to criticize an even stronger version (2). That it is not sufficient is demonstrated by means of examples that invalidate the previous version as well. There are numerous hypotheses

[9]See N. R. Hanson 1983, pp. 60–61.

that if true would correctly explain some observations but are not worth considering on the basis of those observations. To use an earlier example, let O be that I am happy about the news I have just received. Let h be that I have just received the news that I have won the Nobel prize in literature. Hypothesis h if true would correctly explain O. But h is not worth my consideration given O and my background information.[10]

Maxwell and others who reject retroduction as well as the h-d method might suggest that the reason the Nobel hypothesis is not worth considering is that it is absolutely crazy. That is, given my background information b, the probability of h on b is approximately zero. Accordingly, we might write

(2) Given background information b and observations O,
hypothesis h is worth considering if and only if
(a) h if true would correctly explain O
(b) $p(h/O\&b)$ is not (approximately) O.

This can be construed as weaker than the Maxwellian independent warrant condition, since the second clause requires only that the probability of h given O and b not be very close to 0. It does not require that there be some independent reason to think h true, only no overwhelming reason to think h false.

I suggest that (2) is too strong. Neither (a) nor (b) is a necessary condition. Given the background information and a set of observations O, h may be worth considering even if h is incompatible with O—so that h if true would not correctly explain O and $p(h/O\&b) = 0$. For example, given Maxwell's background information, and given observations about the viscosity of gases—including the observation that the coefficient of viscosity varies with the absolute temperature of the gas—Maxwell's molecular hypothesis about viscosity may well have been worth considering. (Maxwell's hypothesis was that molecules exist in various layers in the gas, those in different layers having different mean velocities; those in one layer may pass into another layer, striking the particles in it and exerting a tangential force that produces the viscosity of the gas.) Yet Maxwell's molecular hypothesis about viscosity (together with the rest of his kinetic theory) entailed that the coefficient of viscosity is proportional not to the temperature of the gas but to the square root of the temperature. Accordingly, his hypothesis, if true, would not correctly explain an important observed fact about viscosity, thus violating (2a). Moreover, since h is incompatible with that observed fact, $p(h/O\&b) = 0$, thus violating (2b).

[10] The same problem arises even if we strengthen (1) by requiring that h explain a variety of observations. See footnote 2.

One might be tempted to weaken (2) by requiring not that (a) and (b) be true but that they be reasonable to believe (thus obviating the last objection). But this is still too strong a requirement. Maxwell may have had no reason whatever to believe that his molecular hypothesis about viscosity, if true, would correctly explain various known facts about gaseous viscosity. He did have a good reason to suppose that if true it would correctly explain (in a qualitative way) the *existence* of viscosity. But whether, if true, it would offer a quantitative explanation of the observed relationship between viscosity and other known quantities such as temperature was another matter. His hope was that it would. But he may have had no reason to suppose that it would. Moreover, he may have had no reason to believe anything about the probability of his molecular hypothesis about viscosity, given his background information and given the observation that viscosity varies with temperature. It may have been reasonable to suspend belief on this probability. Still on the basis of the background information and observed facts about viscosity his particular hypothesis about viscosity may well have been worth considering.

Indeed, a hypothesis may be worth considering even when the scientist as yet has no observational data which that hypothesis could in principle explain. Maxwell had no observational data regarding the distribution of molecular velocities.[11] (Such data became available only in the 1920s with the introduction of molecular beam experiments.) Yet in order to determine a theoretical law giving the distribution of molecular velocities, Maxwell introduced the hypothesis that velocity components are independent. This hypothesis was worth considering even though it did not explain observational data that Maxwell had concerning molecular velocity components or the distribution of velocities.

How can these things be so? How can a hypothesis be worth considering even though, if true, it would not explain the data one has, or if one as yet has no data for it to explain?

6. A Broader Proposal. Let me propose a new way of looking at the situation. On the basis of certain considerations Maxwell makes these assumptions:

1. Gases are composed of unobservable particles;
2. These particles are in motion, which is also unobservable;
3. The motion of the particles is responsible for heat transfer;
4. The motion of the particles satisfies Newtonian dynamics.

