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Maxwell, Measurement, and the Modes of Electromagnetic Theory

ABSTRACT

In 1861–62, James Clerk Maxwell published "On Physical Lines of Force," in which he laid out a detailed mechanical model of the ether and argued that it could account not only for electromagnetic phenomena but for light as well. In 1864, he followed with "A Dynamical Theory of the Electromagnetic Field," in which he derived the electromagnetic equations from general dynamical considerations without invoking any mechanical model of the ether. Why the shift? Did Maxwell regard his mechanical model as mere scaffolding, to be cast aside once it had led him to the proper field equations? Or did he remain committed to the goal of a purely mechanical explanation, but find it useful to free his main results, particularly his electromagnetic theory of light, from dependence on the specifics of an admittedly speculative model? To understand the apparent shift Maxwell's thinking underwent between 1862 and 1864, I propose that we look closely at what he was doing in 1863. He spent that year working hard for the British Association Committee on Electrical Standards, collaborating with telegraph engineers to establish the value of the ohm and laying the groundwork for measuring the ratio of electrostatic to electromagnetic units, a key quantity in his electromagnetic theory of light. This experience led Maxwell to adopt for a time an engineering approach that focused on establishing relationships between measureable quantities rather than devising hypothetical mechanisms. Maxwell's electromagnetic work thus had closer ties to the technological context of the day than has generally been recognized.

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The following abbreviations are used: *PM*, *Philosophical Magazine*; *SP*, James Clerk Maxwell, *Scientific Papers of James Clerk Maxwell*, ed. W. D. Niven, 2 vols. (Cambridge: Cambridge University Press, 1891); *SLP*, James Clerk Maxwell, *Scientific Letters and Papers of James Clerk Maxwell*, ed. P. M. Harman, 3 vols. (Cambridge: Cambridge University Press, 1990–2002); *Treatise*: James Clerk Maxwell, *Treatise on Electricity and Magnetism* (Oxford: Clarendon Press, 1873), which will be cited by article number.

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One of the classic questions in the history of nineteenth-century physics centers on James Clerk Maxwell's attitude toward his mechanical models of the ether. Did he look on them as no more than heuristic tools, to be tossed aside once they had helped him find the proper field equations, or did he instead regard them as steps toward a realistic representation of the actual structure of the electromagnetic medium? In his long paper "On Physical Lines of Force," published in installments in 1861–62, Maxwell laid out an elaborate model of the ether, picturing it as an array of tiny spinning vortices interspersed with layers of even smaller "idle wheel" particles.[1] He showed that such a vortex medium could reproduce the main phenomena of electricity and magnetism, including the production of magnetic fields and the induction of electric currents, and could also convey transverse waves very much like—perhaps identical with—those of light. "Physical Lines" marked a major step in the development of field theory and the unification of optics with electromagnetism; with it, Maxwell appeared to be well on his way toward delineating the real mechanical structure of the electromagnetic ether. Yet just over two and a half years later, he sent the Royal Society of London his "Dynamical Theory of the Electromagnetic Field," in which he seemingly abandoned his vortex model and instead derived the equations of the electromagnetic field from general dynamical principles, without invoking any hypothetical mechanical microstructure.[2] Why the shift? Had Maxwell really renounced his vortex model? Had he ever really believed in it, or had he always regarded it as mere scaffolding, to be cast aside when no longer needed? Why had he taken one approach to electromagnetic theory in 1862, and such a seemingly different one in 1864? What does this sequence of moves by Maxwell tell us about the roots and development of his theory of the electromagnetic field, as well as about the deeper attitudes of Victorian physicists toward the nature of physical reality and the means by which they might best seek to grasp and describe it?

1. James Clerk Maxwell, "On Physical Lines of Force," *PM* 21 (1861): 161–75, 281–91, 338–48, and 23 (1862): 12–24, 85–95, repr. in *SP* 1: 451–513.

2. James Clerk Maxwell, "A Dynamical Theory of the Electromagnetic Field," *Philosophical Transactions of the Royal Society of London* 155 (1865): 459–512, repr. in *SP* 1: 526–97.

To understand the apparent shift Maxwell's thinking underwent between 1862 and 1864, we should, I will argue, look closely at what he was doing in 1863. It turns out he spent much of that year, and some months both before and after, working hard for the British Association Committee on Electric Standards, establishing the value of the ohm, clarifying the relationships among electrical measurements, and laying the groundwork for a careful experimental determination of the ratio of electrostatic to electromagnetic units. Maxwell's close collaboration in this period with Fleeming Jenkin and other telegraph engineers led him to adopt, at least for a time and for the purposes at hand, an "engineering approach" to electrical questions in which he focused not on devising hypothetical mechanisms but on formulating demonstrable relations between quantities he could measure and manipulate. Maxwell was not wedded to just one way of doing physics. Sometimes he found it useful to devise hypothetical microscopic mechanisms and trace out their consequences, with hopes of penetrating to the real mechanical substructure of the physical world; other times, he sought instead to formulate macroscopic laws that would be independent of such hypotheses.[3] These shifts did not represent his abandonment of one approach or the other, but rather his attempts to advance scientific understanding at different times along different fronts. Maxwell's work on the British Association Committee shaped his thinking not only about the ohm and electrical measurement, but also about the range of analytical approaches and expository strategies that could be useful in physics, and it became a significant thread not just in his "Dynamical Theory" but also in his later *Treatise on Electricity and Magnetism* (1873) and other works.

MODELS AND STANDARDS

Over the years, many historians, philosophers, and physicists have discussed a sequence of three papers on electromagnetism that Maxwell published between the mid-1850s and the mid-1860s:

3. Note that while the mechanism of the electromagnetic medium that Maxwell discussed in "Physical Lines" was characterized by equations of continuum mechanics that had the same form as the equations used for macroscopic bodies, it was differentiated microscopically by the motion of its parts, as was the version of the kinetic theory of gases Maxwell was developing at the same time; cf. M. Norton Wise, "The Maxwell Literature and British Dynamical Theory," *HSPS* 13, no. 1 (1982): 175–205, esp. 188–89 and 200–201.

1856: "On Faraday's Lines of Force," in which he laid out a fluid-flow analogy to the distribution and interaction of lines of electric and magnetic force;

1861–62: "On Physical Lines of Force," in which he presented his vortex and idle wheel model of the electromagnetic ether and introduced the first "electromechanical" version of his electromagnetic theory of light;

1864: "A Dynamical Theory of the Electromagnetic Field," in which he formulated a set of electromagnetic field equations, including a fully electromagnetic theory of light, based on the general dynamics of a connected system, independently of any detailed model of the medium.

The move from "Physical Lines" to "Dynamical Theory" has drawn particular attention, and it will be my main focus here.[4] The key question has been whether Maxwell believed his vortex model represented the real structure of the ether, at least in part, or instead regarded it as no more than a convenient fiction he could use to help him find the field equations that, on this account, were always his real and final goal.

The evidence pulls in two directions. In "Physical Lines," Maxwell certainly spoke of the vortices very much as if he regarded them as real. Citing Michael Faraday's 1845 discovery of magneto-optic rotation, he declared that William Thomson's 1856 analysis of it proved that "the cause of the magnetic action on light must be a real rotation going on in the magnetic field," and he thought it sufficiently likely that the rotation was performed by tiny "molecular vortices" that, in 1861, he had a special apparatus built with which he tried to measure

4. The sequence from "Faraday's Lines" to "Physical Lines" to "Dynamical Theory" was first emphasized by W. D. Niven in his preface to *SP* 1: xix–xxii. Among subsequent discussions, see Joseph Turner, "Maxwell on the Logic of Dynamical Explanation," *Philosophy of Science* 23 (1956): 36–47; Robert Kargon, "Model and Analogy in Victorian Science: Maxwell's Critique of the French Physicists," *Journal of the History of Ideas* 30 (1969): 423–36; C.W.F. Everitt, *James Clerk Maxwell: Physicist and Natural Philosopher* (New York: Scribner's, 1975), 108–9; Ole Knudsen, "The Faraday Effect and Physical Theory, 1845–1873," *Archive for History of Exact Sciences* 15 (1976): 235–81, on 248–55; Daniel Siegel, "The Origin of the Displacement Current," *HSPS* 17, no. 1 (1986): 99–146; Daniel Siegel, *Innovation in Maxwell's Electromagnetic Theory: Molecular Vortices, Displacement Current, and Light* (Cambridge: Cambridge University Press, 1991); P. M. Harman, *The Natural Philosophy of James Clerk Maxwell* (Cambridge: Cambridge University Press, 1998), 71–90, 98–124; Crosbie Smith, *The Science of Energy: A Cultural History of Energy Physics in Victorian Britain* (Chicago, IL: University of Chicago Press, 1998), 218–38; Bruce J. Hunt, *Pursuing Power and Light: Technology and Physics from James Watt to Albert Einstein* (Baltimore, MD: Johns Hopkins University Press, 2010), 94–110.

their expected gyroscopic effect.[5] The experiment was inconclusive; terrestrial magnetism interfered with the expected effect and prevented Maxwell from establishing more than that the vortices, if they existed at all, must be extremely small. He still believed the magneto-optic evidence to be very strong, however, and he continued to speak of the existence of the vortices as highly probable. On the other hand, he was much more tentative about the idle wheel particles, presenting them as no more than a concrete and readily investigated way to connect the rotations of adjacent layers of vortices. As he acknowledged near the end of the second part of "Physical Lines,"

> The conception of a particle having its motion connected with that of a vortex by perfect rolling contact may appear somewhat awkward. I do not bring it forward as a mode of connexion existing in nature, or even as that which I would willingly assent to as an electrical hypothesis. It is, however, a mode of connexion which is mechanically conceivable, and easily investigated, and it serves to bring out the actual mechanical connexions between the known electro-magnetic phenomena; so that I venture to say that any one who understands the provisional and temporary character of this hypothesis, will find himself rather helped than hindered by it in his search after the true interpretation of the phenomena.[6]

This passage has often been cited as evidence that Maxwell regarded his entire vortex model as "awkward," "provisional and temporary," and frankly unrealistic.[7] But in fact Maxwell used such terms only about the idle wheel particles and the supposition that they were in perfect rolling contact with the vortices; it was only the "mode of connexion" of the vortices that he presented as awkward and unrealistic, not the vortices themselves. From the time he first introduced the idle wheel particles in Part II of "Physical Lines," Maxwell explicitly distinguished their status, which he characterized as merely "provisional," from that of the vortices, whose existence he regarded as "probable."[8] Moreover, when he took up the Faraday effect in his *Treatise* in 1873, he

5. Maxwell, "Physical Lines" (ref. 1), 23: 88, repr. in *SP* 1: 505. On Maxwell's gyroscopic vortex experiment, see ibid., 21: 345n, repr. in *SP* 1: 485–86n; Maxwell to Michael Faraday, 19 Oct 1861, *SLP* 1: 688 and Plate X; and *Treatise*, § 575.

6. Maxwell, "Physical Lines" (ref. 1), 21: 346, repr. in *SP* 1: 486.

7. See, for example, John Hendry, *James Clerk Maxwell and the Theory of the Electromagnetic Field* (Bristol: Adam Hilger, 1986), 174; and Nancy J. Nersessian, *Faraday to Einstein: Constructing Meaning in Scientific Theories* (Dordrecht: Kluwer, 1984), 84.

