2

Mechanical image and reality in Maxwell's electromagnetic theory

As he pursued the task of constructing a unified account of electromagnetic phenomena from a field-theoretic point of view – from his initial explorations under Thomson's tutelage in 1854 through his work on a second edition of the *Treatise on Electricity and Magnetism* in the months before his death in 1879 – Maxwell was unwavering in his basic commitment to a broad mechanical framework, within the confines of which this task was to be carried out. Within this broad mechanical framework, however, there were various methodological options at Maxwell's disposal – traceable to his experiences at both Edinburgh and Cambridge, and also to his interaction with William Thomson – and Maxwell was to make full use of this variety of options, in response to the shifting needs of his evolving research program. In brief, Maxwell started out using an analogical approach to mechanical representation, rooted in Scottish skepticism and reflecting a desire to proceed with minimal physical commitment at the outset; in this context, he presented the mechanical images in his first major paper on electromagnetic theory, "On Faraday's Lines of Force" (1855–6), as purely illustrative, with no claim whatever to realistic status. Subsequently, responding to William Thomson's judgment that the time had come to go beyond mere analogy in electromagnetic theory and to begin the task of constructing a realistic mechanical theory, Maxwell developed his molecular-vortex representation of the electromagnetic field in the paper entitled "On Physical Lines of Force" (1861–2). In that paper he made an explicit commitment to the probable reality of the basic features of the molecular-vortex hypothesis in a manner characteristic of the Cambridge school. Finally, in the later 1860s and the 1870s, Maxwell began a measured retreat from his realistic commitment to the molecular-vortex model, without ever completely giving it up; his attitude toward mechanical representation in that period was complex and nuanced – not reducible to one or the other of the initial options. An understanding of these successive phases in Maxwell's utilization of mechanical models, as well as the varying positions that he adopted with respect to the question of the reality of these mechanical images, is important for our general understanding of the role of mechanical models in nineteenth-century science and also provides the broad context for an understanding of Maxwell's stance with respect to the molecular-vortex model and his utilization of it in connection with his major innovations in electromagnetic theory.

1 *Maxwell and the uses of analogy*

In his paper "On Faraday's Lines of Force" – his first major effort in electromagnetic theory – Maxwell made use of mechanical representation in an analogical sense, at a time when he was not yet ready for a deeper commitment to any mechanical picture. In part, Maxwell supported his choice of an analogical approach, with its avoidance of ontological commitment, by invoking the kind of Scottish skepticism promulgated by Forbes at Edinburgh.[1] For Maxwell, however, the analogical approach was to be employed not as a permanent alternative to the kind of ontologically committed deep theory favored at Cambridge but rather as a prelude to that kind of theorizing; in this, Maxwell followed the example of Thomson rather than Forbes. Maxwell found Thomson's way of using analogies especially attractive, as he indicated in a letter to Thomson: "Have you patented that notion with all its applications?" asked Maxwell in May of 1855, "for I intend to borrow it for a season."[2] Borrow it he did, along with the notion that its primary value was as a temporary expedient, as a prelude to something better.

Maxwell saw himself in a situation in which he could make good use of just such a temporary expedient. "The present state of electrical science," he felt, was "peculiarly unfavourable to speculation." Much was known about electricity, but that knowledge was scattered and fragmentary, and Maxwell could not yet see his way through to a theoretical structure, based on the field approach, that would unify all the ramified phenomena of electricity and magnetism. One might be tempted, in such a situation, to "adopt a [working] hypothesis," which might at least lead to a "partial explanation" of electromagnetic phenomena. In Maxwell's opinion, however, that would be dangerous, because it might foster premature commitment: "If [in this situation] we adopt a physical hypothesis, we see the phenomena only through a medium, and are liable to that blindness to facts and rashness in assumption which a partial explanation encourages." Evincing here more scruple with respect to hypotheses than Herschel or

Whewell – who allowed provisional hypothesizing and theorizing, if carried out with appropriate care – Maxwell showed himself more the follower of Forbes in his concern to "avoid the dangers arising from a premature theory," to avoid being "carried beyond the truth by a favourite hypothesis." In this situation, the analogical approach seemed particularly well suited as a tool of investigation, for it would permit the use of a mechanical representation, which would aid in the task of "simplification and reduction of the results of previous investigation to a form in which the mind can grasp them," but "without being committed" in any way to the literal truth of that mechanical representation. (The possibility of proceeding without any mechanical representation, in a purely mathematical vein, was rejected by Maxwell out of hand, as he believed that disembodied mathematics was bound to be unfruitful; Scottish, Cambridge, and Thomsonian traditions were in agreement on that point.[3])

Merely as an analogy, then, Maxwell proposed a mechanical representation in which an incompressible fluid was pictured as flowing through a porous medium. In "Faraday's Lines," this image was applied to the elucidation of electric fields, magnetic fields, and distributions of electric current; the flow lines of the incompressible fluid were taken to correspond to magnetic lines of force, electric lines of force, or lines of electric current, depending on the particular problem being analyzed. The flow analogy could provide no image of the coexistence and interaction of electric fields, magnetic fields, and electric currents; rather, the flow analogy provided segmented, compartmentalized understanding of each of the three electromagnetic phenomena, each considered in isolation from the other. Such compartmentalization would have been intolerable from the point of view of providing a theory: "No electrical theory can now be put forth, unless it shews the connexion not only between electricity at rest and current electricity, but between the attractions and inductive effects of electricity in both states." The flow representation, however, did not have to be measured against that high standard, for it was intended as a mere analogy, with no claim to either comprehensiveness or truth value. Maxwell explicitly and repeatedly cautioned the reader that the incompressible fluid referred to was an "imaginary fluid," and "not even a hypothetical fluid." The mathematical isomorphism between the equations of percolative streamline flow and the equations describing electric or magnetic lines of force was nothing more than that, and no "physical theory," no specification of the actual "physical nature of electricity" or magnetism, was implied.[4]

There were further reasons for Maxwell's modest stance Wilhelm

Weber's "professedly physical theory of electro-dynamics," formulated in the action-at-a-distance (or charge-interaction) tradition, was by Maxwell's own admission "so elegant [and] so mathematical" that it could not be ignored. Weber had been able to develop a theory that gave a coherent and connected account of the basic phenomena of electricity, electromagnetism, magnetism, and electromagnetic induction – basically all of the phenomena of electricity and magnetism. Maxwell was not able at that point to propose an alternative theory of comparable range based on the field approach, and he evidently was concerned that strong claims in favor of a partial theory of his own – as against Weber's elegant, mathematical, and very comprehensive theory – would invite criticism and perhaps ridicule. Maxwell therefore presented his mechanical picture merely as an analogy – a heuristic device and "temporary" expedient – justifying his effort on grounds of theoretical pluralism: "It is a good thing to have two ways of looking at a subject, and to admit that there *are* two ways of looking at it."[5]