Call this set T_i (initial assumptions). In Maxwell's case T_i consists of those assumptions for which he had some independent warrant. But we

[11]For more on this see my 1986.

need not suppose that this is necessary. Perhaps T_i contains assumptions made by others, or just formulated de novo by the scientist himself—assumptions for which the scientist has no independent warrant and with respect to which he is, for the moment, epistemically neutral.

The assumptions in T_i generate a set of questions, for example,

a. What is the motion of the particles? What paths do they take?
b. What are the forces between particles?
c. What is the shape of the particles?
d. How are velocities distributed among particles?

More generally, T (together possibly with other background assumptions being made) will be said to generate a question Q if T (together with these additional assumptions) entails a *complete presupposition* of Q. A question such as (a) above presupposes a number of propositions, for example,

(i) There are particles;
(ii) There is motion;
(iii) The particles have some motion.

Any proposition entailed by a proposition presupposed by a question will also be said to be presupposed by that question. A complete presupposition of a question is a proposition that entails all and only the presuppositions of that question.[12] Of the three propositions in the above set only (iii) is a complete presupposition of question (a). Since this proposition is entailed by T_i the question (a) is generated by T_i. Similarly, questions (b), (c), and (d) are generated by T_i (together with certain other assumptions Maxwell was making). For example, assumptions (1), (2), and (4), together with the additional assumption (which Maxwell would have taken as plausible) that particles are three-dimensional bodies entails "the particles have some shape". Since this is a complete presupposition of question (c), the latter is generated by T_i. By contrast, question

e. Why did God create unobservable moving particles?

also presupposes (i)–(iii) above. But its complete presupposition:

God created unobservable moving particles for some reason

is not entailed by T_i. Accordingly, T_i does not generate (e).

Sometimes an answer to a generated question is mathematically or logically derivable from the set T_i. But very often it is not. Such an answer, when forthcoming, will be a new underived assumption in the theory. Possibly many such answers to a given question are possible. For ex-

[12]See my 1983, pp. 29ff.

ample, to question (b) one might respond with the law $F = 0$ (except at impact), or with any law of the form $F \alpha 1/r^n$. Such answers to (b) would entail different answers to (a). When is it reasonable to consider one of the assumptions? Let me offer the following sufficient condition:

> Given T_i, a new assumption A is worth considering if it answers a question Q generated by T_i in such a way as to satisfy a set of appropriate instructions for Q.

Instructions are rules for answering a question. They provide constraints on the answer, and include typical methodological considerations, for example,

(1) Very general methodological criteria valued in science. Instructions may require that the answer to Q satisfy some standard of generality (for example, that it employ laws), that it be mathematically formulated, that there be some empirical evidence supporting it, that it be simple, unifying, etc.
(2) More specific empirical constraints. Instructions may require that the answer to Q satisfy Boyle's law, or the principle of conservation of energy, or the principle that there are no preferred directions in space.
(3) Pragmatic constraints. The instructions may require that the answer be one that is mathematically tractable, or one the empirical testing of which is relatively simple.

Obviously given considerations of types (2) and (3), the question of whether some set of instructions is *appropriate* is highly contextual. What specific empirical constraints it is appropriate to impose will depend upon what is known or knowable by the scientist and his community. It would not be appropriate to require Maxwell to propose answers that satisfy special relativity. Similarly, whether a given answer is mathematically tractable or testable depends on the mathematical and empirical procedures usable by those in the scientific community. Indeed, even the applicability of criteria in (1) is context-dependent. These criteria (I have argued in my 1983, ch. 4) provide neither necessary nor sufficient conditions. Whether generality, precision, empirical support, simplicity, etc. are needed for a hypothesis to be worth considering will depend in part on the specific knowledge of the community and on the kinds of answers it is interested in achieving. Yet they are relevant criteria. They set a direction for what kind of hypotheses scientists should try to consider at some point. To determine whether some particular set of instructions is appropriate, these criteria must be used in conjunction with contextual facts.

Moreover, whether a particular answer to Q satisfies appropriate instructions will depend upon whether the question itself is worth pursuing. Questions generated by a theory that pertain to some quantity such as motion or to some quantitative relationship such as mutual forces—questions such as (a) and (b)—are usually of intrinsic interest to those articulating the theory, and will also be of value because of their tendency to aid in further developing the theory, especially mathematically. But this can vary depending on knowledge and interests. (In certain contexts there may be no appropriate instructions for Q even though it is generated by T.)

