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(*b*. Belfast, Ireland, 26 June 1824; *d*. Netherhall, near Largs, Ayrshire, Scotland, 17 December 1907)

physics.

Thomson was the son of James Thomson, who, at the time of his son's birth, was professor of engineering at Belfast. In 1832 he became professor of mathematics at Glasgow. He was the author of several noted texts on differential and integral calculus, and he educated William and another son, James, at home. In 1834 both boys matriculated at Glasgow, where the environment was one characteristic of the Scottish universities of the time, which differed greatly from Cambridge. Whereas at Cambridge there was no chair in natural philosophy, nor much interest in the work of the Parisian analysts of the first third of the century, at Glasgow there was a professorship in natural philosophy (held by William Meickleham. who was succeeded by Nichol and then by William Thomson); there was also a chair in chemistry (held by Thomas Thomson).

Meickleham had a great interest in the French approach to physical science and much respect for it. In 1904 Thomson recalled how, "My predecessor in the Natural Philosophy Chair . . . taught his students reverence for the great French mathematicians Legendre, Lagrange, and Laplace. His immediate successor. Dr. Nichol, added Fresnel and Fourier to this list of scientific nobles."[1] Having been stimulated by Meickleham, Thomson avidly read Fourier's *Théorie analytique de la chaleur and Laplace's Mécanique céleste* during a trip to Paris in 1839. Indeed, Thomson's earliest interests centered on questions drawn from both these treatises. His first published paper (except for an early effort concerning the completeness of Fourier series) involved an attempt to find a method for determining the temperature in a heat-conducting solid outside a closed isothermal surface described within the solid. Thomson approached the problem by employing propositions drawn from Laplace's theory of attraction: the theory that treats the forces exerted by shells of attracting matter. In doing so he forged a formal relationship between the theory of the transfer of heat, on the one hand, and on the other, the general class of theories of attraction, in the particular instance of effects exerted by the electrical fluid.[2]

In the course of his analysis Thomson found that the Coulomb force which is exerted by electrical fluid in a state of equilibrium on the surface of a conducting body within which no fluid exists is mathematically parallel to the flow of heat produced by thermal sources distributed, in place of the electrical fluid, over the surface of the conductor. The formal relation assumes that the empty space in and around the conducting body is replaced by a heat-conducting solid, and that the surface of the conductor is itself replaced by a similarly shaped surface over which sources of heat are distributed in equilibrium. (The term "equilibrium" here means that the thermal surface has a constant temperature and does not enclose any sources of heat, all sources being located at the surface.) In stating this formal relation, Thomson was attempting, at this time (1842), only to find a method for solving problems in heat by use of the theory of electricity in equilibrium. The relationship between the theories of the transfer of heat and of electricity was thus purely formal, their connection being mathematical, not physical.

Between 1841 and 1845 Thomson attended Cambridge. His studies did not influence him as deeply as had those during his years at Glasgow, primarily because of the extreme importance attached to finishing in the first rank of the Senate-House examinations. This emphasis required the expenditure of much thought on the particular kinds of mathematical problems asked. Only rarely were those problems related to any physical question that was not contained in Newton's *Principia*. Thomson did expand his knowledge of the French mathematical techniques and theories during these years, however; and soon after his graduation he journeyed again to Paris, at his fathers suggestion, to work in Regnault's laboratory.

On arriving there for the second time, during the summer of 1845, Thomson was warmly received by Liouville and was soon introduced to Cauchy, Sturm, Biot, Dumas, and Regnault. His studies in Paris were crucial for the subsequent development of British physical science. During this period he developed the technique of electrical images, first read Clapeyron's explication of Carnot's theory of the motive power of heat, and formulated a methodology of scientific explanation that strongly influenced Maxwell.

Thomson's extensive contact with Liouville led him to think more deeply about electrical theory. Liouville had heard of Faraday's work in electrostatics, or at least of the aspects in which Faraday claimed to have found that electrical induction occurs in "curved lines." The conception seemed to conflict with the action-at-a-distance approach, and Liouville asked Thomson to write a paper clarifying the differences between Faraday on the one hand and Coulomb and Poisson on the other. This request prompted Thomson to bring together ideas he had been turning over in his mind during the previous three years.

Even in the 1842 paper on isothermal surfaces, Thomson did not treat the electrical fluid as Poisson had done. He knew of Poisson's mode of mathematical development, but he dealt with the fluid more in the manner of Coulomb–that is, without attributing to the imagined fluid the material properties of actual fluids known to experience. Poisson, for his part, had insisted on conceiving of electricity as a fluid that, like other material entities, occupies a finite region of space: the central problem of his theory was the determination of the actual thickness of the electrical layer at any point of a conducting surface.

In 1842, and again early in 1845, Thomson attempted to envision the physical characteristics of the electrical fluid, and reached disquieting conclusions. He found that if electricity is thought of as a fluid the parts of which exert only inverse-square forces upon one another, then the electrical layer at the surface of a conductor can have no physical thickness at all. That result implied that electricity must be a set of point centers of force. At the time he completely rejected that notion. Later, in 1860, he attributed it to BoŠković. But this rejection made it increasingly difficult for him to conceive of Poisson's fluid as a real physical entity at all. Thus, Liouville's request for a discussion of the issue between Faraday's approach and that of action-at-a-distance led Thomson to attempt a restatement of both theories in terms free from physical hypotheses (as he termed hypotheses concerning unobservable entities). In making that effort, Thomson found himself to be constructing an entire methodology for scientific explanation.[3]

He began by distinguishing the "physical" from the "mathematical" content of Poisson's theory, at first implicitly. In the "mechanical theory of electricity," as he termed the physical hypothesis of the electrical fluid,[4] there are fundamental difficulties which create doubt about the adequacy with which the hypothesis represents the nature of all matter. At the same time the "mathematical theory" developed by Poisson did seem to be extremely powerful and to be capable of dealing correctly with a vast range of particular cases. Prompted by the physical difficulty with Poisson's approach and concerned over the apparent conflict with Faraday's ideas, Thomson began to think more carefully about the actual nature of the action-at-a-distance theory. What did it assert that could be accepted without also accepting the electrical fluid as a real entity? For Thomson was caught between his great admiration for the mathematical power of Poisson's approach and his distaste for the electrical fluid. Hoping to resolve this dilemma, he undertook a series of researches that in effect led him to a proof of the equivalence of Faraday's approach with that of the action-at-a-distance school. He achieved that result by excluding all elements that depended on physical hypotheses from both theories.

From Thomson's new point of view, both the French approach to electrical theory and that of Faraday should consist only of sets of mathematical propositions about the "distribution of electricity" on conducting bodies. Of Coulomb, who had never written like Poisson of the "thickness" of the electrical layer, Thomson said that he had "expressed his theory in such a manner that it can only be attacked in the way of proving his experimental results to be inaccurate." He did not, therefore, believe that Coulomb's approach would stand or fall with the fate of the electrical fluid.