There was other ground on which Maxwell believed he might be criticized: He was presuming to contribute to the theory of electricity and magnetism, while having made no contribution whatever to the experimental side of that field. Only in the twentieth century has the theoretical physicist per se had an acknowledged role; given nineteenth-century norms, Maxwell felt constrained to be both apologetic and moderate in his claims: "By the [analogical] method I adopt," Maxwell wrote, "I hope to render it evident that I am not attempting to establish any physical theory of a science in which I have hardly made a single experiment." Maxwell's deference, one surmises, was primarily toward Faraday, who was eponymously honored in the title of Maxwell's paper; furthermore, Maxwell's main competitor in the realm of mathematical theory – Wilhelm Weber – added to his theoretical prowess impressive experimental credentials. Having no such credentials as an electrical experimenter himself, Maxwell did not presume to present a "true solution" to the problems of electrical science; instead, he offered a heuristic analogy, defining for himself an auxiliary role vis-à-vis the experimental philosophers, helping but not usurping:

> If the results of mere speculation which I have collected are found to be of any use to experimental philosophers, in arranging and interpreting their results, they will have served their purpose, and a mature theory, in which physical facts will be physically explained, will be formed by those who by interrogating Nature herself can obtain the only true solution of the questions which the mathematical theory suggests.[6]

This statement expressed not only the modesty of Maxwell's aims in "Faraday's Lines" but also the hopes he had for the future. He looked forward to something better than mere analogy: He looked forward to "a mature theory, in which physical facts will be physically explained." Thus, his aim in "Faraday's Lines" had "not [been] to establish [a] theory," but his hope for the future was that "a . . . theory . . . will be formed"; the fluid-flow analogy had "not [been] introduced to explain actual phenomena," but the hoped-for theory was to be one "in which physical facts will be physically explained."[7] In 1855, Maxwell was a young man without reputation, diffident and deferent with respect to both Faraday and Weber; he was just beginning to make headway in the task of understanding electromagnetic phenomena and wanted to avoid premature commitment; and he found himself able to devise only a segmented and compartmentalized mechanical representation of electromagnetic phenomena. In these circumstances he made no strong claims for the flow representation, putting it forward merely as an illustrative and heuristic analogy; he looked forward, however, to a better time.[8]

2 *Toward a realistic, comprehensive, and explanatory theory*

The signal that the time had come to go beyond mere analogy and begin to talk in earnest about the nature of things came from William Thomson. Thomson had pioneered the use of physical analogies in electromagnetic theory, and Maxwell had followed him; Thomson had, from the outset, regarded these analogies as merely preliminary steps along the way toward a hoped-for "physical theory,"[9] and Maxwell had agreed; finally, in 1856, Thomson decided that the time had come to talk about the nature of things in electromagnetic theory, and Maxwell was to follow him once again. Maxwell was no blind follower – we have seen that he had his own good reasons for his use of the method of analogy – but he was nonetheless a devoted follower of Thomson, and it is not surprising to find Maxwell following his mentor through this whole sequence.

It was within a year of the publication of Maxwell's "Faraday's Lines" that Thomson decided the time had come to go beyond mere heuristic analogy in the mechanical representation of electromagnetic phenomena. In a paper published in 1856 he announced that he was ready to propose a description of "reality," of the "ultimate nature of magnetism."[10] This new departure must be understood against the broader background of Thomson's developing commitment to a "dynamical" understanding of physical phenomena. That commitment had roots in Thomson's earlier discussions of "mechanical effect" in electrostatic systems, but it began to assume a central position in his thinking only as a result of his encounter with James Prescott Joule at the British Association meeting of 1847, where he "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."[11] Thomson's conversion to the dynamical theory of heat was in fact not quite so abrupt; he continued to defend the caloric theory against Joule's novel views for some years after 1847. In essence, however, Thomson's recollection was correct: He did experience a dramatic conversion to the dynamical theory of heat – by 1851 if not in 1847 – and the general lesson that he abstracted from that conversion experience was that all phenomena are ultimately "dynamical."[12] For Thomson, a "dynamical" theory, as opposed to a "statical" theory, was one in which the forces – and hence effects in general – exerted by a given physical system were referred to internal motions within that system, rather than to primitive attractions or repulsions between its particles. Thus, in Thomson's paradigmatic case of the dynamical theory of heat and gases, gas pressure was the result of internal motions, rather than static repulsive forces between caloric particles within the gas.[13] Thomson, in his dynamical theory of heat, followed Humphrey Davy and W. J. M. Rankine in assuming that the motions that constitute heat are rotary motions associated with individual molecules – "molecular vortices," in Rankine's terminology. Surrounding each "molecular nucle[us]," then, there is vortical motion of the material medium that "interpermeat[es] the spaces between molecular nuclei." The nature of this material medium was not precisely specified; it might be, for example, a "continuous fluid," or it might be a molecular fluid.[14]

The second major input to Thomson's growing conviction that he was now able to specify the "ultimate nature of magnetism" went back to Faraday's discovery, in 1845, of a magnetic action on light. Faraday had discovered that a beam of light propagating in a piece of glass situated in a magnetic field would experience a rotation of its plane of polarization.[15] What was particularly striking to Thomson was that the handedness of the rotation depended on the direction of propagation of the light ray: If a beam of light propagating in one direction through the magnetic field were to experience a right-handed rotation of its plane of polarization, then a beam propagating in the opposite direction would experience a left-handed rotation. The power of optical rotation previously known for certain media, such as sugar solutions, had a definite handedness – dextrorotary or levorotary, depending on the isomer – independent of the direction of propagation of the light beam; such optical rotation could be explained by assuming that the dissolved material consisted of tiny "spiral fibers," of definite handedness, which would rotate the plane of polarization with that given handedness. The Faraday rotation, however, could not be explained in this way, because its handedness was variable. Thomson argued that this could be explained only on the assumption that the magnetic line of force corresponds to an axis of rotation in the medium through which the light propagates: A definite sense of rotation in space would give opposite handedness when referred to opposite directions of propagation. This, in turn, provided for a connection with Rankine's theory of molecular vortices: Thomson concluded that the actual mechanical condition characterizing a region traversed by magnetic lines of force would be one in which the axes of the molecular vortices would all be aligned in one direction, that being the direction of the line of force.[16] A few years later, in a talk at the Royal Institution, Thomson asserted that "a certain alignment of axes of revolution in this [vortical] motion IS *magnetism*. Faraday's magneto-optic experiment makes this not a hypothesis, but a demonstrated conclusion."[17]

The payoff for that way of looking at magnetism, Thomson hoped, would be a unified and realistic theory of electromagnetic phenomena, developed from the field-theoretic point of view, and mechanically founded on the image of molecular vortices. Magnetic forces would be explained not as the result of the static interaction at a distance of magnetic poles or electric currents but rather dynamically, as a result of vortical motions in the intervening medium – the dynamical approach here allying with the field-theoretic outlook in a powerful and mutually reinforcing combination. Electromagnetic induction, Thomson believed, could also be explained in terms of molecular vortices, as involving their inertial resistance to changes in rotational velocity. Thomson's program was sketched only very briefly and vaguely in the paper of 1856, but the paper and the program spoke to Maxwell clearly enough.[18]

"Professor Thomson has pointed out that the cause of the magnetic action on light must be a real rotation going on in the magnetic field," Maxwell wrote approvingly; the time had come to go beyond the analogical approach and begin constructing, on the basis of Thomson's picture of molecular vortices oriented along magnetic field lines, something like the "mature theory" to which Maxwell had looked forward.[19] Maxwell had begun thinking along these lines by early 1857, and he appears to have embarked on the project with considerable enthusiasm.[20] From the outset, as indicated by both the language and the substance of his letters to friends and colleagues in 1857 and early 1858, Maxwell made it clear that this was to be a theory concerning the physical nature of the field, rather than just another illustrative analogy: He was "grinding . . . at a Vortical theory of magnetism," he wrote to Cecil Monro; he was currently concerned with "the physical nature of magnetic lines of force," he wrote to Faraday; and he was designing an apparatus intended to demonstrate the reality of magnetic rotations by direct mechanical means, a drawing of which he included in a letter to Thomson.[21] Maxwell's turn, in his work on the theory of molecular vortices, to an avowed concern with – as John Herschel had put it – "the actual structure or mechanism of the universe and its parts" was thus already evident in Maxwell's communication with colleagues at the outset and was to be even more clearly exhibited in the published work that followed.