Let's see how this works in the case of Maxwell. Maxwell has the set T_i given at the beginning of this section. He proposes to add assumption

p: The force between molecules is zero except at impact.

p answers the question

Q: What is the force between molecules?

which, in virtue of Newtonian dynamics, is generated by T_i. Does p satisfy some set of appropriate instructions for Q? It satisfies *some* of the broad methodological criteria valued in science: it is very general; it is mathematically formulated; and it is quite simple. Moreover, it satisfies the pragmatic constraint of being mathematically tractable. It readily allows a calculation of momentum transfer, and thus a derivation of the pressure law. To be sure, there is no empirical evidence supporting it (or any other force law). But where there is no empirical evidence for this or any other force law, it may still be worth considering if the hypothesis satisfies instructions incorporating other desirable ends. This is precisely the case with Maxwell's force law. It was worth considering because it answered a question generated by a theory he was developing, and did so by providing a general, mathematically formulatable, simple answer, that could readily be used by Maxwell to generate answers to further questions. Moreover, this question was one Maxwell was interested in answering. His avowed aim at the beginning of his paper is to develop kinetic theory in a quantitative way "on strict mechanical principles"—an aim that requires, or at least is facilitated by, some assumption about the forces governing molecules. In the absence of any empirical reason for choosing this rather than another force law, these facts suffice to make his answer worth considering. In the context in which Maxwell was operating—given his knowledge and that of his community—it would not have been appropriate to invoke instructions requiring that the answer considered have empirical support.

If this is so how do we preclude considering "crazy" hypotheses such as the Nobel hypothesis? We begin in this case not with a theory, but with the observed fact that I am happy over the news I have just received, and with the assumption that there is some reason for my happiness. These generate the question "Why am I happy?", which, let us assume, members of the audience want to answer. If empirical support for a hypothesis is not always required, what makes the Nobel hypothesis unworthy of consideration? In this situation we have much more information than in Maxwell's case. Maxwell had no empirical reasons to prefer one force law over another. But we know that philosophers have rarely won the Nobel prize and we know something about my literary talents. Moreover, we know from past experience that there are other much more likely explanations. In short, we do have strong empirical reasons for not considering the Nobel hypothesis. Maxwell, at the outset at least, had no such reasons for not considering contact forces between molecules.

The physicist Feynman in the quote at the beginning of this paper asserts that physics is play. I interpret this to mean that it can be worthwhile to consider and work out theoretical principles even if one hasn't any idea whether they are likely to be correct, even if they have no independent warrant. One can play with an idea and see where it leads, even if there is no reason to think it is true. However, it is crucial to distinguish between (a) having a reason to think a hypothesis is true (having a reason to believe it), and (b) having a reason to consider it. Maxwell had a perfectly good reason to consider (to "play with") the hypothesis that forces between molecules are contact forces, even if he had no reason to think it true or likely. His reason was that it answered a question generated by his theory—one that he was interested in answering so that his theory could be further developed—and did so in a way that satisfied appropriate instructions calling for a general, quantitative, simple answer that is mathematically tractable. He was not in a position to satisfy instructions calling for an answer for which there was independent warrant. Such instructions would have been inappropriate for his situation. They would have prevented further development of the theory.

Having argued that a hypothesis can be worth considering even if one has no reason to think it is true, let me take this further. A hypothesis can be worth considering even if one has some reason—indeed a conclusive one—for thinking it is false. What matters is the aim of the consideration. Here are several situations in which this is possible.

(1) Criticizing a theory. There is a theory accepted by most in the community that I want to criticize. I consider a certain hypothesis from that theory—which on independent grounds I know or believe to be false—and show that it leads to false predictions. The hypothesis is worth considering because of its widespread acceptance. Yet I have strong reason for thinking it false.
(2) Showing that a certain type of theory is possible. In his paper

"On Physical Lines of Force" Maxwell's aim is to show that a mechanical theory of the electromagnetic field is possible by imagining a purely mechanical system that will reproduce known electromagnetic properties. He is not supposing that the particular mechanism he introduces for this purpose is true or even probable. At certain points he introduces hypotheses that he believes are probably false (for example, the idea of vortices within the electromagnetic field connected by particles which are in rolling contact with the vortices).[13] In this situation Maxwell is trying to determine whether there could in principle be a mechanical conception of the electromagnetic field. There is also a premium on hypotheses that can be easily investigated mathematically. Accordingly, it can be reasonable to consider specific mechanisms even if one has independent reasons for believing them false or improbable.

