“Practice vs. Theory”
The British Electrical Debate, 1888–1891

By Bruce J. Hunt*

MAXWELL’S THEORY OF THE ELECTROMAGNETIC FIELD reached maturity in the last third of the nineteenth century; in these same years, the new electrical industries were advancing rapidly and beginning to transform Western life. It took some time for the new theory to penetrate electrical practice; it was not common for engineers to use Maxwellian theory in their work until the 1890s or even later. This study will explore how Maxwell’s followers in Britain used Hertz’s discovery of electromagnetic waves in 1888 to promote their theory and push it into practical applications. This effort was resisted by older electrical engineers, “practical men,” as they called themselves, who saw in it an attempt to rob them of their accustomed control over their own work and to subordinate them to the authority of the theorists. There ensued a lively and sometimes bitter controversy, a contest, as it was called, of “Practice vs. Theory.” The scientists eventually won their point, and the old “half-educated electricians” were left behind, replaced by college-trained electrical engineers.¹

The background to this transition, and the means scientists used to promote it, are the subjects of this study.

MAXWELLIANS, ELECTROMAGNETIC WAVES, AND HERTZ’S DISCOVERY

During the late 1870s and the 1880s Maxwellian theory was developing rapidly, to the point where it could predict new phenomena and have something to say about practical problems. Maxwell had published his Treatise on Electricity and Magnetism in 1873; he died just six years later. He left his theory with a number of gaps and obscurities, and the work of clarifying and extending it, notably in the treatment of electromagnetic waves, fell to a younger group of British physicists.

¹ Department of the History of Science, Johns Hopkins University, Baltimore, Maryland 21218.

An earlier version of this essay was awarded the Henry and Ida Schuman Prize for 1980. I would like to thank Professor Robert Kargon for his help and advice, and the following archives for permission to quote materials in their possession: University College, London; the Royal Dublin Society; the Institution of Electrical Engineers (London); the American Institute of Physics; and Johns Hopkins University.

Although he was a professor at Cambridge, Maxwell made little effort to establish a "school" to promote his theory. J. H. Poynting (1852–1914) was the only one of his students to become a leading Maxwellian—and he had studied gravitation under Maxwell, not electricity. Many of the most important Maxwellians had little to do with Cambridge; they learned Maxwell's theory from his book. But they shared more than an interest in electricity and a characteristic set of Maxwellian ideas; they also formed a network sustained by correspondence and personal friendship. They came to see themselves and were regarded by others as constituting a community of interest. Most of them were physics professors: Poynting at Birmingham, Silvanus Thompson (1851–1916) at London, George Francis FitzGerald (1851–1901) at Dublin, and Oliver Lodge (1851–1940) at Liverpool; H. A. Rowland (1848–1901) of Baltimore was an American member of the group.

FitzGerald and Lodge were particularly prominent in the debate over practice versus theory. FitzGerald, the son of a Protestant bishop, spent almost his entire life at Trinity College, Dublin—as student, fellow, and (after 1881) professor of natural philosophy. Although he was a powerful mathematician, probably his most important contributions to science were the incisive comments and suggestions he threw off in discussions. FitzGerald carried on a particularly vigorous correspondence on electromagnetism with Lodge. Lodge, the son of a prosperous Staffordshire clay merchant, had managed to extricate himself from a business career to pursue science; he was made professor of physics at Liverpool in 1881. Besides being an active experimenter, he was a scientific publicist and especially active as a propagandist for Maxwellian theory.

The third leading Maxwellian in the debate following Hertz's discovery—and an exceptional one in almost every respect—was Oliver Heaviside (1850–1925). Heaviside, an unemployed, largely self-educated former telegrapher, had set out in the 1870s to master electromagnetic theory. Reclusive and eccentric, he made a meager living (about £40 a year) by selling articles to The Electrician, a London trade and technical journal. It was not until the period of the debate, when Hertz's discovery vindicated his mathematics, that Heaviside entered into active correspondence with the other Maxwellians.