8. Maxwell, "Physical Lines" (ref. 1), 21: 282, repr. in *SP* 1: 468. Knudsen, "Faraday Effect " (ref. 4), 551–53, and Siegel, *Innovation* (ref. 4), 47, emphasize that Maxwell continued to believe in the reality of the vortices.

returned to the vortices, saying he could find no other way to account for the action of magnetism on polarized light.[9]

Such is the case for concluding that Maxwell really believed in his vortices. On the other side, we have the fact that in "Dynamical Theory," he only briefly mentioned his vortex model and formulated his entire theory of the electromagnetic field simply in terms of the dynamics of a connected mechanical system, without reference to any hypothetical structure of the ether. Citing "Physical Lines," he said he had "on a former occasion attempted to describe a particular kind of motion and a particular kind of strain, so arranged as to account for the phenomena," but declared that "in the present paper I avoid any hypothesis of this kind"; his use in "Dynamical Theory" of such terms as "electric momentum" and "electric elasticity" was, he said, merely "illustrative, not . . . explanatory."[10] However confidently Maxwell may have spoken of his vortices in "Physical Lines," it certainly appeared that by 1864, he had left them behind. Writing a few years later, the Edinburgh physicist Peter Guthrie Tait, who was presumably in a position to know his close friend's mind, declared that Maxwell had "discarded" his "particular hypotheses as to the molecular vortices" in favor of a theory of the electromagnetic field founded solely on general dynamical principles.[11] One can easily understand how later observers could conclude, as L. Pearce Williams did, that Maxwell had "quietly abandoned" his vortex model.[12]

But why would Maxwell discard the vortices if he continued to believe that the Faraday effect gave strong evidence of their existence? Why, after 1862, did he not turn his efforts to digging out the true microscopic structure of the ether, rather than formulating field equations that ignored any such structure?

There are many reasons for the move Maxwell made in 1864, but an especially important one grew out of his work at just that time for the British Association Committee on Electrical Standards. The committee had been formed at the Manchester meeting of the British Association for the Advancement of Science in September 1861, largely in response to the perceived needs of the British submarine telegraph industry.[13] When British firms began laying

9. Maxwell, *Treatise*, §§ 806–31, esp. § 831.

10. Maxwell, "Dynamical Theory" (ref. 2), 487, repr. in *SP* 1: 563–64.

11. P. G. Tait, *Sketch of Thermodynamics* (Edinburgh: Edmonston and Douglas, 1868), 74; Tait repeated this passage in the 2nd ed. (1877), 90.

12. L. Pearce Williams, *The Origins of Field Theory* (New York: Random House, 1966), 133.

13. Bruce J. Hunt, "Michael Faraday, Cable Telegraphy, and the Rise of British Field Theory," *History of Technology* 13 (1991): 1–19; Simon Schaffer, "A Manufactory of Ohms: Late

telegraph cables in the early 1850s, they initially paid little attention to the electrical condition of their conductors and insulation; they simply covered a length of wire with gutta-percha (a rubber-like natural plastic extracted from Malayan trees), laid it beneath the sea, and hoped for the best. Several early successes were followed by a series of catastrophic failures, culminating in the breakdown of the first Atlantic cable in 1858 and the costly collapse of the Red Sea cable the next year. These reverses prompted a government inquiry whose report called, amongst much else, for the adoption of accurate and agreed standards of electrical resistance. As the report noted, cable manufacturers and operators needed to be able to cite such standards in their contract specifications, and engineers needed reliable resistance coils they could use to help them monitor the condition of their cables and locate faults for repair.[14]

In 1860, Werner Siemens, a leading German electrical industrialist with close ties to the British cable industry, proposed a standard based on the resistance of a thread of mercury one meter long and one square millimeter in section.[15] The new unit began to catch on, but soon drew critics, particularly in Britain. The prominent cable engineers Latimer Clark and Sir Charles Bright presented a paper at the 1861 British Association meeting calling for a connected system of units not only of resistance but also of charge, current, and electromotive force, all to be named for "our most eminent philosophers": "ohma," "farad," "volt," and so on.[16] Given its resemblance to the system that was eventually adopted, it is understandable that many writers have cited Clark

Victorian Metrology and its Instrumentation," in Susan Cozzens and Robert Bud, eds., *Invisible Connections: Instruments, Institutions, and Science (*Bellingham, WA: SPIE Optical Engineering Press, 1992), 23–56; Bruce J. Hunt, "The Ohm is Where the Art Is: British Telegraph Engineers and the Development of Electrical Standards," *Osiris* 9 (1994): 48–63.

14. *Report of the Joint Committee to Inquire into the Construction of Submarine Telegraph Cables*, British Parliamentary Papers, 2744 (1860), vol. 62 (London: Her Majesty's Stationery Office, 1861), xvi; see also the testimony of William Thomson, 118, and Fleeming Jenkin, 139.

15. Werner Siemens, "Vorschlag eines Reproducibaren Widerstandsmasses," *Annalen der Physik* 110 (1860): 1–20, translated as Siemens, "Proposal for a New Reproducible Standard Measure of Resistance to Galvanic Circuits," *PM* 21 (1861): 25–38. On the ensuing controversy between Siemens and the British, see Kathryn Olesko, "Precision, Tolerance, and Consensus: Local Cultures in German and British Resistance Standards," in *Scientific Credibility and Technical Standards in 19th and Early 20th-Century Germany and Britain*, ed. Jed Z. Buchwald (Dordrecht: Kluwer, 1996), 117–56; and Graeme J. N. Gooday, *The Morals of Measurement: Accuracy, Irony, and Trust in Late Victorian Electrical Practice* (Cambridge: Cambridge University Press, 2004), 82–127.

16. Latimer Clark and Sir Charles Bright, "Measurement of Electrical Quantities and Resistance," *Electrician* 1 (1861), 3–4.

and Bright's proposal as the origin of the British Association Committee.[17] In fact the committee got its start from a separate effort led by William Thomson (later Lord Kelvin), and most of the arrangements were already in place before Clark and Bright appeared on the scene. Thomson had taken an active part in the Atlantic cable project and the subsequent inquiry, and was held in high esteem by both scientists and telegraph engineers. He strongly believed that laboratory physicists and practical engineers should adopt a shared set of electrical standards, and though a broken leg kept him from attending the 1861 meeting, he was able, acting largely through his protégé, the cable engineer Fleeming Jenkin, to secure appointment of a committee and the appropriation of £50 from the British Association to fund its work.[18] Initially the terms of the committee focused solely on establishing a standard of electrical resistance, but it soon absorbed Clark and Bright's call for a connected system of units. Bright himself was added to the committee in 1862 and Clark in 1866.[19]

Cable engineers and electrical scientists interacted in important ways on the British Association committee; in particular, Thomson and other scientists used the committee, with considerable though not uncontested success, to assert scientific control over important practices in the cable industry, especially concerning electrical measurement. Over the initial objections of Clark and other engineers, Thomson advocated adoption of the "absolute" system devised in Germany by C. F. Gauss and Wilhelm Weber, in which electric and magnetic units were not founded on the properties of particular material objects, such as Siemens's thread of mercury, but were derived from the fundamental units of time, length, and mass. Thus in Gauss and Weber's system of electromagnetic units, electrical resistance came out as a velocity, which they measured in millimeters per second; as the committee later expressed it, "the

17. See, for example, A. C. Lynch, "History of the Electrical Units and Early Standards," *Proceedings of the Institution of Electrical Engineers* 132A (1985): 564–73; and Graeme Gooday, "Precision Measurement and the Genesis of Physics Teaching Laboratories in Victorian Britain," *British Journal for the History of Science* 23 (1990): 25–51, on 34.

18. *Report of the Thirty-First Meeting of the British Association for the Advancement of Science, held at Manchester in September 1861* (London: John Murray, 1862), xxxix–xl; Hunt, "Ohm" (ref. 13), 57–59.

19. A brief history of the committee can be found in F. E. Smith, ed., *Reports of the Committee on Electrical Standards appointed by the British Association for the Advancement of Science* (Cambridge: Cambridge University Press, 1913), xvii–xxiii; a convenient list of members and their dates of service appears on xxiii–xxiv. Though originally named the "Committee to Report on Standards of Electrical Resistance," from 1862 it was usually known as the "Committee on Electrical Standards" and sometimes the "Committee on Electrical Measurements."

resistance of a circuit is the velocity with which a conductor of unit length must move across a magnetic field of unit intensity in order to generate a unit current in the circuit."[20] The committee noted that Gauss and Weber's system also had the great merit of tying all electric and magnetic units to the unit of work, "the great connecting link between all physical measurements."[21] Thus, for instance, a unit of current passing through a unit of potential difference delivered one unit of power. (In a later version of the terminology the committee itself introduced, 1 amp × 1 volt = 1 watt). Where the values of Gauss and Weber's absolute units were inconveniently large or small, the committee adopted appropriate decimal multiples to adjust them to the needs of telegraphers; for example, it set the value of the "British Association unit of resistance" (later dubbed the "ohm") at 10^7 meters per second, equal to the resistance of about a tenth of a mile of ordinary telegraph cable (and, as the committee noted, very close to the value of Siemens's mercury unit).[22] The traffic between the scientists and engineers on the committee thus ran both ways, as the scientists brought their expertise to bear on the needs of the telegraph industry, while also absorbing and transmitting to the broader scientific community some of the techniques and approaches to electrical measurement that prevailed among engineers.

Maxwell, then a young professor of natural philosophy at King's College London, was not initially a member of the British Association Committee, but he was added to it in 1862 and soon took an active part in its work. Why did he join up and what did he draw from the experience? Maxwell had long taken an interest in new technologies, including telegraphy, but mostly as an observer rather than, like Thomson, a direct participant.[23] Thomson, based in industrial Glasgow, would go on to develop a substantial and lucrative business as a consulting telegraph engineer and instrument manufacturer; Maxwell, a Scottish

20. "Report of the Committee Appointed by the British Association on Standards of Electrical Resistance," *Report of the Thirty-Third Meeting of the British Association for the Advancement of Science, held at Newcastle-upon-Tyne in August and September 1863* (London: John Murray, 1864), 111–24, on 118, repr. in Smith, ed., *Reports* (ref. 19), 58–78, on 69.

21. "Provisional Report of the Committee Appointed by the British Association on Standards of Electrical Resistance," *Report of the Thirty-Second Meeting of the British Association for the Advancement of Science, held at Cambridge in October 1862* (London: John Murray, 1863), 125–35 on 126, repr. in Smith, ed., *Reports* (ref. 19), 1–16, on 2.

22. Ibid., 126–27, 129, repr. in Smith, ed., *Reports* (ref. 19), 2–3, 7.

23. On Thomson's deep involvement in technology and commerce, see Crosbie Smith and M. Norton Wise, *Energy and Empire: A Biographical Study of Lord Kelvin* (Cambridge: Cambridge University Press, 1989), 687–98.

country gentleman with family roots in Edinburgh, always stood somewhat aside from trade and industry, and there is little evidence that he joined the British Association Committee out of a desire to serve the needs of the cable industry, much less to find a place for himself within it. Rather, he appears to have turned to the committee in 1862 in hopes of obtaining measurements to test and if possible confirm his new electromagnetic theory of light, particularly by verifying that the ratio of electrostatic to electromagnetic units was equal, as his theory required, to the speed of light. In this he was only partially successful, but the effort had other important consequences, as Maxwell's exposure to the work of the committee and to engineers' characteristic attitudes toward the quantities they measured led him to reframe his electromagnetic theory in fundamental ways.

