Mechanical Explanation at the End of the Nineteenth Century

by

MARTIN J. KLEIN*

1. The last quarter of the nineteenth century has the reputation of being one of the least exciting periods in the history of scientific thought. Alfred North Whitehead[1] was expressing the generally received view when he characterized this period as "an age of successful scientific orthodoxy, undisturbed by much thought beyond the conventions." He even went so far as to call it "one of the dullest stages of thought since the time of the First Crusade," but this was going too far. As Whitehead himself pointed out, those were the years when "the adequacy of scientific materialism as a scheme of thought for the use of science was endangered." For two hundred years physical theory had had a fixed clear goal, the goal of "the true Philosophy, in which one conceives the causes of all natural effects in terms of mechanical motions."[2] Christiaan Huygens, whom I have just quoted, went on to emphasize the point: "This in my opinion, we must necessarily do, or else renounce all hopes of ever comprehending anything in Physics." Huygens' opinion was almost universally shared, and the search for mechanical explanations of natural phenomena was the accepted and largely successful program of physical theorists until the latter part of the nineteenth century.

During those supposedly stodgy years this traditional goal of the physicist drew serious criticism. Some of the sharpest thinkers of the period questioned not only its attainability but even its desirability and its legitimacy. It was a time of probing and testing, a time when questions of principle were much considered and often hotly debated. The closing decades of the last century did not bring the kind of exciting new ideas that typify the quarter century that followed, but it was an extremely interesting period, nevertheless. Part of this interest comes from the

* Department of the History of Science and Medicine, Yale University.

complexity and variety of the ideas that were current then. Just because of the difficulties in continuing to develop the mechanical explanation of nature and the criticisms raised against such attempts, the mechanists were forced to re-examine and refine their own positions. It is no accident that the most intense polemics against the mechanical program are contemporaneous with the most elaborate, subtle and ambitious efforts to provide mechanical explanations for all of nature, or at least all of inanimate nature.

When discussing a period where the issues are complicated and the points of view are diverse—and for what period would this not be the case?—one must try to avoid the temptations of two simple ways of dealing with the problems. It will not do just to compile a kind of catalogue in which all the various positions that physicists adopted on the methodological issues are stated and grouped into schools of thought. Such compilations are rarely interesting or illuminating and may well be misleading. For as Albert Einstein once wrote: "If you want to find out anything from the theoretical physicists about the methods they use, I advise you to stick closely to one principle: don't listen to their words, fix your attention on their deeds."[3] Nor will it serve to construct a schematic and oversimplified account that purports to give the essence of the thought of the period by omitting its complexities. "There is not an essence of history," to quote Herbert Butterfield,[4] "that can be got by evaporating . . . the incidental or momentary or local things, and the circumstantial elements, as though at the bottom of the well there were something absolute, some truth independent of time and circumstance." All we can do is try to select in such a way that we suggest the richness of the material from which the selection is made, to deal with the "minute particulars" of which historic truth is composed. It is certainly appropriate in this place to remember how Niels Bohr always insisted, with Schiller, that one must concentrate one's maximum force on the smallest point, "im kleinsten Punkte die höchste Kraft."

The point on which I shall concentrate my efforts is the mechanical analogy for the second law of thermodynamics that Hermann von Helmholtz introduced in 1884. Helmholtz's monocyclic systems are of some interest in themselves, but they are of considerably more interest for their impact on Helmholtz's two most active readers—Ludwig Boltzmann and Heinrich Hertz. An analysis of the very different ways in which these individual physicists used this particular class of mechanical

systems to understand phenomena as diverse as the limited convertibility of heat into work and electromagnetic induction should suggest something about the actual status of mechanism at the end of the nineteenth century. It is also no accident that Helmholtz referred to his work as providing only an "analogy" for the second law, and I shall try to explore the significance of that term as an indicator of the changing fortunes of the mechanical world view.

2. By the 1860's the program of mechanical explanation had made enormous progress. Much had been done since Newton gave a superb "example" of the use of rational mechanics in investigating natural forces by explaining the "System of the World", and expressed his wish that "we could derive the rest of the phenomena of Nature by the same kind of reasoning from mechanical principles."[5] Mechanics itself had developed into an extremely powerful and flexible mathematical theory. With its expanded and refined concepts and methods it could now deal with rigid bodies, elastic solids and fluids of various kinds, as well as with systems of particles. There was also a mechanical theory of heat: the new science of thermodynamics, created as such by Rudolf Clausius' memoir of 1850, incorporated the old idea that heat is motion into a conceptual system whose full generality and great scope were not exhibited until Josiah Willard Gibbs's memoirs of the 1870's. Thermodynamics was accompanied by a more detailed mechanical theory of heat, the kinetic theory of gases, which promised to provide a complete mechanical explanation of "the nature of the motion we call heat."[6]

The Newtonian program had also had much success in dealing with electric and magnetic phenomena. The laws of force for both electric charges and magnetic poles at rest were determined before the end of the eighteenth century, and were quickly developed into a mathematical theory of electrostatics. When the phenomena of electromagnetism were discovered, more elaborate laws of force for current elements and moving charges were proposed by Ampère, Weber and others. But the most fruitful electromagnetic theory was James Clerk Maxwell's dynamical theory of the electromagnetic field. Maxwell gave up forces acting at a distance in favor of Michael Faraday's local action in a medium, but he viewed his theory as being essentially dynamical, just as its name implied.

The mechanical theories were not uniformly successful. The kinetic

theory of gases, for example, was beset by difficulties from its very beginning. Maxwell pointed to one of these in his first paper on the subject, when he deduced the value of the ratio of the specific heats of a gas from his theory. "By establishing a necessary relation between the motion of translation and rotation of all particles not spherical," he wrote, "we proved that a system of such particles could not possibly satisfy the known relation between the two specific heats of all gases."[7] This incompatibility between the equipartition theorem and experiment turned out to be an irremovable difficulty, "the greatest difficulty yet encountered by the molecular theory,"[8] as Maxwell later wrote, and one that kinetic theorists long went on hoping was "not necessarily a fatal objection to the theory."[9]

Maxwell's theory of the electromagnetic field provided other difficulties for the mechanical world picture. Maxwell had arrived at the view that light is an electromagnetic wave with the help of a detailed and rather complicated mechanical model, but he never claimed that this model faithfully represented reality. In the later and more complete version of his theory the model disappeared completely, although Maxwell continued to treat the electromagnetic field by the methods of analytical dynamics. His rather subtle views on the nature of mechanical explanation, to which we shall return, puzzled most of his contemporaries. Some (like Lord Kelvin) thought he had not gone far enough in constructing mechanical models; others (like Pierre Duhem) drew back in dismay from the model Maxwell had used, and tried to manage without any mechanical assistance at all.[10] All agreed on one point: the relationship between Maxwell's theory and the old goal of mechanical explanation was not clear.