Work on electromagnetic theory in Britain had entered a relatively quiescent phase somewhat before Hertz's announcement of his discovery in 1888. FitzGerald and Lodge had discussed electromagnetic waves as early as 1879. After some initial confusion, in 1882–1883 FitzGerald worked out how to gen-


erate waves by discharging a condenser and calculated how much energy would be radiated.⁴ He and Lodge were stymied, however, by their inability to devise an adequate detector—"something to feel these rapidly alternating currents with."⁵ During roughly the same period Heaviside, using vector and operational methods of his own devising, had cast Maxwell’s theory into the four "Maxwell’s equations" still in use today and applied them to problems in telegraphy and telephony. The most important of Heaviside’s work came in a long series of articles, "Electromagnetic Induction and its Propagation," begun in 1885 in The Electrician.⁶ The series, largely a mathematical treatment of electromagnetic wave propagation, particularly along wires, was abruptly terminated in 1887 when Heaviside’s application of this theory to telephony, especially his conclusions about the effects of self-induction, led him into a dispute with William Henry Preece, the head of the government telegraph service. Preece, a leading opponent of Maxwellian theory, took steps to block publication of Heaviside’s work.

Early in 1888 Lodge was led back to work on electromagnetic waves by a request that he lecture on lightning conductors. Some preparatory experiments using Leyden jar discharges to imitate lightning convinced him of the importance for lightning conductors of self-induction—the process by which the changing magnetic field produced by a varying current sets up an electromotive force opposing the variation. He also found that if he discharged the jars through long wires, he could set up standing electromagnetic waves along the wires and display resonance effects. Lodge, who was a bit of a showman, was sure this would be the hit of the meeting of the British Association of the Advancement of Science scheduled for September 1888 at Bath.⁷

Electromagnetic waves were indeed the hit of the meeting, but not Lodge’s. Reports came in that Heinrich Hertz (1857–1894) of Karlsruhe had solved the detector problem and, with his simple "resonator," had displayed the waves in free space—a far more striking experiment than Lodge’s, and one that brought into much sharper relief the role of the field in conveying electromagnetic effects.⁸ Hertz later showed how to reflect, refract, diffract, and polarize the waves, illustrating their essential identity with light, in accordance with Maxwell’s theory. Hertz had come to the theory through Hermann von Helmholtz, who had attempted to embrace within a generalized scheme both Maxwell’s theory and the action-at-a-distance electrodynamics of Weber and Neumann then dominant in Germany. It was only in the course of his experiments, in an effort to

---


understand them, that Hertz really moved toward Maxwell. As Heaviside observed to Lodge early in 1889, Hertz "was not a Maxwellian in the first place, but now goes in for M."\(^9\)

The British Maxwellians gave Hertz’s experiments an enthusiastic welcome. Before 1888 their work had been considered quite esoteric and hypothetical; they were sometimes dismissed, according to Heaviside, as "working out a mere paper theory." Hertz’s work gave the Maxwellians an answer to this reproach by putting the theory on a solid experimental foundation. As Heaviside put it, "the very slow influence of theoretical reasoning on conservative minds was enforced by the common-sense appeal to facts," and the Maxwellians could now attract attention and compel assent to the theory from those who were unswayed by its mathematical beauties.\(^10\) Hertz had given the Maxwellians a powerful tool with which to promote their theory, and they used it.

In 1888 FitzGerald was especially well prepared not only to appreciate Hertz’s experiments but, as president of the mathematical and physical section of the British Association, to point out their importance. In Lodge’s words, FitzGerald "pounced on Hertz’s work" when it came out, and in his address to the section at Bath on 6 September he laid the work before the assembled physicists as the experimentum crucis proving Maxwell’s contention that electromagnetic actions are due to an intervening medium. FitzGerald emphasized that this was not just of interest to specialists, but had implications for all of science; he also mentioned its relevance to telegraphy and alternating-current machines.\(^11\)

In the wake of FitzGerald’s address, Hertz’s work was widely acclaimed in the scientific press as having "marked an epoch" in the history of electrodynamics.\(^12\) The British Maxwellians were fully conscious of the value of the work, because it fit so beautifully with their own ideas. Hertz’s papers were translated and his work expounded in public lectures; British physicists repeated and extending his experiments. It all succeeded in "stirring up an amount of interest" in Maxwell’s theory "that was quite wonderful to witness," Heaviside wrote in 1892, particularly for one like himself who had been pursuing the theory of electromagnetic waves for years without stirring up much interest at all. Hertz had, Lodge said, "cut right into the ripe corn of scientific opinion in these islands."\(^13\)

THE PRACTICAL MEN AND THE THEORETICAL ISSUE

Another group in Britain was "ripe" for the lessons of Hertz, though not in the same sense as the Maxwellians. The industrial application of electricity was advancing rapidly in these years, and by the late 1880s electrical engineers were encountering phenomena, particularly in alternating-current circuits and long-distance telephone lines, where their old "rule of thumb" procedures ceased to

---

9 See Russell McCormmach, "Heinrich Hertz," DSB; Heaviside to Lodge, 11 March 1889, in the Lodge Collection, UCL MS Add. 89/50.
These were field effects, precisely the sort of Maxwellian phenomena that Hertz had studied in free space. Lodge along wires, and Heaviside, so to speak, on paper. The increased incidence of these effects intensified an existing conflict between physicists and electrical engineers over who held the requisite knowledge to account for and control the phenomena. The publicity given Hertz’s discovery brought things to a head, and during the next few years the conflict flared into the open over a group of closely related technical issues.