Of course, it may be wondered how Thomson could have employed the phrase "distribution of electricity" without believing that some hypothetical entity is implicated. He did not think so, however. Instead, by 1845 he was drawing a distinction between a "physical hypothesis" and an elementary mathematical law." By a physical hypothesis he meant an assumption concerning the physical existence of an unobservable entity like the electrical fluid or Faraday's contiguous dielectric particles. By an elementary mathematical law he meant a statement that can be directly applied in experiments because its referents are phenomenal entities and mathematical propositions about them. For example, when it is a question of the "distribution of electricity" a phrase that might appear in an "elementary mathematical law," the actual subject concerns the effects produced when a proof-plane is applied to a point of an electrified conductor. The measure of those effects is the twist given to the torsion-bearing thread of an electrometer. Coulomb's laws, therefore, and also those aspects of Poisson's mathematical development of them that do not depend upon the conception of electricity as a physical fluid, were thus actually concise, mathematical laws applicable to the results of such experiments. They were not hypotheses concerning the nature of electricity.

If, however, neither Coulomb's nor Faraday's approaches really did contain physical hypotheses, and if both of them yielded correct laws for the same phenomena, then there should be no conflict. Indeed, thought Thomson, any two theories dealing with the same phenomena, however different they may appear to be, cannot conflict if their most elementary laws can be expressed mathematically with no referents except those that can be interpreted phenomenologically. But having asserted the equivalence of such theories, Thomson had now to provide a method by which equivalence in any particular instance could be demonstrated. He found just such a method in his 1842 connecting of the laws of the transfer of heat with the theory of electricity in equilibrium on conducting surfaces. His reading of George Green's *Essay on the Application of Mathematical Analysis to the Theories of Electricity and Magnetism* (1828) had made the connection even more cogent.

Thomson obtained a copy of Green's work early in 1845. He was especially struck by the proof that a knowledge of the electrical potential at all points suffices to determine both the forces and the distribution of electricity on conducting bodies and permits dispensing with Poisson's postulate of an electrical layer of finite depth. Thomson realized that his own paper of 1842 had actually been founded on a formal equating of temperature with potential, and Green's work convinced him that all of the propositions of the mathematical theory of electricity could be expressed solely in terms of the potential. The relation between the mathematics of heat transfer and of electrical equilibrium thus became even more convincing than he had thought.

This relation made it possible to express the elementary laws of the "mathematical theory" (namely, that containing Poisson's mathematical results but not the conception of electricity as a physical fluid) in the same terms as the laws of the uniform transfer of heat. For Green had actually proved that a knowledge of the potential is sufficient to solve all electrical problems. "We may," Thomson now noted, "employ the elementary principles of one theory, as theorems, relative to the other." That is, the mathematical theory could be expressed in such a fashion that its laws appear as theorems following out of the laws of the transfer of heat when a formal equation is made between temperature and potential.

Construing the mathematical theory in terms of a Fourier heat transfer provided Thomson with a technique for proving the equivalence of Faraday's approach with the mathematical theory. The method was to express the concepts underlying these approaches in propositions drawn from Fourier; if this could be done unambiguously for both, then their equivalence would, he thought, be manifest. If, now, Faraday's principles regarding the induction of electricity on conductors could be connected with the same hypothesis-independent laws to which those of the mathematical theory were linked, then the two methods would come to the same thing. And, indeed, Thomson saw Faraday's system almost as an immediate consequence of the application of Fourier's laws to electrical phenomena (supposing conductors to be separated only by air or a vacuum).

But what of the effects of nonconductors placed between conducting bodies? Thomson had in mind here the properties that Faraday had discovered and had taken for evidence telling in favor of his theory and against that of Poisson. That difficulty would be met, Thomson thought, provided the new law were to be admitted in order to express the effect of a dielectric intermediary, and provided its status were to be that of an elementary mathematical principle, and not of a physical hypothesis. Thomson assumed that the smallest parts of a dielectric under electrical influence possess a "polarity" the laws of which are the same as Poisson's mathematical laws of magnetic polarity. He was thereby able to replace the dielectric by equivalent charged surfaces of electrical equilibrium.[5]

Thomson's conceptions were a powerful technique for analyzing the relationship between theories that seem to conflict. His method for reconciling the systems was to scrutinize them in terms of what each asserted concerning phenomena that could be measured or detected and to try to eliminate whatever was hypothetical. Although there was no guarantee that this critique would always succeed, for Coulomb's and Faraday's theories it did.

Thus it was that Thomson started his policy of eliminating physical hypotheses in order to reconcile Faraday's work with Coulomb's. It was fortified by his aversion to the concept of an imponderable fluid, an idea that was contrary to his beliefs about the nature of matter. For after all his methodology could not be independent of his opinions regarding the actual structure of unobservable entities. Given his particular objections to the electrical fluid, the method that he created between 1842 and 1845 for the purpose of unifying theories had begun to assume a slightly different aspect by 1847. The emphasis now fell upon what the formally equivalent system can tell about the original system as well as upon how it can help eliminate physical hypotheses.

The original purposes, which had been to unify theory and eliminate hypothesis, did not disappear. Instead an element of conceptual elucidation was added that made possible the visualization of the theory in new terms. The elucidatory role of the technique of formal equivalence became very prominent in 1847. In that year Thomson made use of Stokes's 1845 equations for fluids and solids in order to bring out the analogies, first, between the internal linear displacements of an elastic solid and electrostatic force, and, second, between the internal rotational displacements and galvanic and magnetic forces. Thomson's representation was conditioned by Faraday's discovery in 1845 of the rotation of the plane of polarization of light passed through a transparent body subject to magnetic action (the Faraday effect). The rotational nature of the effect led Thomson to characterize magnetic and galvanic forces by internal rotations of elastic media.[6] This new aspect of the process of formal equivalence was elucidatory because Thomson related distinct forces (electric and magnetic) to the internal processes of a single medium. Instead of being entirely distinct phenomena, electrical force and magnetic force were thus linked to a common element.

Yet even here Thomson limited himself to the statement that Faraday's discovery ". . . suggests the idea that there may be a problem connected with the distribution of electricity on conductors, or with the forces of attraction and repulsion exercised by electrified bodies." The purpose of the "mechanical analogy" was different from that of heat representation. The attempt was not to prove the equivalence of theories dealing with the same phenomena, but rather to link theories of different, although related, phenomena (namely, electricity and magnetism) by demonstrating that they could be shown to serve a closely connected set of expressions. The 1845 conception of formal equivalence was not thereby displaced; instead, it was supplemented. Equivalents now made the underlying mathematical connections of phenomena that are quite different more understandable. It was the qualities of clarity and unity that equivalents brought to disparate areas which ultimately had the greatest influence on Maxwell's early work. For it was from Thomson that Maxwell appropriated the equivalence technique as a method for providing unity in place of discord.

After his 1845 sojourn in Paris, Thomson returned to Scotland, where he succeeded to the professorship in natural philosophy at Glasgow, a post that he held for the rest of his life. Neither in Scotland nor in England was there then a university research laboratory or any other in which students could work. Thomson, having had access to Regnault's laboratory, was interested in establishing similar opportunities for students, and he obtained a small sum from the university for that purpose. It made possible the first teaching laboratory in Britain. He was also greatly interested in developing highly accurate measuring instruments, and the facilities of his new laboratory in Glasgow made that possible also.