It was the second law of thermodynamics, however, that proved to be the first insurmountable obstacle for the developing mechanical theories of nature. In 1866 Ludwig Boltzmann began his scientific career with a memoir in which he claimed to have found a "purely analytical, completely general proof" of the second law which reduced it to a theorem in mechanics.[11] Boltzmann's claims were too strong. His mechanical theorem allowed one to define the entropy in terms of mechanical concepts, but only when the system was strictly periodic, an unreasonably severe limitation for anything as complicated as a sample of gas. Even within this limitation Boltzmann had not done anything to explain the irreversibility required by the second law.

In the course of the next few years Boltzmann studied Maxwell's papers on the kinetic theory of gases. He learned from Maxwell that it was essential to describe a gas by statistical methods, that the statistical distribution of molecular velocities was basic to any analysis of the properties of a macroscopic system. Boltzmann made this approach his own, generalizing it and applying it to new problems.[12] Foremost among these was the problem he had started with, and in 1871 Boltzmann gave a new "analytical proof" of the second law.[13] This time the second law, or rather those aspects of the second law that deal with equilibrium and reversible processes, appeared as theorems in a new and as yet unnamed discipline—statistical mechanics. The laws of probability played as essential a part as the laws of mechanics in the new explanation of the second law of thermodynamics.

Although Boltzmann was the one to develop this idea and create the basic structure of statistical mechanics, Maxwell had already seen to the heart of the matter several years earlier. The reason why Maxwell created his often misunderstood demon, that "doorkeeper very intelligent and exceedingly quick, with microscopic eyes," was precisely to point this moral: "The second law of thermodynamics has the same degree of truth as the statement that if you throw a tumblerful of water into the sea, you cannot get the same tumblerful of water out again."[14] Maxwell made the issue even clearer in his supposedly elementary textbook, *Theory of Heat.*

> "If heat in a body consists in a motion of its parts, and if we were able to distinguish these parts, and to guide and control their motions by any kind of mechanism, then by arranging our apparatus so as to lay hold of every moving part of the body, we could, by a suitable train of mechanism, transfer the energy of the moving parts of the heated body to any other body in the form of ordinary motion. The heated body would thus be rendered perfectly cold, and all its thermal energy would be converted into the visible motion of some other body.
>
> Now this supposition involves a direct contradiction to the second law of thermodynamics, but is consistent with the first law. The second law is therefore equivalent to a denial of our power to perform the operation just described, either by a train of mechanism, or by any other method yet discovered. Hence, if the heat consists in the motion of its parts, the separate parts which move must be so small or so impalpable that we cannot in any way lay hold of them to stop them."[15]

Since, in other words, the second law denies the possibility of certain processes that are perfectly consistent with the laws of mechanics, it

must be expressing some essentially nonmechanical aspect of nature. An explanation of the second law cannot be purely mechanical but must also refer to the smallness of the molecules or, equivalently, to their enormous numbers. Statistical description of large systems of molecules is necessary, and not merely convenient, if one is to understand the basis for the second law of thermodynamics.

Maxwell never developed these ideas into a statistical mechanics, leaving the field open for Boltzmann. It was Boltzmann who showed how irreversible behavior could be explained and who obtained an expression for the entropy in terms of the molecular distribution function.[16] Under the pressure of Josef Loschmidt's criticism of his H-theorem of 1872, Boltzmann[17] constructed a fully statistical explanation of the second law, in which irreversibilty was to be understood as the normal evolution of a system into the most probable state, that is, the most probable molecular distribution allowed by its circumstances.

Boltzmann reached this fully statistical interpretation of the second law of thermodynamics in 1877.[18] He evidently believed that the problem was settled, that he had explained the essential features of the second law, and he turned his attention to other matters. His later discussions of this problem, in the 90's, were undertaken only in response to new criticisms, and always consisted of elaborations and more careful restatements of his statistical point of view.[19] There is an exception to this general statement, however, and it brings us finally to Helmholtz and his monocycles.[20] When Boltzmann read Helmholtz's first papers on monocyclic systems in the spring of 1884, he immediately set to work to explore this apparently very different approach to the meaning of the second law. To understand Boltzmann's response we must first look at what Helmholtz had done.[21]

3. Helmholtz's first major work, in 1847, argued for the universal validity of the law of conservation of energy:[22] heat was to be considered as mechanical energy on the molecular scale. He actually proclaimed the mechanical philosophy in its strongest form, asserting that all natural phenomena were to be "referred back to motions of material particles," and that these particles attracted or repelled one another by central forces depending only on their mutual distances. After giving this completely mechanical version of the first law of thermodynamics Helmholtz devoted himself to other branches of science—almost all of them,

it sometimes seems—for the next thirty five years. In 1882 he returned to problems involving the transformation of energy from one form to another and wrote several papers applying thermodynamics to chemical and electro-chemical phenomena. It was apparently in connection with these researches that Helmholtz became actively interested once again in the mechanical foundations of thermodynamics.[23] This time the entropy law rather than the energy law drew his attention.

Helmholtz presumably knew of Boltzmann's statistical interpretation of entropy and the second law, although he did not refer to it explicitly in the series of papers he began to publish early in 1884. His introductory remarks made clear his awareness of the use of statistical descriptions for systems containing many particles, but they also made clear his choice of another approach to the second law. Helmholtz had been studying the properties of a particular class of mechanical systems. These were systems, describable by Lagrange's equations of motion, whose generalized coordinates could be separated into two distinct classes. Coordinates of the first type were cyclic; that is to say, these coordinates did not themselves appear in the Lagrangian function of the system. Their time derivatives, the generalized velocities, did appear in the kinetic energy and so in the equations of motion. These velocities were to be considered as large, so large in fact that the velocities associated with the second class of coordinates could be neglected by comparison. The coordinates of this second class, then, did appear in the Lagrangian, but their time derivatives did not. When only one independent cyclic coordinate was present, Helmholtz called the system monocyclic. But why consider such systems at all, and what did they have to do with thermodynamics?

Helmholtz had a definite analogy in mind. Thermodynamic systems are fundamentally complicated; a sample of gas in a container, for example, is composed of an extremely large number of molecules in rapid irregular motion. The locations of the molecules in the container at any particular time, the molecular coordinates, have no effect (to a first approximation) on the thermodynamic properties of the gas. The molecular velocities, however, determine the molecular kinetic energy, which is proportional to the temperature. The volume of the gas, on the other hand, is a coordinate or parameter of the system that is normally varied at a rate negligibly slow compared to molecular speeds. The two quantities whose interplay constitutes the essence of thermodynamics,

heat and work, are the energy changes of the system produced by changes in the molecular velocities and in the slowly varying parameters (like the volume), respectively.