MAKING MEASUREMENTS

The most famous and important result in "Physical Lines" was Maxwell's identification of light with waves in his electromagnetic medium. This, however, was not at all part of the paper as he initially conceived it. In its first installments, published in the *Philosophical Magazine* in March, April, and May 1861, he showed how his vortices and idle wheels could account mechanically for the existence and operation of magnetic fields, electric currents, and electromagnetic induction, and he closed the May installment with some general remarks about the use of models and analogies in physical theorizing. The tone and structure of this section make it clear that Maxwell considered the paper completed at this point; he gave no hint of more parts to come. But while spending the summer at Glenlair, his estate in Scotland, he returned to his ether model and asked how he might make it account for electrostatic phenomena. His answer—by making the vortex cells elastic—carried with it an unexpected bonus, for he found that his newly elastic medium could carry transverse waves very much like those of light. Moreover, after making some simplifying (and rather questionable) assumptions about the moduli of elasticity of his vortex cells, he found that the speed of the waves came out equal to the ratio of electrostatic to electromagnetic units, a quantity he came to designate as *v*.[24]

24. Maxwell, "Physical Lines" (ref. 1), 23: 15–22, repr. in *SP* 1: 492–500. Siegel, *Innovation* (ref. 4), 136–41, discusses the questionable steps in Maxwell's derivation of the wave speed; see also Simon Schaffer, "Accurate Measurement is an English Science," in *The Values of Precision,* ed. M. Norton Wise (Princeton, NJ: Princeton University Press, 1995), 135–72, esp. 137 and 145.

Maxwell later told Faraday that he had "worked out the formulae in the country," with no access to published measurements of *v* and no way to put an actual number on the speed with which waves would travel in his vortex medium; he knew only that it would equal the ratio of units.[25] Only after he returned to London, probably in late September 1861, did he find that in 1857, Wilhelm Weber and Rudolf Kohlrausch had measured the ratio to be 310,740 x 10^6 millimeters per second, or 193,088 miles per second. To his evident delight, he also found in J. A. Galbraith and Samuel Haughton's *Manual of Astronomy* that Hippolyte Fizeau had measured the speed of light to be 193,118 miles per second.[26] Weber and Kohlrausch had noted the closeness of the two quantities (though they cited a slightly different value for the speed of light), but said it had no real physical significance; in Weber's theory, the ratio of units was related to the speed at which the electrostatic attraction between two oppositely charged particles of electricity would balance their electromagnetic repulsion, and had nothing to do with waves of light.[27] Maxwell saw the matter very differently; as he wrote to Faraday on October 19, 1861, he was convinced that "this coincidence is not merely numerical," but reflected a deep connection between light and electromagnetism. "I think we have now strong reason to believe," he declared, "whether my theory is

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Siegel rightly emphasizes that Maxwell had settled on identifying *v* with the speed of the waves *before* he saw Weber and Kohlrausch's measurement; see Daniel M. Siegel, "Author's Response," *Metascience* 4 (1993), 31.

25. Maxwell to Michael Faraday, 19 Oct 1861, *SLP* 1: 685.

26. Ibid.; also Maxwell to C. J. Monro, ca. 20 Oct 1861, *SLP* 1: 690. Joseph Galbraith and Samuel Haughton, *Manual of Astronomy* (London: Longmans, 1855), 36, gave Fizeau's value of the speed of light as 169,944 "geographical miles" (of 6000 feet) per second; converting to statute miles, Maxwell arrived at 193,118 miles per second. Fizeau's own published value, reported in H. Fizeau, "Sur une expérience relative à la vitesse de propagation de la lumière," *Comptes Rendus* 29 (1849): 90–92, was in fact 70,948 "lieues de 25 au degré"; this converts to 195,937 miles per second, but in "Physical Lines" (ref. 1), 23: 22, repr. in *SP* 1: 500, Maxwell mistakenly transcribed Fizeau's number as 70,843, and so gave the speed of light as 195,647 miles per second; see Siegel, *Innovation* (ref. 4), 211–12, n. 21, and Schaffer, "English Science" (ref. 24), 145–47.

27. On Weber and Kohlrausch's measurement of the ratio of units, see Oliver Darrigol, *Electrodynamics from Ampère to Einstein* (Oxford: Oxford University Press, 2000), 66. Darrigol also discusses Gustav Kirchhoff's 1857 derivation, based on Weber's theory, of the speed with which electricity would propagate in a thin wire, and Weber's reasons for concluding that the closeness of this value to the speed of light was not physically significant; see 72–73. Note that Weber's conception of electric currents as counterflowing streams of oppositely charged particles led him to focus on the ratio of electrostatic to electrodynamic units, which was larger than Maxwell's ratio of electrostatic to electromagnetic units by a factor of $\sqrt{2}$.

a fact or not, that the luminiferous and the electromagnetic medium are one."[28]

That v should equal the speed of light was not the only empirically testable consequence of Maxwell's vortex theory, and his immediate motive in writing to Faraday was to obtain other experimental data. As he told Faraday, his vortex model had led him 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."[29] In particular, Maxwell's model predicted that the specific inductive capacity (or relative permittivity) of a transparent dielectric would be equal to the square of its index of refraction, and he asked Faraday if he knew of good measurements of these quantities for different substances. Maxwell's theory of the Faraday effect also implied that the rotation of a beam of polarized light would be proportional to the strength of the magnetic field through which it passed, and so to the size, density, and rotational speed of the underlying vortices, and he asked if Faraday could point him toward any measurements bearing on the question. Faraday pencilled "Verdet" in the margin of Maxwell's letter, a reference to the experiments on magneto-optical phenomena that Émile Verdet had been publishing since 1854.[30] By the time Maxwell sent an account of his vortex model to William Thomson on December 10, 1861, he had worked Verdet's results into his developing theory, suggesting, for example, that he could explain the seemingly anomalous results Verdet had found for light passing through ferro-magnetic solutions by assuming that, in them, the magnetic vortices set the iron molecules themselves spinning in the opposite direction.[31]

Maxwell wrote up two new installments of "Physical Lines," published in January and February 1862, in which he showed how making the vortex cells elastic would enable his model to account for light waves and magneto-optic rotation.[32] In a much-quoted passage, he cited the closeness between the ratio of units and the speed of light, and declared 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*."[33] He clearly thought he

28. Maxwell to Michael Faraday, 19 Oct 1861, *SLP* 1: 685–86.

29. Ibid., 683.

30. Ibid., 684–85 and n.10.

31. Maxwell to William Thomson, 10 Dec 1861, *SLP* 1: 692–98, esp. 697, n.28 and n.29; Maxwell, "Physical Lines" (ref. 1), 23: 89–90, repr. in *SP* 1: 507–508.

32. Maxwell, "Physical Lines" (ref. 1), 23: 12–24, 85–95, repr. in *SP* 1: 489–513.

33. Ibid., 12: 22, repr. in *SP* 1: 500; emphasis in original.

was onto something big and eagerly sought more and better experimental evidence. Even when he found that other published measurements of the speed of light did not match the ratio of units nearly as closely as did Galbraith and Haughton's report of Fizeau's, Maxwell emphasized that the various values lay on either side of Weber and Kohlrausch's number, and he strongly implied that he expected the values to converge as measurements of both quantities improved. He also continued to seek better measurements of specific inductive capacity, writing to Thomson that "I think Fleeming Jenkin has found that of gutta percha caoutchouc &c." and asking "where can one find his method, and what method do you recommend."[34] This was Maxwell's first known mention of Jenkin; it would be far from the last.

Very few letters to Maxwell from this period have survived, and we have no record of any responses he may have received from Faraday or Thomson. Nor do we have evidence of any direct contact between Maxwell and Jenkin at this time. Both men were then in London, however, and Jenkin was just beginning his work as Secretary of the British Association Committee. Given Maxwell's growing interest in electrical measurements and his question to Thomson, it seems likely that he got in touch with Jenkin in the early months of 1862. In any case, we know that Maxwell formally joined the British Association Committee in October 1862, and thereafter worked closely with Jenkin, first assembling the necessary apparatus and then performing the experiments that first established the value of the ohm.[35]

34. Maxwell to William Thomson, 10 Dec. 1861, *SLP* 1: 697. Although Maxwell and Jenkin both attended the Edinburgh Academy in the 1840s, there is no evidence they were acquainted then; see Gillian Cookson and Colin Hempstead, *A Victorian Scientist and Engineer: Fleeming Jenkin and the Birth of Electrical Engineering* (Aldershot: Ashgate, 2000), 16–18. On Jenkin's work on the inductive capacities of gutta-percha and rubber, see Bruce J. Hunt, "Insulation for an Empire: Gutta-Percha and the Development of Electrical Measurement in Victorian Britain," in *Semaphores to Short Waves*, ed. Frank A. J. L. James (London: Royal Society of Arts, 1998), 85–104, on 98–99.

35. Bright's brother and son later claimed that Bright and Clark's 1861 British Association paper "formed the sequel to a letter addressed by Bright to Prof. J. Clerk Maxwell, F. R. S., some months previously, on the whole question of electrical standards and units"; see E. B. Bright and Charles Bright, *The Life Story of the Late Sir Charles Tilston Bright*, vol. 2 (2 vols., London: Constable, 1899), 21. Maxwell, however, had not shown any interest in electrical units before Oct 1861, and it seems unlikely that months earlier Bright would have written to him out of the blue on the subject. It seems more likely that Maxwell wrote to Bright in search of experimental data in the fall of 1861, around the time of his letters to Faraday and Thomson, and perhaps elicited a reply about electrical standards.

In its 1862 report, the British Association Committee had adopted 10^7 meters per second as its unit of electrical resistance, but that was only the prelude to the difficult task of constructing a physical standard (essentially a coil of wire) that would accurately embody the new unit.[36] In the wake of the 1862 meeting, that task was assigned to a London-based subcommittee consisting of Jenkin, Maxwell, and Balfour Stewart, the director of the Kew Observatory and an expert on terrestrial magnetism. They planned to use a method devised by Thomson in which a small magnet attached to a needle was suspended within a large coil of wire. Initially the needle simply pointed northward, but as the coil was spun around a vertical diameter, its motion through the earth's magnetic field induced a current within it that deflected the needle. The neat trick here was that since both the original force directing the needle to the north and the new one arising from the induced current were proportional to the strength of the earth's magnetic field, the net deflection was independent of that field and depended solely on the size of the coil, its speed of rotation, and crucially, its electrical resistance. The spinning coil method was conceptually simple, but carrying it out to high precision involved many subtleties. For instance, the need for a very steady rate of spin prompted both Maxwell and Jenkin to take up the theory of governors, while Maxwell's observations of the decaying oscillations of the deflected needle led him to experiments on the viscosity of air that later provided a crucial test of his kinetic theory of gases.[37]

Once they had set up and tested the apparatus, Maxwell, Jenkin, and Stewart spent May and June 1863 spinning their coil and taking readings in a basement room at King's College London. As Maxwell later described the procedure, "the Secretary" (Jenkin) cranked the driving wheel, "the Astronomer" (Stewart) timed the rotation of the coil, and Maxwell himself tracked the

36. The British Association Committee initially intended to construct a material standard as close to 10^7 meters per second as possible and then treat that as the unit, in much the same way that the meter, though in principle set at 10^{-7} of the distance from the equator to the pole, came to be defined as a length marked on a metal bar kept near Paris. See "Provisional Report, 1862" (ref. 21), 129–30, repr. in Smith, ed., *Reports* (ref. 19), 7. Later the Committee simply defined the "B. A. unit," or ohm, as 10^7 meters per second, though it had continuing difficulties constructing a material standard that matched its defined value.

