

## WHAT TO DO IF YOU WANT TO DEFEND A THEORY YOU CANNOT PROVE: A METHOD OF “PHYSICAL SPECULATION”\*

In 1875 the theoretical physicist James Clerk Maxwell published a paper in *Nature* entitled “On the Dynamical Evidence of the Molecular Constitution of Bodies.”<sup>1</sup> In it he argues for the existence of molecules and for various claims of the molecular theory that he and others had been developing, including that molecules satisfy dynamical principles of classical physics. He does so without any experimental proof for his fundamental claims. Since he regards this as contrary to a prominent methodological view that the defense of a theory requires experimental proof, at the outset he announces that he will employ a different scientific method. It is designed for developing and defending theories that postulate objects that, at the time, cannot be observed, and that make claims about such objects that, at the time, cannot be demonstrated to be true by observation and experiment.

Two questions are of special interest to Maxwell. First, can you use the method to develop and defend a theory about unobservables in a way that can make it possible to be justified in believing the theory (or at least the set of its central and distinctive assumptions) to be true, without being able to experimentally prove that it is true?<sup>2</sup> Second, can you do so in a way that is sufficiently precise and complete to answer a range of questions about the unobservables postulated, even if you have no epistemic warrant for some of these answers? Maxwell gives an affirmative answer to both questions. He refers to his method as a “method of physical speculation.” He takes it to be different from an inductive method of a sort espoused by Newton and Mill, which he regards as too demanding for his purposes, and from “the method of hypothesis” (or hypothetico-deductivism), which he deems too weak.

\*For important criticisms and suggestions, I am indebted to Linda Suzanne Brown, Victor DiFate, Richard Richards, and the editors of this JOURNAL.

<sup>1</sup> Reprinted in *The Scientific Papers of James Clerk Maxwell*, W.D. Niven, ed. (Dover, 1965), Volume 2, pp. 418–38.

<sup>2</sup> Maxwell, as well as other authors to be considered here (including Newton, Mill, and Whewell), were scientific realists who believed that in principle empirical arguments could be given that prove the existence of unobservables. Accordingly, my discussion will be conducted within a realist framework. (For my own defense of scientific realism, see “Is There a Valid Experimental Argument for Scientific Realism?” this JOURNAL, XCIX, 9 (September 2002): 470–95.) However, the issue to be treated here does not depend on what attitude one takes about realism. Anti-realists can substitute some version of “saving the phenomena” for “truth,” and then raise Maxwell’s question by asking how to defend a theory when you cannot prove that it “saves the phenomena.”

Maxwell gives only a very brief general description of his method, leaving his readers the task of understanding what it is from seeing how he actually employs it in defending his molecular theory. Whether it is worthy of being called a “method” at all, or just a general strategy, or something else, I believe that it is important for philosophers to consider. It is, indeed, different from standard scientific methods advocated not only in the nineteenth century but today as well, including hypothetico-deductivism, inductivism, and Inference to the Best Explanation (IBE). It is a method that many scientists (whether knowingly or not) have employed in developing and defending a theory they could not prove. Maxwell’s position is not that following the method will necessarily yield truth, or justified belief, or even a theory worth considering, but that it can do so if the development and defense are sufficiently good, and that it is a reasonable and useful strategy to follow when experimental proof is not available. I propose to formulate and illustrate the method, see how it differs from the others noted above, and explore and defend its virtues. I begin with a characterization of the contrasting methods.

#### I. CONTRASTING METHODS OF DEFENSE

*I.1. The Newton-Mill-Whewell Tradition of Proof.* Within the empiricist tradition of the sort with which Maxwell was familiar, especially in the works of Newton, Mill, and Whewell, is the view that one defends a scientific theory by attempting to prove that it is true. For these writers “proving” a theory in empirical science consists in giving arguments involving appeals to experiments and observations that allow one to conclude, beyond reasonable doubt, that the theory is true. Inductivists such as Newton and Mill advocate doing so by offering causal-inductive arguments from experiments and observations to universal causal laws. According to Newton, one constructs such arguments on the basis of his four “Rules for the Study of Natural Philosophy.” The first two of these rules allow one to infer a single cause from the same type of observed effects; and the third and fourth rules allow one to infer the truth of an inductive generalization that such a cause operates within the entire class of phenomena in question.<sup>3</sup> Newton speaks of propositions derived using these rules as being “deduced from the phenomena and made general by induction,” and he regards them as having “the highest evidence a proposition can have in this [experimental] philosophy.”<sup>4</sup>

<sup>3</sup>Newton, *The Principia: Mathematical Principles of Natural Philosophy*, I. Bernard Cohen and Anne Whitman, trans. (Berkeley: California UP, 1999), pp. 794–96. For a discussion of these rules see my *Particles and Waves* (New York: Oxford, 1991), chapter 2.

<sup>4</sup>From a letter to Cotes in 1713. Reprinted in H.S. Thayer, ed., *Newton’s Philosophy of Nature* (New York: Hafner, 1953), p. 6.

For Mill, using his “Four Methods of Experimental Inquiry,” we vary circumstances under which phenomena of one type follow those of another, and by doing so we can determine whether causation exists and how general it is.<sup>5</sup> As in the case of Newton, such causal-inductive arguments should establish these laws with as much certainty as is possible in empirical science. Mill speaks of arguments of these sorts as providing “proof” of the propositions. Indeed, in his initial definition of “induction” he defines it as “the operation of discovering and proving general propositions.” In cases typical in the theoretical sciences where effects are explained by reference to multiple causes, Mill introduces his “deductive method,” which requires three steps in order to infer the truth of a theoretical system: causal-inductive generalizations from observations to a set of causal laws comprising the system; “ratiocination,” which involves inferences showing how this set, if true, can explain and predict various observable effects; and verification of new effects predicted. Only if these three steps are followed, and not simply the last two, can one infer the truth of the theoretical system and regard it as proved.

By contrast, Whewell, who rejects the inductive methodology of Newton and Mill, advocates a robust form of IBE. If the universal causal laws in question not only explain the phenomena used to generate them, but explain and predict phenomena of types different from those that generated the laws to begin with, then Whewell says there is a “consilience of inductions,” and we have no basis for any reasonable doubt.<sup>6</sup> If this continues over time as new phenomena are discovered, and does so in such a way that the theory is simple and coherent, then one can infer with the highest possible certainty that the theory is true.

