4 Fear, Shunning, and Valuelessness: Controversy over the Use of "Cambridge" Mathematics in Late Victorian Electro-Technology

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Cambridge sets the fashion in mathematics. No one but a Cambridge man is supposed to be capable of anything in mathematics. A few Dublin men have had a glimmering of knowledge, but an Oxford graduate is in outer darkness, and an unfortunate who has read by himself and never been to any University at all is doomed to eternal ignorance. In spite of this we hope that someone may write a book on practical mathematics which contains just subjects which are necessary for engineers, and nothing more. We do not mean a book of simplified explanations. They are delusive. . . .
—letter from a Leeds "Electrician" to *Industries*, April 1891[1]

The electrical engineer must not be only an overgrown wireman, a mechanical engineer with a little electrical knowledge, a mathematician, a financier, a lacquered brass and sealing-wax varnish instrument maker, a physicist, or a manager of men. He must be all of these, in different proportions in different men.
—James Swinburne, presidential address to Institution of Electrical Engineers, 1902[2]

Historians now routinely examine technoscientific controversies to map past sociocultural practices that are otherwise difficult to recover. My concern is with controversies that enable us to recover the ways in which *training* was central to articulating a new multifaceted discipline and defining the proper credentials of its practitioners. Across Europe, America, and Japan in the 1880s and the 1890s, physicists, mathematicians, electricians, telegraph engineers, mechanical engineers, and civil engineers all tried their hand in a new field designated as "electrical engineering." Practitioners of each group sought to annex this exciting and potentially lucrative new technological domain—whether in direct current (d.c.) or alternating current (a.c.) forms—as an extension of their own expertise. Yet the project of developing electrical light and power posed enormous theoretical, technical, practical, commercial, and political challenges that no single existing group had comprehensive skills to handle. Most especially there was prolonged disagreement about whose training was best suited for handling the unprecedentedly complex phenomena of a.c. technology—especially in understanding the effects of the electromagnetic parameter of "self-induction."

I focus on a sometimes heated argument about how to theorize the performance of one important but recalcitrant electromagnetic technology: the a.c. generator commonly known as an alternator. John Hopkinson's controversial 1883–84 theory of alternators was articulated using techniques he had learned as a Mathematics undergraduate at Cambridge from 1867 to 1871, and was thus intelligible only to those with a university-level training in mathematics. Nevertheless, engineers trained in the technical college and workshop could at least challenge both Hopkinson's idealistic analogical reliance on the mathematics of purely mechanical systems and the practical inferences he drew from treating self-induction as if it were as unvarying as a flywheel's "moment of inertia." This sort of treatment was second nature to a Cambridge graduate inculcated into seeking neat analytical equations within a mechanistic paradigm, but "practical" engineers more familiar with the operations of a.c. machinery than Hopkinson saw this simplifying assumption as entirely unjustified and liable to result in error. Preferring to use more trustworthy if less concise graphical methods to analyze alternator performance, these self-styled "practical men" often disparaged what they more generally saw as delusive and thus valueless equations produced by Cambridge-trained "mathematicians." As I show below, this divergence of educationally grounded practice could produce polemical results. I discuss how Hopkinson's 1894 lecture to the Institution of Civil Engineers on "the relations of mathematics to engineering" prompted criticism that led Hopkinson to suggest that too many engineers unduly "feared and shunned" the powerful techniques of Cambridge analysis.

The particularity of this dispute to the credentials of *Cambridge* mathematics and especially to training in the new field of electrical engineering is rather significant. After all, few doubted the cogency of Cambridge mathematics training for developing late Victorian theories of electromagnetism and thermodynamics.[3] Then again, Hopkinson's disagreement with the "practical men" is also clearly particular both to his own idiosyncratic mode of deploying Cambridge mathematical techniques beyond the syllabus of the university's Tripos examinations and his early reluctance to heed advice from "practical" engineers. I show that James E. H. Gordon, a contemporary of Hopkinson trained in Cambridge mathematics, was more amenable to artisanal wisdom and was thus able with his engineering workshop assistants to develop *practical* forms of a.c. technology exactly contemporaneously with Hopkinson's disputed theorizing. Examining Gordon's case enables us to see how Cambridge graduates had at least as much to learn about electrical engineering from artisan-engineers as vice versa. Indeed I argue that once constructive dialogue was established in the following decade, a new pedagogy for mathematizing electricity was synthesized from both graphical and Cambridge traditions in the discipline—one product of which featured importantly in Richard Feynman's early career.