The basic scientific point in dispute was the role of the field in conveying electrical energy, in particular, the importance of self-induction. The “practical men,” led by Preece, wanted to keep to the simple formulas and procedures that had worked for them in the past, and they denied that self-induction—“retardation,” as they called it—played a significant role. It was “a bug-a-boo,” Preece said, and it was only because of the theorists’ “mania for self-induction” that they kept injecting it into every electrical discussion. The theoretical camp, in contrast, held that “the despised self-induction is in fact the great moving agent” by which all electrical energy is conveyed, and that even engineers could no longer afford to ignore it.14

Even without Hertz’s discovery, the British Association meeting was scheduled to be the scene of a battle in this war of “Practice vs. Theory.” Lodge had sparked the controversy in his lectures before the Society of Arts in spring 1888 by declaring that when a lightning conductor was struck, its self-induction was far more important than its resistance, and that consequently the orthodox methods of lightning protection were seriously flawed. The claim directly contradicted the recommendations made in 1882 by a Lightning Rod Conference of which Preece had been a member; that report had ignored self-induction and prescribed a fat copper conductor of very low resistance as the ideal protection from lightning.15 Preece and his colleagues in the practical camp thought Lodge’s conclusions absurd and resolved to put this theoretician in his place. Toward that end, a public discussion was set for the British Association meeting.

In his experiments Lodge had simulated lightning by discharging Leyden jars. Preece and others questioned whether this was a valid analogy, and it was indeed the weakest part of Lodge’s argument.16 The more substantial issue, however, was the alleged effect of self-induction in radically increasing the impedance of a conductor to a rapidly alternating current such as resulted from a Leyden jar discharge or, according to Lodge, a lightning bolt. According to Maxwellian theory, as elaborated by Heaviside, Poynting, and Lord Rayleigh (J. W. Strutt), this was essentially a field effect, and it involved nothing less than the nature of an electric current.

To most “practical men” like Preece, a current in a wire was much like the flow of water in a pipe.17 There might be some modifications—picture the pipe as elastic, to simulate capacitance, for instance, or filled with baffles to simulate resistance—but it was basically a simple and intuitive picture, and a remarkably useful one. The essential insight of Maxwell’s theory, however, was to focus on

the field, not the wire, as FitzGerald put it, "According to Maxwell’s view, there is a great deal more going on outside the conductor than inside it." The most striking implication of this view was one Poynting (and independently Heaviside) had derived from Maxwell’s theory in 1884—the so-called "Poynting flux," according to which electrical energy is conveyed not by the current flowing in the wire, but by the ether in the space surrounding it. In 1885 FitzGerald sought to illustrate this with a model he devised (and actually built) consisting of a plane array of wheels set on vertical axles and connected to their neighbors by rubber bands. The spinning of the wheels corresponded to a magnetic field and strain in the bands to an electric field. An electric current was characterized entirely in terms of the magnetic field around it; a conductor was simply a region where the bands could slip and dissipate energy into heat. This illustrated the Poynting flux very well: energy flowed into a wire sideways from the "whirling machinery" in the ether. The model also illustrated self-induction on Maxwellian principles: when a current was started or reversed, it had to react against the momentum of this whirling machinery, or, in electrical terms, against the self-induction. The self-induction thus acted like a flywheel to oppose any change in the current.

In a Leyden jar or lightning discharge through a small resistance, the current oscillated, according to Lodge's calculations, with a frequency of about a million cycles per second. At such frequencies the self-induction would, in the Maxwellian view, not only become an important factor in opposing the rush of the current, but would completely overshadow the ordinary resistance of the wire. The contrast with Preece's view becomes clear: to him, a lightning conductor should simply be able to convey a large amount of electricity quickly, rather like a fat drainpipe. But according to Lodge, the real aim should be to minimize the self-induction. Providing a conductor of very low resistance was at best a waste of copper, Lodge said, since the self-induction would dominate the situation anyway.

In replying to Lodge, Preece had more at stake than discrediting the notion of self-induction. Nor was he defending merely the honor of the Lightning Rod Conference and the practices of the Post Office, though these were important considerations. His main aim was rather to assert the primacy of practice over theory.