Newton, Mill, and Whewell do recognize that propositions are introduced into science without proof. Newton calls them “hypotheses,” and although in the *Principia* he claims that they “have no place in experimental philosophy,” he does in fact employ them, clearly labeling them as such. Overall his view seems to be that you can introduce them and consider their implications, but you are not justified in inferring that they are true, even if the implications are experimentally verified, since conflicting hypotheses may be equally successful. Mill has a very similar view. According to Whewell, if the theory explains known

<sup>5</sup>John Stuart Mill, *A System of Logic* (London: Longmans, 1872, 8<sup>th</sup> ed.), Book III, chapter 8.

<sup>6</sup>William Whewell, *The Philosophy of the Inductive Sciences, Founded upon Their History* (London: Parker, 1840; reprinted London: Routledge/Thoemmes, 1996), Volume 2, chapter 5, section 10.

phenomena and predicts new ones only of the same type, you can conclude that the theory is “valuable,” or even (so far at least) “verified” by positive instances. But this is not sufficient for proof, which is what Whewell seeks in “testing,” since such theories often turn out to be false.

For purposes of contrast with Maxwell, then, I shall understand these “proof-demanding” writers to be claiming (a) that it is one of the principal aims of scientists to provide empirical proof of a theory; (b) that scientists are justified in believing a theory only if they have such proof; and (c) that merely showing that observations constitute positive instances of the theory, or are entailed or explained by it, or even (following contemporary Bayesians) that the probability of the theory is increased by these observations, is not sufficient for proof. While Maxwell agrees with (a) and (c) he rejects (b).

*I.2. The Method of Hypothesis.* What can you do to defend a theory in the absence of experimental proof? One standard approach is to employ some version of the “method of hypothesis.”

Maxwell writes:

The method which has been for the most part employed in conducting such inquiries 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 someone else does not invent another hypothesis which agrees still better with the phenomena.<sup>7</sup>

Maxwell rejects this “method of hypothesis” on grounds that apply to even more sophisticated versions, namely, that its users have no empirical basis from which to generate their hypotheses. Because of this, either they leave “their ideas vague and therefore useless,” or else they engage in an “illegitimate use of the imagination.” By the former Maxwell means that one thing users of the method of hypothesis sometimes do is invent very general hypotheses that are not sufficiently precise or developed to be tested. By the latter he means that the fact that hypotheses accommodate the phenomena by itself constitutes insufficient empirical warrant for those hypotheses, since there may be other conflicting hypotheses that accommodate the phenomena at least as well if not better.<sup>8</sup> Even if Maxwell were to agree (which he does not)

<sup>7</sup> *The Scientific Papers of James Clerk Maxwell*, Volume 2, p. 419.

<sup>8</sup> This is implied in the passage quoted above, and even more explicitly in Maxwell’s book *Matter and Motion* (New York: Dover, first published 1877), p. 122. The “method of hypothesis” that Maxwell is here rejecting is much more basic than the more sophisticated IBE espoused by Whewell. The latter requires not only agreement with “actual phenomena” (presumably observed phenomena), but also with predicted ones, espe-

that the type of “verification” claimed by the method of hypothesis provided some support for a hypothesis, it cannot provide enough to justify a belief in the hypothesis. Maxwell wants a method that can do the latter when proof by “methodized experiment and strict demonstration” is not available. He also seeks a method that will enable one to provide a set of hypotheses that are not “vague and therefore useless.” This is the entering point for the “method of physical speculation.”

## II. WHAT IS MAXWELL’S METHOD?

In very general terms, it is a method, or strategy, or procedure to be used when developing and defending a theory about “unobservables,” when whatever experimental evidence exists is not sufficient to establish the theory. It is a method designed for, or at least particularly appropriate for, theories in which the “unobservables” comprise a micro-system of which some observable macro-system is claimed to be composed, and in which the claim is that the behavior of the micro-system causes or determines that of the macro-system. In what follows I will offer a general characterization of the method that goes well beyond what Maxwell himself provides. In doing so I will distinguish four components and illustrate each by reference to what Maxwell actually does in his “physical speculations” about molecules.

*Component 1: Independent Warrant.* First, whatever reasons one can offer should be given in favor of the existence of the postulated unobservables that determine macro-behavior, in favor of the central and distinctive principles introduced, and in favor of supposing that such principles are applicable to these unobservables. Such reasons can be of different sorts and may include: (a) appeals to experimental results and observations, arrived at independently of the theory in question and usually from other domains; these may provide a causal-inductive or an analogical basis for supposing that the macro-system is composed of some type of unobservables that produce some of the observed behavior of the macro-system;<sup>9</sup> (b) a methodological appeal

---

cially ones of a type different from those used to generate the hypothesis in the first place; and it requires simplicity and coherence over time in the face of new observations. However, Maxwell, like Mill before him, would find these (additional) Whewellian criteria neither necessary nor sufficient justification for an inference to the truth of a hypothesis. Maxwell never mentions Whewellian “predictivity” or “coherence” as being required by his “method of physical speculation.” And while he cites the ability of the kinetic theory to explain a variety of different observed facts as counting in favor of that theory, this by itself is not sufficient for him to conclude that the theory is true.

<sup>9</sup>In his 1855 paper “On Faraday’s Lines of Force,” Maxwell defends a “method of physical analogy” for dealing with “electrical science” so as to produce a “simplification

to the “fundamental” character and simplicity of the principles being applied to those unobservables; and (c) an inductively based appeal to the success of these principles in other domains when applied to objects with the same or similar properties as those attributed to the unobservables. The reasons offered may vary in their strength, but they are not of the form “if we make these assumptions then we can explain and predict such and such phenomena,” and they are not sufficiently strong to prove that the theory is true. In other writings I have said that such reasons supply “independent warrant.”<sup>10</sup> They provide some epistemic reasons for believing the hypotheses in question that are independent of the explanatory and predictive power of the assumptions.

Maxwell seeks to develop and defend a general molecular theory of gases and liquids that governs, relates, and interprets properties and phenomena such as pressure, volume, temperature, density, specific heat, and diffusion. In accordance with the “method of physical speculation,” the first thing he wants to provide are some reasons for making the molecular assumptions he does, including, most importantly, the assumption that bodies are composed of molecules, and that these satisfy classical principles of dynamics. He offers three different sorts of reasons.