Thomson's studies in France not only led him to a new approach to electrical theory and to an interest in experimental work and instrumentation: he was also introduced to Sadi Carnot's theory of the motive power of heat, as developed analytically by Clapeyron in 1834. Thomson was deeply impressed by the power of Carnot's theory (although he had read only Clapeyron's explication of it at this date), and especially by the rationalization it afforded for the production of mechanical effect by thermal processes. In an 1848 paper Thomson employed Carnot's theory for the first time in an attempt to establish an "absolute" thermometric scale. The old scale was a merely "arbitrary series of numbered points of reference sufficiently close for the requirements of practical thermometry." By an absolute scale Thomson meant one based on some completely general natural law. That law he took from Carnot–a given amount of heat passing between two given temperatures can produce at most a certain amount of work. In the old scale, based on the air thermometer, the amount of work done by a standard quantity of heat in falling through one degree varied at different points of the scale, as Clapeyron had shown. An absolute scale would be one in which the "value of a degree" would be independent of temperature. Thomson constructed this measure using the results of Regnault, Steele, and others.[7] (The modern Kelvin scale was defined later, following the elucidation of the concept of the conservation of energy.)

Carnot's work was generally unknown, even in Paris; and his theory was new not only to Thomson but to the entire British physical community of the 1840's. A few months after the 1848 paper Thomson presented to the Royal Society of Edinburgh a general account of Carnot's findings. Thomson noted that Carnot's theory was founded on the concept that heat is a substance that, when employed in a complete cycle of operations, enters the body acting as an engine in a given amount. At the end of the cycle it is entirely removed, and the engine remains in its initial state. Carnot had likened the operation of such a heat engine to that of a column-of-water device in which a quantity of water falling through a fixed distance from a given height produces an invariable quantity of motive power, the water being fully transferred from its original height to a reservoir at a lower level. In Carnot's view heat acts in a similar manner: a given quantity of heat abstracted by an engine from a high-temperature reservoir effects changes in the volume of the engine and. at the end of a cycle in which the engine is restored to its original condition, the heat taken from the high-temperature source has been totally transferred to a reservoir at a lower temperature, the amount of power produced being fixed by the temperatures of the reservoirs and the quantity of heat transferred.

Even at this early date, Thomson was ambivalent in his views concerning the concept of heat as substance despite the seeming necessity of accepting that idea if Carnot's theory were to be employed. In his "Account of Carnot's Theory" (1849) he wrote,

. . . all those assumptions depending on the idea that heat is a *substance*, invariable in quantity, not convertible into any other element, and incapable of being *generated* by any physical agency: in fact the acknowledged principles of latent heat; would require to be tested by a most searching investigation before they ought to be admitted, as they have usually been, by almost every one who had been engaged on the subject.[8]

Thomson's ambivalence had two distinct sources. The first lay in his skepticism about the propriety of employing imponderable entities. Further, Fourier's theory of the transfer of heat had left open the question of the nature of heat, and Thomson felt this openness to be one of the most important characteristics of the theory. Thus, he would not have accepted the material theory of heat without misgivings, even had he not known the work of Joule, whom he met at the 1847 meeting of the British Association. That work furnished a second set of reasons for Thomson's ambivalence.

Joule believed that heat and mechanical effect are but different aspects of matter, motion, and force. Sensible heat, he supposed, is in reality the "living force of the particles of the bodies in which it is induced;" when in a latent form, heat consists in the "separation of particle from particle, so as to cause them to attract one another through a greater space." Living force and "attraction through space"–by which Joule intended not force alone but a force acting across a given space–are convertible and "equivalent" because "living force may be produced by the action of gravity [or any attractive or repulsive force] through a given distance of space." Conversely, particulate motion can be transformed into particulate arrangement under the action of force. Whenever the living force of macroscopic bodies disappears, an equivalent of either particulate live force or of mechanical rearrangement effected against force is produced.

Thus, according to Joule, heat can be absolutely generated by mechanical action or, indeed, by any physical agency in which mechanical force is the ultimate source of action (as, for example, in an electromagnetic engine turned by hand). Conversely, heat should be absolutely destroyed in those circumstances in which thermal agency effects mechanical action. Joule attempted to support his contentions through a series of experiments involving electromagnetic engines and the internal motions of viscous fluids. In the former case Joule argued that heat is actually generated in a current-bearing conductor by the action of the current directly and not by any transfer of heat from hotter to colder parts of the body. In the latter case, he produced motions in a viscous fluid by means of a paddle wheel run by mechanical force. Although the motions seem to disappear after a time, they are actually converted into the living force particles of the fluid–heat–as is evidenced by a rise in temperature.

These two experiments at best demonstrated, Thomson carefully noted, that heat can be produced by mechanical effect; but they did not prove the converse–that it can be destroyed through conversion into effect. Joule's experimental demonstrations enabled Thomson to keep his distance ; from the material hypothesis of heat, although he continued to accept what he believed to be Carnot's basic principles. He did not embrace Joule's wider schema of heat as motion and "attraction through space"; indeed, it is highly unlikely that he knew the details of Joule's ideas on these points in 1849 beyond the rather vague assertion that heat is particulate *vis viva*. Thomson probably did not yet fully grasp the conception of latent heat as the arrangement of particles under the action of force. Indeed, his presentation of "Carnot's axioms" would suggest that he did not.

In the 1849 "Account" Thomson rarely referred to the impossibility of perpetual motion, conceived then as the impossibility of obtaining motive power without a corresponding alteration in other conditions. He began directly with his own version of ; Carnot's two axioms, regarding them as the basis of a theory that makes possible the calculation of the mechanical effect that can be produced *solely* by thermal agency. He gave the following as the principal "questions to be resolved by a complete theory of the subject"; these questions, to which Carnot's theory provides one possible answer, were essentially pragmatic:

1.What is the precise nature of the thermal agency by means of which *mechanical effect* is to be produced, without effects of any other kind?

2.How may the amount of this thermal agency necessary for performing a given quantity of work be estimated?

Thomson answered the first question by a deduction from what he called Carnot's "fundamental axiom," *viz*., that ". . . at the end of a cycle of operations, when a body is left in precisely its primitive physical conditions, if it has absorbed any heat during one part of the operations, it must have given out exactly the same amount during the remainder of the cycle." On the basis of this axiom, Thomson concluded that "the origin of motive power . . . must be found in the agency of heat entering the body and leaving it" because no other effects are produced during a complete cycle. The precise mode in which mechanical effect is produced by heat transfer is specified in Carnot's second axiom: "The thermal agency by which mechanical effect may be obtained, is the transference of heat from one body to another at a lower temperature."

Combining these two propositions made it possible to derive a theorem of great importance, which states that a fixed quantity of effect, the maximum which can be obtained, is produced by all reversible engines from the transferral of a given quantity of heat between two specified temperatures: "A perfect thermodynamic engine is such that, whatever amount of mechanical effect it can derive from a certain thermal agency; if an equal amount be spent in working it backwards, an equal reverse thermal effect will be produced." The foregoing has the standing of a theorem, and not a definition, because the criterion of reversibility is implicated in the first axiom through the notion of cyclical processes. The theorem affords the answer to the second question because it is used to calculate the effect produced by the transfer of heat between two fixed reservoirs by a perfect engine.