(3) Producing idealizations. I introduce a hypothesis that I have some independent reason for believing to be false; yet it may be approximately true. If the hypothesis has other virtues (mathematical tractability, etc.), it may be worth considering.

No doubt those who defend the idea that a hypothesis is worth considering only if it has some independent warrant will reply that they are not speaking of situations of types (1)–(3). They are speaking of:

(4) Proposing a theory in order to correctly explain a range of phenomena. While doing (1)–(3) may serve as a useful prolegomenon to (4), it is not sufficient. What I have been arguing is that in doing (4), no less than in doing (1), (2), and (3), hypotheses which there is no reason to believe true or probable may be worth considering.

There is a moral here for agencies that provide financial support for scientific research projects. Suppose that in 1858 Maxwell had submitted a proposal to a government agency to support his theoretical research in kinetic theory. He proposes to "lay the foundation of such investigations on strict mechanical principles" by deriving consequences from a set of assumptions he makes about molecules. For some of these he has some independent warrant. But for a number of the central ones he has no such warrant; he has no reason at all to think they are true. Nor does his pro-

[13]Maxwell (1965, vol. I, p. 487) writes: "The conception of a particle having its motion connected with that of a vortex by perfect rolling contact may appear somewhat awkward. I do not bring it forward as a mode of connexion existing in nature, or even as that which I would willingly assent to as an electrical hypothesis. It is, however, a mode of connexion which is mechanically conceivable, and easily investigated. . . ."

posal contain a description of any experiments that he will conduct to test these or any other assumptions in the theory. Whether his proposal should be funded will depend on several factors in addition to its scientific value (for example, the amount of money available, the quality of other proposals, his scientific credentials, etc.). But most importantly, it will depend on whether the theory he proposes to develop is worth considering. Accordingly, in his proposal to the funding agency he should provide good reasons for considering the theory. If he has reasons for thinking that the theory, or some part of it, is true, he should say what they are. But he may not yet be in a position to do so. Depending on the circumstances, it may be legitimately decided to fund his proposal to work out the idea that molecules are perfectly elastic spheres subject to contact forces, even though there are no reasons to think that this idea is true. Funding agencies should not require such reasons as a necessary condition for support.

As noted, Maxwell had independent warrant for some of the assumptions of kinetic theory. But suppose he didn't have. Suppose that he had simply appropriated the four kinetic theory assumptions comprising T_i from Clausius and others, and could offer no independent warrant for any of them. He then proposes to consider the hypothesis (p) that the force between molecules is zero except at impact. On the proposal of the present section, this hypothesis could still be worth considering. There is no requirement that the assumptions in the initial set have independent warrant. We must keep in mind, however, that the condition for "worth considering" is relativized to the assumptions in the initial set: p is (or is not) worth considering, given T_i. Relative to some other set the verdict may be quite different. But this just prompts the question: what about the initial set itself? Are these hypotheses worth considering? On the present account they could be even in the absence of independent warrant for any of them. How could this be so? Here is one possible scenario.

Maxwell begins with certain background information about known regularities exhibited by gases—regularities concerning pressure, viscosity, heat transfer, and so forth. On the basis of these he makes the supposition

> *S*: There is some set of (relatively simple, unified) hypotheses about gases that can explain, or can be further developed to explain, the variety of observed regularities associated with gases.

S generates the question

> *Q*: What set of (simple, unified) hypotheses about gases can explain, or can be further developed to explain, the variety of observed regularities associated with gases?