Also evident at the outset, in the correspondence with Thomson, was that Maxwell was following Thomson in seeing the idea of molecular vortices as furnishing a possible connection between electromagnetic theory and the dynamical theory of heat: Thermodynamic issues, dealing with the conversion of motion to heat and the irreversibility of this process, came up in the correspondence, and Maxwell discussed the relevance of the molecular-vortex picture to these issues in a letter to Thomson. Thomson was committed to, and Maxwell was beginning to buy into, a vision of the theory of molecular vortices as a broad, comprehensive, deep theory of the type favored at Cambridge, unifying disparate areas in the manner described in Whewell's account of "consilience"; the impetus toward unification in Maxwell's thinking was to be clear also in the published work that followed, both in terms of the unification of electromagnetic theory itself and in terms of the assimilation of optics to electromagnetic theory. There was a gestation period of about four years before any of Maxwell's thoughts concerning molecular vortices saw print; the paper "On Physical Lines of Force" then appeared in a series of four installments over a period of eleven months in 1861–2.[22]

As Maxwell indicated in the introduction to Part I of "Physical Lines," the promise of Thomson's vision, with respect to the difficulties that had hindered the development of a serious electromagnetic theory, was twofold: The theory of molecular vortices promised comprehensiveness, and it promised explanatory power. One of the major limitations of the flow analogy in "Faraday's Lines" had been its inability to give any account of the connections and interactions among electric fields, magnetic fields, and electric currents. The molecular-vortex picture, on the other hand, gave promise of just that comprehensive and connected coverage that the flow picture lacked: "If, by the [molecular-vortex] hypothesis," Maxwell

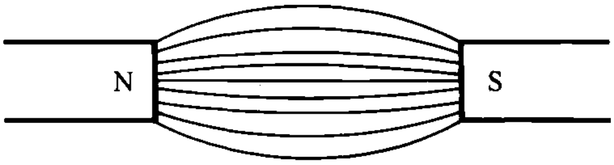

Figure 2.1. Magnetic lines of force running between unlike poles.

wrote, "we can connect the phenomena of magnetic attraction with electromagnetic phenomena and with those of induced currents, we shall have found a theory which, if not true, can only be proved to be erroneous by experiments which will greatly enlarge our knowledge of this part of physics." Thus, a theory having comprehensive coverage was not guaranteed to be true, but it was, by virtue of that comprehensive coverage, to be regarded as a serious candidate for truth; such a theory was to be regarded in any case as having truth value, whether true or false, in contradistinction to the flow analogy of "Faraday's Lines," which was to be regarded as neither true nor false, but only illustrative.[23]

The molecular-vortex representation was to be distinguished from the fluid-flow picture also in that the molecular-vortex representation was to be regarded as having explanatory power, whereas the fluid-flow representation did not. The issue of explanatory power was a central issue for Maxwell, and it is worth discussing at some length. Consider, for example, the case of two unlike magnetic poles, exerting mutual attractive forces on each other (Figure 2.1). As Faraday had conceptualized this situation in a paper entitled "On the Physical Character of the Lines of Magnetic Force" (1852) – this is clearly the immediate referent of the title of Maxwell's own paper – the lines of magnetic force behave as if they have a tendency to contract along their lengths and also to repel each other; acting in this way, they tend to pull the unlike magnetic poles together. The attribution of this behavior to the magnetic lines of force provides an explanation or account of the attraction between the unlike poles, in terms of the system of lines of magnetic force existing in the space surrounding the magnets. Faraday distinguished between a merely *geometrical* treatment of the lines of force, dealing descriptively with their distribution in space, and a *physical* treatment of the lines of force, dealing with their dynamical tendencies that give rise to the actual forces exerted. In the former context, "the term *magnetic line of force*" applied; in the latter context, one spoke of a "*physical line of [magnetic] force.*" Clearly, the titles of Maxwell's two papers reflect this usage, indicating at the outset the merely geometrical and descriptive character of the flow representation, as opposed to the physical and explanatory character of molecular vortices.[24]

In "Faraday's Lines," then, Maxwell had presented a mechanical representation of magnetic lines of force that adequately modeled the geometrical distribution of magnetic lines of force in space – this was given by the flow lines – but gave no purchase for understanding the forces of attraction or repulsion between magnetic poles. No tendencies of the magnetic lines to repel each other and contract along their lengths were derivable from the flow picture; consequently, magnetic forces could not be explained or accounted for by that picture. Maxwell had been explicit and insistent on this point: "By referring everything to the purely geometrical idea of the motion of an imaginary fluid, I hope to . . . avoid the dangers arising from a premature theory professing to explain the cause of the phenomena." Reviewing the matter in the introduction to "Physical lines," Maxwell stated again that in "Faraday's lines" he had been "using mechanical illustrations to assist the imagination, but not to account for the phenomena." Referring in a similar vein to Thomson's paradigmatic mechanical analogies of 1847, Maxwell observed that "the author of this method of representation does not attempt to explain the origin of the observed forces . . . but makes use of the mathematical analogies . . . to assist the imagination." Maxwell's avowed purpose in "Physical Lines," by contrast, was "to examine magnetic phenomena from a mechanical point of view, and to determine what tension in, or motions of, a medium are capable of *producing* the mechanical phenomena observed." He was seeking for a way in which "the observed resultant forces may be *accounted for.*" This was where the molecular-vortex representation showed its superiority: Assuming that the magnetic line of force represented the axis of a molecular vortex, it was easy to demonstrate that centrifugal forces would tend to make each vortex tube expand in thickness, thereby tending to increase the spacing between magnetic lines; at the same time, owing to the incompressibility of the fluid in the vortex tubes, those tubes would tend to shrink in length, giving the magnetic lines a corresponding tendency to contract along their lengths. This *physical* behavior of the magnetic lines was what was needed to *explain*, to *account for*, magnetic forces.[25]