37. Maxwell to William Thomson, 11 Sep 1863, *SLP* 2: 112–16, includes discussion of "Jenkin's governor" and Maxwell's own; see also James Clerk Maxwell, "On Governors," *Proceedings of the Royal Society of London* 16 (1868), 270–83, repr. *SP* 2: 105–20. Concerning Maxwell's work on the viscosity of air, see Maxwell to G. G. Stokes, 9 June 1863, *SLP* 2: 96, n.10.

deflection of the needle through a telescope.[38] Unexpected complications soon turned up, arising from such things as small shifts in the direction of the earth's magnetic field and difficulties in regulating the temperature of the coil; even the passing of steamers on the Thames could affect the sensitive magnetic needle. Maxwell's biographer, Lewis Campbell, said in 1882 that Jenkin had preserved "a mass of correspondence, containing numerous suggestions made by Maxwell from day to day in 1863–64," but apart from one quoted by Campbell, those letters have all been lost.[39] Maxwell also consulted with Thomson, sending him accounts of the "spins" and inviting him to observe a few and give advice when he was in London in early June.[40]

As Maxwell and Jenkin refined their procedures, they became confident they could bring their results to a high level of precision. They made more spins over the winter and spring of 1863–64, with Stewart's place being taken by Charles Hockin, a recent Cambridge graduate who later became a leading cable engineer.[41] They had hoped to be able to issue certified coils at the British Association meeting in September 1864, but the work of duplicating the standards went slowly, and Elliott Brothers, the London instrument makers, began to sell unofficial coils that autumn to "persons who were unwilling to wait for the final experiments by the Committee."[42] Certified standards were finally ready in February 1865, when Jenkin sent the *Philosophical Magazine* a brief notice announcing that, for a small charge, "copies of the Standard of Electrical Resistance chosen by the Committee on Electrical Standards

38. Maxwell to Robert Dundas Cay, 21 Aug 1863, *SLP* 2: 103; James Clerk Maxwell, Balfour Stewart, and Fleeming Jenkin, "Description of an Experimental Measurement of Electrical Resistance, made at King's College," Appendix D to "Report, 1863" (ref. 20), 163–76, on 174, repr. in Smith, ed., *Reports* (ref. 19), 156. See also I. B. Hopley, "Maxwell's Work on Electrical Resistance: The Redetermination of the Absolute Unit of Resistance," *Annals of Science* 13 (1957): 265–72. On the King's College laboratory, see Jordi Cat, *Maxwell, Sutton, and the Birth of Color Photography: A Binocular Study* (New York: Palgrave Macmillan, 2013).

39. Lewis Campbell and William Garnett, *The Life of James Clerk Maxwell* (London: Macmillan, 1882), 317 and 336–37.

40. Maxwell to William Thomson, 29 May 1863, *SLP* 2: 88–92; also 93–94 and 98–101.

41. See the obituary of Hockin in *Electrician* 8 (1882): 409–10.

42. On standards made by Elliott Bros. in 1864, as well as a set made by Siemens and Halske of Berlin for the government telegraphs in India, see "Report of the Committee on Standards of Electrical Resistance," *Report of the Thirty-Fourth Meeting of the British Association for the Advancement of Science, held at Bath in September 1864* (London: John Murray, 1865), 345–49, on 345, repr. in Smith, ed., *Reports* (ref. 19), 159–66, on 159. See Maxwell to Hockin, 7 Sep 1864, *SLP* 2: 164, expressing "hope there will be resistance coils at the British Association," and Gooday, *Morals* (ref. 15), 107–10, on Augustus Matthiessen's laborious efforts with Hockin to make reliable duplicates of the resistance standards.

appointed by the British Association in 1861, can now be procured by application to me as Secretary to the Committee."[43] As Maxwell wrote to Tait on March 7, 1865, "The true origin of Electrical Resistance as expressed in B. A. units is Fleeming Jenkin Esq[re] 6 Duke Street Adelphi W. C. Price £2..10 in a box."[44] The committee also sent standard coils gratis to government telegraph departments around the world and to several eminent physicists, including Weber, Gustav Kirchhoff, and James Joule.

The new standard was soon taken up by instrument makers, scientific laboratories, and perhaps most importantly by the companies that made and laid telegraph cables. In an 1873 review of Jenkin's *Electricity and Magnetism*, a textbook aimed at "practical men," Maxwell made it clear that he believed the real call for precision measurement and accurate electrical standards came from engineers and businessmen rather than from academic scientists. Echoing remarks Jenkin had made in the opening pages of his book, Maxwell observed:

> At the present time there are two sciences of electricity—one that of the lecture-room and the popular treatise; the other that of the testing-office and the engineer's specification. The first deals with sparks and shocks which are seen and felt, the other with currents and resistances to be measured and calculated. The popularity of the one science depends on human curiosity; the diffusion of the other is a result of the demand for electricians as telegraph engineers.[45]

Agreed standards and reliable measurements were vital to any commercial exchange, and those drawing up contracts for expensive telegraph systems and submarine cables wanted to be sure they were getting exactly what they paid for. Siemens's mercury unit was cited in some telegraph specifications in the early 1860s, but it largely gave way to the new British Association unit later in the decade, particularly in cable telegraphy. The cable industry was dominated by British firms and at the time constituted by far the largest market for precision electrical measurement; the British Association Committee knew that if it could persuade the cable industry to adopt its standards, the battle would practically be won. In 1865 the committee boasted that "[t]he new unit has been actually employed to express the tests of the Atlantic Telegraph

43. Fleeming Jenkin, "Electrical Standard," *PM* 29 (1865): 248.

44. Maxwell to P. G. Tait, 7 Mar 1865, *SLP* 2: 214.

45. [James Clerk Maxwell], review of *Electricity and Magnetism*, by Fleeming Jenkin, *Nature* 8 (1873): 42–43, repr. in *SLP* 2: 842–44, on 842; Fleeming Jenkin, *Electricity and Magnetism* (London: Longmans, 1873), vii.

Cable," but certified standards were not yet available when its specifications (both for the cable that snapped during laying in August 1865 and the one that was successfully completed the following year) were drawn up.[46] There was a brief lull in cable laying after 1866, but in 1868, companies led by "cable king" John Pender launched a wave of new projects that would set the pattern for the entire industry. Beginning with the contract for the Malta–Alexandria cable, drawn up in May 1868, these called for the resistance of both the copper conductor and the gutta-percha insulation to be measured in "B. A. units" or, as they soon came to be called, "ohms" and "megohms."[47]

Problems with the British Association resistance standards began to turn up in the 1870s, as we shall see, and their values later had to be adjusted. But in the late 1860s, the new system appeared to be a clear success; its units and standards had taken firm root in the cable industry and were rapidly becoming central to the daily practice of telegraph engineers. Its work seemingly done, in 1870, the British Association Committee dissolved itself.

"ELEMENTARY RELATIONS"

Maxwell's work for the British Association Committee had two sides, experimental and conceptual. His experimental work led to the establishment of the ohm and the associated system of units and standards; his conceptual work resulted in an important but often overlooked paper on "The Elementary Relations Between Electrical Measurements." Written with Jenkin and first published as an appendix to the committee's 1863 report, "Elementary Relations" was devoted to formulating the basic principles of electrical measurement as clearly and simply as possible, with a minimum of speculation or hypothesis.

46. "Report of the Committee on Standards of Electrical Resistance," *Report of the Thirty-Fifth Meeting of the British Association for the Advancement of Science, held at Birmingham in September 1865* (London: John Murray, 1866), 308–11, on 311, repr. in Smith, ed., *Reports* (ref. 19), 190–95, on 194. See Bern Dibner, *The Atlantic Cable* (Norwalk, CT: Burndy Library, 1959).

47. See the cable contracts in DOC/ETC/1/114, held by the Porthcurno Telegraph Museum, Porthcurno, Cornwall. The first, dated 11 May 1868, called for the resistance of the conductor of a new cable from Malta to Alexandria to be "9 B. A. units per nautical mile" and that of its gutta percha covering to be "not less than 200 millions of B. A. units per nautical mile." By the time the contract for a cable from Marseille to La Calle (Algeria) was signed on 5 Feb 1870, the terminology had changed: resistance of the conductor was specified to be "not more than 12.15 ohms be nautical mile" and that of the gutta percha covering "not less than 150 megohms per nautical mile." There was occasional confusion; for instance, the 15 Aug 1870 specification for a new Anglo-Mediterranean cable referred throughout to "shms" and "megshms."

It fits right between "Physical Lines" and "Dynamical Theory," and adding it to our previous list of three papers helps clear up some otherwise puzzling aspects of Maxwell's apparent shift in approach between 1862 and 1864, and illuminates themes that would run through his later work.[48]

In 1901, Richard Glazebrook, a leading figure for many years at the Cavendish Laboratory in Cambridge, the first head of the National Physical Laboratory (effectively the British standards bureau), and a great expert on precision electrical measurement, declared with perhaps only slight exaggeration that Maxwell and Jenkin's 1863 paper laid "the foundation of everything that has been done in the way of absolute electrical measurement since that date."[49] Surveying the field in 1907, E. B. Rosa and and N. E. Dorsey of the National Bureau of Standards in Washington called the paper "masterly" and quoted long passages from it; it long remained a touchstone for those engaged in precision electrical measurement.[50] "Elementary Relations" has attracted little attention from historians, however, no doubt in part because it was omitted from W. D. Niven's 1890 edition of Maxwell's *Scientific Papers* and also from Peter Harman's recent edition of Maxwell's *Scientific Letters and Papers*. Simply getting its title straight has proven tricky; in his excellent *Dictionary of Scientific Biography* article on Maxwell, later published separately as *James Clerk Maxwell: Physicist and Natural Philosopher*, Francis Everitt inexplicably cited it as "On the Elementary Relations of Electrical Quantities," an error John Hendry, Daniel Siegel, and Harman all later repeated.[51]

48. James Clerk Maxwell and Fleeming Jenkin, "On the Elementary Relations between Electrical Measurements," *Report of the Thirty-Third Meeting of the British Association for the Advancement of Science, held at Newcastle-upon-Tyne in August and September 1863* (London: John Murray, 1864), 130–63, and in the separately issued *Report of the Committee appointed by the British Association on Standards of Electrical Resistance* (London: Taylor and Francis, 1864), which retained the same pagination. "Elementary Relations" appeared with minor revisions in *PM* 29 (1865): 436–60, 507–25, and with more extensive changes in Fleeming Jenkin, ed., *Reports of the Committee on Electrical Standards appointed by the British Association for the Advancement of Science* (London: Spon, 1873), 59–96; the latter version was reprinted in Smith, ed., *Reports* (ref. 19), 86–140. Citations here will be to the version in the 1863 *British Association Report*.