Now, in accordance with the present proposal, given the supposition S, the set T_i containing kinetic theory assumptions can be worth considering. T_i answers question Q generated by S. And, in the extreme case, even in the absence of any independent warrant for T_i, it may do so in a way satisfying appropriate instructions for Q. For example, the context may be one in which no other simple, unifying theory has as yet been proposed, or in which any others proposed (for example, caloric theory) have devastating objections, although T_i does not. To be sure, there may be independent warrant for the supposition S. But since S makes no assumptions about the content of the set of simple, unifying hypotheses, there need be no independent warrant for theory T_i. In the absence of independent warrant for any of the assumptions of kinetic theory, those assumptions can still be worth considering.

7. Implications for Hypothetico-Deductivism and Retroduction. Hypothetico-deductivism is correct in one important respect. In order to consider a hypothesis, to take it seriously, one does not need any reason to think it is true or probable. In the context of discovery there need be no inference from any data to the truth or probability of a hypothesis before one attempts to construct derivations from that hypothesis to testable conclusions. However, to say this much one need not adopt some of the other tenets of the h-d position. Hypothetico-deductivists seem eager to avoid making restrictions on which hypothesis it is worth considering (with the possible exception that it provide an answer to a question being raised). Their restrictions are saved for the context of justification in which the hypothesis is being tested. Here I part company with them. Even if one does not demand a reason to believe each hypothesis being considered, there are constraints to be imposed on which hypotheses to consider. The "context of discovery" is not irrational or arational or one in which "anything goes". There may be good reasons for considering a given hypothesis, or for considering hypothesis 1 before hypothesis 2. Maxwell had very good reasons for considering the hypothesis that gas molecules exert contact forces, even if he had no reason for supposing this true or probable.

Similarly, I reject that version of retroduction which requires at least some reason to think each of the assumptions of one's theory to be true before one draws new conclusions from the theory and begins empirical testing. Instead I would support that version which requires only reasons for considering h (which need not be reasons for thinking h true). However, I reject the retroductive position that makes the requirement that h if true would correctly explain some observations we have made either a necessary or a sufficient condition for h's being worth considering. A hypothesis h may not be worth considering even when h, if true, would correctly explain O (for example, the Nobel hypothesis). And a hypothesis may be worth considering even when if true it would not correctly explain any observed data we have. The hypothesis may be incompatible with our data (as in the case of Maxwell's viscosity hypothesis). Or we may as yet have no observational data which that hypothesis could explain (as in the case of Maxwell's independence assumption about components of molecular velocity). In the latter case the hypothesis was worth considering even though it did not explain observational data Maxwell had, but because it enabled him to derive, in a fairly simple way, a quantitative answer to the question "How are velocities distributed among particles in a gas?" This question is generated by the initial assumptions of his theory. It is one that Maxwell was particularly interested in pursuing, both for its own sake and for enabling the kinetic theory to be given further mathematical development.

REFERENCES

Achinstein, P. (1983), *The Nature of Explanation*. New York: Oxford.

———. (1985), "The Method of Hypothesis: What Is It Supposed to Do and Can It Do It?", in P. Achinstein and O. Hannaway (eds.), *Observation, Experiment and Hypothesis in Modern Physical Science*. Cambridge, Massachusetts: MIT, pp. 127–145.

———. (1986), "Theoretical Derivations", *Studies in History and Philosophy of Science, 17*: 375–414.

Feynman, R. (1965), *The Character of Physical Law*. Cambridge, Massachusetts: MIT.

Hanson, N. R. (1958), *Patterns of Discovery*. Cambridge, England: Cambridge.

———. (1983), "The Logic of Discovery", in P. Achinstein (ed.), *The Concept of Evidence*. Oxford: Oxford University Press, pp. 53–62.

Hesse, M. (1974), *The Structure of Scientific Inference*. Berkeley: University of California Press.

Joule, J. (1965), "On Matter, Living Force, and Heat", in S. G. Brush (ed.), *Kinetic Theory*, Vol. I. Oxford: Pergamon Press, pp. 78–88.

Maxwell, J. C. (1965), *Scientific Papers*. New York: Dover.

———. (1875), *Theory of Heat*. London: Longmans, Green, and Co.

McLaughlin, R. (1982), "Invention and Appraisal", in R. McLaughlin (ed.), *What? Where? When? Why?* Dordrecht: Reidel, pp. 69–100.

Peirce, C. (1960), *Collected Papers*. Edited by C. Hartshorne and P. Weiss. Cambridge, Massachusetts: Harvard.