The contrast between the illustrative mechanical analogy of "Faraday's Lines" and the explanatory mechanical theory of "Physical Lines" can be developed in a more formal manner. Consider a physical system described by a set of variables $\{F_i, G_i\}$, where the F_i are variables that represent observable mechanical forces, and the G_i are the other variables describing the system. A mechanical representation of that physical system would refer to a mechanical system represented by variables $\{f_j, g_j\}$, whose interrelationships were isomorphic to those of some subset $\{F_j, G_j\}$ of the $\{F_i, G_i\}$. (The larger the subset, the more complete the mechanical representation.) Whether that mechanical representation were to be construed as illustrative or explanatory would depend on the nature of the f_j: If the set $\{f_j\}$ were empty, or if the f_j were not themselves forces, then the mechanical representation would be merely illustrative; if, on the other hand, there were some f_j and they were forces, then the mechanical representation could be said to have explanatory power. In the molecular-vortex representation of "Physical Lines," for example, there are forces f_m, produced by the centrifugal forces of the rotating vortices, that are isomorphic to the forces F_m exerted in magnetostatic situations. In this case, the system of molecular vortices can be *identified with* the magnetic field, the forces f_m then being identified with the forces F_m, which is possible because they are variables of the same kind; the behavior of the molecular vortices, which explains the f_m, then also explains the F_m – that is, the theory of molecular vortices *explains* magnetic forces. In the fluid-flow representation of "Faraday's Lines," on the other hand, the set $\{f_j\}$ was empty: The variables relevant to magnetostatics, for example, were all of the g_j class – representing pole strength, field strength, and so forth – and no forces f_m corresponding to magnetostatic forces F_m were exhibited; therefore, there could be no explanation of magnetic forces by that mechanical representation – it could only be illustrative. In general, because the set $\{f_j\}$ was completely empty in the fluid-flow representation, that mechanical representation could have no explanatory power.[26]

Maxwell's assertion that the mechanical representation based on molecular vortices was explanatory rather than merely illustrative was thus no mere rhetorical device, but rather had a precise technical meaning. Because the mechanical representation based on molecular vortices was explanatory, and also because it provided comprehensive and connected coverage of the whole range of electromagnetic phenomena, Maxwell felt justified in referring to it as "the theory of molecular vortices." By calling it a theory, Maxwell indicated that it was definitely something more than a "mechanical illustration . . . to assist the imagination": It was at least a candidate for reality – perhaps "true" and perhaps "erroneous," but in any case not merely illustrative.[27]

3 *On the reality of molecular vortices*

Although the theory of molecular vortices was a candidate for reality, the strength of its candidacy was subject to vicissitudes. The four installments of "Physical Lines," taken together with other evidence from the period, furnish a rich record of the variations and nuances in Maxwell's views during the period in 1861–2 when he was working intensively on the theory. Parts I and II of the paper were published in March through May of 1861, and the nuances of Maxwell's views contained therein would appear to represent primarily a range of essentially coexisting sentiments, rather than a development in time. As discussed earlier, the tone of Part I was enthusiastic concerning the promise of the theory of molecular vortices, and guardedly optimistic concerning the theory's candidacy for reality. In Part II, however, there were crosscurrents. On the one hand, the basic "hypothesis of vortices" was characterized as a "probable" hypothesis. Also classified as probable was Maxwell's judgment concerning the sizes of the vortices: "The size of the vortices is . . . probably very small as compared with that of a complete molecule of ordinary matter." Maxwell went on to observe that although the precise sizes of the vortices could not be specified by electromagnetic measurements alone, they could be determined if one were able to measure directly the mechanical angular momentum carried by the vortices. That, in turn, "might be detected by experiments on the free rotation of a magnet," utilizing the type of apparatus that Maxwell had described in outline to Thomson back in 1858. Maxwell had indeed already "made experiments to investigate this question," but he had "not yet fully tried the apparatus" and did not report results.[28] It is clear, however, from Maxwell's continuing concern with this experiment, that in his estimation the vortices could well have been real enough to have had detectable angular momentum. He therefore continued to work on the experiment, but, as he complained to both Faraday and Thomson some months later, he had "not yet overcome the effects of terrestrial magnetism in marking the phenomenon."[29] Indeed, a successful measurement of the effect continued to elude Maxwell, as he reported retrospectively in the *Treatise on Electricity and Magnetism* (1873). His continuing efforts to detect the vortices, however, testified to their continuing candidacy for realistic status.[30]

Maxwell was not optimistic, on the other hand, concerning another part of the theory. In extending the theory to include electric currents (the procedures used to extend the theory are discussed in detail in Chapter 3), Maxwell had postulated that between the vortex cells there were interposed monolayers of small spherical particles, which rolled without slipping on the surfaces of the vortices (Figure 2.2), thus coupling the vortex rotations in the manner of "'idle wheel[s]'"; these were, moreover, to be regarded as *movable* idle wheels, and, "according to our hypothesis, an electric current is represented by the transference of the[se] moveable

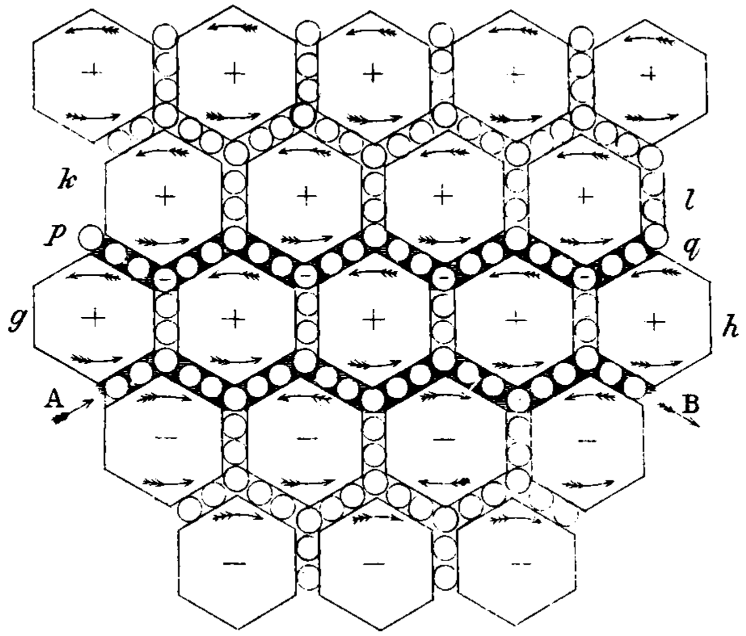

Figure 2.2. Vortex cells separated by monolayers of small spherical particles. (From Maxwell, "Physical Lines," Plate VIII, opposite p. 488.)

particles." The mathematics of these "moveable particles" turned out very neatly, but Maxwell nevertheless had to admit that the hypothesis could be regarded only as "provisional," of "temporary character." Indeed, the "conception" was "awkward," and Maxwell did "not bring it forward as a mode of connexion existing in nature, or even as that which [he] would willingly assent to as an electrical hypothesis." (Part of Maxwell's distaste for the movable particles probably stemmed from the fact that they constituted a sort of electrical fluid in his theory, and Maxwell, as a good follower of Faraday, believed in the primacy of the field and found the notion of electrical particles or fluids repugnant.) Maxwell, then, was careful to distinguish between a part of the theory that he regarded as a "probable" hypothesis and a good candidate for reality, and another part, which he regarded as a "provisional" hypothesis – a kind of placeholder in the theory – and a very poor candidate for reality. (Herschel, for example, had allowed for both kinds of hypotheses in his theory of scientific method.[31])