Now, if one has a mechanical device, like a wheel, rotating rapidly about a fixed axis, its energy depends only on the angular velocity of the wheel and not on its instantaneous orientation. In this respect the rotating wheel is analogous to the molecular motions in a gas. One can imagine a device, like a governor, attached to the axle in such a way that the energy of the system also depends on the coordinates of certain masses, coordinates that can be varied at a rate which is slow compared to the speed of rotation. Helmholtz's idea was that such a purely mechanical system, monocyclic in the sense described before, might provide an analogy for the complicated systems of thermodynamics, not only with respect to the classification of its coordinates but also with respect to the relationships between heat and work. The essential point was to show that energy provided to the system as heat (that is, as a change in the kinetic energy of the cyclic coordinate) could not be completely converted into work (that is, into a slow change in the auxiliary parameters, the second class of coordinates referred to above). In other words Helmholtz had to show that the analogue of the differential heat had an integrating factor proportional to the kinetic energy of the system.

To see how this goes in the simplest possible case let L be the Lagrangian of the system. Lagrange's equations have the form,

$$\frac{d}{dt}\frac{\partial L}{\partial \dot{q}} - \frac{\partial L}{\partial q} = F \tag{1}$$

where q is a generalized coordinate, $\dot{q}$ is the corresponding generalized velocity, and F is the external generalized force acting on this coordinate. For the single cyclic coordinate q_a of our monocyclic system one has, by definition,

$$\frac{\partial L}{\partial q_a} = 0 \tag{2}$$

so that

$$F_a = p_a \tag{3}$$

where p_a is the corresponding generalized momentum,

$$p_a = \frac{\partial L}{\partial \dot{q}_a} \tag{4}$$

For the remaining coordinates, q_b, which are slowly varying, one can drop the terms in $(\partial L/\partial \dot{q}_b)$ and write simply

$$F_b = -\frac{\partial L}{\partial q_b} \tag{5}$$

for these coordinates. The kinetic energy T of the system can be written simply as

$$T = \frac{1}{2}\dot{q}_a p_a \tag{6}$$

and the differential of the total energy U can easily be put in the form

$$dU = \dot{q}_a dp_a + \sum_b F_b dq_b \ . \tag{7}$$

The first term is the energy change produced by a change in the velocity of the cyclic coordinate, and is identified with the heat dQ added to the system. The second term is the work done on the system by a change in the slowly varying parameters, q_b. One can now write dQ in the form

$$dQ = \dot{q}_a dp_a = T\, d\, \ln(p_a)^2 \tag{8}$$

with the help of equation (6). We see that dQ is the product of the kinetic energy and the differential of a state function, $\ln(p_a)^2$. If kinetic energy is identified with temperature, then the state function $\ln(p_a)^2$ will be the analogue of the entropy of the system.

Helmholtz carried the analysis a great deal further, treating more general monocyclic systems and showing how two such systems can be coupled in much the same way that thermodynamic systems are coupled isothermally. In other words, he developed a rather complete mechanical analogy for the laws of thermodynamics. Helmholtz was quite aware of the fact that real thermodynamic systems are not monocyclic, and that he was giving a mechanical *analogy* for thermodynamics and not a mechanical *explanation* of thermodynamics.

He made this point emphatically in response to a criticism from Clau-

sius. The actual thermal motions in a system like a gas or a solid were unknown but certain to be complicated. "In these circumstances," Helmholtz wrote, "it seems completely rational to me to look for the most general conditions under which the most general physical characteristics of thermal motion can appear in other well known classes of motion. It is in this sense, of course, that I have called special attention to the analogies between the behavior of thermal motion and of the monocyclic motions I have investigated, but I have declared from the outset that thermal motion is not strictly monocyclic. I have, accordingly, never claimed to have given 'an explanation' of the second law of thermodynamics."[24]

4. Boltzmann, who *had* tried to give an explanation of the second law, and who was convinced that he had succeeded in giving one in terms of statistical mechanics, was, nevertheless, fascinated by Helmholtz's work. He was quite ready to accept Helmholtz's approach to the second law in parallel with his own and even to develop it further himself. Boltzmann already knew and appreciated the idea of a mechanical analogy: he had learned it from the writings of the master of this concept—James Clerk Maxwell. Maxwell had made effective use of dynamical analogies in developing his electromagnetic theory, to the bewilderment of his continental critics. We must have a look at Maxwell's analogies as an essential aspect of his view of mechanical explanation.

Maxwell proposed the use of analogies in his first paper on electromagnetism, which he began by pointing out that "the present state of electrical science seems peculiarly unfavorable to speculation."[25] Under these circumstances, Maxwell thought, one had to reject two common ways of dealing with the extensive but unstructured body of experimental results and "intricate mathematics." A "purely mathematical" approach—formal, phenomenological and free of physical content—would make one "entirely lose sight of the phenomena to be explained." The alternative—introducing a physical hypothesis about the basic nature of electricity—was decidedly premature and would make one "see the phenomena only through a medium," making one "liable to that blindness to facts and rashness in assumption which a partial explanation encourages." What he wanted was "some method of investigation which allows the mind at every step to lay hold of a clear

physical conception without being committed to any theory . . ., so that it is neither drawn aside from the subject in pursuit of analytical subtleties, nor carried beyond the truth by a favorite hypothesis."

To make it possible to think physically and concretely without having to adopt a dubious physical hypothesis, Maxwell urged the use of analogies. He meant by an analogy "that partial similarity between the laws of one science and those of another which makes each of them illustrate the other." He had in mind a definite example of such an analogy, recently demonstrated and used by William Thomson, an "allegorical representation of the case of electrified bodies by means of conductors of heat."[26] The laws of electrostatic potential theory and the laws of stationary heat conduction apply to completely different physical situations; the forces between charged particles and the flow of heat through a continuous medium have no common physical features. But because the mathematical equations of the two theories are identical in form, one can solve a problem in the theory of attractions by thinking physically about the analogous problem in the theory of heat conduction, and vice versa. Maxwell's paper exploited just such an analogy between the flow of an incompressible fluid and Faraday's lines of magnetic force. He emphasized though that however suggestive such an analogy might be, it was no substitute for "a mature theory, in which physical facts will be physically explained." The subject was not ready for such a theory in 1855. Maxwell was arguing against a premature commitment to the physical hypothesis of a force acting at a distance between moving charges, like the force proposed by Weber. He wanted to keep open the possibility of a theory based on Faraday's local action of a field of force, even though he could not yet construct such a theory.