A reason he proposes for assuming that bodies are composed of molecules of the sort postulated is that “whatever may be our ultimate conclusions as to molecules and atoms, we have experimental proof that bodies may be divided into parts so small that we cannot perceive them” and that by “particle” he means a small, possibly unobservable, part of a body, not some ultimate or indivisible “atom.” In his 1875 paper Maxwell does not say what such “experimental proof” is, but it is likely that he is thinking of various claims, made in his book *Theory of Heat* (first published in 1871), starting with the idea that it has been experimentally established that heat is not a substance (caloric) but a form of energy.<sup>11</sup> The energy of a body, he continues in that book, is either kinetic energy due to motion, or potential energy due to the

---

and reduction” of the known laws of electricity and magnetism. He constructs an analogy between the electromagnetic field and a purely imaginary incompressible fluid. The analogy enables him to offer a physical representation of the field while at the same time avoiding speculations about unobservable constituents of the field. This is very different from Maxwell’s later “method of physical speculation,” which insists on such speculations, and allows analogical arguments if they provide some warrant for them. For extended discussions of these issues, see my *Particles and Waves*.

<sup>10</sup> See my *Particles and Waves*.

<sup>11</sup> By the mid-nineteenth century, experiments conducted by Rumford and Davy at the end of the eighteenth century, showing both that if caloric exists it must be weightless and that mechanical work can produce an indefinite quantity of heat, were

body's position with respect to other bodies. But, he claims, heat cannot be the latter, because the presence of another body is not necessary for heat radiation. So it is due to motion, but not that of the body as a whole, since a body radiates heat even when stationary. He concludes, "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."<sup>12</sup>

Maxwell offers two sorts of reasons for applying dynamical principles to the postulated set of unobservables. The first, which is empirical, is that such principles have been successful in astronomy and electrical science. Maxwell does not explicitly draw the inductive inference from this that such methods will therefore be successful for the kinds of phenomena he is concerned with. But this does seem implicit in his thought. The second involves claims that are methodological or conceptual. One is that, on his view, and that of most nineteenth century physicists, dynamical explanations of phenomena are complete so that no further explanations are "necessary, desirable, or possible." Another is at least an implicit appeal to simplicity, when he says that "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."<sup>13</sup> Here the idea is that the basic molecular assumptions he is and will be making will satisfy a standard of simplicity by explaining macro-systems composed of bodies in terms of micro-systems composed of bodies, and so introduce no new ontological category.

Having presented some reasons in support of the assumption that gases and liquids are composed of molecules and that they are subject

considered decisive against the caloric theory. Also, experiments by Joule in the 1840s on heat produced by the friction of bodies established a quantitative relationship between mechanical work and heat. Maxwell is thinking of the latter when near the beginning of his book he writes: "Such evidence [as to the nature of heat] is furnished by experiments on friction, in which mechanical work, instead of being transmitted from one part of a machine to another, is apparently lost, while at the same time, and in the same place, heat is generated, the amount of heat being in an exact proportion to the amount of work lost. We have, therefore, reason to believe that heat is of the same nature as mechanical work, that is, it is one of the forms of Energy"—*Theory of Heat* (London: Longmans, 10<sup>th</sup> ed.), p. 7.

<sup>12</sup> *Theory of Heat*, pp. 311–12.

<sup>13</sup> *The Scientific Papers of James Clerk Maxwell*, Volume 2, p. 420.

to dynamical principles, Maxwell proceeds by formulating a virial equation derived by Clausius from classical mechanics as applied to a system of particles constrained to move in a limited region of space, and whose velocities can fluctuate within certain limits. The equation relates the pressure and volume of a gas or fluid to the total kinetic energy of the system of particles of which it is composed, the forces of attraction or repulsion between the particles, and the distances between them. Maxwell writes the equation as follows:

$$pV = 2/3T - 2/3\Sigma\Sigma(1/2Rr).^{14}$$

In using this equation, Maxwell and Clausius are *assuming* that gases and fluids are composed of unobservable particles; this is not something which is proved by proving the equation itself. (In his discussion Maxwell uses the term “particle” and “molecule” interchangeably.) Since the equation is derived from classical mechanics, support for which comes from observations of the behavior of observable bodies, he is also supposing that such observations provide some independent warrant for the claim that if the postulated particles exist they satisfy the virial equation as well. (As we will see next in considering the second part of Maxwell’s method, he uses this equation to explain and give molecular interpretations of known gaseous phenomena.)

None of the facts Maxwell cites as independent warrant, separately or together, *establishes* Maxwell’s initial hypothesis that gases are systems of particles or molecules satisfying the Clausius virial equation. But they do constitute at least some reason in favor of such a hypothesis. In Maxwell’s own terms, such a hypothesis cannot “be derided as mere guess-work.”

*Component 2: Derivations and Explanations of Known Phenomena.* Second, the macro-system’s known properties, laws, and experimentally established deviations from these laws should be explained by invoking properties of, and principles governing, the unobservables that comprise the postulated micro-system. More specifically, known properties

<sup>14</sup>The quantity on the left represents the pressure of the gas or fluid multiplied by the volume of its container, and can be directly measured;  $T$  is the kinetic energy of the total system of particles;  $R$  is the force of attraction or repulsion between two particles; and  $r$  is the distance between two particles. The quantity  $1/2Rr$  Clausius calls the virial of the attraction or repulsion. The sum is double since the virial for each pair of particles must be determined and then the entire sum of these is taken. Clausius’s paper was published in German in 1870, with an English translation in *Philosophical Magazine*, XL (1870): 122–27. The latter is reprinted in Stephen G. Brush, *Kinetic Theory*, Volume 1 (Oxford: Pergamon, 1965), pp. 172–78. The general theorem yields the result that the mean value of the kinetic energy of such a system of material particles equals the mean value of the virial. In the special case of a gas, considered to be composed of such material particles, where the gas is acted on by an external pressure  $p$  and confined to a volume  $V$ , the theorem can be expressed in the form Maxwell gives it above.



of the macro-system should be characterized as determined by, or identical with, certain properties attributed to the micro-system. And laws governing the macro-system, and deviations from them, should be derived from assumptions regarding the micro-system defended in the first component. If they are so derived, then whatever justification for the assumptions is claimed can be claimed not just on the basis of the independent warrant but also on the basis of known laws and phenomena that are derivable from those assumptions.<sup>15</sup>

Maxwell considers a gas with observable properties of temperature, volume, pressure, specific heat, and so on, which is subject to known laws and known deviations from them, and explains these properties, laws, and deviations using known properties and laws governing dynamical systems in general involving bodies in motion. Employing Clausius's virial equation, and assuming that the pressure and volume of a gas are simply the pressure and volume of the postulated molecular system and that the temperature of the gas is proportional to the mean kinetic energy of the molecules, Maxwell derives Boyle's law for gases; and using the virial equation he explains why known deviations from the law occur at low temperatures and high densities (see Appendix). He considers the derivation of Boyle's law, and of deviations from that, to count in favor of the theory, even to provide at least some reason (or part thereof) for thinking the theory is true. But this is so only if there is some independent warrant for basic assumptions in the theory.