Given the broader significance of this story, historians should see the central issue as more than just the credibility of John Hopkinson's idiosyncratic form of Cambridge theorizing. Nor need they acquiesce in contemporary characterization of opposing positions simply in terms of the *training* of individuals involved. Rather we might consider other factors that were at stake: gender (competing masculine identities), socio-economic class (middle-class graduates versus industrial artisans), institutional power and professional credibility. In a sense education was a crucial forum for defining or redefining all these issues in late-nineteenth-century British culture. Since the early 1870s, new forms of primary, secondary, and tertiary education enabled broader competition for diversifying opportunities in an industrial society ever less constrained by traditional boundaries of social class and power.[4] Amongst all this change, one lingering traditional peak of the British educational system was the Cambridge Mathematics Tripos, which had produced many elite scientists, engineers, clergy, politicians and civil servants throughout the nineteenth century.

As Andrew Warwick has explained, the top performer in each year's Mathematics Tripos examination—the Senior Wrangler—was held in especially high public esteem, publicly celebrated in newspapers and accorded intellectual supremacy and respect enjoyed by few other contemporaries. And in this regard we should note that John Hopkinson's prima facie authority in the debate discussed below was rooted in his publicly acknowledged status as Senior Wrangler of 1871.[5] Yet as Hopkinson found to his discomfiture, especially in 1894, his educationally defined high status was questioned by an ever more confident electrical engineering community. This was defining its own new elites and prerogatives to the extent that it could challenge a Cantabrigian authority speaking on matters pertaining to their expertise in a.c. technology. Defining their identity in ways that directly opposed and limited the writ of a Cambridge Senior Wrangler was, I suggest, an effective means for electrical engineers to lessen the social power that Hopkinson and other Senior Wranglers had so freely (and hitherto uncontentiously) drawn from their Cambridge pedigree.

In two senses, then, this chapter is a corollary to Warwick's definitive historical study of the Cambridge Mathematics Tripos, *Masters of Theory: Cambridge and the Rise of Mathematical Physics*. I comment both on the demise of the now long-vanished fetishism of Senior Wrangler authority and on the previously undocumented challenges that Cambridge Wranglers faced when unreflexively deploying their university-bred practices to new contexts of enquiry.[6]

Practice-Laden Mathematics: Cambridge Tripos vs. Engineering Graphics

Historians of electrical technology have noted that two major disputes arose in the late 1880s concerning the significance of self-induction in technological theorizing. Rather than considering broad socio-economic or educational explanations to account for the origin of these conflicts, enquiry has focused on the outcome of these encounters as the application of abstract Maxwellian "Theory" to defeat particular forms of engineering "Practice." Accordingly much attention has been paid to the ways in which a group of "Maxwellians" drew upon Maxwell's *Treatise on Electricity and Magnetism* of 1873 and adapted it to resolve problems in both physics and electrical engineering. Although John Hopkinson latterly drew selectively upon some aspects of Maxwell's work, we should by contrast see his predominant practice as epitomizing pre-Maxwellian Cambridge mathematical physics—not as propounding Maxwellian electromagnetic theory.[7]

Bruce Hunt's account of one such debate from 1888 to 1891 considers how Maxwellian theorists discredited claims by chief Post Office Electrician, William Preece, that a lightning conductor's performance was determined primarily by its electrical resistance. Maxwellians argued instead for the primacy of self-induction in governing how lightning strikes were discharged.[8] Without explicitly using the "practice" vs. "theory" dichotomy, Dominic Jordan explains similarly how, between 1886 and 1889, the same Maxwellians had appealed to the theory of self-induction to explain and resolve distortion problems in telephone lines. Both accounts take as their explanandum the defeat of Preece's view that self-induction was a "choking" effect that ought to be expunged completely from both technologies for effective working.[9] In both cases the engineering problem was solved by, among others, Oliver Heaviside, Oliver Lodge, and Silvanus Thompson using Maxwellian theory to stipulate an *optimal* rather than minimal value for self-induction.