Part II of "Physical Lines" ended on a down note. As I shall argue in Chapter 3, Maxwell encountered difficulty in extending his theory to embrace electrostatics, and he did not anticipate, when he submitted Part II of the paper for publication, the triumphant further extension of the theory that he was to publish as Parts III and IV after an eight-month hiatus. (It would appear that the new breakthrough was accomplished in the summer of 1861, some months after the publication of Part II.) Maxwell thus closed Part II with what was clearly intended to be a final conclusion to the paper, and he evidently was not too happy with what he had wrought. Through the omission of electrostatics, the theory fell short of the comprehensive coverage for which he was striving, and he remained still at a disadvantage vis-à-vis Weber's more comprehensive theory. In a somewhat cynical closing statement, Maxwell observed that "those who [had] been already inclined" toward the field point of view might find his paper worthwhile. Others might be predisposed to "look in a different direction for the explanation of the facts" – clearly the reference was to Weber's action-at-a-distance theory – and Maxwell knew he was not going to win over those others with the incomplete theory he had presented. Unable, in that situation, to take a strong stand on behalf of his own theory, Maxwell had to content himself with taking a shot at Weber's theory: "Those who look in a different direction for the explanation of the facts, may be able to compare this theory with that . . . which supposes electricity to act at a distance with a force depending on its velocity, and therefore not subject to the law of conservation of energy." Thus, at that point, Maxwell was using the dynamical theory of heat and the associated conservation law not only as one of the foundations of his own theory but also as a normative principle on the basis of which to criticize alternative theories.

Continuing in a critical and dyspeptic vein, Maxwell summed up his own contribution as follows: "We have now shewn in what way electromagnetic phenomena may be imitated by an imaginary system of molecular vortices." This statement is clearly out of tune with Maxwell's stance in the rest of "Physical Lines"; it echoes, instead, Maxwell's tone of diffidence with respect to Weber in "Faraday's Lines." Maxwell had hoped to construct a theory that would rival Weber's in comprehensiveness; he had failed, and he was disappointed. Apparently, however, his disappointment spurred him on to further efforts, which were crowned with spectacular success.[32]

Part III of "Physical Lines" was published in January 1862, and there, by extending the theory to electrostatics and by providing an explanation of electrical forces in terms of stresses in the medium, Maxwell finally achieved the full comprehensiveness and explanatory character that he had sought. He was now able to "explain the condition of a body with respect to the surrounding medium when it is said to be 'charged' with electricity, and account for the forces acting between electrified bodies, [thereby] establish[ing] a connexion between all the principal phenomena of electrical science." The extension to electrostatics was accomplished by assigning elastic properties to the molecular vortices, making the system of vortices in space – the "magneto-electric medium" – capable of sustaining elastic waves. Using values of electrical parameters measured by Wilhelm Weber and Rudolph Kohlrausch – Weber was at this point being enlisted as an ally – Maxwell was able to calculate the velocity of elastic waves in the magnetoelectric medium, arriving at a value that agreed precisely with (in fact was bracketed by) existing measurements of the speed of light in air or vacuum. Maxwell's conclusion – the route to which will be discussed in detail in Chapter 5 – was that "we can scarcely avoid the inference that *light consists in the transverse undulations of the same medium which is the cause of electric and magnetic phenomena.*" Maxwell's use of italics was well justified: he, and soon his colleagues, judged this to be a result of immense consequence, and historical perspective has reinforced that assessment (see further Chapter 6).[33]

The result was also of the greatest importance as concerned the status of the theory of molecular vortices. First, given the widespread belief – at Cambridge and beyond – in the real existence of the luminiferous medium, the identification of the luminiferous and magnetoelectric media became an argument for the real existence of the magnetoelectric medium, with its vortex structure. At the level of particulars, certain features of the vortices – their elasticity, for example – were no longer ad hoc to electromagnetic theory, but rather could be seen as the natural extension of a theoretical structure already in place, namely, the wave theory of light. Relatedly, the generality and broad range of the resulting unified theory of electromagnetism and optics argued powerfully for the physical significance of the theory of molecular vortices. Indeed, this was a most spectacular example of Whewellian consilience. Moreover, as Maxwell stressed in a letter announcing the new results to Faraday, the new, unified theory predicted various relationships between electromagnetic and optical phenomena, experimental verification of which would help to establish the truth value of the theory by demonstrating its predictive power (a criterion emphasized by both Herschel and Whewell): "The conception I have hit on has led, when worked out mathematically, to some very interesting results, capable of testing my theory, and exhibiting numerical relations between optical, electric, and electromagnetic phenomena, which I hope soon to verify more completely." In a parallel letter to Thomson, Maxwell acknowledged Thomson's seminal role in the development of the theory of molecular vortices, sketched the application of the theory of molecular vortices to electricity and magnetism, making clear the comprehensiveness and explanatory character of the theory, and discussed the experimental basis and implications of the unification with optics. In all three announcements of the new results – the public one in "Physical Lines" and the private ones to Faraday and Thomson – Maxwell's confidence in the theory of molecular vortices, now with a vastly enhanced range of applicability, was in marked contrast to his earlier vacillations. Further arguments for the reality of molecular vortices were to be presented in the final installment of "Physical Lines," which appeared one month later.[34]

4 *The mathematics and physics of linear and rotatory vectors*

Maxwell's most focused argument in favor of the reality of molecular vortices was given in Part IV of "Physical Lines." It was a mathematical argument, but it was not presented as pure and abstract mathematics; it was, rather, according to Maxwell's enduring commitment, "embodied mathematics" – mathematics represented in mechanical examples.[35] The central mathematical relationship on which Maxwell focused was that exemplified in the relationship between electric current and magnetic field, that is, Ampère's circuital law in differential form:

$$\begin{aligned} p &= \frac{1}{4\pi}\left(\frac{d\gamma}{dy} - \frac{d\beta}{dz}\right) \\ q &= \frac{1}{4\pi}\left(\frac{d\alpha}{dz} - \frac{d\gamma}{dx}\right) \\ r &= \frac{1}{4\pi}\left(\frac{d\beta}{dx} - \frac{d\alpha}{dy}\right) \end{aligned} \tag{2.1}$$

where α, β, γ are the Cartesian components of the vector representing magnetic field intensity, p, q, r are the components of the vector representing electric current density, and the differential operators d/dx, d/dy, d/dz represent partial differentiation with respect to Cartesian coordinates x, y, z (Maxwell did not use a special symbol for partial differentiation). In order to illustrate the deeper meaning of this equation, Maxwell listed a series of mechanical examples to which this equation could be applied: (1) If α, β, γ represents linear displacement or change of location, then p, q, r represents rotatory displacement or change of location. (2) If α, β, γ represents linear velocity, then p, q, r represents rotational velocity. (3) If α, β, γ represents a force or push, then p, q, r represents a torque or twist. Common to these examples is the circumstance that α, β, γ has "*linear* . . . character," representing a motion or a thrust in a certain direction, whereas p, q, r has "*rotatory* character," representing a rotation or a twist about a certain axis. Further mechanical examples exhibited the inverse relationship: (4) If α, β, γ represents rotatory displacement or motion in a continuous medium, then p, q, r represents linear displacement or relative motion in that medium. (5) If α, β, γ represents the rotational velocities (angular velocities) of the vortices in Maxwell's theory, then p, q, r represents the averaged linear flow density of the idle-wheel particles postulated in that theory. In these examples, it is α, β, γ that represents a rotatory motion about some axis, whereas p, q, r represents an associated linear motion. Maxwell's conclusion from the two sets of instances was that equations (2.1) in general represent a kind of relationship that obtains "between certain pairs of phenomena, of which one has a *linear* and the other a *rotatory* character"; if α, β, γ is linear, then p, q, r is rotatory, and if α, β, γ is rotatory, then p, q, r is linear.[36]