*Component 3: Theoretical Development.* The postulation of the set of unobservables satisfying the properties and principles introduced will suggest a range of questions about what properties and principles in addition to those introduced in components 1 and 2 these unobservables satisfy. To the extent possible, the theorist should attempt to develop the theory further by formulating and answering these questions. Doing so will usually require the introduction of new theoretical assumptions about the unobservables for which there may or may not be independent warrant, and derivations of new results that may or may not be testable by known means. Judging from the amount of time Maxwell devotes to it, this "theoretical development" of the theory—which can go well beyond what is contained in the two components above—is a crucial part of Maxwell's idea. Its focus is on providing more and more information about the postulated micro-system,

<sup>15</sup> A formal probabilistic representation of this idea is as follows. Let  $h$  be a hypothesis or set of hypotheses, let  $i$  be the independent warrant for  $h$ , and let  $e$  describe a set of known laws and other phenomena derived from  $h$ . Then  $p(h/i \& e) \geq p(h/i)$ . So if  $h$ 's probability on the independent warrant is high it will remain at least as high on additional data  $e$  if  $e$  is derived from  $h$ . For more discussion of this probabilistic representation, see my *Particles and Waves*.

whether or not this yields testable predictions and explanations of properties of the macro-system.

Maxwell introduces a series of questions about the unobservable molecules he postulates, including these: What is the mean distance traveled by a molecule before striking another molecule (the mean free path)? What is the motion of molecules after collision? Are all directions of rebound equally likely? What is the distribution of molecular velocities? He introduces various new assumptions which enable him to answer these and many other questions.<sup>16</sup> In the case of the last question, Maxwell derives a distribution law, now bearing his name, which relates the number of molecules with velocities between given limits to the total number of molecules in the sample of gas and to the velocities themselves. In doing so he makes various new assumptions about molecules, including that molecular components of velocity in different directions are independent, and that the fraction of molecules in a unit volume does not depend on their direction but only on their speeds. He had no way of experimentally verifying these assumptions, or experimentally determining any of the quantities in the law, and hence no way of experimentally verifying the law.<sup>17</sup> It is a “purely theoretical” conclusion.<sup>18</sup>

*Component 4: Unsolved Problems.* In addition to formulating, defending, and developing the theory in accordance with the three points noted above, problems with the theory should be noted. These can include a reference to known laws and properties of the macro-system that have not yet been explained, as well as to experimental results that are not in accord with certain consequences of the theory. This, of course, is not a way of defending the theory. But it is a way of suggesting aspects of the theory that need further development, and of defending the “theorist” by showing that he is aware of these aspects.

Maxwell derives some conclusions from his theoretical assumptions that are contradicted by experiments. The most important of these he considers to be a derivation (first done in 1860) of the ratio of the spe-

<sup>16</sup>In an earlier 1860 paper on kinetic theory, Maxwell sets out all of these questions as tasks to perform, and making various new assumptions he derives a set of “propositions” that provide answers to them.

<sup>17</sup>Direct experimental tests of Maxwell’s law became possible only in the twentieth century with molecular beam experiments. In the 1875 paper under discussion Maxwell gives only a qualitative account of the law, which he had formally derived in his 1860 paper. For an account of that derivation, see my *Particles and Waves*, pp. 171–73.

<sup>18</sup>Another derived conclusion is one that gives actual numerical values for mean velocities of molecules of oxygen, nitrogen, and hydrogen at 0 degrees centigrade. Although these are derived using experimentally measurable values for volume, pressure, and mass of a gas, these velocity values could not themselves be checked experimentally by Maxwell.

cific heat of a gas at constant pressure to its specific heat at constant volume. According to theoretical calculations, in the best case, assuming that molecules are mere material points incapable of rotation, the ratio is 1.66, whereas the observed value is 1.4. This difference Maxwell considers “too great for any real gas.” And if we suppose that molecules can vibrate, so that there are at least six degrees of freedom, the theoretical calculation of specific heat ratios will be a maximum of 1.33, which is too small for hydrogen, oxygen, nitrogen, and several other gases. Maxwell says that he considers this “to be the greatest difficulty yet encountered by the molecular theory.”<sup>19</sup> In addition to this problem Maxwell mentions several properties of gases, including electrical ones, that neither he nor anyone else had explained in molecular terms.

### III. WHAT CAN ONE CONCLUDE ABOUT A THEORY DEVELOPED USING MAXWELL'S METHOD?

Let us divide the assumptions made by a theory postulating unobservables into two sorts: those for which there are independent warrant arguments, and those for which there are not. In Maxwell's method the most fundamental assumptions of the theory should be of the first sort. If they are, and if the arguments supplied are sufficiently strong, then one can claim to be justified in believing them to be true, even if the assumptions postulate unobservables, and even if the assumptions cannot at the time be proved experimentally. And if a range of observed phenomena is explained by derivation from these assumptions, then justification for the assumptions can be claimed not just on the basis of the independent warrant but on the basis of the explained phenomena as well.<sup>20</sup>

The assumptions for which no independent warrant is given are ones for which conditional claims are usually made: if we assume such and such then we can derive the following result, which may or may not be testable. If it is not testable, then we certainly cannot conclude that we are justified in believing the assumptions leading to that result, or the result itself (for example, Maxwell's assumptions leading to his distribution law). If the result is testable and determined to be true but there is no warrant for the assumptions, then, since Maxwell explicitly rejects the method of hypothesis, he will not conclude that one is justified in believing the explanatory assumptions. What, then, can one conclude about such assumptions?

For Maxwell, nothing epistemic. Yet an important way of defending a theory is by showing how it can be developed theoretically. According to Maxwell this involves formulating assumptions precisely, often

<sup>19</sup> *The Scientific Papers of James Clerk Maxwell* (1875), Volume 2, p. 433.

<sup>20</sup> See note 15.

mathematically; adding new theoretical assumptions about the unobservables postulated in response to questions about the properties and behavior of those entities; and deriving consequences. Frequently in such a development new theoretical assumptions are introduced for which no independent warrant is given, and theoretical consequences are drawn that are not testable. In response to questions he posed regarding molecular velocities, Maxwell developed his theory by adding (un-argued for) assumptions about the independence of component molecular velocities, leading to a derivation of his (untestable) molecular distribution law. In doing so he did not provide any new or increased epistemic reason to believe his general molecular assumptions or the specific ones needed for the derivation. Nor is such theoretical development what some have called an “aesthetic” criterion of goodness that adds beauty or simplicity to the theory. (A particular theoretical development may be quite complex and un-beautiful.<sup>21</sup>) Nor is a theoretical development of this sort engaged in simply to show that the theory is “worthy of pursuit.” In telling us much more about the entities and properties introduced than is done in central assumptions, its purpose is to add some measure of completeness to the theory by answering a range of questions that might be prompted by considering the fundamental assumptions, and to do so with precision. Completeness and precision are nonepistemic virtues Maxwell regards as valuable for their own sake, and not just for leading to conditional explanations and predictions of phenomena (if they even do so), or just for leading to tests of the theory (again if they do), or just for providing reasons to pursue the theory.<sup>22</sup> Without a theoretical development, he suggests, the basic assumptions are “vague,” in the sense of being underdeveloped and imprecise.