Hunt and Jordan make few direct allusions to educational matters in explaining the anatomy of these debates. This is prima facie reasonable since none of the three key figures mentioned above acquired their Maxwellian approach through formal education: Lodge and Thompson both learned practical physics from the distinctly non-Maxwellian Frederick Guthrie in London, and Heaviside was famously an autodidact. Indeed, as Hunt and Buchwald have shown, early Maxwellian commitments were generally developed outside of formal educational experience.[10] Moreover, in cases where educational traditions did inform electrical engineers' adoption of Maxwellianism, it reflected the relative autonomy of "practical" and "Cambridge" sub-communities discussed above. For example, Ron Kline has shown that working engineers in the United

States developed effective informal "practical" theories for the alternating-current (a.c.) induction motor that diverged considerably from the Maxwellian theoretical canon. This exasperated the Cambridge-trained Serbo-American Michael Pupin who clearly expected deference from less privileged engineers to his particular Maxwellian version of electrical engineering theory.[11] From Kline's study it would seem that the historian cannot identify any single form of pedagogy to account for electrical engineers' development of efficacious theories and practices in early a.c. technologies.

Looking deeper, however, we find that some important educational issues were operative in framing the debate. Ido Yavetz has pointed out that late Victorian electrical engineers' talk about an opposition between "practice" and "theory" should be seen as a *partisan representation* of a complex socio-professional tension—not a literal fact.[12] He notes that it was Preece who rhetorically fashioned the lightning conductor debate using the "practice vs. theory" dichotomy to articulate his opposition as man of "established practice" to the Maxwellians' controversial theory of lightning conductor design.[13] Yavetz pinpoints the context-specificity of this labeling by observing that Preece invoked no such dichotomy in the telephony debate discussed by Jordan.[14] What *was* common to both these cases, however, was the molding of Preece's response by his prior training as a telegraphic electrician. His assumption that self-induction was of negligible importance (or even to be expunged) was borrowed from William Thomson's 1850s canonical modeling of telegraph signal transmission solely in terms of capacitance and resistance. Thus we might reinterpret controversies in which Preece was embroiled as focusing not so much on the virtues of Maxwellian "theory" per se but on the viability of extending the learned telegraphic exemplar to other areas of technological design. And as far as the other side in the controversies on self-induction is concerned, Silvanus Thompson and Oliver Lodge were both institutional professors—at Finsbury Technical College London, and University College Liverpool, respectively—committed to using their training programs to inculcate new approaches to the theoretical analysis of electrical technology. Pedagogy was thus a major consideration in the longer term ramifications of the "Practice vs. Theory" debate to the extent that it was the institutionalized means of extending successful strategies for theorizing machines to future generations of practitioners.

In his studies of nineteenth-century mathematical physics, Andrew Warwick has persuasively shown that such processes of theorizing were "practice-laden" in character and specific to localized regimes of training. In other words he subverts the dichotomy of "theory vs. practice" by looking in a Wittgensteinian vein at the practice *of* theory.[15] According to Warwick's education-centered account, practitioners' theorizing activities are shaped by a set of well-rehearsed problem-solving *strategies* and specific techniques learned as students to bring a disciplined and conventionalized mathematical order to bear on material situations. To that extent Warwick sympathizes with the well-known Kuhnian view that the formation of disciplinary identity hinges rather crucially on learning from common exemplars in a discipline's educational canon. Yet whereas Kuhn's account focuses on globally current paradigms, Warwick emphasizes that the characteristic strategies and techniques of theoretical practice are learned in ways *localized* to a particular educational institution, and indeed to a particular curricular scheme within that institution. He shows compellingly how difficult it was for those at other locations (German universities and British schools) who had not taken the Mathematics Tripos at the University of Cambridge to solve the characteristic problems set by its examiners—even when presented with complete specimen solutions to such problems.[16]