Both axioms are necessary for the derivation of the perfection theorem, and the first axiom is additionally necessary when analyzing a cycle of operations to ensure that, in a complete cycle, the engine returns to its primitive state. Yet Thomson, troubled by the conception of heat as a substance, felt that he had to justify the fundamental (first) axiom and attempted to do so by asserting that "no operation is known by which heat can be absorbed into a body without either elevating its temperature, or becoming latent, and producing some alteration in its physical condition." But heat engines must return to their initial states at the end of a cycle if–and here is the true motive for Thomson's insistence on the first axiom–calculation of the effects that have purely thermal origins is to be possible. It is again evident that Thomson had not yet grasped Joule's conception of latent heat as converted heat. He had not assimilated the idea that a body does not have to part with heat *as* heat in order to be returned to its primitive state. Instead, it can effect the conversion of heat into "attraction through space," that is, into the configuration of external bodies between which forces act, and thereby return to its original condition. In Thomson's opinion heat had to be transferred as heat in order to appear once again as sensible or latent; it cannot "disappear."

Thus, what Thomson called Carnot's "fundamental axiom" asserted the inconvertibility of heat' in the view he then took of it. At this stage in his thought, it was not because convertibility of heat to something else would permit perpetual motion that he objected to it. Rather, he wished to maintain what he took to be Carnot's theory, because abandoning either axiom would ruin any theory of the origins of mechanical effect from thermal agency. He justified the first and fundamental axiom by the lack of any known operation in which heat can be absorbed without the appearance of extraneous effects. It is obvious in retrospect that this position was tenuous because it actually depended upon Thomson's inability to conceive of latent heat as converted heat. It can therefore be seen that in 1849 Thomson did not so much hold to the conception of the materiality of heat as he held to certain propositions that appeared to be founded on that idea, but that are in reality acceptable on empirical and pragmatic grounds. Carnot had himself, toward the end of his short life, come to view the material theory of heat as untenable and had accepted the view that heat is a mode of motion. He had despaired of making his theory of motive power consonant with the kinetic model of heat. Thomson, however, knew nothing of Carnot's later ideas.

Despite Thomson's having accepted the two axioms, in particular the first, he was still not confident of the idea that heat is a substance. He reasoned that Carnot's fundamental axiom, while it "may be considered as still the most probable basis for an investigation of the motive power of heat," might, along with "every other branch of the theory of heat . . . ultimately require to be reconstructed upon another foundation, when our experimental data are more complete." Thomson's ambivalence depended in part upon his aversion to imponderable entities. Joule thought as he did, and wrote

In our notion of matter two ideas are generally included, namely those of *impenetrability* and *extension*. . . . Impenetrability and extension cannot with much propriety be reckoned among the *properties* of matter, but deserve rather to be called its definitions, because nothing that does not possess the two qualities bears the name of matter. If we conceive of impenetrability and extension we have the idea of matter, and of matter only.

Joule's comments were printed in the Manchester *Courier* of 5 and 12 May 1847, but he may very well have discussed them with Thomson at the British Association meeting of that same year.

Thomson was also impressed by Joule's conception of conservation in general. As Joule had written in the *Courier*:

. . . the phenomena of nature, whether mechanical, chemical, or vital, consist almost entirely in continual conversion of attraction through space, living force, and heat into one another. Thus it is that order is maintained in the universe–nothing is destroyed, nothing ever lost, but the entire machinery, complicated as it is, works smoothly and harmoniously. And though, as in the awful vision of Ezekiel, "wheel may be in the middle of wheel," and every thing may appear complicated and involved in the apparent confusion and intricacy of an almost endless variety of causes, effects, conversions, and arrangements, yet is the most perfect regularity preserved–the whole being governed by the sovereign will of God.

Thomson was clearly affected by this conception, for in a footnote to his 1849 "Account" he remarked:

When thermal agency is spent in conducting heat through a solid, what becomes of the mechanical effect it might produce? Nothing can be lost in the operations of nature–no energy can be destroyed. What effect then is produced in place of the mechanical effect which is lost? . . . "[A similar problem seems to exist in the question of the] mechanical effect lost in a fluid set in motion in the interior of a rigid closed vessel, and allowed to come to rest by its own internal friction; but in this case the foundation of a solution of the difficulty has been actually found, in Mr. Joule's discovery of the generation of heat by the internal friction of fluids in motion.

It can be seen that in 1849 Thomson was deeply puzzled by the apparently complete disappearance of effect in certain cases. His thoughts were more clearly defined during the following two years as the result of two experiments. One of them clearly lent weight to Carnot's theory, and the other seemed to support that part of Joule's conceptions in which heat is thought to be generated. (Although Thomson had, from 1847 on, been willing to consider the latter possibility, only in 1850 did he accept it as a proven fact.)

The first experiment, performed by his brother, James, confirmed the deduction from Carnot's theory (with both axioms) that the freezing point of water must be lowered when the pressure is increased. One of the most important aspects of that work lay, not so much in its direct test of the two axioms, as in its use of the theorem derived from them regarding the perfection of reversible engines. It was this theorem that James Thomson most frequently employed in his demonstration, thereby giving it a significance greater than that normally associated with a derivative proposition. The work on the lowering of the freezing point of water, therefore, helped to shift attention from the two axioms toward the theorem on the perfection of the reversible engine. This shift was important for William Thomson's subsequent reformulation of Carnot's theory, in that he was to reconstruct the theory by substituting for Carnot's dual axioms a single one from which the perfectibility theorem followed directly.[9]

The second circumstance was more complex than the first and involved the properties of saturated steam under high pressure escaping through an orifice. Rankine had observed that, when saturated steam is permitted to expand, the heat that becomes latent during expansion is greater than the heat that the expanding vapor would normally release as a result of its concomitant drop in temperature. If no part of the vapor is to become liquid, it follows that some external source must supply the extra heat necessary to maintain saturation. More specifically, as a gas expands adiabatically, its temperature drops; the drop in temperature was taken as an indication that a certain amount of the free heat of the gas had become latent. The expansion of the gas was supposed to be the result of this absorption of heat. (The modern explanation of the effect depends upon the negative slope of the temperature-entropy curve in the vapor portion of the liquid-vapor curve. The implication is that the specific heat of saturated steam is negative, meaning that heat is absorbed as its temperature falls. The physical explanation of the effect is that, as the vapor has its temperature lowered, it expands so much to avoid supersaturation that the external work that it performs is greater than the drop in its internal energy. The situation thus requires that heat be absorbed.)