49. R. T. Glazebrook, *James Clerk Maxwell and Modern Physics* (London: Cassell, 1901), 55–56.

50. E. B. Rosa and N. E. Dorsey, "A Comparison of the Various Methods of Determining the Ratio of the Electromagnetic to the Electrostatic Unit of Electricity," *Bulletin of the Bureau of Standards* 3 (1907): 605–22, on 605.

51. Everitt, *Maxwell* (ref. 4), 100; Hendry, *Maxwell* (ref. 7), 200, 294; Siegel, *Innovation* (ref. 4), 215 n.17; Harman, *SLP* 2: 8. The paper is briefly discussed (under the correct title) in Harman, *Natural Philosophy* (ref. 4), 64–65, mainly in connection with dimensional analysis, and in Thomas K. Simpson, *Maxwell on the Electromagnetic Field: A Guided Study* (New Brunswick, NJ: Rutgers University Press, 1997), 409 n.9. It is taken up more fully in Salvo d'Agostino,

Set next to the path-breaking "Physical Lines" and the magisterial "Dynamical Theory," it is easy to see how "Elementary Relations" could be overlooked. It is not a flashy paper, and at least on the surface, it contains little that would have seemed obviously new; long stretches are devoted to carefully defining such things as magnetic induction and electromotive force and to clarifying how such quantities are related to each other. Instead of advancing dazzling new experimental results or bold theoretical ideas, "Elementary Relations" sought simply to make readers' ideas about electrical and magnetic phenomena clearer and more definite, and to relate those ideas as closely as possible to quantities that could actually be measured. Its ultimate aim was to facilitate fruitful collaboration and exchange. "Whenever many persons are to act together," Maxwell and Jenkin observed, "it is necessary that they should have a common understanding of the measures to be employed," and their aim in "Elementary Relations," they said, was "to assist in attaining this common understanding as to electrical measurements."[52] They were in effect seeking to carve out what Peter Galison has called a "trading zone," a realm in which scientists and engineers who might have very different practical aims and theoretical commitments could nonetheless act in concert when making and comparing electrical measurements.[53]

Aside from a note toward the end that is credited to Maxwell, it is hard say which of the coauthors wrote any particular part of "Elementary Relations," and it is perhaps best to treat all of it as reflecting both men's views.[54] Maxwell and Jenkin evidently saw eye-to-eye on most subjects, and they certainly became close friends. Maxwell later took to calling Jenkin "the Fleemingo," and Jenkin asked Maxwell to serve as the godfather of his youngest son, Bernard Maxwell Jenkin, born in 1867.[55]

"Experiment and Theory in Maxwell's Work: The Measurements for Absolute Electromagnetic Units and the Velocity of Light," *Scientia* 113 (1978), 469–80, esp. 472, but d'Agostino's argument is vitiated by his misattribution to Maxwell of the 1863 "Report" of the Committee on Standards of Electrical Resistance, which was written by William Thomson; see S. P. Thompson, *Life of Sir William Thomson, Baron Kelvin of Largs*, vol. 1 (London: Macmillan, 1910), 419.

52. Maxwell and Jenkin, "Elementary Relations" (ref. 48), 130.

53. On "trading zones," see Peter Galison, *Image and Logic: A Material Culture of Microphysics* (Chicago, IL: University of Chicago Press, 1997), 803–05.

54. Maxwell and Jenkin, "Elementary Relations" (ref. 48), 160–61; this "Note," including a discussion of how magnetic measurements would differ if made in "a sea of melted bismuth" rather than in air, was omitted when the paper was reprinted in 1873 and 1913.

55. Maxwell to Tait, 7 Nov 1874, *SLP* 3: 135; on Maxwell as godfather to Bernard Maxwell Jenkin, see Cookson and Hempstead, *Jenkin* (ref. 34), 17.

In the opening lines of the paper, Maxwell and Jenkin placed their effort squarely within a technological context. "The progress and extension of the electric telegraph," they declared, "has made a practical knowledge of electric and magnetic phenomena necessary to a large number of persons who are more or less occupied in the construction and working of the lines, and interesting to many others who are unwilling to be ignorant of the use of the network of wires which surrounds them." The situation called for a careful analysis of foundations, they said, for "between the student's mere knowledge of the history of discovery and the workman's practical familiarity with particular operations which can only be communicated to others by direct imitation, we are in want to a set of rules, or rather principles," to guide us in applying abstract laws to achieve specific ends.[56]

The thrust of "Elementary Relations" is strongly anti-hypothetical, in places what we might, a little anachronistically, call "operationalist." Thus Maxwell and Jenkin defined "electric quantity" or "charge" not by invoking an imponderable fluid, or even strains in a surrounding field, but by noting that "[w]hen two light conducting bodies are connected with the same pole of a voltaic battery, while the other pole is connected to the earth, they may be observed to repel one another. . . . Bodies, when in a condition to exert this peculiar force one on the other, are said to be electrified, or charged with electricity. These words are mere names given to a peculiar condition of matter."[57] For the purposes of their paper, Maxwell and Jenkin did not wish to commit themselves to anything beyond what could be seen and measured; an electric current, they said, did not necessarily represent the flow of anything material, and should be regarded as simply a state into which certain bodies are thrown under certain circumstances, as when a wire is connected across the poles of a battery. Similarly, they said, "in speaking of a quantity of electricity, we need not conceive it as a separate thing, or entity distinct from ponderable matter, any more than in speaking of sound we conceive it as having a distinct existence." We nonetheless often find it convenient to speak of the "velocity of sound," and in the same way "we may speak of electricity, without for a moment imagining that any real electric fluid exists."[58] Jenkin would take the same tack ten years later in his *Electricity and Magnetism*, saying that although for convenience he sometimes spoke of electricity as flowing like

56. Maxwell and Jenkin, "Elementary Relations" (ref. 48), 130.
57. Ibid., 136.
58. Ibid.

a fluid, "it is quite unnecessary to assume that the phenomena are due to one fluid, two fluids, or any fluid whatever."[59]

The members of the British Association Committee knew that to win the widest possible acceptance for their proposed system of standards, they needed to take care that it not be seen as tied to any one national tradition or any particular theory of electromagnetism, whether Faraday and Maxwell's field theory on the one hand or Weber's action-at-a-distance theory on the other. The committee sought, Maxwell and Jenkin said, to form a system that "bears the stamp of the authority, not of this or that legislator or man of science, but of nature."[60] Toward that end, Maxwell and Jenkin formulated their definitions and accompanying equations in ways that simply related together macroscopic objects and forces that one could measure and manipulate, while founding the entire system on universal units of mass, length, and time. It was an approach that George Chrystal and others would later describe as "businesslike"; moreover, it reflected a characteristic engineering approach to such problems.[61]

As Edwin Layton and others have observed, engineers typically focus on macroscopic phenomena and seek to formulate empirically verifiable relations between quantities they can measure and control. They are concerned with practical outcomes and generally see little value in speculating about unseen entities that will not affect the result. Physicists, on this account, are more concerned with ferreting out the physical microstructure underlying such macroscopic phenomena, and are not averse to introducing hypotheses about molecules and the like to help them do so; indeed, Layton pointed to the contrast between a macroscopic and a microscopic focus as one of the "mirror-image" differences between engineers and scientists.[62] Jenkin himself embodied both sides of this divide. Well known for the great breadth of his interests,

59. Jenkin, *Electricity and Magnetism* (ref. 45), 1.

60. Ibid., 131.

61. George Chrystal, review of *Treatise on Electricity and Magnetism*, by James Clerk Maxwell, *Nature* 25 (21 Jan 1882): 237–40, on 238.

62. Edwin Layton, "Mirror-Image Twins: The Communities of Science and Technology in 19th-Century America," *Technology and Culture* 12 (1971): 562–89, on 569. See also David Channell, "The Harmony of Theory and Practice: The Engineering Science of W.J.M. Rankine," *Technology and Culture* 23 (1982): 39–52, on 51–52; and Walter G. Vincenti, *What Engineers Know and How They Know It: Analytical Studies from Aeronautical History* (Baltimore, MD: Johns Hopkins University Press, 1990), 129–36. Of course, some branches of physics, notably thermodynamics, focus on macroscopic phenomena and leave aside microphysical considerations, but note that thermodynamics had strong engineering roots in the study of steam engines; see

he was on occasion happy to play the natural philosopher and speculate about such things as the structure of atoms and the inner workings of the ether, notably in an influential 1868 essay on Lucretian atomism.[63] In his engineering writings, however, Jenkin left such microphysical hypotheses firmly aside; his concern there was solely with what he could measure and manipulate. His *Electricity and Magnetism* contains scarcely a word about the ultimate nature or microphysical foundations of its ostensible subjects; he opened it not by asking "What is Electricity?" but by describing how to detect and measure "Electric Quantity."[64]

In an important 1987 paper on the ways science and technology interacted in the development of the induction motor, Ronald Kline argued that in this case Layton's distinction did not hold, since everyone involved, both scientists and engineers, used equations drawn from Maxwell's 1864 "Dynamical Theory" that were already cast in macroscopic form and did not involve any hypothetical microstructure. Kline wrote that "Maxwell [had] himself made the fundamental translation of knowledge between electrophysics and electrotechnology," driven by a desire to understand the workings of certain electrical instruments, particularly ones involving spinning coils like those he had used on the British Association Committee to establish the value of the ohm.[65] I would go further and argue that Maxwell's focus on such instruments, and his characteristic way of dealing with them, were themselves driven by a larger technological context, that associated with the formation of the British Association Committee itself. Maxwell's 1861–62 "Physical Lines of Force" is about the hypothetical microstructure of the ether; his 1864 "Dynamical Theory of the Electromagnetic Field" is not. His 1863 paper with Jenkin on "Elementary Relations" was in effect a transition piece that served to carry him, at least for a time and for specific purposes, to a less hypothetical, more macroscopic—and more engineering-oriented—approach to electrical phenomena.

D.S.L. Cardwell, *From Watt to Clausius: The Rise of Thermodynamics in the Early Industrial Age* (Ithaca, NY: Cornell University Press, 1971).

63. Fleeming Jenkin, "The Atomic Theory of Lucretius," *North British Review* 48 (1868): 211–42, repr. as "Lucretius and the Atomic Theory" in Sidney Colvin and J. A. Ewing, eds., *Papers Literary, Scientific, &c. by the late Fleeming Jenkin, F. R. S., Vol. 1* (2 vols., London: Longmans, 1887), 177–214; see also Crosbie Smith, "Engineering the Universe: William Thomson and Fleeming Jenkin on the Nature of Matter," *Annals of Science* 37 (1980): 387–412.

64. Jenkin, *Electricity and Magnetism* (ref. 45), 1.

65. Ronald Kline, "Science and Engineering Theory in the Invention and Development of the Induction Motor, 1880–1900," *Technology and Culture* 28 (1987): 283–313, on 313.