In further contrast to Kuhn's early account in *The Structure of Scientific Revolutions,* Warwick does not describe the long-term significance of education as the deep and deterministic inculcation of physical ontologies. After all, Kuhn's own work showed that in later stages of their careers scientists could and often did completely reject metaphysical commitments acquired in their early training.[17] By contrast, practitioners were much less inclined to abandon the higher-order strategies for problem solving they had learned in undergraduate or graduate study. Indeed Kuhn's more considered view of education emphasized its role as familiarizing students with puzzle-solving exemplars that they could use as heuristics to deal with unfamiliar problem situations. In this vein, Warwick argues that intensive mathematical training by specialist coaches for the Cambridge Mathematics Tripos equipped them with powerful techniques—algebra, trigonometry, calculus and differential equations—for extending exemplary problem-solutions to new situations.[18] He establishes this important point by showing how the later-nineteenth-century Mathematics Tripos nurtured a very characteristic kind of research publication among those of its graduates that went on to academic careers—especially those coached by Edward Routh. For example, Warwick compares the Tripos papers of John Henry Poynting and his first paper, and identifies a standard strategy to both—one that was uniquely characteristic of a Cambridge graduate.[19]

A particular feature of Cambridge-style analysis noted by Warwick was the very restricted form for what could count as a solution to a Tripos problem. A crucial desideratum for such Cambridge examinees was to achieve answers that were not only elegant and concise, but above all *analytic*, that is, closed-form algebraic expressions. Following the pattern of exemplar solutions in classical mechanics, neither an infinite series nor an overtly approximate solution would do. Tripos examiners were thus expected to set Tripos problems that had neat—if unworldly—solutions, avoiding topics that did not

lend themselves to such analytic answers. As Warwick indicates, candidates were expected to make appropriate approximations so that a problem could be made to fit the pattern of learned exemplars and then solved to yield the required form of answer. Such a use of approximations to accomplish standard practice was obviously *not* dictated by the physics of the problem situations, but rather the conventions of examination setting local to the University of Cambridge.[20]

Indeed, to understand the Mathematics Tripos we should consider it as a conventionalized expression of the university's pedagogical goals to instill in its students the intellectually rigorous discipline of arriving at elegant concise solutions to abstract problems. Given the strongly self-referential nature of the Cambridge Mathematics Tripos, we cannot infer that the Tripos examiners intended or expected problem-solving strategies nurtured by it to be extended directly to extrinsic practical contexts. As we shall see, however, this was precisely what John Hopkinson did so controversially on several occasions in the 1880s and the 1890s, even going so far as to solve problems in published papers with the characteristic Tripos "rider"—specialized solutions to variants on the original problem. As will be explored further below, Hopkinson using Tripos problem-solving methods, produced equations for the performance of electromagnetic machinery precisely of the form required by Tripos examiners—over a decade after he had ceased to be a Cambridge undergraduate.

At a few points in his presentation of his Wranglerish alternator theory to fellow engineers, Hopkinson did try to adopt the techniques of graphical analysis with which the vast majority would have been more familiar. This was most obvious on the very first airing of his theory at the Institution of Civil Engineers in April 1883. Yet on that occasion his quick reversion to Wrangler tricks incomprehensible to most fellow engineers, and his condescension to them in public debates, certainly did not win their deference or enthusiasm. Few ordinary electrical engineers were likely ever to have the time or opportunity to master the simultaneous differential equations used by Hopkinson and his Tripos ilk from 1884 onwards in articulating a.c. theory.

To bridge the obvious gap between the Cambridge and graphical traditions of electrical engineering, a handful of a.c. specialists set out soon afterwards to translate Hopkinson's a.c. theory into the more familiar Euclidian idiom of graphical analysis. One was fellow Cambridge graduate Thomas Blakesley (34th Wrangler, 1869) who, after a civil engineering apprenticeship, became a freelance a.c. consultant and then Lecturer in Mathematics and Physics at the Royal Naval College in 1885. Blakesley's popular handbook *Papers on Alternating Currents of Electricity for Students and Engineers* (1889) grew out of articles written for *The Electrician,* the Physical Society of London, and the *Philosophical Magazine* since 1885. Exemplifying the use of the "geometrical method" to treat a.c. problems Blakesley devoted his opening chapter to the vexing topic of self-induction. This chapter set out to augment the elementary reader's presumed knowledge of how electromotive force was governed by resistance with an analysis of how self-induction was equally important in determining electromotive forces in alternating current circuits. For a sinusoidal form of a.c oscillation, these effects could be represented as the rotation of a straight line representing electromotive force around a point (only later referred to as a vector), the length and direction of that line being determined by the effects of self-induction and resistance. Using this geometrical technique, Blakesley painstakingly took his reader through the notion that self-induction in an a.c. circuit produces a time-lag between cyclical changes in current and potential difference. Blakesley next instructed his readers how to calculate the phase angle (as it was later known) of this retardation.[21]