Preece was a fitting spokesman for the "practical men," for he was in many ways typical of the group, though more articulate and certainly more of a public figure than most engineers. Born in 1834, he was about fifteen years older than most of the Maxwellians. Like many other practical electricians, Preece began his career with the old Electric Telegraph Company, where, in the absence of any systematic training programs, he had to learn by doing. He was an able manager, and after the British telegraphs were taken over by the government, he became Chief Electrician at the Post Office, in charge of the country's tel-

---


egraphs (and later its telephones) and a leading authority on electrical engineering. Preece had a long list of honors: he was president of the Society of Telegraph Engineers in 1880 and again in 1893 (after it was renamed the Institution of Electrical Engineers), of the Institution of Civil Engineers in 1898, and of the British Association’s engineering section in 1888. In 1899 he was knighted. He also had the scientific prestige of having been elected a fellow of the Royal Society in 1881, and he served on its council in 1887–1889. But Preece never pursued the theoretical side of electricity; like many Britishers of the day, he was strongly suspicious of all theories and abstractions. “Stern experience,” he declared, was “the best of all teachers—superior to all the theory in the universe.”

He and most other “practical men” regarded Maxwellians like Lodge, and especially Heaviside, as upstarts who had too little respect for the “stern experience” of their elders and placed too much reliance on a mere theory.

Preece’s attitude was amply demonstrated in his suppression of Heaviside’s work, already alluded to above. As early as 1873, when Heaviside published a paper on duplex telegraphy, Preece had found it “most pretentious and impudent,” and he and his colleagues vowed “to pot Oliver, somehow.” Heaviside’s work in the 1880s on cable transmission again drew Preece’s ire. Preece had long held that self-induction (“retardation”) was the enemy of the telegrapher and the telephonist, and he asserted that long-distance telephony was only possible over lines of very low self-induction. He had experiments done, and from the quality of speaking on copper lines he argued that these must have a very low inductance—less than 1%, in fact, of the value arrived at by anyone else’s methods of calculation. It was, Heaviside later wrote Lodge, “really dreadful stuff, rank quackery.” In fact, as Heaviside pointed out, it was the relatively high inductance of the copper lines, along with their low resistance, that made them good for telephony. He had found mathematically from Maxwell’s theory that distortion could be lessened, even eliminated, by properly loading cables with extra self-induction. In an article sent to The Electrician in 1887, he described Preece’s paper in characteristically blunt terms as “radically wrong... in methods, reasoning, results, and conclusions.”

But that article was not published. Preece had taken measures to keep Heaviside’s work on self-induction from seeing print. Heaviside had collaborated with his brother Arthur, a Post Office telegraph engineer, on an article on telephony for the Journal of the Society of Telegraph Engineers and Electricians. Before being published, it had to be cleared by the Post Office censors. Preece used his position to block it, and sent Arthur a letter Oliver described as “savage and even insulting.” Preece was especially upset with Oliver’s section of the paper, which contradicted Preece’s own views on self-induction. As Heaviside said later, “the official censor ordered it all to be left out, because he considered that the Society was saturated with self-induction, and should be given credit

21 Ibid., pp. 109–110.
24 Heaviside to the editor of Philosophical Magazine, 18 July 1887 (draft), Heaviside Papers, Institution of Electrical Engineers, London.
for knowing all about it." When Heaviside submitted versions of the work elsewhere, "a certain peculiar concurrence and concatenation of circumstances" led to its being turned down by four other journals, "the resultant effect of which was to screen Mr. Preece from criticism." 25 C. H. W. Biggs, who had consistently supported Heaviside, was removed as editor of The Electrician in October 1887, and publication of Heaviside's series "Electromagnetic Induction and its Propagation," which had been running since 1885, was abruptly stopped. The new editor, W. H. Snell, simply told Heaviside that his papers did not attract enough readers; in fact, he said, "I have not been able to discover any." As Heaviside said later, "exactly when I came to making practical applications in detail of my theory," as he was doing in the 1887 articles, he was brought "to a dead stop." 26 Preece and his "practical men" would not stand for a "mere mathematician" like Heaviside intruding self-induction and Maxwellian theory into the preserves of engineering practice.