A few years later Maxwell elaborated his analogy in the series of papers entitled, "On Physical Lines of Force".[27] He could now represent the relationships of the electromagnetic field variables with the help of an elaborate mechanical construction involving a latticework of molecular vortices in an ideal fluid, with adjacent vortices coupled by means of electrical particles acting like idle wheels. Maxwell carefully pointed out once again that all this was only an analogy. The use of particles to couple the vortices was not intended to be "a mode of connexion existing in nature,"[28] but rather only "a mode of connexion which is mechanically conceivable and easily investigated," one that would help

rather than hinder the "search after the true interpretation of the phenomena." The analogy did its work: it made it possible for Maxwell to deduce that "light consists in the transverse undulations of the same medium which is the cause of electrical and magnetic phenomena."[29]

In his full-fledged "Dynamical Theory of the Electromagnetic Field"[30] of 1865 the detailed mechanical analogy almost completely disappeared. It was mentioned only in a passing reference halfway through the long memoir. Maxwell had now dropped the specific mechanical analogy, but he was still constructing a mechanical theory. He viewed the field as "a complicated mechanism capable of a vast variety of motion," and asserted that "such a mechanism must be subject to the general laws of Dynamics."[31] Maxwell compared the old and new versions of his theory in a letter to his friend Peter Guthrie Tait, writing: "The former is built up to show that the phenomena [of electromagnetism] are such as can be explained by mechanism. The nature of the mechanism is to the true mechanism what an orrery is to the Solar System. The latter is built on Lagrange's Dynamical Equations and is not wise about vortices."[32]

Maxwell's intentions have often been misunderstood. The new approach was not the complete mechanical explanation of electromagnetism that he would have wanted to achieve. His use of Lagrange's methods allowed him to proceed without detailed knowledge of "the nature of the connexions of the parts of the system,"[33] and this was an advantage, but it did not mean that his search for a detailed mechanical explanation was abandoned. That search simply had to be postponed until the true nature of the electric current was known. Then, Maxwell wrote in his *Treatise*, we would have "the beginnings of a complete dynamical theory of electricity in which we should regard electrical action, not, as in this treatise, as a phenomenon due to an unknown cause, subject only to the general laws of dynamics, but as the results of known motions of known portions of matter, in which not only the total effects and final results, but the whole intermediate mechanism and details of the motion, are taken as the objects of study."[34]

Maxwell looked on the Lagrangian method as a way of avoiding the need to "frame a hypothesis" about the particular mechanical structure of the system under study, whether this system was the electromagnetic field or the molecules of a gas.[35] The power of the method lay in the fact that "the final equations . . . are independent of the particular form

of [the mechanical] connexions."[36] In a note published a few years after the *Treatise* Maxwell went a step further, recommending the Lagrangian method as "presenting to the mind in the clearest and most general form the fundamental principles of dynamical reasoning." He deplored the "too frequent practice" of inventing "a particular dynamical hypothesis," deducing results from it by means of the equations of motion, and then supposing the hypothesis to be supported by the validity of these results. "The true method of physical reasoning," he wrote, "is to begin with the phenomena and to deduce the forces from them by a direct application of the equations of motion."[37] This was the method he tried to pursue himself.

I doubt very much that the Newtonian echoes in these quotations are accidental. Maxwell was, after all, particularly sensitive to language. When Maxwell criticized the framing of unnecessary hypotheses, or argued that one should go from the phenomena to the forces, he could hardly have been unaware of the uses of those phrases in the General Scholium and the Preface to the *Principia*. I think he was deliberately identifying his approach with Newton's, and putting his avoidance of assumed forces like Weber's, or of special molecular models, on a par with Newton's rejection of the particular Cartesian mechanisms. Newton had been quite prepared to consider the possibility that gravity had an underlying cause, and to speculate on the possible role of the aether here, but would not incorporate such speculations into the *Principia* because he had "not been able to discover the causes of those properties of gravity from phenomena." Just so Maxwell hoped for an eventual dynamical theory of electricity where "the whole intermediate mechanism and details of the motion" would be worked out, but not until the nature of electricity and the electric current could be deduced in a manner "warranted by experimental evidence."[34]

5. Ludwig Boltzmann, that avid student of Maxwell's writings, admired and appreciated his subtle views of the various aspects of mechanical explanation.[38] This sympathetic reading of Maxwell prepared Boltzmann to accept Helmholtz's monocycle analogy for the second law in the proper spirit. He set to work on it as soon as it appeared, extending and developing Helmholtz's ideas in a number of directions.[39]

Although Helmholtz proposed a mechanical analogy, one must not imagine that he had described a particular mechanical model, like

Maxwell's vortices and idle wheels. He did not care for such constructions, and preferred the "safer" way of relying on general equations, even though they were the equations of a mechanical system. Helmholtz's analogy was, therefore, comparable rather to Maxwell's dynamical theory of the *Treatise*. It is typical of Boltzmann, however, that he began by illustrating Helmholtz's ideas with the help of several very specific mechanical examples. These examples were more than just illustrations. They allowed Boltzmann to show that the monocycle analogy was less general than Helmholtz had claimed it to be. The kinetic energy was not always an integrating factor for the differential heat in monocyclic systems, and there were even situations when no such integrating factor existed at all, cases without thermodynamic parallels.

Boltzmann explored the relationships between the monocycle analogy and his own previous work in several different ways. Although the individual systems used in the statistical approach to the second law were not monocycles, Boltzmann showed how an ensemble of such systems could exhibit monocyclic properties. Boltzmann also discussed the connection between Helmholtz's work and his own early attempt to find a purely mechanical explanation of the second law. He emphasized the value of Helmholtz's clear distinction between two types of coordinates —the slowly varying parameters of the system and the rapidly varying cyclic coordinates, analogues of the molecular coordinates. This distinction made it possible to define the crucial difference between heat and work within a mechanical theory, something that could not otherwise be done in a purely mechanical theory.[40]

The three papers Boltzmann wrote on monocyclic systems between 1884 and 1886 did not exhaust his interest in the subject. He already noted in the first of these papers that Maxwell had, in effect, used a monocyclic system when he discussed electromagnetic induction in the *Treatise*. Boltzmann developed this idea at great length in his Munich lectures on Maxwell's theory a few years later.[41] In these lectures the cyclic system becomes the central mechanical concept for the construction of a theory of electric circuits and their interactions. The reasoning is simple: when a current flows in a closed circuit, some kind of motion is taking place. When the current is a steady one, the motion must be such that whenever one particle leaves a certain point, another identical one moving with the same velocity must immediately arrive at that point, just as in the uniform rotation of a disk. In other words, the

coordinates of the electricity must be cyclic, and the energy of the the current can only depend on the corresponding velocities.