Steam remains saturated during its expansion after its escape at high pressure through an orifice (an expansion rapid enough to be essentially adiabatic). Its condition is evident in that it does not scald, as it would had it become partially liquefied. By mid-1850 Thomson saw this phenomenon as conclusive evidence in favor of Joule's contention that heat can be generated from something else: Thomson thought he knew the source of this extra heat. "There is no possible way," he wrote to Joule, "in which the heat can be acquired except by the friction of the steam as it rushes through the orifice. Hence I think I am justified in saying that your discovery alone can reconcile Mr. Rankine's discovery with known facts." It was at this time also that Thomson first learned of Clausius' work of the previous April; although he had not read it by October, he commented that Clausius' methods "differ from those of Carnot only in the adoption of your [Joule's] axiom instead of Carnot's. . . ." Within five months Thomson himself had assimilated in full the "dynamical theory of heat" and had so modified Carnot's theory as to be in accord with it–an act accomplished independently, without any detailed knowledge of Clausius' work of May 1850.[10]

Thomson's central concern during these months was the discovery of a principle from which the essential elements of Carnot's theory could be derived while dispensing with the fundamental axiom concerning the conservation of heat *qua* heat. This was not a simple task; without the fundamental axiom, it is extremely difficult to produce a measure of the effect resulting from purely thermal action. The beginning of a solution lay in the new meaning given to the concept of "latent heat" by the dynamical theory, where it is thought of as the work done against the internal, molecular forces of a body. They then store it in the resulting molecular configuration. This notion relieved Thomson of his earlier concern that a body can be returned to its initial state only if the entire amount of heat that has entered it leaves in the same form. He now understood that heat can be converted into a "new" form–"attraction through space" in Joule's terminology–and yet be entirely removed from the transferring engine because the "latent heat" of the engine, its forced molecular configuration, can be directly converted through the performance of work into something of the same kind, namely, the "attraction through space" of external bodies. The problem, therefore, became the discovery of a principle that could be taken as the expression of the essential contents of Carnot's theory once conversion had been admitted. In other words, now that heat could be envisioned as both *vis viva* and molecular configuration, the problem ceased to concern the way in which purely thermal actions could be measured, for thermal actions were reducible to mechanical processes. The problem now was to formulate the theory of heat engines.

As noted above, James Thomson's 1850 paper on the freezing point of water placed great emphasis on the ideal nature of reversible engines. The maximum mechanical effect from the transfer of a quantity of heat between two temperatures will be obtained from engines that yield a mechanical effect when run forward equal to that expended on them when run backward. Although derived from Carnot's two axioms, by 1850 Thomson had come to see this proposition as the central element of the theory. The theorem as stated is insufficient. It is necessary also to stipulate that the engine returns to its initial condition at the end of a cycle. Thomson had earlier believed that this requirement necessitated the acceptance of Carnot's fundamental axiom, but by March 1851 he knew that this was incorrect. Yet the theorem itself could not be derived if either of the two axioms was rejected. Neither was it tenable in its original form, if the dynamical theory was accepted, because it employed the concept of the complete transfer of heat. Thomson was able to reformulate the theorem without referring to transfer of heat. Instead, he referred to heat "quantity": "If an engine be such that, when it is worked backwards, the physical and mechanical agencies in every part of its motion are all reversed, it produces as much mechanical effect as can be produced by any thermodynamic engine, with the same temperatures of source and refrigerator, from a given quantity of heat." (The "quantity of heat" appears to be that which is abstracted from the high-temperature reservoir.) Thomson attributed this formulation to Carnot and Clausius, but the attribution to Carnot is misleading, because Carnot's statement referred to the transfer of heat *in toto* and not simply to heat "quantity" and thermal reservoirs. It was in the proposition on the ideal nature of reversible engines that Thomson located the essential content of Carnot's theory. This proposition, along with Joule's, is one of the two central propositions of the theory of heat engines.

Nonetheless, Thomson was not satisfied with the proposition on the perfection of reversible engines. He felt the need for a more elementary principle because the proposition did not appear to be self-evident, as Carnot's axioms had prior to Joule's work. Working independently of Clausius, Thomson now developed the concept of a new kind of perpetual motion and then deduced the perfection of reversible engines from the postulate of its impossibility. This new perpetual motion, were it possible, would produce useful effects solely by the conversion of heat directly into work–a possibility that is not in conflict with either Joule's proposition or with the impossibility of that kind of perpetual motion in which something for nothing is obtained. Thomson asserted the impossibility of what was later termed perpetual motion of the second kind in the following words: "It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter by cooling it below the temperature of the coldest of the surrounding objects."[11]

After 1844 (at the latest) Thomson felt that Fourier's principles had been overlooked by those "geologists who uncompromisingly oppose all paroxysmal hypotheses, and maintain not only that we have examples now before us, on the earth, of all the different actions by which its crust had been modified in geological history, but that those actions have never, or have not on the whole, been more violent in past time than they are at present." Thomson had early thought the Uniformitarian approach to be untenable. He believed that if both the earth and the sun had once been molten balls cooling through radiation (the earth forming a crust and the sun, because of its peculiarly high temperature and the nature of its substance, remaining an "incandescent liquid mass"), then the dissipation of heat required by Fourier's laws must necessarily have been much more rapid in the past than in the present. If this were so, then clearly such phenomena as the winds, which depend upon thermal gradients, must have been much more vigorous in past times. In 1844 these views were merely beliefs. Because Thomson had not developed in detail the solutions to such problems of heat transfer, he had not investigated the modifications which such circumstances as the solidification of the earth's outer crust might have produced in the rate at which heat is dissipated. Most important, he did not have the full range of data needed for deducing numerical values.

By late 1852 Thomson's beliefs about the inadequacy of Uniformitarian assumptions had been made even more cogent as a result of the second law of thermodynamics. He now thought Fourier's theory of the conduction of heat to be "a beautiful working out of a particular case belonging to the general doctrine of the 'Dissipation of Energy.'" Thomson's earlier beliefs were now reinforced by the energy dissipation conception. He reasoned that geological actions, being ultimately mechanical and due, in cases like volcanism, to internal gradients in the heat of the earth, must have gradually decreased in intensity over time as energy was "dissipated" (or, as he later put it, as "potential energy is exhausted"). Even if the earth as a whole has not cooled appreciably since its formation, it must still follow that volcanic action must have been more intense in the past. For it is a particular case of the conversion of heat into mechanical effect, and the inevitable equalizing of temperature throughout the substance of the earth must ultimately lead to a state of quiescence. By 1852 Thomson also thought that Fourier's laws require both the earth and the sun to have cooled substantially over time (although he had not provided a detailed argument for this latter assertion). And however that may be, he thought that the second law of thermodynamics requires volcanic action to have been much more intense in the past than at present. (If, however, the sun has not cooled appreciably, then atmospheric phenomena on earth–though not necessarily geological–might not have altered radically over time.)

By 1862 Thomson had provided detailed support for the contention that the sun has cooled. It had previously been held that the sun, although an incandescent mass radiating heat, might have remained at about the same temperature, its heat replenished either by the influx of meteors or by the effects of shrinking under the influence of gravity, or by both factors. Thomson discounted these contentions by making numerical estimates. In the first case, he argued, the mass increase attendant on the meteoric influx would have affected planetary motion. In the second case, even if heat is generated by shrinkage–given reasonable estimates of the specific heat of the sun's mass obtained from "Stokes's principles of solar and stellar chemistry" (spectroscopic theory)–the correlation is very poor between the actual amount of heat radiated and the amount that would be provided by gravitational collapse. It is, therefore, most likely, argued Thomson, that the sun has cooled considerably and that it is an incandescent liquid mass receiving no heat from without. On that basis, Thomson further calculated, given his estimate of the solar specific heat and the present rate of radiation, that the sun probably has "not illuminated the earth for 100,000,000, and almost [certainly for not more than] 500,000,000 years."