This attitude underlay Preece's remarks in the debate at Bath. Preece made the point that as Chief Electrician of the Post Office he supervised 500,000 lightning conductors, and thus could draw on voluminous practical experience, while Lodge had nothing but a theory and a few experiments. Yet Lodge presumed to tell practical men how to order their affairs. Preece's repeated criticisms of those who were "slaves of mathematics" and who announced conclusions based on "mere mathematical development" instead of practical experience, led Rayleigh to comment that listening to Preece "one felt that 'mathematician' was becoming almost a term of abuse." Preece's main target, however, was not the mathematical physicist content to work out his results in an ivory tower, but the young fellows coming out of the Technical Institutes with "a smattering of mathematics" who "thrust upon the electrical world conditions and conclusions with a coolness and effrontery that was simply appalling." It was this tendency, which he said was "growing to a very serious extent," that Preece sought to combat: he wanted to keep higher mathematics and those trained in higher mathematics from dominating electrical engineering, and to retain control in the hands of practical men like himself. 27 To do this, he had to show that mathematically complicated factors such as self-induction, high-frequency oscillations, and various field effects—in short, Maxwell's theory—were irrelevant to electrical practice.

How far Preece went in rejecting theory is shown in a remarkable letter he wrote to Lodge in October 1888. After accusing Lodge of being the "principal culprit" in stirring up the controversy, he said, "You rather accept the dictum that the practical man is behind the theoretical one. Now I have been as you know very actively engaged on the practical side for 35 years, and I cannot recall to mind one single instance where I have derived any benefit from pure theory"

27 "Discussion," Electrician, 1888, 21:644–645, 674, 679; cf. Elec. Rev., 1888, 23:394, where a "practical man" complains that "when some young shaver shoots off his school learning" in mathematics, it might as well be in Italian for all the good it did most electricians.
—a statement that may well have been true. But Preece also denied that
theory had led Sir William Thomson (Lord Kelvin) to improvements in sub-
marine cables, or John Hopkinson to better dynamo designs; the work had all
been done by practical men, "without any help from your so called 'Theorist.'"
As a parting shot, Preece declared that "the new views of electricity that you
are now propounding, and that are being so admirably developed by Hertz, are
simply those which I have been preaching and teaching for the last 20 years and
it seems to me that the teaching of Hertz is absolutely the reverse of your own
views as propounded by yourself in your early papers in 'Nature.'"28 Lodge,
who already believed Preece to be "something of an ignoramus," sent the letter
to others to convince them of Preece's ignorance. Thomson called the letter
"really quite too monstrous," and said, "as to submarine cables he is of course
very much adrift." Lodge's old professor, George Carey Foster of University
College, London, whom Preece had claimed as an ally, found the letter "a very
fancy revelation" of Preece, whom he considered "a thorough-paced humbug"
in his scientific views. Foster advised Lodge to tell Preece "(in parliamentary
language) that what he says about Hertz and himself is absolute rot and ast-
tounding self-conceit, and generally that his estimate of the value of science to
practice is absurd."29

THE PRACTICAL PROBLEM

Preece was not likely to be converted by anything Lodge might tell him; as he
observed in another letter, after a renewed debate on lightning conductors, "You
and your friends believe one thing—W. H. P. and the old boys believe another,
and no one seems inclined to be convinced even against his will."30 But Preece
and "the old boys" faced more than professors' arguments; advances in elec-
trical practice itself meant that complicated field effects of the sort dealt with
in Maxwell's theory were cropping up more and more. With the invention of
the telephone, relatively high frequency signals (electromagnetic waves, in fact)
were being sent along wires. Preece found it increasingly difficult to ignore the
effects of self-induction—though he tried—in the long-distance lines coming into
use in the late 1880s.31 Again, increased use of alternating currents for power
transmission meant that field effects that could be ignored for direct currents
now had to be attended to.

Although Maxwellian ideas seemed terribly counterintuitive, even nonsensi-
cal, to many of the old "water-in-a-pipe" practical men, they did explain field

28 Preece to Lodge, 26 Oct. 1888, in Lodge Collection, UCL MS Add. 89/86. Preece was appar-
ently under the impression that Hertz's work was related to his own interest in methods of space
telegraphy by induced currents (see Baker, Preece, pp. 254–257); they are in fact very different.
There was further confusion on this point in 1896–1897, when Preece promoted the work of Marconi
for some time before appreciating that it was an application of Hertz's waves (see Jolly, Lodge, pp.
124–128).
29 Lodge to Heaviside, 23 Sept. 1888 (draft); W. Thomson to Lodge, 26 Jan. 1889; G. C. Foster
to Lodge, 29 Oct. 1888; Lodge Collection, UCL MSS Add. 89/50, 89/107, 89/38.
30 Preece to Lodge, 17 May 1889, Lodge Collection, UCL MS Add. 89/86.
31 On the problems with Preece's "KR Law" for telephony, which ignored self-induction, see "A
Law which is No Law" (editorial), Electrical World (New York), 1893, 21:117; cf. D. G. Tucker,
"The First Cross Channel Telephone Cable: The London-Paris Telephone Links of 1891," Trans-
effects. S. A. Varley, for instance, called the Poynting flux, the Maxwellian idea of the ether as the conveyor of electromagnetic energy, "a curiously complex hypothesis," the product of over-active imaginations; he concluded that if this was the sort of thing Maxwellians were trying to palm off on the public, then "we of the older schools . . . will not just yet have to retire to a back seat."