The content and character of Maxwell's argument can be highlighted by comparison and contrast with a modern treatment of the same issue. Modernly, and in fact building on Maxwell's continuing work in this area, one maintains his distinction between two kinds of vectors: Maxwell's linear quantities are now denoted true vectors or polar vectors, and Maxwell's rotatory quantities are now designated pseudovectors or axial vectors. The distinction between these two kinds of vector quantities is, however, established in a modern treatment on the basis of their transformation characteristics – in particular with respect to spatial reflection – rather than by reference to mechanical examples. Maxwell, conversely, relied exclusively on mechanical examples to establish the distinction and to illustrate the relationship between the two kinds of vectors, as in equations (2.1).[37] Maxwell was, after all, a mechanical philosopher: not a mechanical philosopher of the eighteenth-century type, with their qualitatively distinct subtle matters, but a characteristic British mechanical philosopher of the post-1850 period, who espoused an ontology of matter and motion in which "all matter must in itself be the same, and can be modified only by differences of arrangement and motion and by being actuated by different systems of force." Given that ontology, the reality underlying the electromagnetic field had to be mechanical, and the equations describing electromagnetic relationships, ultimately, were the expression of mechanical conditions. Mechanically, equations (2.1), by virtue of their mathematical structure, had to represent the relationship between a linear mechanical motion or force and a rotational motion or torque. Either magnetism was rotational and electric current linear, or magnetism was linear and electric current rotational. The mathematical relationship of equations (2.1), taken together with the mechanical ontology, guaranteed this; all that remained was to make a decision between the two possibilities.[38]

The way to tell whether a given phenomenon had linear or rotatory character, Maxwell suggested, was to look at its effects: "All the direct effects of any cause which is itself of a longitudinal character, must themselves be longitudinal, and . . . the direct effects of a rotatory cause must be themselves rotatory." Maxwell proceeded to inventory the effects of electric currents, to judge whether they were to be classified as linear or rotatory in character. In the first place, Maxwell observed, "electric currents are known to produce effects of transference in the direction of the current." In the electrolysis of water, for example, hydrogen is moved in one direction along the current line, and oxygen in the other, thus indicating a linear character for the electric current; there was no known rotational effect of electric current (Faraday had in fact searched for such an effect but found none), so that Maxwell felt confident in characterizing the electric current as linear. Given the previous argument concerning equations (2.1), this result was sufficient to demonstrate not only that electric current was linear in character but also that magnetism was rotatory; any additional information bearing directly on the rotatory character of magnetism would then introduce a reassuring redundancy into the argument.[39]

As directly concerned the nature of magnetism itself, Maxwell first argued that magnetism produced no known linear effects. (The magnetic lines of force and their actions on magnetic poles appear to be linear, but, as Maxwell argued, some phenomenon, such as electrolysis in the electrical case, where the opposite ends of the line of action are physically distinguished, is needed to establish that the line is not merely an axis of rotation; no such phenomenon, however, had been found for the magnetic line of force. This is a subtle point, which still bedevils students.) Magnetism did, however, produce a rotatory effect, namely, the Faraday rotation – "the rotation of the plane of polarized light when transmitted along the lines of magnetic force." That, of course, was the phenomenon that had provided the basis for the whole line of thinking that Maxwell was pursuing. Maxwell acknowledged Thomson's role in "point[ing] out that the cause of the magnetic action on light must be a real rotation going on in the magnetic field." Also invoked was Thomson's argument concerning the handedness of the Faraday rotation, leading to the conclusion that "the direction of rotation is directly connected with that of the magnetic lines, in a way which seems to indicate that magnetism is really a phenomenon of rotation."[40]

What Maxwell had added to Thomson's original argument for the reality of magnetic rotations was a fourfold redundancy: Given Maxwell's argument concerning the mathematical character of equations (2.1) and their associated mechanical significance, four kinds of evidence converged on the conclusion that magnetism was rotational: (1) linear effects of electric current, as in electrolysis; (2) lack of rotatory effects of electric current, as generally established and as further tested in Faraday's experiments; (3) lack of linear effects of magnetism, as generally established and as supported by Maxwell's argument concerning the lack of any physical distinction between the two ends of a magnetic line of force; and (4) rotatory effects of magnetism, as established experimentally by Faraday and as interpreted theoretically by Thomson. Maxwell added further redundancy to the argument in favor of the rotational character of magnetism by enlisting even the views of the action-at-a-distance theorists – Ampère and Weber, in particular – to the effect that electric currents involved linear transport of electric charge, whereas magnetism was a manifestation of current loops.[41]

The strength and weakness of this argument in Part IV of "Physical Lines" for the rotatory character of magnetism was its generality, its independence of the specifics of the theory of molecular vortices. The details of the theory of molecular vortices were not supported by this argument, and where those details were questionable – as in the case of the idle-wheel particles – they remained questionable. On the other hand, as this argument was not tied to those details, it could retain its general force for Maxwell even when he backed away from the details. In fact, as will become apparent in the sequel, Maxwell never did give up the belief that there was "a real rotation going on in the magnetic field." Given the mechanical ontology, the mathematics of linear and rotatory vector quantities, and the experimental evidence concerning the linear and rotatory characteristics, respectively, of electric currents and magnetic fields, the conclusion for "a real rotation" was simply unavoidable for Maxwell.[42]

5 *The decline of the theory of molecular vortices*

After the publication of "Physical Lines," Maxwell began a measured retreat from the mechanical concreteness and detail that characterized his presentation of the theory of molecular vortices there. His next major paper on electromagnetic theory, "A Dynamical Theory of the

Electromagnetic Field" (1864–5), exemplified this trend in his thinking. Clearly, dissatisfaction with the weakest link in the theory – the idle-wheel particles – was a crucial reason for this retreat. There were, however, other reasons, having to do with the broader development of Maxwell's research program: His research on gas theory, in the 1860s, took a direction that had negative implications concerning molecular vortices, and his concern to develop the electromagnetic theory of light in a manner that would be acceptable to a broader audience led also to a deemphasis on molecular vortices. The result of these was to redirect Maxwell's efforts in electromagnetic theory toward a more phenomenological emphasis, without, however, engendering either a return to the analogical approach or a complete loss of faith in the reality of molecular vortices.[43]

The original stronghold of the hypothesis of molecular vortices had been the theory of heat and gases, and one of the most attractive features of Thomson's suggestion, in 1856, that the hypothesis of molecular vortices be applied to electromagnetic phenomena was the broad unification this promised. In the later 1850s, however, most notably as a result of the work of Rudolph Clausius, the idea that the motion of gas molecules giving rise to gas pressure was translatory – characteristically in straight lines – rather than rotatory began to look more and more appealing. Maxwell's initial response to this situation, in the years around 1860, was to maintain his primary allegiance to molecular vortices, employing them as the basis for a physical theory in "Physical Lines," while utilizing the linear-motion picture as the basis of a physical analogy in gas theory, with no commitment to it as a realistic representation of nature. In this way, conflict between the respective requirements of electromagnetic theory and gas theory was minimized. This accommodation, however, proved to be unstable.[44]