Accordingly, in using “the method of physical speculation” one may be able to conclude that a theory is defensible both epistemically and nonepistemically. It has the epistemic virtue that its fundamental

<sup>21</sup> This was Duhem’s criticism of Kelvin’s theoretical development of the wave theory of light in the latter’s *Baltimore Lectures* (reprinted in Robert H. Kargon and Peter Achinstein, eds., *Kelvin’s Baltimore Lectures and Modern Theoretical Physics* (Cambridge: MIT, 1987)). Kelvin developed the theory by proposing conflicting theoretical models of the ether to interpret various optical phenomena. He developed the theory by answering a series of questions about the structure of the ether, but the development lacked coherence.

<sup>22</sup> It would be misleading to say that for Maxwell, the “theoretical development” constitutes simply what some philosophers have called a “logic of pursuit.” Maxwell’s aim is to employ a method that can be used to show both epistemic and nonepistemic virtues of a theory without proving it. He wants more than simply giving reasons for pursuing the theory or taking it seriously. He wants reasons for believing it to be true, and for concluding that it is a good theory. The “theoretical development” may provide part of one’s reasons to pursue a theory, but so will the other components of the method; and, as I have emphasized, that is not the *raison d’être* of this component.

assumptions and perhaps others have independent warrant; and, depending on the strength of this warrant, and on the known phenomena derived from them, this may be enough for one to be justified in believing those assumptions. It has the nonepistemic virtue of being developed with some measure of completeness and precision.

#### IV. IS THIS REALLY A NEW METHOD?

Is Maxwell correct in claiming that there are genuine differences between his method and more standard ones mentioned earlier? It is clearly different from the “method of hypothesis,” as formulated by Maxwell, since the latter, unlike the former, requires no independent warrant at all for its hypotheses. As a result, unless it can be shown that any competing system is less probable, the most that one can conclude from the fact that the hypotheses explain or predict observational facts is that these hypotheses are “possible,” or even “confirmed” or “verified” by the facts, but not that these facts justify believing that the hypotheses are true.

There are two important differences between Maxwell’s method and those of Newton, Mill, and Whewell. One pertains to “theoretical development.” To be sure, the methods of Newton, Mill, and Whewell involve producing derivations of observable phenomena from the basic assumptions. And Whewell, like Maxwell, emphasizes the idea of developing a theory over time by adding new assumptions in response to phenomena not yet explained. However, Maxwell is also concerned, very importantly, with developing new theoretical assumptions about the unobservable entities postulated, *whether or not those assumptions are actually employed in explaining observable phenomena or are even capable of being verified at the time*. And, unlike Whewell’s idea of “coherence,” which is an epistemic criterion supposed to guarantee the highest measure of justified belief, Maxwell’s “theoretical development” idea does not guarantee any measure of justified belief but nevertheless contributes to a defense of the theory by exhibiting nonepistemic virtues of the theory.

A second difference between Maxwell’s method and those of Newton, Mill, and Whewell is that the latter, but not the former, are based on the idea that inference to the truth of a scientific proposition or theory requires proof, which these methods are designed by their proponents to enable scientists to provide. Newton and Mill draw a sharp distinction between proof and possibility. Whewell recognizes that there are situations in which you have less than proof (which requires “consilience”) but more than mere possibility—for example, when your hypothesis predicts as well as explains phenomena of the same type as those prompting the hypothesis in the first place. By contrast, Maxwell’s method is based on the idea that although proof is always

desirable, a range of situations exists in which you have less than proof and more than possibility, or Whewellian success in explaining and predicting phenomena of the sort that prompted the theory. In such situations, depending on the strength of the independent warrant and of the explanations offered, you may be able to infer that your theory (or at least its set of fundamental assumptions) is true, while at the same time recognizing that more theoretical development and experimental support are needed and that unsolved problems remain.

Is there a difference between Maxwell's method and those of Newton, Mill, and Whewell over the *types* of epistemic arguments that can be employed in defense of a theory? Maxwell is clearly denying Whewell's claim that "consilience" is sufficient for inference; independent warrant—warrant other than the explanatory and predictive success of the theory—is also necessary. This is something with which Newton and Mill would agree. (It is Mill's first step in his "deductive method.") The relevant difference here is over the strength of the arguments required, not over types. Unlike Newton and Mill, Maxwell has in mind cases in which none of the arguments, individually or collectively, suffice to prove the assumptions. Why do they fail to do so?

Recall just the two empirical arguments Maxwell gives for assuming that molecules exist and that they obey laws of dynamics. The first is a causal-eliminative argument from the theory of heat (in his 1871 book), which starts with the claim that experiments show that heat is a form of energy, not a substance; then it moves to the claim that it must be kinetic energy rather than potential, since observations show that heat radiation does not depend on relative positions of bodies (which potential energy does); then, since hot bodies do not necessarily exhibit observable motion, it concludes that the motion must be that of parts of the body too small to observe, parts Maxwell will call molecules. The argument is certainly not decisive, since it makes assumptions that could be, and indeed were, questioned, for example, that energy of motion requires bodies in motion—an assumption denied by "energeticists" who rejected molecular theory, such as Ostwald later in the nineteenth century.<sup>23</sup>

The second empirical argument Maxwell offers is one for supposing that molecules in motion obey laws of dynamics. The argument is simply an inductive generalization from the fact that such laws have been successful in astronomy and electrical science. In the absence of

<sup>23</sup> Friedrich Wilhelm Ostwald, "Emancipation from Scientific Materialism," (1895), reprinted in Mary Jo Nye, ed., *The Question of the Atom* (Los Angeles: Tomash, 1986), pp. 337–54. This essay contrasts with Ostwald's later 1908 conversion to atomic theory, which will be discussed in section vi below.

conflicting information, although this gives some reason for supposing these laws hold for domains both large and small, it is by no means decisive, since the phenomena in the domains cited are so different; and, of course, its conclusion was abandoned in the twentieth century with the advent of quantum mechanics.