In "Elementary Relations," Maxwell and Jenkin sought to place electrical and magnetic measurements on a concrete, unhypothetical foundation that all scientists and engineers could endorse. They saw this as a first step toward winning universal acceptance for the British Association system of units and standards, particularly in the face of Siemens's competing mercury unit. They also needed to explain in a simple and accessible way the principles behind "absolute" electric and magnetic measurement—a subject that, as originally presented by Gauss and Weber, struck most telegraph engineers as abstruse, forbidding, and of little practical value. An arbitrary material standard of resistance (such as Siemens's column of mercury) combined with a similarly arbitrary standard of electromotive force (such as that given by a Daniell's cell or other stable voltaic battery) would have provided a coherent system of electrical units, as the British Association Committee freely admitted, and would have met the main needs of telegraphers.[66] Few engineers in the 1860s thus saw much value in relating their electrical measurements to mechanical units of mass, force, and work, as was done in Weber's system; telling them the resistance of a length of wire could be expressed as so many meters per second had little appeal to telegraphers who simply wanted to compare the resistance of one wire to that of another. Latimer Clark was among those who initially dismissed Weber's units as impractical, saying in 1862 that he hoped the newly formed British Association Committee would not "recommend the adoption of Weber's absolute units, or some other units of a magnitude ill adapted to the peculiar and various requirements of the electric telegraph."[67] In "Elementary Relations," however, Maxwell and Jenkin reiterated that simple decimal multiples could be used to define practical units of convenient size, while also emphasizing, as had Thomson, that the absolute system had the great merit of linking all physical measurements to units of energy. For Thomson, energy (or work) was the ultimate measure of value, grounded, as Crosbie Smith has shown, in an overarching view of what really counts in the physical world.[68] Moreover, in urging that the system of electrical units be tied to the concept of energy, Thomson and the committee showed what proved to be valuable foresight, for although energy considerations rarely came up in telegraphy, the rise of the electric power industry later made them fundamental to

66. "Report, 1863" (ref. 20), 114, repr. in Smith, ed., *Reports* (ref. 19), 62–63.
67. Latimer Clark, letter to the editor, *Electrician* 1 (1862): 129.
68. Smith, *Science of Energy* (ref. 4), 121–25.

electrical engineering practice. Indeed, Clark himself later acknowledged what he called "the enormous value of an absolute system" of electrical units.[69]

Along with energy considerations, Maxwell and Jenkin made analysis of the dimensions of physical quantities a central feature of "Elementary Relations." Each time they introduced a quantity, they took care to express its dimensions in terms of products and ratios of length, time, and mass, or L, T, and M, and showed how attention to dimensions clarified the relationships among quantities. Thus energy, with the dimensions L^2M/T^2, could be regarded as the product of force, LM/T^2, and distance, L, or of momentum, LM/T, and velocity, L/T. Joseph Fourier had drawn up a table of dimensions as early as 1822, but Maxwell and Jenkin appear to have worked out the underlying principles independently, and their treatment of dimensional analysis in "Elementary Relations" played a major part in spreading knowledge of the subject more widely. They introduced the now familiar bracket notation, [LM/T], in the 1873 reprint of the British Association Committee reports, edited by Jenkin.[70]

Maxwell and Jenkin made a special point of establishing the dimensions of *v*, the ratio of electrostatic to electromagnetic units. By analyzing the force between two charges, they had already shown that in the electrostatic system, charge has the dimensions $L^{3/2}M^{1/2}/T$; similarly, by analyzing the force between two current-carrying wires, they had shown that in the electromagnetic system, charge has the dimensions $L^{1/2}M^{1/2}$. The ratio of the two systems' units thus came out as L/T, a velocity—one whose magnitude, they emphasized, was independent of any particular theory of electrical action or choice of basic units.[71] Citing Weber and Kohlrausch's measurement of *v* as 310,740,000 meters per second, they noted that this was "a velocity not differing from the estimated velocity of light more than the different determinations of the latter

69. Latimer Clark to William Thomson, 3 May 1883, quoted in Smith and Wise, *Energy and Empire* (ref. 23), 687.

70. On the history of dimensional analysis, including Fourier's contributions, see John J. Roche, *The Mathematics of Measurement: A Critical History* (London: Athlone Press, 1998), 188–207, and the 1877 article by J. C. M[axwell], "On Dimensions," *Encyclopedia Britannica*, 9th edn., vol. 7, (Edinburgh, 1875–89), 240–41, repr. *SLP* 3: 517–20. Although Maxwell and Jenkin did not mention Fourier's earlier work in the original 1863 version of "Elementary Relations" (ref. 48), they added a note about it to the 1873 revision, 89n., repr. in Smith, ed., *Reports* (ref. 19), 132n.

71. Maxwell and Jenkin, "Elementary Relations" (ref. 48), 149; see also *Treatise*, § 768, and James Clerk Maxwell, "On a Method of Making a Direct Comparison of Electrostatic and Electromagnetic Force; with a Note on the Electromagnetic Theory of Light," *Philosophical Transactions* 158 (1868): 643–57, on 643–44, repr. in *SP* 2: 125–43, on 125–26.

quantity differ from each other."[72] Although Maxwell and Jenkin made no overt reference in "Elementary Relations" to the electromagnetic theory of light, they were clearly dropping hints.

Weber and Kohlrausch had found *v* by measuring a quantity of electricity first in electrostatic units, by charging a condenser of known capacity to a potential that they measured with an electrometer, and then in electromagnetic units, by discharging the condenser through an electrodynamometer and gauging the degree and duration of its deflection.[73] Over the summer of 1863, Maxwell and Jenkin discussed two other methods, based on finding common measures of electromotive force and of resistance, and in "Elementary Relations" they added two more, based on finding common measures of current and of capacity; the capacity method, they said, "would probably yield very accurate results," as indeed later proved to be the case.[74]

Like Thomson before him, Maxwell emphasized that the ratio of units was an important quantity quite apart from the electromagnetic theory of light. It came into play whenever energy passed between its electromagnetic and electrostatic forms, as in a submarine cable carrying a pulse of current and its concomitant wave of voltage, and so figured in calculations of the limits on the speed of signalling.[75] Noting the practical and scientific importance of an accurate knowledge of the ratio of units, Maxwell and Jenkin announced in "Elementary Relations" that "a redetermination of *v* will form part of the present Committee's business in 1863–64."[76] However, the most promising methods for finding *v* depended on comparing it to a resistance whose value was already known in absolute units (so that *v* would actually be measured in "B. A. units," or ohms), and the redetermination of *v* thus had to wait until the committee had completed its work on the new resistance standard.

In the meantime, Maxwell set about refashioning his electromagnetic theory to take maximum advantage of the new determination of *v* once it became available. His experience on the British Association Committee and especially

72. Maxwell and Jenkin, "Elementary Relations" (ref. 48), 149.

73. Darrigol, *Electrodynamics* (ref. 27), 66.

74. Maxwell to Fleeming Jenkin, 27 Aug 1863, in Campbell and Garnett, *Maxwell* (ref. 39), 336–37; Maxwell and Jenkin, "Elementary Relations" (ref. 48), 153–54; Rosa and Dorsey, "Comparison" (ref. 50), 616–17. In a letter to G. G. Stokes, 15 Oct. 1864, *SLP* 2: 188, Maxwell said he and Jenkin planned to measure *v* by the capacity method, but they never did so.

75. Maxwell, "Direct Comparison" (ref. 71), 644, repr. in *SP* 2: 126; see also Schaffer, "English Science" (ref. 24), 149.

76. Maxwell and Jenkin, "Elementary Relations" (ref. 48), 149.

in writing "Elementary Relations" had brought home to him the persuasive value of basing his theoretical claims as directly as possible on measureable quantities and the demonstrable relations among them. A speculative hypothesis like his vortex and idle wheel model might be a wonderfully fruitful source of new ideas, but it was unlikely to carry much weight with skeptical critics. Once he had a reliable measurement of the ratio of units in hand, Maxwell wanted to be able to cite it as evidence that light was indeed waves in the electromagnetic medium without facing objections that he had derived the supposed connection between *v* and the speed of light from a fanciful mechanical model. As he and Jenkin had done in their examination of "The Elementary Relations Between Electrical Measurements," Maxwell sought to strip his theory of its hypothetical elements and reduce it to what could, he argued, be derived from measureable phenomena. The result was his "Dynamical Theory of the Electromagnetic Field."

"DYNAMICAL THEORY" AND THE RATIO OF UNITS

Maxwell and Jenkin finished writing "Elementary Relations" in the fall of 1863 and, with Hockin, completed their spinning coil experiments the following spring.[77] While laying plans to measure the ratio of units, Maxwell turned to composing his "Dynamical Theory of the Electromagnetic Field." He began by justifying its name: it was a theory of the field, he said, because it concerned the space surrounding electric and magnetic bodies, and a dynamical theory "because it assumes that in that space there is matter in motion, by which the observed electromagnetic phenomena are produced."[78] Unlike in "Physical Lines," however, he did not proceed to lay out a detailed mechanical model of the medium and then set about explicating its workings. Instead, he simply started with the laws of energy that governed any connected dynamical system and, drawing on what had become the standard tools of Cambridge mathematical physics, used a Lagrangian analysis to derive what he argued were the necessary relations among electric and magnetic quantities. Maxwell's methods were those of a Cambridge wrangler, but his motivation for using them as he did was rooted in the measurement-based approach of "Elementary Relations."

77. James Clerk Maxwell, Fleeming Jenkin, and Charles Hockin, "Description of a Further Experimental Measurement of Electrical Resistance Made at King's College," Appendix A to "Report, 1864" (ref. 42), 350–51, repr. in Smith, ed., *Reports* (ref. 19), 166–67.

78. Maxwell, "Dynamical Theory" (ref. 2), 460, repr. *SP* 1: 527.

Maxwell wrote most of "Dynamical Theory" at Glenlair over the summer and added the finishing touches after returning to London in the fall; on October 27, 1864, he submitted it to the Royal Society for publication in the *Philosophical Transactions*, traditionally the favored repository for the least speculative contributions to science.[79] The way he described his paper in letters to friends is revealing. Writing to Hockin from Glenlair on September 7, Maxwell said he had now "cleared the electromagnetic theory of light from all unwarrantable assumption, so that we may safely determine the velocity of light by measuring the attraction between bodies kept at a given difference of potential, the value of which is known in electromagnetic measure"—that is, by measuring the ratio of units.[80] Back in London on October 15, he sent Thomson detailed plans for such a measurement, adding: "I can find the velocity of transmission of electromagnetic disturbances indep[enden]t of any hypothesis now & it is $= v$."[81] Writing the same day to G. G. Stokes, Maxwell said he now had "materials for calculating the velocity of transmission of a magnetic disturbance through air founded on experimental evidence without any hypothesis about the structure of the medium or any mechanical explanation of electricity or magnetism."[82]

In all of these letters, Maxwell framed his "Dynamical Theory" not as his definitive formulation of field theory, or even of the electromagnetic theory of light, but primarily as a way to link the ratio of units to the speed of light without relying on a mechanical hypothesis like that in "Physical Lines." After citing the wave theory of light to establish that space is filled with a medium capable of storing and conveying energy by the motion and elasticity of its parts, he used a Lagrangian analysis to show that any medium capable of exerting the known electric and magnetic forces would also carry waves at a speed given by the ratio of units—which Weber and Kohlrausch's measurements had shown to be the speed of light, or something very close to it. Maxwell emphasized that in "Dynamical Theory" he was able to do all of this without invoking a detailed model like the vortices and idle wheels of "Physical

79. Maxwell moved from Glenlair to London between 27 Sep and 15 Oct 1864; see his letters to William Thomson of those dates, *SLP* 2: 172 and 175. The Royal Society of London received "Dynamical Theory" on 27 Oct 1864; it was "read" at the meeting of 8 Dec 1864, and published some time after Mar 1865; see *SLP* 2: 189 n.3.