In the course of the 1860s, Maxwell's investment in and commitment to the linear picture – that is, to what has been ever since the standard kinetic theory of gases – increased substantially. Exhibiting the same kind of progression from an analogical stage to a theoretical stage that we have already seen in his electromagnetic theory, Maxwell in 1866 published what he was prepared to call a "Theory of Gases"; once again, just as in the electromagnetic case, the step to the theoretical stage was justified by the comprehensiveness and explanatory character of the given mechanical representation. The methodological progression in Maxwell's gas theory thus mirrored quite faithfully the methodological development of his electromagnetic theory; as far as content was concerned, however, the direction in which Maxwell's gas theory was developing in the first half of the 1860s tended to undermine the foundations of his electromagnetic theory. Not that there was any direct conflict between the gas theory and the electromagnetic theory – molecular vortices in the ether were perfectly consistent with translational motion of gas molecules – but the grand synthesis on the basis of molecular vortices as originally envisaged by Thomson appeared to have been ruled out, and some of the appeal of applying molecular vortices to electromagnetic phenomena was thereby lost.[45]

In part, it was the very success of the theory of molecular vortices that led to its downfall. This theory had provided the context for a unified treatment of electromagnetism and optics on the basis of the mechanics of one universal medium or ether. The further development and consolidation of this unification of electromagnetism and optics then became the new focus of Maxwell's continuing research. On the experimental side, he became quite productively involved in work on electrical measurements and standards, oriented toward more precise determination of the ratio of electrical units, on the basis of which the connection with light had been established. On the theoretical side, he worked toward the establishment of a more direct connection between electromagnetism and optics. The original connection between the two, as established in "Physical Lines," has aptly been designated an "electro-mechanical"[46] theory of light, rather than an electromagnetic theory of light: In "Physical Lines," Maxwell had argued from electromagnetic phenomena, by way of the theory of molecular vortices, to the mechanical properties of the magnetoelectric medium; then, from the mechanical properties of that medium, Maxwell had deduced the propagation of transverse elastic waves in it, with all the characteristic properties of light waves. Early on, however, Maxwell surmised that a more direct and theoretically parsimonious establishment of this result should be possible, and a more direct argument, "cleared . . . from all unwarrantable assumption," was likely to be more palatable to the Continental action-at-a-distance electricians, who were skeptical of the whole field-primacy approach of Faraday, Thomson, and Maxwell. Maxwell was in fact successful in devising a more parsimonious argument, which proceeded directly from the electromagnetic equations – as appropriately modified – to the calculation of electromagnetic waves propagating at the velocity of light. This, finally, was truly an "electromagnetic theory of light," and it formed the centerpiece of Maxwell's paper "A Dynamical Theory of the Electromagnetic Field."[47]

In this paper, then, in consonance with the exigencies of his research programs in both gas theory and electromagnetic theory, Maxwell retreated from the specifics of the theory of molecular vortices, but not from the general framework. He still insisted on the existence of a mechanical medium in space that was both the carrier of light waves and the seat of electric and magnetic fields; he was still willing to propose, as a "very probable hypothesis," that magnetic and electric fields were manifestations respectively of "motion" and "strain" in that medium; and he still judged that the Faraday rotation gave "reason to suppose that th[e] motion [underlying the magnetic field was] one of rotation, having the direction of the magnetic force as its axis." Beyond this Maxwell was not willing to go, and he did not even use all of this in developing the mathematical theory. Basically, all that Maxwell used were the equations of electromagnetic phenomena as established by experiment, together with the assumption that these reflected conditions in a connected mechanical medium pervading space and capable of storing, exchanging, and transmitting kinetic and potential energy. This warranted Maxwell in treating the field variables (and other electromagnetic variables) as generalized mechanical variables, in the sense of the Lagrangian formalism. The result was a "dynamical theory" of the electromagnetic field, which was still a mechanical theory, but abstract and general rather than concrete and pictorial.[48]

6 *Molecular vortices in the* Treatise on Electricity and Magnetism

Maxwell's primary methodological commitment in the later 1860s and the 1870s was to the dynamical approach, which abjured completely the use of concrete mechanical images; the dynamical approach was fully developed and centrally positioned in the *Treatise on Electricity and Magnetism* (1873). The *Treatise*, however, was intended to be a comprehensive work, treating all aspects of electromagnetic phenomena, and some of these were not amenable to a treatment by macroscopic dynamical theory, without any assumption as to mechanical details. In particular, an entire chapter was devoted to the Faraday rotation (important because of its bearing on the electromagnetic theory of light), and here molecular vortices played a central role. Maxwell began his analysis – characteristically for the *Treatise* – by "consider[ing] the dynamical condition[s]" attendant upon the Faraday rotation, that is, by applying the Lagrangian formalism; he arrived thereby at the following – by now familiar – result, stated in the abstract terminology characteristic of that formalism: "the consideration of the action of magnetism on polarized light leads . . . to the conclusion that in a medium under the action of magnetic force something belonging to the same mathematical class as an angular velocity, whose axis is in the direction of the magnetic force, forms a part of the phenomenon." He then proceeded to interpret this result in the light of his mechanical ontology: Given a universe of matter and motion, the "something belonging to the same mathematical class as an angular velocity" must in fact *be* an angular velocity of some rotating portion or portions of the medium filling space. Experiment indicated that no sizable angular momenta were associated with these rotations, so the rotating portions of the medium had to be small, and the conclusion was that "we must therefore conceive the rotation to be that of very small portions of the medium, each rotating on its own axis." "This," once again, was "the hypothesis of molecular vortices"; Maxwell used the hypothesis basically in this unadorned form, not fleshing it out as he had in "Physical Lines."[49]

In a final summary of his ultimate views concerning the reality of molecular vortices, Maxwell again invoked the distinction that he had made in "Physical Lines" between the status of the vortices themselves – they had been a "probable" hypothesis – and the status of the system of idle-wheel particles that coupled the motions of the vortices – this had been a "provisional," "temporary," and "awkward" hypothesis. Concerning the vortices themselves, the "probable" hypothesis, Maxwell had this to say in the *Treatise:* "I think we have good evidence for the opinion that some phenomenon of rotation is going on in the magnetic field, that this rotation is performed by a great number of very small portions of matter, each rotating on its own axis, this axis being parallel to the direction of the magnetic force, . . ." Concerning this part of the theory of molecular vortices, then, Maxwell was fully as sanguine in the *Treatise* as he had been in "Physical Lines"; his language in the *Treatise* – "I think we have good evidence for the opinion that . . ." – was, if anything, a bit stronger than in "Physical Lines."[50]

Concerning the *existence* of a mechanism coupling the motions of the individual vortices, there was also good evidence: "I think we have good evidence . . . that the rotations of the . . . different vortices are made to depend on one another by means of some kind of mechanism connecting them." The *particular* connecting mechanism envisaged in "Physical Lines" – that is, the system of idle-wheel particles – was, on the contrary, not to be taken seriously:

> The attempt which I then made to imagine a working model of this mechanism must be taken for no more than it really is, a demonstration that mechanism may be imagined capable of

> producing a connexion mechanically equivalent to the actual connexion of the parts of the electromagnetic field. The problem of determining the mechanism required to establish a given species of connexion between the motions of the parts of a system always admits of an infinite number of solutions.