What Maxwell is saying is that despite the lack of certainty in such cases, we provide what empirical and methodological arguments we can. Furthermore, he is saying, we do not need to base our belief in the assumptions of a theory on such “independent warrant” arguments alone but on these together with the fact that the assumptions can be used to explain known laws and deviations. And he is saying that a theory can be defended not only on epistemic grounds, but on non-epistemic ones as well, including the precision and completeness of its theoretical development.

#### V. A MAXWELLIAN BELIEF STATE; EPISTEMIC IMPLICATIONS AND OBJECTIONS

Despite the lack of proof, Maxwell’s own belief state with regard to his kinetic-molecular theory was a quite confident one, which might be characterized as follows: (1) he believed that molecules exist and that the independently warranted dynamical assumptions about them were true; (2) he believed that he was justified in so believing; (3) he believed that neither he nor anyone else had sufficient experimental evidence to demonstrate that the assumptions he was making in the theory are true. Claims (1) and (2) about Maxwell can be supported by examining many of his published and unpublished writings in the 1870s, and not just the 1875 paper in question.<sup>24</sup> Claim (3) is clearly made in his 1875 paper.<sup>25</sup>

<sup>24</sup> Here is a passage from an 1875 article he wrote on atoms for the *Encyclopedia Britannica*: “Having thus justified the hypothesis that a gas consists of molecules in motion, which act on each other only when they come very close together during an encounter, but which, during the intervals between their encounters which constitute the greater part of their existence, are describing free paths, and are not acted on by any molecular force, we proceed to investigate such a system.” This contrasts with Maxwell’s much more skeptical epistemic state around 1860 during the time of his first kinetic theory paper. In that period Maxwell took his theory to be, as he described it, “an exercise.” Writing to Stokes in 1859, he says, “I do not know how far such speculations may be found to agree with facts, ..., and at any rate as I found myself able and willing to deduce the laws of motion of systems of particles acting on each other only by impact, I have done so as an exercise in mechanics. Now do you think there is any so complete a refutation of this theory of gases as would make it absurd to investigate it further so as to found arguments upon measurements of strictly ‘molecular’ quantities before we know whether there be any molecules?” (Elizabeth Garber, S. Brush, and C. Everitt, eds., *Maxwell on Molecules and Gases* (Cambridge: MIT, 1986), p. 279).

<sup>25</sup> *The Scientific Papers of James Clerk Maxwell*, Volume II, p. 420. In an 1873 lecture entitled “Molecules,” Maxwell divides various claims of molecular theory into “three ranks,”

Let us call a belief state of the sort Maxwell was in (one satisfying (1)–(3) above) a “confident but less than perfect one” with respect to a hypothesis  $h$  (which I will abbreviate as CLP( $h$ )). Now admittedly one can be in such a state without being justified in believing  $h$ . But my claim is that one can also be in such a state and be justified in believing that  $h$  is true. Suppose I own 85% of the tickets in a fair lottery, one ticket of which will be drawn at random, and I believe that I will win because I own 85% of the tickets. I am justified in believing this even if I have not proved or demonstrated that I will win. Or suppose that I am a detective trying to solve a crime, and that I have a good deal of information that suspect number 1 is the perpetrator: the motive, means, and opportunity all fit, as do the descriptions of some witnesses. On the basis of these facts I come to believe that this suspect is guilty—even though, let us say, not all the evidence fits exactly, and even though I need more direct and positive evidence for a court of law. In the sort of case I am imagining I am justified in believing what I do, even if I cannot yet prove it. In relevant respects, in 1875 Maxwell’s belief state with regard to molecular theory was analogous to these.

Now for some objections.

*Objection 1.* In the lottery and detective cases, as well as in Maxwell’s case, we need to distinguish what justification a person offers for his belief, on the one hand, from whether his belief is really justified, since (the opponent might say) in these cases the person in question does not really have sufficient evidence to be justified in his belief. One has this only if the justification is sufficient for knowledge. Although CLP( $h$ )-states are possible, and someone in such a state may offer a justification for believing  $h$ , this is not sufficient for knowledge that  $h$  is true. Such a position has in fact been taken in epistemology by Jonathan Sutton, who distinguishes a “loose” and a “strict” sense of justification.<sup>26</sup> In a case such as my lottery example, he argues that although in a “loose” sense (which is used colloquially and is championed by most epistemologists) I am justified in believing that I will win, in a “strict” sense I am not, because I do not know that I will win. In a strict sense I am justified in believing only that I will *probably* win.

---

which vary with degree of certainty and completeness of the knowledge about the molecular assumptions. In the first rank are assumptions about relative molecular masses and velocities, while in the third (yielding what he calls only “probable conjecture”) are assumptions about absolute molecular masses and diameters (see Volume II, p. 371).

<sup>26</sup>Jonathan Sutton, *Without Justification* (Cambridge: MIT, 2007).



*Reply.* To do justice to Sutton's position one would need to carefully examine each of the epistemic arguments and advantages he offers for employing his "strict" sense of justification. Elsewhere I have drawn a distinction somewhat similar to his between "veridical" and "nonveridical" senses of expressions such as "good reason to believe," "evidence," and "sign or symptom of," in which the veridical sense requires the truth of the hypothesis in question, the nonveridical does not.<sup>27</sup> For purposes of this paper it suffices to say the claims about justified belief in the lottery, detective, and Maxwellian cases are being made in my "nonveridical" sense. They can also be made in Sutton's "loose" sense, which does not require either truth or knowledge of the truth.<sup>28</sup> Maxwell's general epistemic position with respect to molecular theory fits both descriptions. It is one that other theoretical scientists are frequently in.<sup>29</sup>

*Objection 2.* As has been noted, even in 1875 Maxwell recognized problems with the theory, including theoretical derivations of specific heat values that were incompatible with observed values, and the inability of the theory to explain various known properties of gases. Since there were such problems and since they caused Maxwell to have some doubts concerning the theory, how could Maxwell believe the theory, let alone have confidence in his beliefs?

<sup>27</sup> *The Book of Evidence* (New York: Oxford, 2001). The distinction I draw is not exactly Sutton's, since for my "veridical" sense it is required that the hypothesis (which is justified, for which there is evidence, and so on) be true, not that anyone know this, which is what Sutton requires in addition to truth.

<sup>28</sup> If we do the latter, then we would reformulate these claims by saying that ("strictly speaking") in the lottery, detective, and Maxwellian cases one is justified in believing that the claims made are probably true. And we would reformulate Maxwell's aim in employing his method of physical speculation to be one of showing how one can develop and defend a theory without experimental proof by showing that one is justified in believing it is probably true.