Thomson's deduction of a maximum limit for the age of the sun was in direct conflict with those geological Uniformitarians who assumed that geological time cannot be given absolute limitations. Thomson soon presented a second paper supporting his earlier belief that the earth also must have been much hotter in the past. He showed that Fourier's laws of the transfer of heat require that, given the present rate of decrease of the heat of the earth with depth, the earth must have solidified from its primordial molten state not less than 20,000,000 and not more than 400,000,000 years ago. These limits were rigorous deductions from Fourier's laws applied to the case of a molten sphere cooling through emission of radiant heat. They include a probable estimate of the magnitude of the effect due to the formation of the earth's crust; and they hold good provided that the earth has no sources of energy beyond its own central heat.

Thomson's original intention in writing the 1862 papers had been to attack Lyell and the extreme Uniformitarian approach to geology. The Uniformitarians, he felt, looked upon the sun "as Fontenelle's roses looked upon their gardener. 'Our gardener,' say they, 'must be a very old man; within the memory of roses he is the same as he has always been; it is impossible that he can ever be other than he is.'" Lyell had asserted that an absolute geochronology is not useful and most likely not possible. By 1865, however, British geologists were uncertain that Lyell had been correct on this point, and there was disagreement over whether the history of the earth could be absolutely dated. Some believed that it could be grouped in distinct sequences (Pre-Cambrian, Cambrian, etc.) with arbitrary time spans. Thomson unwittingly entered the midst of the geochronology controversy with his insistence that absolute times can, and indeed must, be assigned.

The idea that the central heat of the earth accounts for volcanic action and other geological processes involving thermal variations was assumed by most geologists of the time; and Thomson's criticism implied that, if that idea be accepted, it followed that for a long period the surface of the earth had been the scene of violent and often abrupt changes. This last point was especially damaging to the Darwinians, who had assumed that evolution, being a very slow, gradual process, must occur within the context of uniform geological change. For thirty years the geological and biological community had either to ignore the findings of physics, which few could do comfortably, or else with Huxley attempt to satisfy the demands of Thomson's limitations as best they could.[12]

It was Thomson's acceptance of the dynamical theory of heat and his subsequent reformulation of the axioms of thermodynamics that had led him to several of the conclusions resulting in the geological controversy. The effect of the dynamical theory was not limited to areas directly associated with thermal processes, however. In accepting the conception that sensible heat is particulate *vis viva* and that latent heat is the stored effect of molecular configuration, Thomson, for the first time in his career, deliberately did employ unobservable entities and make use of physical hypotheses. It must be recognized that Thomson's earlier exclusion of unobservables had been dependent upon his inability to attribute truly material properties to such entities. In contrast, the dynamical theory of heat did not require the acceptance of any particular conception of the ultimate nature of material particles. It required only that these particles exist and exhibit the properties of mass, motion, and the power to exert forces (although this latter aspect presents several difficulties). The idea that heat is a mode of particulate motion opened to Thomson a new approach to all physical theory, therefore. He wrote in 1872 of his thoughts before he had accepted the dynamical theory

. . . [before 1847] I did not . . . know that motion is the very essence of what has hitherto been called matter. At the 1847 meeting of the British Association in Oxford, I learned from Joule the dynamical theory of heat, and was forced to abandon at once many, and gradually from year to year all other, statical preconceptions regarding the ultimate causes of apparently statical phenomena.

In 1855 Thomson wrote John Tyndall a letter in which he referred to the "mechanical qualities" of the medium that pervades all space. He now believed the ultimate explanation of electromagnetic phenomena lay in the structure of that medium, conceived dynamically. On 10 May 1856 Thomson submitted to the Royal Society a paper that employed the dynamical properties of molecular entities to explain the Faraday effect. By then he was willing to employ microstructural entities which possess the requisite material properties, and he argued that, from any galvanic current, there extends a moving spiral that coils about the line of magnetic force passing through the center of the axis of the current. Indeed, he intimated that the current itself consists of the trapping of a segment of this spiral in ponderable matter. Light waves are propagated by transverse vibrations of the particles of the moving spirals, and the plane of polarization of the waves will be rotated in a sense dependent upon the motion of the spiral, which, in turn, depends upon the direction of the magnetic force.[13] (See Fig. 1.)

Thomson believed that this representation afforded the ultimate representation of all electromagnetic effects. Magnetic forces were to be due to the screw motions of the spiraling helices which, when fixed in matter, become currents, and electrostatic forces to their compressions:

We now look on space as full. We know that light is propagated like sound through pressure and motion

. . . . If electric force depends on a residual surface action, a resultant of an inner-tension, experienced by the insulating medium, we can conceive that electricity itself is to be understood as not an accident, but an essence of matter. Whatever electricity is, it seems quite certain that electricity in motion IS heat; and that a certain alignment of axes of revolution in this motion is magnetism. . . .[14]

It was this vision of Thomson's that Maxwell seized upon between 1857 and 1862 in his search for a new approach to electromagnetic theory.

Despite his influence upon Maxwell, Thomson was highly skeptical of Maxwell's approach to electromagnetic theory. The reasons for that skepticism lay in Thomson's developing beliefs regarding the nature of the hypotheses to be used in physical explanations. Although the dynamical theory of heat had opened Thomson's mind to the role of unobservable entities in theory, he still had very definite opinions on the nature of the admissible entities. "I have been led," he wrote in 1858 in an unpublished note.[15] ". . . to endeavour to explain some of the known properties of sensible matter by investigating the motion of [a fluid filling the interstices between detached solid particles] on strict dynamical principles." The sole attributes of the space-filling fluid are extension, incompressibility, and inertia; these, together with the laws of mechanics, constitute the "dynamical principles" to be used as the basis of physical theory. Evident in this is a clear reflection of Thomson's early revulsion from imponderable fluids and his strong conviction that it is in extended matter and inertial motion that the ultimate explanation of all physical processes is to be sought.

Yet one phenomenon above all stands in the path of any general theory of the type he desired, and that is elasticity. If nothing is to be admitted beyond matter and motion, then elastic reaction to compression and distortion must be explicable without recourse to "force" in the Newtonian sense. In 1858 Thomson had no theory that could explain elasticity solely on the grounds of extension and inertia. What put him on the track of a solution was Tait's 1867 translation of Helmholtz' 1858 paper on vortex motion in inviscid, incompressible fluids: such fluids are analyzable without recourse to any principles beyond inertia and extension. In his work, Helmholtz, with whom Thomson maintained a close personal friendship, had shown that linear fluid vortices, defined as lines drawn in the fluid along which the angular momentum of differential fluid elements is constant, influence one another's motions through the instantaneously propagated pressures that their existence produces in the fluid as a whole. The effects are instantaneous because the fluid is incompressible. Closed lines of vortex motion exhibit striking patterns: two such figures appear to repel or attract one another in a complex manner dependent upon their mutual orientation and the angular momenta of their constituent elements. These effects are the results solely of pressures produced in the medium.