Boltzmann's *Lectures on Maxwell's Theory* demonstrates the art of constructing mechanical analogies in a very highly developed form. Paul Ehrenfest, who studied with Boltzmann, has described this aspect of his master's work, an aspect rather neglected by Boltzmann's biographers. "Boltzmann's presentation of the Maxwell theory starts with just these mechanical analogies—in contrast to all other presentations of this theory. In Boltzmann's lectures one had ample time at the beginning just to learn the amazing reactions that relatively very primitive, purely mechanical systems can carry out—reactions of a type that one would never expect from a mechanical system so long as one instinctively thinks only of the planetary system. The imagination had to be trained by studying these systems so that it could push on further and further to the construction of a mechanism, free of contradictions in all its details, which could explain the complicated properties of the luminiferous aether."[42]

"Mechanical representations," Ehrenfest wrote in another passage, "were the material from which Boltzmann preferred to fashion his creations .... He obviously derived intense aesthetic pleasure from letting his imagination play over a confusion of interrelated motions, forces and reactions until the point was reached where they could actually be grasped. This can be recognized at many points in his lectures on mechanics, on the theory of gases, and especially on electromagnetism. In lectures and seminars Boltzmann was never satisfied with just a purely schematic or analytical characterization of a mechanical model. Its structure and its motion were always pursued to the last detail. If, for example, several strings were used to illustrate certain kinematical relations, then the conceptual arrangement had to be devised in such a way that the strings would not become entangled. In those great works of his whose results always encompass an immense domain, the simple example is also given an exhaustive and loving treatment."[43]

Boltzmann's models for inductively coupled circuits show exactly what Ehrenfest was describing. He began by treating a simple device, a monocycle, which nevertheless could serve as a mechanical model for a Carnot cycle.[44] An appropriately coupled combination of three of these satisfied the equations of two coupled circuits, with self and mutual induction.[45] This did not satisfy Boltzmann, however, because the

coefficients of self and mutual induction could not be varied independently in the model, and he proceeded to redesign the system to allow for this possibility.[46] This perfected design not only could be constructed —it actually was constructed to Boltzmann's specifications at a workshop in Graz.[47] An unwary reader glancing through the pages of Boltzmann's book on electromagnetic theory and noting the diagrams might easily imagine that he had picked up a treatise on the design of engineering mechanisms by mistake. Boltzmann's *Lectures on Maxwell's Theory* can surely be paired with Kelvin's *Baltimore Lectures* as representing the High Baroque phase of the mechanical world view.

6. Helmholtz's monocyclic analogy for the second law made a powerful impression on at least one other reader in addition to Boltzmann. That was Heinrich Hertz, Helmholtz's own best-loved pupil. Hertz worked at a book on mechanics during the last three years of his tragically short life, and finished it, except for final revision and polishing, only two months before his death. This book, *The Principles of Mechanics Presented in a New Form*, appeared posthumously in 1894.[48]

Hertz had re-examined the fundamental ideas of mechanics and arrived at a new way of organizing, presenting and thinking about the subject. He wrote the whole book in the classical Euclidean style, except for a long philosophical introduction. Philosophers of science have found Hertz's work to be rich in suggestions, and have admired its logical structure.[49] Almost seventy years ago Bertrand Russell referred to its principles as being "so simple and so admirable."[50] Physicists have generally considered Hertz's system to be beautiful but valueless; treatises on dynamics rarely do much more than mention Hertz's principle of the straightest path or least curvature.

The best known feature of Hertz's system of mechanics is his elimination of force as a fundamental concept. He was dissatisfied with what he called the "logical obscurity" that surrounded the idea of force. Hertz described one result of this logical obscurity in a sentence that most teachers of mechanics would be happy to endorse warmly: "I would mention the experience that it is exceedingly difficult to expound to thoughtful hearers that very introduction to mechanics without being occasionally embarrassed, without feeling tempted now and again to apologize, without wishing to get as quickly as possible over the rudiments, and on to examples which speak for themselves. I fancy

that Newton himself must have felt this embarrassment . . ."[51] Hertz argued that force was introduced into physics because we cannot account for motion, even for such a simple motion as that of a falling stone, by using only "what can be directly observed." "If we wish to obtain an image of the universe which shall be well-rounded, complete, and conformable to law," he wrote, "we have to presuppose, behind the things which we see, other, invisible things . . . Force is such an invisible thing," as is energy, but Hertz proposed another choice.

"We may admit," he argued, "that there is a hidden something at work, and yet deny that this something belongs to a special category. We are free to assume that this hidden something is nought else than motion and mass again—motion and mass which differ from the visible ones not in themselves but in relation to us and to our usual means of perception."[52] This was to be Hertz's basic assumption—that the motions of visible masses are to be accounted for by their direct couplings to other concealed masses which are themselves in motion. Hertz's proposal was an explanation in the Cartesian tradition, a mechanics from which dynamics would be eliminated and which would consist exclusively of kinematics.[53]

Hertz was attempting to explain force as the effect of kinematical constraints linking the observed motion of the visible masses to the hidden motions of hidden masses. He took this to be a natural next step in the process of mechanical explanation, referring to the success of the kinetic theory of heat and of Maxwell's electromagnetic theory, both of which were based on hidden motions of unseen masses. And Hertz emphasized the significance of Helmholtz's studies on cyclical systems, in which he had "treated the most important form of concealed motion fully, and in a manner that admits of general application." The hypothesis of hidden motions had explained "the forces connected with heat" and at least some aspects of electromagnetic forces; Helmholtz's analysis suggested to Hertz that one could explain all forces this way.[54] Hertz might even have described his work as a mechanical explanation

One could attain a better approximation to the truth "by tracing back the supposed actions at a distance to motions in an all-pervading medium whose smallest parts are subjected to rigid connections." It was over the ground of the aether that "the decisive battle between these different fundamental assumptions of mechanics must be fought out."[55] Hertz's system was to furnish the mechanics of that electromagnetic aether whose existence his own experiments did so much to establish.