<sup>29</sup> Although in the *Principia* Isaac Newton demands "deductions from the phenomena," which require the satisfaction of his four methodological rules, and, he believes, yield the kind of certainty he seeks, in several Queries in the *Opticks* he gives arguments in favor of the particle theory of light and against the competing wave theory that by his own admission do not furnish such certainty. Yet Newton believed that light consists of unobservable particles, and pretty clearly he believed he was justified in so believing. For example, Query 29 begins as follows: "Are not the Rays of Light very small Bodies emitted from shining Substances. For such Bodies will pass through uniform Mediums in right Lines without bending into the Shadow, which is the Nature of the Rays of Light." (Newton continues to show how various known properties of light can be explained on the particle theory.) Newton is giving arguments for the particle theory that put him in the same epistemic position that Maxwell was in with respect to molecular theory. These arguments, he thought, justify his belief in the particle theory without giving decisive proof.

Although I will not argue for it here, I think that quite similar things can be said about Darwin's reasoning in *The Origin of Species* for natural selection as the mechanism for evolution. Darwin believed his hypotheses, believed he was justified in so believing, and yet explicitly recognized that he lacked decisive proof.

*Reply.* Maxwell did not doubt that the theory, in its essentials, is true. He doubted that all the assumptions he was making were true, without being able to point to specific ones as being particularly dubious. And he believed that the theory had not yet been sufficiently developed to deal with the inconsistencies or with unanswered questions concerning electrical and certain other known properties of gases. But doubts of these sorts were not enough to shake his confidence that the fundamental ideas are correct and that these problems would be worked out.

*Objection 3.* Maxwell's 1875 paper is entitled "On the Dynamical Evidence of the Molecular Constitution of Bodies." Evidence need not provide proof. It need not even provide reasons sufficient for belief. All it has to do is supply some reasons for increasing one's degree of belief in the theory. And that is all that Maxwell was in fact doing, or was justified in claiming to do.

*Reply.* This objection presupposes an increase-in-degree-of-belief (or probability) position on evidence, which I have criticized elsewhere,<sup>30</sup> and will not pursue here. Suffice it to say that, on Maxwell's view, one can give evidence that is strong enough to justify belief, and goes beyond simply increasing one's degree of belief, without giving proof; and that is precisely what he was trying to do in his 1875 paper and in other writings during this period.

#### VI. TWO "DECISIVE" EXPERIMENTS

Given that Maxwell's "evidence" for his molecular assumptions was not decisive, what sort of evidence would be? For proof of the existence of molecules Maxwell was not demanding experiments making molecules "directly observable," but only experimental results from which their existence and properties could be inferred with certainty. Let me begin by noting two different experimental results that were regarded as decisive not only by many who were believers already but by at least some initially skeptical scientists as well. The arguments presented, one for the existence of molecules, the other for electrons, were of the same general type. I want to ask what such arguments possessed that Maxwell's did not that, at least in some cases, made believers out of skeptics. Neither of the experimental results I will mention made the postulated unobservables "observable"; nor did they need to do so to be decisive.

For an initially skeptical scientist I choose Friedrich Wilhelm Ostwald, Professor of Physical Chemistry at the University of Leipzig, and winner of the Nobel Prize in chemistry in 1909. In 1896 Ostwald published a paper entitled "Emancipation from Scientific Materialism,"<sup>31</sup> in which

<sup>30</sup> *The Book of Evidence.*

<sup>31</sup> See note 23 above.

he lays out his fundamental objections to atomism.<sup>32</sup> For our purposes here the most important is his claim that no way had yet been found to experimentally measure, with any degree of certainty, quantities associated with atoms or molecules; this measurement criterion Ostwald took to be necessary and sufficient for proving the existence of a postulated unobserved entity.

However, in 1908 Ostwald was converted by experiments of two physicists: those of Jean Perrin in 1908 on Brownian motion, which Ostwald claims, “justify the most cautious scientists in now speaking of the experimental proof of the atomic nature of matter”; and by experiments of J.J. Thomson (in the mid-1890s) on the counting of gas ions and (in 1897) on cathode rays, leading to Thomson’s discovery of the electron. In other writings I have discussed both sets of experiments at length.<sup>33</sup> Here I will very briefly outline the arguments based on experiments of Perrin for the existence of molecules in order to indicate first, the type of reasoning involved; second, why the argument was so convincing to Ostwald and to many (though not all) scientists; and third, how it differed from Maxwell’s “evidence” for molecular theory.

Perrin’s argument contains two stages. In the first, he offers a general qualitative causal-eliminative argument from experiments on Brownian motion, the haphazard motion exhibited by small microscopic particles suspended in a liquid. In the 1880s various experiments had been performed, principally by Leon Gouy, to determine whether this observed Brownian motion was caused by forces external or internal to the fluid in which the motion occurred. Gouy examined a range of possible external causes. When these were reduced or eliminated, the Brownian motion continued unabated. Perrin concluded that the motion of the observable Brownian particles is caused internally by their bombardment with unobservable molecules comprising the fluid.<sup>34</sup>

The second stage of Perrin’s argument invoked experimental results that completely convinced Ostwald of the existence of molecules. (The first stage by itself Ostwald would probably have regarded as no more convincing than Maxwell’s “independent warrant” arguments.) In 1908 Perrin conducted a series of experiments in which he prepared tiny (“Brownian”) particles visible though a microscope, each of which had the same mass and density, and inserted them into a cylinder containing a dilute liquid of known density and temperature. He derived an equation containing terms for the number of microscopic Brownian particles per unit volume at the upper and lower levels of the cylinder,

<sup>32</sup> For a discussion of these objections, see my “Atom’s Empirical Eve: Methodological Disputes and How to Evaluate Them,” *Perspectives on Science*, xv (2007): 359–90.

<sup>33</sup> See *Particles and Waves* and *The Book of Evidence*.