But Poynting's idea did, in fact, clarify a number of phenomena of alternating currents; as Lodge told Varley, this was "no question of flights of fancy or luxuriance of imagination" but of "downright fact."32 For instance, in a rapidly alternating current only the outer part of the wire actually carries the current: the current is being driven by the "whirling machinery" outside the wire, and this can convey its effect only a little way into the wire before it reverses. Thus the current sloshes back and forth along the surface while the interior of the wire remains quiescent. This is called the "skin effect," and it clearly had to be taken into account in alternating-current power engineering. Lodge had found evidence of the skin effect in his Leyden jar experiments, and at Bath Sir William Thomson added his voice to those endorsing the effect; citing Heaviside's work, he announced that for a frequency of 150 cycles per second, such as was then supplied by some commercial dynamos, the current penetrated only a few millimeters into the wire, so that a very thick conductor was a waste of copper.33

Although many practical men accepted the skin effect entirely on faith, relying on the enormous reputation of Thomson ("When Sir W. Thomson says so," Heaviside quoted one as saying, "who can doubt it?")34, others continued to hold out against the new views. According to Preece, Thomson's findings had "starled the electrical world"—or at least that large part of it unfamiliar with Maxwell's theory. One of the intransigent engineers was Samuel Alfred Varley (1832–1921). Very much an old-style practical man, Varley left school at fourteen to learn telegraph engineering in the workshop of his older brother Cromwell Fleetwood Varley, who was later prominent in the laying of the Atlantic cable.35 In 1866 S. A. Varley was among the inventors of the self-exciting dynamo, work for which he felt he never received proper credit. He often expressed the belief that pioneers like himself were being pushed aside by glib-talking, mathematics-spouting professors of physics, many of whom, it seems, were Maxwellians. These professors, Varley claimed, were trying to establish a monopoly of authority; reaching deep into the bag of traditional British invective, he denounced Maxwellianism as "scientific Popery." Maxwell's field theory, Varley said, "rests solely on hypothesis," and Hertz's experiments "do not seem to bear very directly on practical electrodynamics." As for the Poynting flux and the skin effect, these were absurd and unproved; Varley contended, in strictest "water-in-a-pipe" form, that an electric current "harmonises in every respect with the transmission of force through a hydraulic system."36

While Varley had some support from other "practical men," the Maxwellians for the most part ignored him as an extreme case. They found the more moderate John T. Sprague worth refuting. Sprague, a self-described "half-educated electrician," had written a widely read book on electricity in 1875.37 He carried on a long battle against Maxwell's theory, and particularly the Poynting flux, in letters to and articles for The Electrician in 1891. According to Sprague, the Maxwellians were using the advent of field effects in technical applications to push their own erroneous views. "The fact is," he declared,

that it is only since alternating currents became of practical importance, and the effects of make and break had to be explained and formulated, that these new imaginations became serious; the professors found it easier to invent theoretical explanations than to study material facts, so they converted our old enemy—retardation—into conduction by the ether.

Rejecting FitzGerald's claim in his Bath address that Hertz's experiments proved that "non-conductors can and probably always do, as Professor Poynting has taught us, transmit electromagnetic energy," Sprague countered that "As far as I can see the Hertz experiments prove nothing," and that the Maxwellian theory was nonsense.38

Sprague was answered, principally by Silvanus Thompson (in consultation with Heaviside), and his views roundly discredited. Thompson pointed to Hertz's experiments as "the most striking evidence that electric energy can be, and is conveyed by the medium."39 The Maxwellians held that Sprague's refusal to acknowledge those experiments condemned him, like Varley, as close-minded and hopelessly out of date, and that the old views could be maintained only by closing one's eyes to established facts. In an editorial summing up the 1891 debate, The Electrician concluded that Thompson and the Maxwellian side had won decisively, and that Sprague's assumption that the energy flowed in the wires, despite its long acceptance by practical electricians, had no real basis in fact.40

But Sprague had pointed to the heart of the dispute. Practical men like himself knew that alternating current and related developments were bringing new phenomena into play, but they did not want to give up their old, commonsense ways of dealing with electricity. They had been working with electric currents for years with great success, and they resented "the professors" telling them they had been all wrong. They did not, as Varley had said, want to take a back seat to the upstart Maxwellians. The practice versus theory debate was an attempt by the practical men to hold their own ground against the incursions of the scientists.