In one central passage of his book Hertz discussed the general concept of dynamical analogies or models. He took mechanical explanation to be only the construction of such dynamical analogies, and explicitly stated that "it is impossible to carry our knowledge of the connections of natural systems further than is involved in specifying models of the actual systems." Hertz saw "the relation of a dynamical model to the system of which it is regarded as the model" to be "precisely the same as the relation of the images which our mind forms of things to the things themselves."[56]

There was one real trouble with Hertz's system. He gave no examples to show how one could account for particular mechanical phenomena by means of a particular arrangement of hidden masses in motion. Worse than that—it was apparently not possible to construct such explanations for even very simple mechanical problems. Boltzmann, whose mechanical ingenuity had already been amply demonstrated, was unable to find a mechanism, of the general type allowed in Hertzian mechanics, which would give a kinematic reduction for such a simple and basic problem as the elastic collision of two spheres. He questioned, therefore, whether the theory would be of much value for physics, "in spite of all its philosophical beauty and completeness,"[57] and he did not follow Hertz's approach in his own lectures on mechanics.[58]

7. The idea that physics must make its goal the mechanical explanation of nature did not go unchallenged. These challenges multiplied and grew bolder late in the nineteenth century. Ernst Mach, certainly the most cogent of the critics of mechanism, was already writing in 1871 "that one can hold, treasure, and also turn to good account the results of modern natural science without being a supporter of the mechanical conception of nature."[59] He was, however, aware that "the view that physical phenomena can be reduced to processes of motion and equilibrium of molecules is so universally spread that, at the present time, one

can only let people know that one's convictions are opposed to it with caution, guardedly, and at the risk of rousing the opinion that one is not up to date and has not grasped the trend of modern culture."[60]

Mach developed his ideas on this subject at greater length years later in *The Science of Mechanics*. His "historical-critical" presentation of mechanics was aimed at destroying existing illusions about the nature of that science and replacing them with a sound positivist approach. He stated flatly: "The view that makes mechanics the basis of the remaining branches of physics, and explains all physical phenomena by mechanical ideas, is in our judgment a prejudice." The historical priority of mechanics was not an adequate reason for assuming its logical priority. "We have no means of knowing yet," Mach went on, "which of the physical phenomena go *deepest*, whether the mechanical phenomena are perhaps not the most superficial of all, or whether all do not go *equally deep*."[61]

A variety of antimechanists were willing to accept Mach's statements, and some were willing to go much further in their reduction of the status of mechanics. I am referring to the energeticists—Wilhelm Ostwald and Georg Helm—and their French ally, Pierre Duhem. The epic battle in which Boltzmann defended the flags of mechanism and atomism against all attacks by the energeticists is not part of my concern here. But one aspect of the energeticists' case is relevant. Shunning the idea that phenomena such as chemical reactions should be explained sooner or later with the help of the concepts of force and motion applied to atoms and molecules, they claimed the authority of Josiah Willard Gibbs in support of their point of view. Helm thought that Gibbs's writings were "free of such prejudice for the mechanics of atoms," that he had "established the strict consequences of the two laws with no hankering or yearning after mechanics," and that his work exhibited the true subject matter of theoretical science in its "naked and pure" form.[62] Before Gibbs, according to Helm, thermodynamics had never really been free of the corrupting effects of molecular hypotheses and mechanism.

Helm was wrong about both Gibbs and his predecessors. Rudolf Clausius had been scrupulously careful to separate his work on thermodynamics from his ideas on molecular science, realizing that the latter were much less likely to command general assent. Thermodynamics textbooks for almost a century preserved the form of development originally given by Clausius, in which neither molecules nor mechanics play any part.

Clausius did believe that an explanation of the laws of thermodynamics on the basis of molecular mechanics was possible, necessary and fundamental, but then so did Willard Gibbs.

There are indications of this in Gibbs's memoir on heterogeneous equilibrium, such as the discussion of what has become known as the Gibbs Paradox. While Gibbs published nothing but a very brief abstract on statistical mechanics before his book on that subject appeared in 1902, he certainly suggested his belief in the fundamental character of molecular science before then. This was done, appropriately enough, in the obituary notice of Clausius' work that Gibbs wrote in 1889, where he wrote approvingly of Clausius' interest in "the nature of the molecular phenomena of which the laws of thermodynamics are the sensible expression."[63] Gibbs's comments in this article indicate how thoroughly he had studied the writings of Boltzmann and Maxwell and how much he agreed with their approach to the subject. He was already lecturing at Yale on the deduction of the laws of thermodynamics by an application of the theory of probability to molecular mechanics in 1889–90, and gave courses along this line every few years thereafter.[64]

It is quite true that in the Preface to his *Statistical Mechanics* Gibbs was very cautious about claiming physical validity for the theory.[65] There were too many unsolved problems in the theory of radiation and other related areas, as well as in the theory of specific heats based on the equipartition theorem. For that reason Gibbs proposed the pursuit of "statistical inquiries as a branch of rational mechanics," as a way to "avoid the gravest difficulties" by "giving up the attempt to frame hypotheses concerning the constitution of material bodies." And he carefully labelled his treatment of the statistical mechanical foundations of thermodynamics as the construction of "thermodynamic analogies", using the same terminology as Maxwell and Helmholtz, and for the same reasons.

Gibbs's readiness to seek out mechanical explanations, and his intellectual distance from the energeticists, are also evident in some of his writings on optics. He must have been one of the first physicists to give a course of lectures on Maxwell's electromagnetic theory, since his courses on this subject go back to 1885 at least.[64] His optical papers written several years earlier refer to Maxwell's work.[66] Gibbs was willing, nevertheless, to discuss elastic solid theories of the luminiferous aether, even in the late 80's, although he concluded that the electrical

theory was superior just because "it is not obliged to invent hypotheses" in order to eliminate longitudinal waves.[67]

Gibbs's cautiousness and his reluctance to engage in general methodological discussions should not obscure the fact that he was, after all, squarely in the tradition of mechanical explanation.

8. That tradition did not come to a sudden end because of the attacks of the energeticists. In 1892 H. A. Lorentz started a new stage in the development of electromagnetic theory with his paper on the applications of Maxwell's theory to moving bodies. This paper contained the first version of his theory of electrons, but Lorentz began it by treating the electromagnetic field itself as a dynamical system. The success of the theory of electrons did not depend on this dynamical theory of the field, however, and Lorentz quietly dropped it. In the later versions of his theory he made no attempt to arrive at the field equations from a mechanical starting point.[68] This is typical of the way mechanical explanation ceased to be the program for the development of physics. One could try to treat the electromagnetic field as a dynamical system but it was awkward and profitless to do so. If the supposed mechanical explanation provided no new insight, and if it led to no further progress, in what sense did it provide an explanation? As Einstein put it: "One got used to operating with these fields as independent substances without finding it necessary to give oneself an account of their mechanical nature; thus mechanics as the basis of physics was being abandoned, almost unnoticeably, because its adaptability to the facts presented itself finally as hopeless."[69]

By the turn of the century it looked to many physicists, particularly the younger generation of theorists, as though the electron theory would supplant mechanism with a new electromagnetic world view.[70] Mechanical concepts like mass would then be explained by means of electromagnetic theory, rather than the other way around. Even Boltzmann, who had fought so valiantly in defense of mechanism, was ready in 1904 to give up the field to the new world view, and wished it success.[71] He felt he was surrendering to an intellectually honorable conqueror, which would not have been so if energetics had carried the day.