In 1867 Thomson opened a paper entitled "On Vortex Motion" as follows: "The mathematical work of the present paper has been performed to illustrate the hypothesis that space is continuously occupied by an incompressible frictionless fluid acted on by no force, and that material phenomena of every kind depend solely on motions created in the liquid."[16] In subsequent papers Thomson attempted to demonstrate that closed vortex tubes (tori whose surfaces are formed of closed, linear vortex filaments) behave toward one another much as the material particles that constitute bodies are supposed to act: the tubes "repel" one another, and, under certain conditions, they "attract"–actions that are the results, not of "force," but of material motions alone. Thomson ultimately had to admit that his program could not be expanded to include electromagnetism or the electromagnetic theory of light, gravitation, and chemical phenomena; but throughout the period 1860–1880 he was imbued with the conception of "vortex atoms"–that is, with the idea that all material particles are actually vortex tubes in an all-encompassing medium. Although their initial creation is inexplicable mechanically, he believed that their actions provide the most fruitful basis for theoretical speculation. Helmholtz had shown that vortices, once established by some unknown means in an inviscid medium, have unalterable qualities that can neither be generated nor destroyed by mechanical processes, and so are eternal. Among these qualities, as noted above, is a pseudoelasticity by which the vortices tend to avoid one another in an apparently elastic manner. Thomson saw in this the possibility of identifying the elastic atoms of kinetic theory with perpetually circulating vortices.

It was the strength of this conviction that accounts for his cold reaction to Maxwell's work. Maxwell, in 1861-1862, and again in 1864, had perforce to employ an unreduced "elasticity," a resistance to distortion of form, in his equations for the dynamical processes of the medium. Further, in their 1864 form the ethereal processes are difficult to visualize–they are dynamical, employing as primordial concepts material substance, momentum, and energy–but the structure of the medium is not necessarily supposed to be continuous, nor is it specified. Thomson regarded elasticity as a property that must be reduced completely to the effects of material motion. By 1867 he was greatly excited by the possibilities of his fluid medium, and he regarded Maxwell's system as at best a way station on the road to a more adequate representation. He could not accept a primordial elasticity. (For that matter, not many of Maxwell's British successors could accept it, and Maxwell himself was troubled by the question.) Also Thomson was not attracted to any dynamical theory that could not be fitted into the framework of a continuous, readily visualized medium. Although these hopes kept him from accepting Maxwell's scheme, and gradually opened a gap between him and the new generation of British "electricians" of the late 1870's and early 1880's, still his conceptions were generally regarded as the best chance for a truly dynamical theory. And Maxwell himself, despite Thomson's disagreement with his theory, was impressed by this vision. He saw in it at least an attempt to achieve ultimate mechanical simplicity; he, too, believed in this goal, having strayed only so far as he felt necessary in his approach to electromagnetism. Maxwell wrote of Thomson's vortices as follows:

. . . the greatest recommendation of this theory, from a philosophical point of view, is that its success in explaining phenomena does not depend on the ingenuity with which its contrivers "save appearances" by introducing first one hypothetical force and then another. When the vortex atom is once set in motion, all its properties are absolutely fixed and determined by the laws of motion of the primitive fluid, which are fully expressed in the fundamental equations. The disciple of Lucretius may cut and carve his solid atoms in the hope of getting them to combine into worlds; the follower of Boscovich may imagine new laws of force to meet the requirements of each new phenomenon; but he who dares to plant his feet in the path opened by Helmholtz and Thomson has no such resources. His primitive fluid has no other properties than inertia, invariable density, and perfect mobility, and the method by which the motion of this fluid is to be traced is pure mathematical analysis.[17]

Thomson's concern with the dynamical foundations of physical science, and his insistence that material substance be clearly conceived, are strikingly evident in the *Treatise on Natural Philosophy*, which he wrote with Peter Guthrie Tait in the early 1860's. "Thomson and Tait," as the work is generally known, and Maxwell's later *Treatise on Electricity and Magnetism* were the most influential British physical texts of the last half of the nineteenth century. Originally Thomson had envisioned a multivolume series giving a complete representation of all physical theory, including material processes, heat, light, and electricity and magnetism. He and Tait produced only the first two volumes, however–on kinematics and dynamics. The unfinished character of the work is evident in its continual references to future portions–as, for example, to a planned section on the properties of matter. Thomson and Tait presented in full the kinematics of point particles and the dynamics of motion under force; they placed heavy emphasis upon the dynamics of material media; and they made detailed use both of a new formulation of Lagrangean mechanics and the conservation of energy. The *Treatise on Natural Philosophy* introduced a new generation of British and American physical scientists to the details and concepts of mechanics.

In his attempt to achieve an alternative to Maxwell's theory of light, Thomson could find no clear help in his vortices, and in 1888 he began to look into the reasons for the failure of the elastic-solid theory of the luminiferous ether. He had particularly in view George Green's 1837 medium. Green had been obliged to assume that the ether is incompressible, in order to avoid instability and to remove longitudinal waves. On these grounds, however, he was unable to obtain Fresnel's tangent-law for the reflection and refraction of light for waves polarized normally to the plane of incidence. Encouraged by his vortex theory of the medium, Thomson at first imagined an inviscid fluid permeating the pores of an incompressible, spongelike solid, but he soon found that this structure only augmented the older difficulties. He therefore began to consider whether Green's comments on stability might not be open to doubt. As Thomson considered the problem, it occurred to him that the kind of instability envisaged by Green, that of a spontaneous shrinkage of finite ether volumes, would not occur "provided we either suppose the medium to extend all through boundless space, or give it a fixed containing vessel as its boundary."

By supposing the ether to have no resistance to compression by volume, Thomson was able to show that the hypothesis adopted makes it possible to obtain all of Fresnel's laws, while eliminating the longitudinal wave and avoiding instability. The work had an immediate and widespread impact on the Maxwellian community. Within a month Richard Tetley Glazebrook had written a paper successfully applying the new conception to double refraction, dispersion, and metallic reflection. In the United States, Willard Gibbs compared the new theory to the electrical theory:

It is evident that the electrical theory of light has a serious rival, in a sense in which, perhaps, one did not exist before the publication of William Thomson's paper in November last. Nevertheless, neither surprise at the results which have been achieved, nor admiration for that happy audacity of genius, which seeking the solution of the problem precisely where no one else would have ventured to look for it, has turned half a century of defeat into victory, should blind us to the actual state of the question.

It may still be said for the electrical theory, that it is not obliged to invent hypotheses, but only to apply the laws furnished by the science of electricity, and that it is difficult to account for the coincidence between the electrical and optical properties of media, unless we regard the motions of light as electrical. . . .[18]

Despite Thomson's deep theoretical concerns, he was always strongly interested in physical instrumentation. He felt that existing instruments were inadequate for precisely determining important physical constants. Instrumentation became even more important as electrical phenomena began to be employed in Britain's increasingly complex industrial economy, and Thomson was involved in the design and implementation of many new devices.

His interest and reputation brought him to the attention of a consortium of British industrialists who, in the mid-1850's, proposed to lay a submarine telegraph cable between Ireland and Newfoundland. Telegraphy by then was a well-developed and extremely profitable business, and the idea of laying such a cable was not new. The undertaking provides perhaps the first instance of a complex interaction between large-scale industrial enterprise and theoretical electricity. Thomson was brought in early in the project as a member of the board of directors, and he played a central role.