<sup>34</sup> Jean Perrin, *Atoms* (Woodbridge, CT: Ox Bow, 1990).

the mass and density of the particles, the density of the liquid, the height of the cylinder, the temperature of the liquid, and, most importantly, Avogadro's number  $N$  (the number of molecules in a gram molecular weight of a substance). All of these quantities except for the latter were experimentally measurable. When Perrin performed various experiments with different types of microscopic particles and different fluids he determined that Avogadro's number was the same in all cases, approximately  $6 \times 10^{23}$ . He concluded from this that molecules exist.<sup>35</sup>

J.J. Thomson's argument for the existence of electrons (or "corpuscles," as he called them) is in important respects parallel to Perrin's arguments for molecules.<sup>36</sup>

In the first stage of their arguments both physicists cite experiments yielding results that purport to show the existence of the object postulated without providing any measurements of the object's properties. Their arguments are of a causal-eliminative type. They begin with an observed phenomenon: Brownian motion, in the case of Perrin, and cathode rays, in the case of Thomson, which, it is claimed, given the background information, is likely to have one of several different types of causes that are specified. Then it is asserted that experiments make it very probable that all but one of these causes is eliminated, leaving the hypothesis which postulates the unobservable entity in question as the probable cause of the phenomenon. In the second stage of the argument experiments yielding other effects of the inferred entity are cited, from which certain magnitudes associated with these causes are derived and experimentally measured to be approximately the same in various different types of experiments performed. In neither case did the experimenters make the entities inferred "observable."

It is not my claim that arguments containing both of these stages are in general necessary to decisively establish claims of the existence of unobservable entities, but only that in the cases in question there were arguments of these types, and they did in fact convince not only the experimenters themselves, but at least some initial skeptics. Nor is it my claim that all arguments of these types are in fact decisive (whether or not they are deemed to be so), since it depends on how well the premises, or other assumptions implicitly made, are themselves established.<sup>37</sup>

What is the difference between these cases in which experiments for the postulated entities are decisive, or considered to be so, and

<sup>35</sup> Perrin, *ibid.*, p. 105. Once an experimental value for Avogadro's number is obtained, then an experimental value for a weight in grams of a given molecule is determined by dividing this by the gram molecular weight.

<sup>36</sup> See *Particles and Waves*, chapters 10–11.

<sup>37</sup> Although the arguments of Thomson and Perrin were considered decisive by many, there were some skeptics. For example, in Thomson's case, there were some

Maxwell's empirical arguments for molecules, which he himself did not characterize as decisive? In all three cases appeals are made to experiments, and in all three cases numerical values associated with the postulated entities are given. The difference is in the strength of the empirical arguments. Maxwell's causal-eliminative argument for the existence of molecules from experiments on heat makes it probable that bodies contain "parts too small to be observed separately," which Maxwell called molecules. But it did not make it probable enough to be decisive since it did not decisively preclude other possible causes of heat phenomena. Nor was his inductive argument decisive from the success of dynamical principles in other domains to their applicability to the inferred molecules, since, as he notes, such principles have been successfully applied only to macro-bodies. And although Maxwell gives some theoretical estimates for various molecular speeds, he had no experimental way to verify these calculations. Accordingly, in the 1870s, although Maxwell had some empirical arguments for the existence of molecules and for assumptions he was making about them, even if these arguments provided a reasonable basis for believing the theory true, they were by no means conclusive, or regarded by Maxwell as such.

Had he lived, then, what Maxwell might have said in response to Ostwald's late conversion to the molecular theory in 1908 is this: True, in the 1870s "methodized experiments" that provide a "strict demonstration" of molecular theory did not exist. Nevertheless, in accordance with the "method of physical speculation," epistemic arguments were given that furnished a reasonable basis for believing the central assumptions of the theory, and in addition nonepistemic ones were presented showing how the theory can be developed theoretically. One does not need "strict demonstration" to accomplish these purposes.

PETER ACHINSTEIN

Yeshiva University  
Johns Hopkins University

---

who questioned whether (in the first stage of the argument) Thomson was right in supposing that the most probable cause of electrical deflection in cathode tubes was the passage of charged particles with (classical) mass. (George FitzGerald, for example, thought that it was at least as probable that they were "aetherial" free charges.) In the second stage of the argument in which Thomson experimentally determines a constant mass-to-charge ratio, some questions might be raised, since the experimental values Thomson actually obtained had a significant range in both sets of experiments he performed to obtain this ratio. For these and other issues pertaining to the decisiveness of Thomson's argument, see George E. Smith, "J.J. Thomson and the Electron, 1897–1899," in Jed Z. Buchwald and Andrew Warwick, eds., *Histories of the Electron* (Cambridge: MIT, 2001), pp. 21–76.

## APPENDIX: AN EXAMPLE OF MAXWELL'S EXPLANATORY STRATEGY

Examining the Clausius equation given in section II, we may conclude that the pressure of a gas depends on the kinetic energy  $T$  of the system of molecules, which is due to the motion of the molecules, and to the quantity  $1/2Rr$ , which depends on the forces between them. Maxwell now argues that the pressure of a gas cannot be explained by assuming repulsive forces between molecules. He shows that if it were due to repulsion then the pressure of a gas with the same density but in different containers would be greater in a larger container than in a smaller one, and greater in the open air than in any container, which is contrary to what is observed. If we suppose that the molecules of the gas do not exert any forces on each other, then the Clausius virial equation reduces to  $pV = 2/3T$ . Then since  $T$  is the kinetic energy of the system of particles, where  $M$  is the mass of the gas, that is, the mass of the system of molecules, and since  $T = 1/2Mc^2$ , where  $c$  is the mean velocity of a molecule, Maxwell derives the equation  $pV = 1/3Mc^2$ . The latter is Boyle's law, on the assumption that the temperature of a gas is proportional to the mean kinetic energy of the molecules.

Now, continues Maxwell, it is known that real gases deviate from Boyle's law at low temperatures and high densities. And he asks whether the second factor in Clausius's equation dealing with forces between molecules, which was ignored in deriving Boyle's law, can be invoked to explain actual deviations from that law found in experiments. These experiments show that as the density of a gas increases its pressure is less than that given by Boyle's law. Hence, the forces between the molecules must on the whole be attractive, rather than repulsive. In the virial equation this is represented by a positive virial. Experiments also show that as the pressure of a gas is increased, it reaches a state in which a very large increase in pressure produces a very small increase in density, so that the forces between molecules are now mainly repulsive.<sup>38</sup>

<sup>38</sup> In the paper Maxwell goes on to give mechanical interpretations, analyses, explanations, and calculations involving other gaseous phenomena, using probabilistic ideas first introduced by Clausius and developed by Maxwell and Boltzmann. For example, he derives a theoretical estimation of the speeds of molecules. In a highly rarefied gas, as noted, where the second term in the Clausius equation can be ignored, we can derive  $pV = 1/2Mc^2$  in which  $p$  (pressure),  $V$  (volume), and  $M$  (mass of the gas) are measurable quantities, and  $c$  = mean velocity of a gas molecule. Maxwell proceeds to give some calculations: at 0 degrees centigrade, the average velocity of a molecule of oxygen is 461 meters per second; that of nitrogen is 492, and of hydrogen 1844. Of course, these are theoretical calculations, not experimental results.