The mechanical world view did not disappear so quickly or so completely as had been expected. The electron theory based on particular structural hypotheses never achieved its goal. Much more valuable as a

guide was the principle of relativity which seemed to some in 1906 to be only a reactionary holdover from mechanich.[72] The need for a physics that could deal with the full range of the properties of matter, a need expressed by Pierre Duhem in the language of energetics[73], was finally met by a new mechanics. The quantum mechanics, for all its striking differences from past ideas is still, as Bohr often called it, "a rational generalization of classical physics," and physicists are still interested in determining what a very young James Clerk Maxwell called "the particular go"[74] of each phenomenon.

NOTES

1. Alfred North Whitehead, *Science and the Modern World* (New York, 1925), p. 148.
2. Christiaan Huygens, *Treatise on Light*, transl. S. P. Thompson (London, 1912), p. 3.
3. Albert Einstein, *The World As I See It* (New York, 1934), p. 30.
4. Herbert Butterfield, *The Whig Interpretation of History* (Reprinted New York, 1965), p. 68.
5. Isaac Newton, *Mathematical Principles of Natural Philosophy. A.* Motte's translation revised by F. Cajori (Berkeley, 1934), pp. xvii–xviii.
6. Rudolf Clausius, "The Nature of the Motion Which We Call Heat", *Phil. Mag. 14* (1857), 108.
7. James Clerk Maxwell, "Illustrations of the Dynamical Theory of Gases," *Phil. Mag. 20* (1860), 37. Reprinted in *The Scientific Papers of James Clerk Maxwell*, ed. W. D. Niven (Cambridge, 1890), *1*, p. 409.
8. J. C. Maxwell, "On the Dynamical Evidence of the Molecular Constitution of Bodies," *Nature 11* (1875), 357, 374. Reprinted in *Scientific Papers, 2*, p. 433.
9. Henry William Watson, *A Treatise on the Kinetic Theory of Gases*, 2nd ed. (Oxford, 1893), p. 87.
10. Pierre Duhem, *The Aim and Structure of Physical Theory*, transl. P. P. Wiener (Princeton, 1954), pp. 55–104, especially pp. 71–72.
11. Ludwig Boltzmann, "Über die mechanische Bedeutung des zweiten Hauptsatzes der Wärmetheorie," *Wiener Berichte 53* (1866), 195. Reprinted in L. Boltzmann, *Wissenschaftliche Abhandlungen*, ed. F. Hasenöhrl (Leipzig, 1909), *1*, 9–33. This collection will be referred to as Boltzmann, *Wiss. Abh.* and page references to Boltzmann's work are made to this reprint.
12. L. Boltzmann, "Studien über das Gleichgewicht der lebendigen Kraft zwischen bewegten materiellen Punkten," *Wiener Berichte 58* (1868), 517. *Wiss. Abh. 1*, 49–96.
13. L. Boltzmann, "Analytischer Beweis des zweiten Hauptsatzes der mechanischen Wärmetheorie aus den Sätzen über das Gleichgewicht der lebendigen Kraft," *Wiener Berichte 63* (1871), 712. *Wiss. Abh. 1*, 288–308.

14. J. C. Maxwell to J. W. Strutt, 6 December 1870. Reprinted in R. J. Strutt, *Life of John William Strutt, Third Baron Rayleigh* (Madison, 1968), 47. See also M. J. Klein, "Maxwell, His Demon, and the Second Law of Thermodynamics," *American Scientist 58* (1970), 84.
15. J. C. Maxwell, *Theory of Heat* (London, 1871). My quotations are from the 4th London edition of 1875, pp. 153–154.
16. L. Boltzmann, "Weitere Studien über das Wärmegleichgewicht unter Gasmolekülen," *Wiener Berichte 66* (1872), 275. *Wiss. Abh. 1*, 316–402.
17. L. Boltzmann, "Bemerkungen über einige Probleme der mechanischen Wärmetheorie," *Wiener Berichte 75* (1877), 62. *Wiss. Abh. 2*, 116–122.
18. L. Boltzmann, Über die Beziehung zwischen dem zweiten Hauptsatze der mechanischen Wärmetheorie und der Wahrscheinlichkeitsrechnung respektive den Sätzen über das Wärmegleichgewicht," *Wiener Berichte 76* (1877), 373. *Wiss. Abh. 2*, 164–223.
19. I have analyzed the development of Boltzmann's ideas in Chapter 6 of my book *Paul Ehrenfest. The Making of a Theoretical Physicist* (Amsterdam, 1970).
20. Hermann von Helmholtz, (a) "Studien zur Statik monocyklischer Systeme," *Berliner Berichte (1884)*, pp. 159, 311; (b) "Principien der Statik monocyklischer Systeme," *Journal für die reine und angewandte Mathematik 97* (1884), 111, 317.
21. In addition to the references of note 20, see also (a) J. Larmor and G. H. Bryan, "On Our Knowledge of Thermodynamics, Specially with Regard to the Second Law," *B.A.A.S. Reports 61* (1891), 96; (b) H. von Helmholtz, *Vorlesungen über Theoretische Physik, VI, Vorlesungen über Theorie der Wärme*, ed. F. Richarz (Leipzig, 1903), p. 338.
22. H. von Helmholtz, *Über die Erhaltung der Kraft* (Berlin, 1847). English translation reprinted in S. G. Brush, ed., *Kinetic Theory* (London, 1965), *1*, p. 89.
23. Leo Koenigsberger, *Hermann von Helmholtz*, transl. F. A. Welby (Reprinted New York, 1965), p. 348.
24. H. von Helmholtz, "Studien zur Statik monocyklischer Systeme III," *Berliner Berichte* (1884), 757.
25. J. C. Maxwell, "On Faraday's Lines of Force," *Trans. Cambr. Phil. Soc. 10* (1855), 27. *Scientific Papers 1*, p. 155.
26. J. C. Maxwell to W. Thomson, 13 September 1855. Reprinted in Sir Joseph Larmor, ed., *Origins of Clerk Maxwell's Electric Ideas* (Cambridge, 1937), p. 17.
27. J. C. Maxwell, "On Physical Lines of Force", *Phil. Mag. 21* (1861), 161, 281, 338; *23* (1862), 12, 85. *Scientific Papers 1*, p. 451.
28. J. C. Maxwell, *Scientific Papers 1*, p. 486.
29. *Ibid.*, p. 500.
30. J. C. Maxwell, "A Dynamical Theory of the Electromagnetic Field," *Trans. Roy. Soc. 155* (1865), 459. *Scientific Papers 1*, p. 526.
31. J. C. Maxwell, *Scientific Papers 1*, p. 533.
32. J. C. Maxwell to P. G. Tait, 23 December 1867. Quoted in C. G. Knott, *Life and Scientific Work of Peter Guthrie Tait* (Cambridge, 1911), p. 215.
33. J. C. Maxwell, *A Treatise on Electricity and Magnetism*, 3rd ed. 1891 (Reprinted New York, 1954), *2*, p. 213.