80. Maxwell to Charles Hockin, 7 Sep 1864, in Campbell and Garnett, *Maxwell* (ref. 39), 340, repr. in *SLP* 2: 164.

81. Maxwell to William Thomson, 15 Oct 1864, *SLP* 2: 180.

82. Ibid., 187–88.

Lines." In a sense he was extending to the electromagnetic medium the macroscopic, measurement-based approach that he and Jenkin had followed in "Elementary Relations," though now assisted by the full apparatus of analytical dynamics.

Maxwell did not leave the vortices out of "Dynamical Theory" altogether, however. Having drawn on the wave theory of light to establish that space must be filled with a medium possessing both density and elasticity, he cited Thomson's analysis of the Faraday effect as proof that, in a magnetic field, portions of this medium must be rotating around the lines of force—that is, must form vortices. This led us, Maxwell said, "to the conception of a complicated mechanism," filling all space and "capable of a vast variety of motion," yet with its parts all connected in definite but as yet unknown ways.[83] But instead of proceeding to imagine a specific mode of connection, as in "Physical Lines," Maxwell now asked how, on general dynamical principles, the observable electric and magnetic phenomena must be related to one another, whatever the details of the underlying machinery.

Tait was right when he later wrote that Maxwell had based his "Dynamical Theory" on energy principles rather than a detailed mechanical model, but he went too far when he said that Maxwell had "discarded" his hypotheses about molecular vortices.[84] After seeing a draft of Tait's chapters, Maxwell wrote to him in December 1867. "There is a difference," he said,

> between a vortex theory ascribed to Maxwell at p. 57, and a dynamical theory of Electromagnetics by the same author in Phil Trans 1865. The former is built up to show that the phenomena are such as can be explained by mechanism. The nature of this mechanism is to the true mechanism what an orrery is to the Solar System. The latter is built on Lagranges Dynamical Equation and is not wise about vortices.[85]

The point of the last remark, of course, was that Maxwell *was* wise about vortices, and while he acknowledged that the connecting mechanism he had described in "Physical Lines" was as awkward and artificial as that of an orrery, he had scarcely more doubt that the vortices really existed and were spinning on their axes than he did that the planets really circled the sun. Even as he shifted his mode of formulating electromagnetic theory, Maxwell remained convinced that the Faraday effect proved that molecular vortices (or something

83. Maxwell, "Dynamical Theory" (ref. 2), 464, repr. in *SP* 1: 533.

84. Tait, *Sketch* (ref. 11), 74.

85. Maxwell to P. G. Tait, 23 Dec 1867, in *SLP* 2: 337.

very much like them) must exist in a magnetic field. His aim in "Dynamical Theory" was not to do away with the vortices, but to see how far he could get in explaining optical and electromagnetic phenomena without them.

Using Lagrangian methods, Maxwell proceeded to develop the equations of the electromagnetic field, taking the "electromagnetic momentum" or vector potential as his starting point. Early on he introduced the "displacement" that an electromotive force produces in a dielectric, comparing it to the elastic yielding machinery undergoes when subjected to a force. Though not an ongoing electric flow, such displacement was "the commencement of a current," he said, and its variation constituted a transient current that, when added to the conduction and convection currents, served to close otherwise open circuits.[86] After deriving a long list of relations among various electric and magnetic quantities, Maxwell combined several of them to form a wave equation, from which he extracted what he regarded as his most important result: that transverse waves of magnetic force would propagate through the electromagnetic medium at a speed of $\sqrt{k/4\pi\mu}$ —in free space, simply the ratio of units.[87]

Maxwell drew several experimentally testable consequences from this result, including that the square of the index of refraction of a transparent medium would be equal to the product of its specific dielectric capacity and its specific magnetic capacity.[88] He noted with evident satisfaction that his theory explained why most transparent solids are good insulators, whereas most good conductors are opaque, and suggested that apparent exceptions, such as the transparency of many electrolytes and the anomalously low opacity of gold leaf, might be traced to lower resistive losses at very high frequencies.[89] He also discussed propagation in crystalline media, but said nothing about reflection or refraction, having failed to satisfy himself concerning the proper boundary conditions. Recalling that Stokes, who was far more expert in the intricacies of the wave theory of reflection, had once told him that "the subject was a stiff one to the best skilled in undulations," Maxwell decided it would be best,

86. Maxwell, "Dynamical Theory" (ref. 2), 462, 480, repr. in *SP* 1: 531, 554.

87. Ibid., 497–99, repr. in *SP* 1: 577–80.

88. Ibid., 466, repr. in *SP* 1: 535. When Ludwig Boltzmann measured these quantities for various materials in 1873, he found they fit Maxwell's relationship reasonably well; see Darrigol, *Electrodynamics* (ref. 27), 259 n.129, and Jed Z. Buchwald, *The Creation of Scientific Effects: Heinrich Hertz and Electric Waves* (Chicago, IL: University of Chicago Press), 208–14.

89. Maxwell, "Dynamical Theory" (ref. 2), 504–5, repr. in *SP* 1: 587.

given the uncertainties, simply to leave it aside.[90] Nor did he address magneto-optic rotation in "Dynamical Theory"—an ironic omission, since it had been Thomson's analysis of the Faraday effect that first put Maxwell on the path that led him to "Physical Lines of Force" and so to the electromagnetic theory of light. Maxwell found, however, that the general Lagrangian methods to which he had restricted himself in "Dynamical Theory" were not competent on their own to account for magneto-optic rotation; as he wrote to Thomson in January 1873, while drafting the corresponding section of his *Treatise*, "It is very remarkable that in spite of the *curl* in the electromagnetic equations of all kinds Faradays twist of polarized light will not come out without what the schoolmen call local motion."[91] In "Dynamical Theory," Maxwell had tried to see how far he could get toward explaining optical and electromagnetic phenomena without invoking his molecular vortices. Here was his answer: pretty far, but not all the way to magneto-optics.

Having shown theoretically in "Dynamical Theory" that the electromagnetic medium would carry waves at a speed given by v, Maxwell next began marshalling the experimental evidence that the measured value of v in fact matched the measured speed of light. The best available value of the latter was 298,000 kilometers per second, measured just two years before by Léon Foucault using a rotating mirror apparatus. In "Dynamical Theory," however, Maxwell also cited Fizeau's older figure of 314,858 kilometers per second, as well as the value of 308,000 kilometers per second derived from the aberration of starlight, perhaps because including the two latter numbers yielded an average that, as he said, "agrees sufficiently well" with the only measurement of v then available, Weber and Kohlrausch's figure of 310,740 kilometers per second.[92] Maxwell, however, was clearly looking toward new and better measurements of v, as he and Jenkin had declared in "Elementary Relations" and as he had outlined in his letters to Hockin and Thomson. Indeed, the inclusion of the last section of "Dynamical Theory," on calculating the self-induction of

90. Maxwell to G. G. Stokes, 15 Oct 1864, in *SLP* 2: 186; see P. M. Harman, "Through the Looking-Glass, and What Maxwell Found There," in A. J. Kox and Daniel M. Siegel, eds., *No Truth Except in the Details: Essays in Honor of Martin J. Klein* (Dordrecht: Kluwer, 1995), 78–93. See also *SLP* 2: 182–85, a draft section, not included in the published "Dynamical Theory," in which Maxwell wrestled with the boundary conditions for reflection. Later in the 1870s, H. A. Lorentz and G. F. FitzGerald independently found ways to include reflection and refraction in Maxwell's theory; see Darrigol, *Electrodynamics* (ref. 27), 190–92, 323–24.

91. Maxwell to William Thomson, 22 Jan 1873, *SLP* 2: 784.

92. Maxwell, "Dynamical Theory" (ref. 2), 499, repr. in *SP* 1: 580.

a coil of wire, made little sense except in this context. The problem had no obvious connection to the rest of the paper, but it was important for the experimental determination of the British Association ohm and so for Maxwell's planned new determination of *v*.[93]

Maxwell hoped and expected that the measured values of *v* and the speed of light would converge as new and better experiments were performed, but for a long time, they stubbornly refused to do so. Once they had certified "B. A." resistance standards in hand, Thomson and a team of his Glasgow students, several of whom later became cable engineers, set about carefully measuring the electromotive force of a battery with both an electrometer and an electrodynamometer, arriving by 1868 at a value for *v* of 28.25 ohms or (taking the ohm at its intended value of 10^7 meters per second) 282,500 kilometers per second.[94] That same year Maxwell and Hockin, using a method that balanced the electrostatic attraction of two disks against the electromagnetic repulsion of two coils, found *v* to be 28.8 ohms or 288,000 kilometers per second. Given the great difficulty of the measurements involved, this was arguably not a bad match with Foucault's 298,000 kilometers per second for the speed of light, and it was certainly a striking fact that the two quantities should be even approximately equal.[95] But the 3% gap between Foucault's value for the speed of light and Maxwell and Hockin's for *v* was a far cry from the margin of just 0.02% that had fired Maxwell's enthusiasm in 1861, when he first compared Fizeau's reported value for the speed of light with Weber and Kohlrausch's for *v*. Throughout the 1860s and 1870s, the measured values of the two quantities remained too discrepant and uncertain to win over those like Thomson who doubted that light was really waves in the electromagnetic medium.

93. Ibid., 506–12, repr. in *SP* 1: 589–97. See also Kline, "Induction Motor" (ref. 65).

94. W. F. King, "Description of Sir Wm. Thomson's Experiments made for the Determination of *v*, the Number of Electrostatic Units in the Electromagnetic Unit," *Report of the Thirty-Ninth Meeting of the British Association for the Advancement of Science, held at Exeter in August 1869* (London: John Murray, 1870), 434–36; see also Schaffer, "English Science" (ref. 24), 150.

95. Maxwell, "Direct Comparison" (ref. 71), 644, 651, repr. in *SP* 2: 126, 135. See also I. B. Hopley, "Maxwell's Determination of the Number of Electrostatic Units in One Electromagnetic Unit of Electricity," *Annals of Science* 15 (1959): 91–108. Schaffer, "English Science" (ref. 24), 153, suggests that Maxwell took steps to "force" the measured value of *v* to match the known speed of light more closely, but Maxwell insisted that the measurements he rejected "were condemned on account of errors observed while they were being made," before he had calculated the values of *v* they implied; Maxwell, "Direct Comparison" (ref. 71), 650, repr. in *SP* 2: 135. Jenkin's 1873 reprint of *Reports of the Committee on Electrical Standards* (ref. 48), 92, included Foucault's value for the speed of light, given as 29.8 ohms, in the list of values of *v*.