40 "The Transfer of Energy" (editorial), Electrician, 1891, 27:270.
THE OUTCOME OF THE DEBATE

The issue of authority lay at the heart of the conflict between practice and theory, and made it especially bitter. A cartoon in the December 1888 *Electrical Plant*, a London trade journal, graphically illustrates the attitude of the "practical men" toward the "professors." Depicting the lightning-conductor debate, it shows Preece, in his hand a lightning rod with the flag of "Experience" attached, standing in triumph, his foot to the throat of the vanquished Lodge, labeled "Experiment." An editorial in *The Engineer*, quoted by Lodge in his report on the Bath meeting, further illustrates how deep the split was between scientists and engineers, and that the issue at stake was one of relative standing. It was time, the editorial declared, to quash "the preposterous pretensions put forward by some men of so-called pure science" and to recognize that "the world owes next to nothing to the man of pure science. . . . the engineer, and the engineer alone, is the great civilizer. The man of science follows in his train."41

The scientists and their supporters quite understandably opposed this view, asserting that they, too, had an important role to play in practical affairs and could claim some credit for the world's progress.42 Some sought to minimize the element of conflict: at Bath Sir William Thomson had told Preece that there ought not to be antagonism but rather "a genuine alliance between mathema-


42 See [Oliver Lodge], "Empiricism vs. Science," *Nature*, 1888, 38:609; for evidence that this was written by Lodge, see, e.g., P. G. Tait to Lodge, 6 Nov. 1888, Lodge Collection, UCL MS Add. 89/103.
ticians and engineers.’’ The mathematicians, however, saw their own role as one of leading and directing the work of the engineers when it touched on theoretical matters. Heaviside made this clear shortly after the Bath meeting in a letter to The Electrician titled ‘‘Practice vs. Theory—Electro-Magnetic Waves.’’ There he declared it ‘‘the duty of the theorist to try to keep the engineer . . . straight, if the engineer should plainly show that he is behind the age, and has got shunted onto a siding. The engineer should be amenable to criticism.’’43 This issue of who in fact had the right and duty to tell whom when they were wrong lay at the core of the practice versus theory debate.

The importance of this issue appears from several events connected with the debate. Electrical engineers generally recognized the authority of Sir William Thomson in scientific matters, accepting, for example, his endorsement of the skin effect, as noted above. But less eminent physicists were likely to be rebuffed in their efforts to ‘‘keep the engineer straight,’’ as Heaviside had been in 1887. Preece’s position had certainly helped in that dispute: he was a high government official and prominent in scientific and engineering societies. Heaviside, by contrast, was a nobody: an unemployed ex-telegrapher living with his parents in a seedy part of north London. As Charles Süsskind has observed, it is perhaps understandable that in a quarrel between these two, ‘‘it was not immediately obvious to all concerned that the latter was right.’’44

Eventually, of course, authority in electrical matters fell to the theorists. Hertz’s experiments played an important part in this outcome: they raised Maxwell’s theory from a paper theory to something solid, that even ‘‘practical men’’ had to admit and take notice of. It suddenly became the center of attention, particularly the sort of mathematical studies Heaviside had been doing. ‘‘The interest excited has been immense,’’ Heaviside wrote a few years later, ‘‘and the theorist can now write about electromagnetic waves without incurring the reproach that he is working out a mere paper theory.’’ Those like Varley and Sprague who dismissed Hertz’s experiments as irrelevant could be charged simply with ignoring the evidence; The Electrician declared that ‘‘even the practical electrical engineer cannot safely neglect a new departure so fundamental and wide-reaching in extent.’’45

The discovery of electromagnetic waves by Hertz and Lodge also marked the turning point for Heaviside himself, bringing him a sort of instant respectability. Sir William Thomson dragged him into the limelight in his January 1889 presidential address to the Institution of Electrical Engineers, praising his theory of cable loading, and in 1891 Heaviside was made a fellow of the Royal Society.46 His theoretical work on waves in space and along wires began, at last, to attract some attention; there were calls for reprints and for new work. The Electrician began to publish his papers again, beginning in 1891 the series entitled ‘‘Electromagnetic Theory,’’ which ran until 1902 and was republished in three volumes. (Snell had fallen ill in mid-1889 and died 5 March 1890; his replacement