The directors had entrusted the technical details of the project to an industrial electrician, E. O. W. Whitehouse; and the many difficulties that plagued it from the outset resulted from Whitehouse's insistence on employing his own system of electrical signaling, despite theoretical objections from Thomson. Thomson had developed a very sensitive apparatus, the mirror-galvanometer, to detect the minuscule currents transmitted through miles of cable, but Whitehouse refused to use it. The Whitehouse-Thomson controversy stemmed primarily from Whitehouse's jealousy of Thomson's reputation. Thomson had asserted that the length of cable would, by a process of statical charging of its insulation, substantially reduce the rate at which signals could be sent unless small voltages were used, so small that only his galvanometer could detect the currents.

The first attempt to lay the cable, in 1857, ended when it snapped and was lost. The second attempt, a year later, was successful, but the large voltages required by the Whitehouse method reduced the ability of the cable to transmit signals rapidly, just as Thomson had predicted. Whitehouse privately recognized the inadequacy of his own instruments and surreptitiously substituted Thomson's galvanometer while claiming success for his own methods. This deception was soon discovered, and the ensuing controversy between Whitehouse, the board of directors, and Thomson combined theoretical science, professional vanity, and financial ignominy. A third cable was laid in 1865, and, with the use of Thomson's instruments, it proved capable of rapid, sustained transmission. Thomson's role as the man who saved a substantial investment made him a hero to the British financial community and to the Victorian public in general; indeed, he was knighted for it. It also was the foundation for a large personal fortune.

As the Atlantic cable affair demonstrates, Thomson was deeply involved in applying instrumentation designed for sensitive measurements to industrial concerns. His involvement in industry did not stem from a great desire to further the application of science to technology, although that motive was certainly there among others. Rather it was a consequence of his interest in instrumentation itself. The two central concerns of his life were the application of the ideas of mechanics to physics and the development of sensitive measuring devices. By the time of his death, Thomson, then Baron Kelvin of Largs, while perhaps behind the times in his adherence to dynamical modes of thought, was generally looked upon as the founder of British physics. Together with Helmholtz in Germany, he had been the foremost figure in transforming–indeed, in creating–the science of physics as it was known in 1900.

NOTES

1. S. P. Thompson, *The Life of William Thomson*, I.

2. W. Thomson, "On the Uniform Motion of Heat in Homogeneous Solid Bodies, and its Connexion With the Mathematical Theory of Electricity," in *Cambridge Mathematical Journal*, **3** (1843), 71–84.

3. See S. P. Thompson, *op. cit.*, I.

4. W. Thomson, "Demonstration of a Fundamental Proposition in the Mechanical Theory of Electricity," in *Reprint of Papers on Electrostatics and Magnetism*, pp. 100–103.

5. W. Thomson, "On the Mathematical Theory of Electricity in Equilibrium," *op. cit.*, 15–37.

6. W. Thomson, "On a Mechanical Representation of Electric, Magnetic, and Galvanic Forces," in *Mathematical and Physical Papers*. I (Cambridge. 1911), 76–80.

7. W. Thomson, "On an Absolute Thermometric Scale, Founded on Carnot's Theory of the Motive Power of Heat, and Calculated From the Results of Regnault's Experiments on the Pressure and Latent Heat of Steam," *ibid.*, **I** , 100–106.

8. W. Thomson, "An Account of Carnot's Theory of the Motive Power of Heat, With Numerical Results Deduced From Regnault's Experiments on Steam," in *Transactions of the Royal Society of Edinburgh*, **16** (1849), 541–574.

9. James Thomson, "Theoretical Considerations on the Effect of Pressure in Lowering the Freezing Point of Water," in *Cambridge and Dublin Mathematical Journal*, **5** (1850), 248–255.

10. W. Thomson, "On a Remarkable Property of Steam Connected With the Theory of the Steam-Engine," in *Philosophical Magazine*, **27** (1850), 386–389.

11. W. Thomson, "On the Dynamical Theory of Heat; With Numerical Results Deduced From Mr. Joule's 'Equivalent of a Thermal Unit' and M. Regnault's 'Observations on Steam,'" in *Transactions of the Royal Society of Edinburgh*, **20** (1853), 261–288.

12. On the problems see P. Lawrence, "The Central Heat of the Earth," Ph.D. dissertation, Harvard University, 1973.

13. W. Thomson, "Dynamical Illustrations of the Magnetic and Helicoidal Rotatory Effects of Transparent Bodies on Polarized Light," in *Proceedings of the Royal Society*, **7** (1856), 150–158.

14. W. Thomson, Royal Institution Friday Evening Lecture, in *Reprint of Papers on Electrostatics and Magnetism*, pp. 208–226.

15. Cambridge University Library. MS Add. 7342, box I. notebook **IV** , pp. **I–II** . Quoted in Ole Kundsen, "From Lord Kelvin's Notebook: Ether Speculations," in *Centaurus*, **16** (1966), 41–53.

16. W. Thomson, "On Vortex Motion," in *Philosophical Transactions of the Royal Society of London*, **25** , pt. 1 (1868), 217–260.

17. J. C. Maxwell, *Scientific Papers of James Clerk Maxwell* (New York, 1952), 445–484; from “Atomism,” in *Encyclopaedia Britannica*, 9th ed., **II** (1875).

18. J. Willard Gibbs, “A Comparison of the Electric Theory of Light and Sir William Thomson’s Theory of a Quasi-Labile Ether,” in *American Journal of Science*, **37** (1889), 467–475.

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All of Thomson’s papers are collected in two sets, *Reprint of Papers on Electrostatics and Magnetism* (London, 1872); and *Mathematical and Physical Papers*, 6 vols. (Cambridge, 1911). Occasional papers are not reprinted in the former collection, but in their absence a reference to their location in the journals is given. The *Treatise on Natural Philosophy*, 2 vols. (Oxford, 1867), written with P. G. Tait, went through numerous editions.

The only full-scale biography is S. P. Thompson, *The Life of William Thomson, Baron Kelvin of Largs*, 2 vols. (London, 1901). Although uncritical, it contains a vast selection from Thomson’s correspondence and includes a complete bibliography of Thomson’s papers. Thomson’s “green notebooks.” kept throughout his life, are unpublished but are available at the University Library, Cambridge. Brief accounts of Thomson’s life and work include David Murray, *Lord Kelvin as Professor in the Old College of Glasgow* (Glasgow, 1924): Alexander Russell, *Lord Kelvin, His Life and Work* (London–Edinburgh–New York, 1939); and A. P. Young, *Lord Kelvin, Physicist, Mathematician, Engineer* (London, 1948).

See also Herbert Norton Casson, “Kelvin, His Amazing Life and Worldwide Influence,” in *Efficiency Magazine* (1930); John Ferguson, “Lord Kelvin: a Recollection and an Impression,” in *Glasgow University Magazine*, **20** , no. 9 (1908); Kelvin Centenary Oration and Addresses Commemorative (London, 1924); Agnes Gardner King, *Kelvin the Man. A Biographical Sketch by His Niece* (London, 1925); Émile Picard, “Notice historique sur la vie et l’oeuvre de Lord Kelvin,” in *Annuaire de l’Académie des sciences de Paris* (1920); Robert H. Silliman, “William Thomson: Smoke Rings and Nineteenth-Century Atomism,” in *lsis*, **54** (1963), 461–474; E. C. Watson, “College Life at Cambridge in the Days of Stokes, Cayley, Adams, and Kelvin,” in *Scripta mathematica*, **6** (1939), 101–106.

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