A further complication arose from the fact that the values of *v* found by Maxwell, Thomson, and several later experimenters were based on the putative value of the ohm, and as time went on it became increasingly clear that all was not well with the original British Association standards. Although the committee had expressed confidence that the resistance of its coils lay within 0.1% of their intended value, tests in the 1870s by German, British, and American physicists indicated the certified standards were off by ten or even twenty times that amount.[96] In 1880, the British Association Committee was reconstituted and Lord Rayleigh, Maxwell's successor at the Cavendish Laboratory in Cambridge, set out to repeat the determination of the ohm using the original spinning coil apparatus. He also carefully reviewed Maxwell and Jenkin's procedures and calculations, and though he was unable to pinpoint the precise source of the discrepancy, he found they had made several significant errors, including transposing numbers when measuring their spinning coil and miscalculating its self-induction. Rayleigh concluded that the 1865 B. A. unit was 1.3% too small—about 9,870 kilometers per second instead of the intended 10,000.[97] As scientists in Britain and around the world developed improved methods of absolute electrical measurement, they gradually hammered out a corrected set of standards in their laboratories and in a series of international congresses.[98]

By the time measurements of the ratio of units and the speed of light definitively began to converge around 1890, Maxwell's theory had already largely won out on other grounds, notably those provided by Heinrich Hertz's dramatic discovery of electromagnetic waves in 1888. Had the gap between measurements of *v* and the speed of light persisted, it would have raised serious problems for Maxwell's theory, but in the event the eventual convergence in the values, though welcome, played a relatively small role in drawing adherents to the theory.[99]

96. For the claim that the resistance of the British Association coils "does not probably differ from true absolute measurement by 0.08 per cent," see "Report, 1864" (ref. 42), 346, repr. in Smith, ed., *Reports*, 161; for later evidence that the discrepancy was in fact much larger, see Schaffer, "English Science" (ref. 24), 161–62, and Olesko, "Precision" (ref. 15), 137–43.

97. Lord Rayleigh and Arthur Schuster, "On the Determination of the Ohm in Absolute Measure," *Proceedings of the Royal Society of London* 32 (1881), 104–41.

98. Larry Lagerstrom, "Constructing Uniformity: The Standardization of International Electromagnetic Measures, 1860–1912" (PhD dissertation, University of California, Berkeley, 1992).

99. On improvements in the measurement of *v* between the 1870s and 1907 and their convergence with measurements of the speed of light, see Schaffer, "English Science" (ref. 24), 163,

Maxwell's strategy in the mid-1860s of trying to clinch the case for his electromagnetic theory of light by showing that the ratio of units equalled the speed of light met with only equivocal success. Discrepancies in measurements and uncertainties over the precise value of the ohm proved too great to overcome the skepticism of Thomson and others, and when he addressed the issue in his *Treatise* in 1873, Maxwell felt he could say no more than that his theory was "not contradicted" by the available measurements.[100] But what he had seen as a preliminary step in that strategy, of showing on general dynamical grounds that the electromagnetic medium would carry waves at a speed given by the ratio of units, proved remarkably fruitful, for it played a large part in leading him to formulate his general equations of the electromagnetic field.

CONCLUSION: "A TREATISE ON ELECTRICAL MEASUREMENT"

George Chrystal was a Scottish-born Cambridge wrangler who worked closely with Maxwell at the Cavendish Laboratory from 1874 until 1878 and later became professor of mathematics at the University of Edinburgh. He knew Maxwell and his work well, and when the second edition of Maxwell's *Treatise* appeared in 1881, two years after Maxwell's death, Chrystal was asked to review it for *Nature*. The resulting review is valuable and interesting in many ways, not least for a striking remark bearing on Maxwell's work for the British Association Committee. Maxwell's *Treatise on Electricity and Magnetism*, Chrystal declared, is "in the strictest sense a Treatise on Electrical *Measurement*," for it "looks at electrical actions almost exclusively as measurable." Indeed, he said, much of the *Treatise* represented "a continuation of the labours of its author in conjunction with the rest of the distinguished band of electricians who formed the Committee of the British Association on Electrical Measurements."[101]

Chrystal's remark reinforces the point, argued above, that Maxwell's work for the British Association Committee was not a mere side project for him, but

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and Rosa and Dorsey, "Comparison" (ref. 50), 606–07. On the growing acceptance of Maxwell's theory in the 1880s and 1890s, see Jed Z. Buchwald, *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century* (Chicago, IL: University of Chicago Press, 1985), and Bruce J. Hunt, *The Maxwellians* (Ithaca, NY: Cornell University Press, 1991).

100. Maxwell, *Treatise* § 787.

101. Chrystal, review of *Treatise* (ref. 61), 238.

lay firmly in the main line of his scientific development, and had a lasting effect on the way he approached electrical questions—including in his *Treatise.* The *Treatise* has customarily been seen as a work of electromagnetic *theory*, indeed, as the prime expression of Maxwell's own field theory and the foundation of its later development.[102] It was certainly that. But anyone who sits down and actually reads through its two thick volumes is likely to be struck by how little of the *Treatise* focuses on field theory, much less on Maxwell's own distinctive contributions to the subject. Instead one finds its pages filled with long accounts of one- and two-fluid electrical theories, extended discussions of the mathematical intricacies of such things as spherical harmonics and confocal surfaces, and detailed descriptions of the workings of guard ring electrometers and other electrical and magnetic instruments. As Andrew Warwick has shown, different groups read the *Treatise* in very different ways and for very different purposes, even just within Cambridge. Those attending W. D. Niven's intercollegiate lectures might delve into the physics of Maxwell's theory (as did George Francis FitzGerald, Oliver Heaviside, and a scattered band of others outside Cambridge), but most Tripos coaches turned to the *Treatise* solely for its treatment of mathematical techniques and told their students to skip almost everything we would now regard as "Maxwell's theory," whereas experimenters at the Cavendish generally focused instead on Maxwell's accounts of instruments and how to use them.[103]

Besides the college lecture hall, the Tripos coaching room, and the nascent Cavendish Laboratory, Maxwell's *Treatise* should also be seen in relation to a fourth context: the testing rooms of the great cable telegraph companies. The British Association Committee had been set up in 1861 largely to meet the needs of the growing British cable industry, and in his review of the *Treatise*, Chrystal emphasized how profoundly the development of electrical units and standards, and the ideas associated with them, had affected technological practice. "Instead of the old vague, unscientific, and still more, unbusinesslike

102. This is reflected, for instance, in Thomas K. Simpson, *Figures of Thought: A Literary Appreciation of Maxwell's "Treatise on Electricity and Magnetism"* (Santa Fe: Green Lion Press, 2005), which focuses entirely on ten chapters in Part IV of Maxwell's *Treatise* and thus leaves aside 85% of the book.

103. On the mathematical Tripos examination at Cambridge and the culture of "coaching" for it, as well as on the reception of Maxwell's theory elsewhere at Cambridge, see Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (Chicago, IL: University of Chicago Press, 2003), 286–356; on the reception and development of Maxwell's theory outside Cambridge, see Hunt, *Maxwellians* (ref. 99).

statements of quantity and intensity," he noted, "we have the precise ideas of electromotive force, resistance, current, and so on, measured in their respective units, the volt, the ohm, the ampère." Now, he declared with evident satisfaction, "electrical commodities can be bought and sold by rule and measure, as heretofore cloth, coals, or horse-power."[104] Here Chrystal was echoing remarks Maxwell had made in his 1873 review of Jenkin's *Electricity and Magnetism* and that William Thomson had often made about the commercial measurement of physical quantities.[105] Crosbie Smith, Norton Wise, and Simon Schaffer have all drawn attention to the ways commercial values and concerns informed the work of the British Association Committee on Electrical Standards.[106] Chrystal's point (and mine) is that these values and concerns shaped Maxwell's wider work as well, and are reflected both in his *Treatise* and in the crucial shifts his thinking underwent in the 1860s and 1870s.

The most striking of these shifts came, of course, in 1863–64, when Maxwell was fresh off his work on "Physical Lines" and had just joined the British Association Committee. At this crucial juncture, as he sought to capitalize on his discovery that light was evidently a disturbance in the electromagnetic medium and that its speed was given by the ratio of electrostatic to electromagnetic units, he considered how he could present his results in a way that would win them the widest possible support. He never gave up his belief that magnetic fields are filled with molecular vortices; indeed, when he returned to the Faraday effect toward the end of his *Treatise*, he said he still thought "we have good evidence for the opinion that some phenomenon of rotation is going on in the magnetic field" and that "this rotation is performed by a great number of very small portions of matter, each rotating on its own axis."[107] But Maxwell's collaboration with Jenkin on "Elementary Relations" and on the determination of the ohm, and his exposure to engineers' characteristic ways of thinking, had brought home to him the value of framing his results not in terms of unseen microstructures, but as far as possible in terms of relations among measurable quantities. He was thus led to step back from the vortex

104. Chrystal, review of *Treatise* (ref. 61), 238.

105. [Maxwell], review of Jenkin (ref. 45). Thomson argued that the farad should be defined in terms of a "real purchaseable tangible object" (i.e., a condenser of specified capacity) rather than a quantity of charge; see William Thomson to Maxwell, 24 Aug 1872, in *SLP* 2: 749 n.8.

106. Smith and Wise, *Energy and Empire* (ref. 23), 687–98; Smith, *Science of Energy* (ref. 4) 277–87; Schaffer, "Manufactory" (ref. 13), 24–26; and Schaffer, "English Science" (ref. 24), 135–36.

107. Maxwell, *Treatise*, § 831.

FIG. 1. Portrait of James Clerk Maxwell, depicting him with the spinning coil apparatus he and Fleeming Jenkin used in 1863–64 to determine the value of the ohm. Painted by R. H. Campbell and presented to the Institution of Electrical Engineers by L. B. Atkinson in 1929, it hung for many years in the Maxwell Room at the London headquarters of the IEE (now the Institution of Engineering and Technology). *Source*: IET Archives (www.theiet.org/resources/library/archives), UK0108 IMAGE 1/1/0319.01.

model of "Physical Lines" and instead formulate the general equations of his "Dynamical Theory."

Maxwell was a man of many sides. As the titles of at least two books about him attest, he is often—and rightly—viewed as a "natural philosopher," perhaps one of the last in a long British line.[108] But he had another side, too, as

108. Everitt, *Maxwell* (ref. 4); Harman, *Natural Philosophy* (ref. 4).

seen in his work for the British Association Committee on Electrical Standards, and while he was no William Thomson, knee-deep in Glasgow commerce and industry, Maxwell developed closer links to the practicalities of electrical engineering than have often been recognized. He was, for instance, an early member of the Society of Telegraph Engineers and took an active part in its work promoting accurate electrical measurement. It is fitting that the portrait of Maxwell that was presented in 1929 to the Institution of Electrical Engineers, as the STE had by then been renamed, and that for many years adorned the "Maxwell Room" at the Institution's London headquarters, depicts him sitting with the spinning coil apparatus that he and Jenkin had used to determine the ohm, and that had done so much to shape his thinking on electrical questions (fig. 1).[109]

109. On Maxwell's early membership in the Society of Telegraph Engineers, see Rollo Appleyard, *The History of the Institution of Electrical Engineers, 1871–1931* (London: Institution of Electrical Engineers, 1939), 43, and *SLP* 3: 201 n.8. The portrait of Maxwell by R. H. Campbell was presented to the IEE in 1929 by L. B. Atkinson; as of 2014, it is in storage while the headquarters of the Institution of Engineering and Technology (successor to the IEE) is being renovated. I thank Jonathan Cable of the IET Archives for this information.