This agnostic statement notwithstanding, Maxwell observed that certain things were more definitely known even about the connecting mechanism: "Electromotive force arises from the stress on the connecting mechanism [and] electric displacement arises from the elastic yielding of the connecting mechanism." Thus, certain *general* mechanical properties of the connecting mechanism were known, along with their relationships to electromagnetic phenomena.[51]

An infinite number of different mechanisms could be imagined that would fulfill these specifications, and Maxwell clearly did not entertain the hope that one could ever determine which of these was the "actual connexion" existing in nature. The effort that Maxwell had made in "Physical Lines" to envisage a concrete example of such a mechanism had not, however, been a worthless exercise; it had provided a "demonstration that mechanism may be imagined capable of" fulfilling the given specifications. Such a concrete mechanism, not realistically intended, but intended instead to show that a mechanism of the sort required was possible, was called by Maxwell a "working model." A working model is similar to a physical analogy in that it is a concrete and pictorial mechanical representation, with imaginary rather than realistic status. There is, however, an important difference: The working model must be able to *produce* the mechanical effect in question – that is, it must be a model that really *works*, in the sense of accomplishing the effect. The working model furnishes a possible explanation of the effect, just because it is able to produce the effect. One may judge it highly improbable that the given working model faithfully represents the details of the actual situation, either because the working model is manifestly awkward or artificial, or simply because one knows that there is an infinite number of possible working models, so that the a priori probability that a given one is the true one is vanishingly small. Nevertheless, the working model is "capable of producing" the observed effects, and in that sense represents a *possible* explanation. A physical analogy, however, will in general not represent even a possible explanation, because the mechanical system envisaged is not capable of producing the phenomenon in question. (In the formal language introduced earlier, a working model is described by variables $\{f_i\}$ that are forces, whereas a physical analogy is not.) Thus, even as concerned the connecting mechanism – the weakest part of the theory of molecular vortices – Maxwell was not retreating back to the physical analogy stage.[52]

To sum up Maxwell's final position concerning the theory of molecular vortices, he regarded part of the theory as a hypothesis for which "we have good evidence," and part of the theory as a "working model." In order to get a comprehensive theory of electromagnetic phenomena, one would have to put the two parts together; evaluating the resultant theory by its weakest link, one would have to characterize the whole as merely a "working model." Maxwell, however, chose to maintain the separation of the two parts in his characterization of the status of the theory, thus highlighting the strong and continuing commitment he had to the reality of the central core of the theory, the vortices themselves.[53] In addition, Maxwell judged certain general mechanical features of the theory to be firmly established throughout, including both the vortices and the connecting mechanism.

> The following results of the theory, however, are of higher value:
>
> (1) Magnetic force is the effect of the centrifugal force of the vortices.
>
> (2) Electromagnetic induction of currents is the effect of the forces called into play when the velocity of the vortices is changing.
>
> (3) Electromotive force arises from the stress on the connecting mechanism.
>
> (4) Electric displacement arises from the elastic yielding of the connecting mechanism.[54]

Maxwell's popular lectures and writings through the 1870s continued to echo his judgment in the *Treatise* that the core of the molecular-vortex theory was still sound. In a talk at the Royal Institution "On Action at a Distance," Maxwell argued that "strict dynamical reasoning" demonstrated the existence of "molecular vortices . . . rotating, each on its own axis," with "magnetic force[s] . . . aris[ing] from the centrifugal force of the . . . vortices." In his article "Ether" for *Encyclopaedia Britannica,* Maxwell's message was similar: "Sir W. Thomson has shewn" that there is "a rotational motion in the medium when magnetized," which "must be a rotation of very small portions of the medium each about its own axis, so that the medium must be broken up into a number of molecular vortices." Finally, in the article "Faraday" for *Britannica,* Maxwell observed that the discovery of the Faraday rotation, though it had not led to much in the way of "practical application," had nevertheless been "of the highest value to science, as furnishing complete dynamical evidence that wherever magnetic force exists there is matter, small portions of which are rotating about axes parallel to the direction of that force." To the end, then, Maxwell maintained his allegiance to the image of whirling vortices as the basis of the magnetic field.[55]

Conclusion

Viewed over the course of his entire career as a theorist of electricity and magnetism, Maxwell's use of mechanical representation was varied and pluralistic, reflecting the various formative influences on his work as well as the developing needs of his research program. The Scottish emphasis on analogy, rooted in a combination of Baconian empiricism and Common Sense philosophical sophistication, and transmitted through James D. Forbes, William Hamilton, and William Thomson, was evident in Maxwell's initial use of the method of physical analogy. Then, with growing confidence in the foundation of his work, Maxwell embraced the hypothetical method of John Herschel, William Whewell, and the Cambridge school; in this context, Maxwell utilized the hypothesis of molecular vortices as the foundation for a broad and ramified "physical theory" of electricity and magnetism, put forward with considerable realistic intent. Finally, there set in a period of disillusionment concerning at least certain aspects of the theory of molecular vortices, resulting in a measured retreat from that kind of detailed and explicit mechanical theorizing; in this final phase of Maxwell's work, a limited and nuanced continuing reliance on certain aspects of the theory of molecular vortices coexisted with a predominant emphasis on a more abstract variety of mechanical theory – based on the Lagrangian formalism – that completely renounced the use of concrete mechanical images. If one were to look only at Maxwell's starting and ending points, one would conclude that he took primarily a skeptical stance toward mechanical representation. That, however, would be to ignore the middle period, which was the period of strong mechanical commitment, and also the period of intensive innovation in Maxwell's electromagnetic theory.

Maxwell's turn away from concrete mechanism in the later 1860s and 1870s – echoing his initial skepticism of the 1850s – was of great consequence for the subsequent methodological and foundational development of physical science: Maxwell's turn away from mechanical models was one of the precipitating events in the decline of the mechanical worldview and the transition to the more abstract physical formalisms of the twentieth century. Consequently, much historical and philosophical analysis has emphasized this skeptical element in Maxwell's approach to mechanical models.[56] The other aspect of Maxwell's approach to mechanism – the strong commitment to the molecular-vortex model in the middle period – has received less attention. Indeed, viewed from a twentieth-century perspective, the molecular-vortex model may appear to be "bizarre" and "outlandish," and hence not worthy of serious attention.[57] Maxwell, however, took that model very seriously at the time, and so must we if we are to fully understand his work. In particular, the two major innovations in Maxwell's electromagnetic theory – the displacement current and the electromagnetic theory of light – received their initial formulations in the context of the molecular-vortex model. If we are to understand the origins of these crucial novelties, in terms of the context of nineteenth-century mechanical commitment that gave birth to them in Maxwell's work, it will be necessary to look more closely at the details of the construction of the molecular-vortex model.