44 Süsskind, ‘‘Heaviside,’’ DSB.
as editor, A. P. Trotter, was more receptive to Heaviside’s work.) Heaviside’s earlier efforts were collected in Electrical Papers, published in two volumes in 1892. It was a far cry from 1887, when he had been prevented from publishing by Preece and the Post Office censors. By 1894, Heaviside could tell FitzGerald: “I do not mind the eminent [i.e., Preece] a bit, now that he is no longer able to sit upon me and suppress me as he used to.”

Hertz’s discovery, along with a group of related developments, also helped give self-induction a new respectability. FitzGerald’s glorification of Hertz, Lodge’s experiments on lightning conductors, and Thomson’s studies of alternating currents all came together at Bath to focus attention on self-induction and Maxwell’s theory of the field. As Thomson put it, with what Heaviside called “a happy union of epigrammatic force and scientific precision,” self-induction was “in the air.” Hertz’s electromagnetic waves, Heaviside went on, were now “also in the air,” and Preece’s protestations notwithstanding, “it is the ‘great bug’ self-induction that keeps them going.” Heaviside was moved to burst into verse:

Self-induction’s “in the air,”
Everywhere, everywhere;
Waves are running to and fro,
Here they are, there they go.
Try to stop ’em if you can
You British Engineering man!

Shortly after Bath, Heaviside was able to declare that “self-induction, despite strenuous efforts to stop it, goes on moving; nay, more, it is accumulating momentum rapidly, and will, I imagine, never be stopped again.”

The same could be said for Maxwell’s theory as a whole. Hertz had given it an impetus that made it the most wide-reaching physical theory by the end of the century, and also carried it deep into the practical world of electrical engineering. “Despite strenuous efforts to stop it,” the theory found its way into practice, transforming the electrical industry into the prime example of a science-based, indeed, a theory-based industry.

CONCLUSION

Hertz’s discovery of electromagnetic waves marked the triumph of Maxwell’s field theory, and gave it both the opportunity and the means to expand. It had become “an imperial science,” according to Lodge, annexing optics and later a string of other branches of physics. The Maxwellians soon annexed electrical engineering as well, extending their theory into the practical world. The practice

47 Heaviside to FitzGerald, 30 May 1894, FitzGerald Collection. “The eminent scienticulist,” often shortened to “the eminent” or “em. sci.,” was one of Heaviside’s many nicknames for Preece, which included “the man of brass,” “the bouncer,” and “Mr. Prigs.” On Snell see Electrician, 1890, 24:437, 473.
versus theory debate of 1888–1891 marked the attempt of the practical men to beat back this invasion. But Maxwell’s theory had been so strengthened by the experimental evidence of Hertz and others, and the ground of practical electricity so well prepared by technological progress, that electrical engineering fell quite quickly to the Maxwellians.

The old-style “half-educated electricians” were not, for the most part, converted to Maxwellianism, but were simply left behind. As a letter to The Electrician during the debate put it, “usually the only cure for the ‘practical’ generation is to die off, and for a new generation to arise.”50 That new generation—Britain’s first generation of academically trained electrical engineers, appearing at the end of the 1880s—was raised from the first on Maxwellian principles. Such Maxwellians as FitzGerald, Lodge, and Silvanus Thompson played leading roles in promoting electrical engineering education.51 These new engineers, trained in theory and mathematics, were able to handle the new technologies involving alternating currents and field effects far better than were their “practical” predecessors, who were displaced as electricity simply got beyond them. The quintessential “half-educated electrician” Edison, after a long struggle against alternating current, essentially gave up work in power engineering once it was adopted. He retained mathematical physicists to answer questions on electricity. “I’ve come to the conclusion,” he said ruefully in 1892, “that I never did know anything about it.”52 Authority in the electrical world had passed into the hands of the scientists.

50 Letter to Electrician, 1888, 21:749, apparently by Lodge; see Heaviside Notebook 3a, entry 157, Heaviside Papers.
51 FitzGerald launched the electrical engineering program at Dublin; see his letter to H. A. Rowland, 26 October 1882, in Rowland Collection, Johns Hopkins University. Lodge started the Liverpool program; Past Years, p. 149. Thompson was the principal of the Finsbury Technical College in London, an early center of electrical engineering education, and was the author of several widely used textbooks.