34. *Ibid.*, p. 218.
35. J. C. Maxwell, *Matter and Motion* (Reprinted New York, 1952), p. 122.
36. J. C. Maxwell, *op. cit.*, note 33, p. 200.
37. J. C. Maxwell, *Scientific Papers 2*, pp. 308–309.
38. See Boltzmann's translation of Maxwell's paper, *Über Faraday's Kraftlinien* in the Ostwald's Klassiker Series, No. 69 (Leipzig, 1895), pp. 97–102.
39. L. Boltzmann, (a) "Über die Eigenschaften monozyklischer und anderer damit verwandter Systeme," *Journal für die reine und angewandte Mathematik 98* (1884), 68; (b) "Über einige Fälle, wo die lebendige Kraft nicht integrierender Nenner des Differentials der zugeführten Energie ist," *Wiener Berichte 92* (1885), 853; (c) "Neuer Beweis eines von Helmholtz aufgestellten Theorems betreffend die Eigenschaften monozyklischer Systeme," *Göttinger Nachrichten* (1886), 209. *Wiss. Abh. 3*, pp. 122, 153, 176.
40. This point was strongly emphasized by Maxwell. Helmholtz's sharp distinction between the two classes of coordinates, and a corresponding restriction of the possible ways in which energy could be added to the system, made it possible to avoid Maxwell's criticism of any mechanical theory of the second law. For further discussion and references to Maxwell see my paper on Maxwell's demon, cited in note 14.
41. L. Boltzmann, *Vorlesungen über Maxwells Theorie der Elektricität und des Lichtes* (Leipzig, *1*, 1891, *2*, 1893).
42. Paul Ehrenfest, "Ludwig Boltzmann," *Mathematisch-Naturwissenschaftliche Blätter 3* (1906). Reprinted in Paul Ehrenfest, *Collected Scientific Papers*, ed. M. J. Klein (Amsterdam, 1959), p. 134.
43. *Ibid.* p. 135.
44. L. Boltzmann, *op. cit.*, note 41, *1*, p. 8.
45. *Ibid.*, p. 26.
46. *Ibid.*, p. 42.
47. *Ibid.*, pp. 48–49.
48. Heinrich Hertz, *The Principles of Mechanics Presented in a New Form*, transl. D. E. Jones and J. T. Walley (London, 1899).
49. See the Introductory Essay and Bibliography by Robert S. Cohen in the reprinted edition of Hertz's *Mechanics* (New York, 1956).
50. Bertrand Russell, *The Principles of Mathematics* 2nd ed. (New York, 1943), p. 494. (This edition was reprinted unchanged from the original edition of 1903).
51. H. Hertz, *op. cit.*, p. 6.
52. *Ibid.*, p. 25.
53. See P. Duhem, *L'évolution de la mécanique* (Paris, 1903), pp. 157–168.
54. H. Hertz, *op. cit.*, p. 26.
55. *Ibid.*, p. 41.
56. *Ibid.*, p. 177.
57. L. Boltzmann, "Anfrage, die Hertzsche Mechanik betreffend," *Wiss. Abh. 3*, p. 641.
58. L. Boltzmann, *Vorlesungen über die Principe der Mechanik 1* (Leipzig, 1897), pp. 1–6, 37–42.

59. Ernst Mach, *History and Root of the Principle of the Conservation of Energy*, transl. P. E. B. Jourdain (Chicago, 1911), p. 54.
60. *Ibid.*, p. 39.
61. E. Mach, *The Science of Mechanics*, transl. T. J. McCormack, 6th ed. (Lasalle, Illinois, 1960), pp. 596–597.
62. Georg Helm, *Die Energetik* (Leipzig, 1898), pp. 144–146.
63. Josiah Willard Gibbs, "Rudolf Julius Emanuel Clausius," *Proc. Amer. Acad. 16* (1889), 458. Reprinted in *The Scientific Papers of J. Willard Gibbs* (New York, 1906; reprinted 1961), *2*, pp. 261–267. See my article "Gibbs on Clausius," *Historical Studies in the Physical Sciences*, ed. R. McCormmach (Philadelphia, 1969) *1*, p. 127.
64. Information from the Gibbs manuscripts at the Beinecke Rare Book and Manuscript Library, Yale University.
65. J. W. Gibbs, *Elementary Principles in Statistical Mechanics* (New Haven, 1902; reprinted New York, 1960), pp. vii–xii.
66. J. W. Gibbs, *Scientific Papers, 2*, p. 210.
67. *Ibid.*, p. 246.
68. See Tetu Hirosige, "Origins of Lorentz' Theory of Electrons and the Concept of the Electromagnetic Field," *Historical Studies in the Physical Sciences 1* (1969), pp. 191–209.
69. Albert Einstein, "Autobiographical Notes," in *Albert Einstein: Philosopher-Scientist*, ed. P. A. Schilpp (New York, 1949), pp. 25, 27.
70. See, for example, Max Abraham, *Theorie der Elektrizität, II, Elektromagnetische Theorie der Strahlung* (Leipzig, 1905), p. 147.
71. L. Boltzmann, *Vorlesungen über die Principe der Mechanik 2* (Leipzig, 1904), pp. 137–139.
72. See T. Hirosige, "Theory of Relativity and the Ether," *Japanese Studies in the History of Science*, No. 7 (1968), 48–49.
73. P. Duhem, *op. cit.*, note 53, pp. 197–208.
74. L. Campbell and W. Garnett, *The Life of James Clerk Maxwell* (London, 1882), p. 28.
75. I gratefully acknowledge the support for this work provided by a grant from the National Science Foundation.