The next major author seeking to bring transparency and accessibility of graphical methods to a.c. engineering was Gisbert Kapp. A mechanical engineering graduate of the Zurich polytechnic, Kapp took up British citizenship in 1875, becoming a consultant engineer and editor of *Industries* and the *Elektrotechnische Zeitschrift*. From 1887 he began to publish a series of articles for *The Electrician* titled "Induction coils graphically treated." Kapp's use of a clock diagram (see figure 4.1) drew on Blakesley's practice of using rotating vectors to represent the cyclical variation of, and inductive lag between, electro-motive force (e.m.f.) and current in an a.c. circuit.

As Ron Kline notes, even when such geometrical methods were extended to Kapp's more sophisticated clock-face diagrams, they could not give the generality of analytical techniques as adopted, for example, by John Hopkinson. Even so engineers could use trigonometric tables or a ruler and compass on a carefully constructed clock diagram to establish what they needed to know for practical work: the basic phase and magnitude parameters of a steady-state electrical circuit.[22] If engineers could get by with Blakesley's and Kapp's practical shortcuts there was no obvious benefit for busy engineers to use more technical and time-consuming mathematics just to achieve a higher level of rigor or abstraction.

As we shall see, the longer-term successful take-up of the clock-diagram technique in modeling electromagnetic machines was no mere representational cul-de-sac nor second-rate solution. Hopkinson's dogmatic insistence on the use of Wranglerish jargon, opaque derivations, and mystifying equations only reinforced the impression of ordinary engineers that Cambridge mathematics had little to offer them in advancing their understanding of a.c. technology. Overall, then, my story challenges prevailing historiographical assumptions that those educated in "advanced" mathematical techniques necessarily determine the development of techno-scientific disciplines.

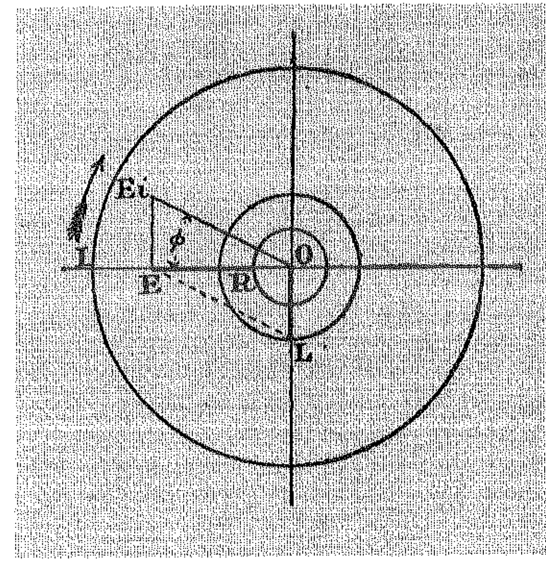


Figure 4.1
An early example of a (pre-vector) "clockface" diagram for representing the cyclical variation of—and phase displacements between—the parameters of an alternate current circuit. The maximum current is represented by the line *OI*, rotating anticlockwise so that the projection of *OI* on the vertical axis gives the actual value of current at a given moment. Similarly the line *OE* represents imposed potential difference (with similar projection onto horizontal axis), *OR* the potential difference lost through resistance, and *OL* the self-inductive element, always perpendicular to current. The parallelogram defined by the lines *OE* and *OL* gives the resultant magnitude of the induced electromotive force *OEi* and the phase angle ϕ between potential difference and current in the circuit. Source: G. Kapp, "Alternate current machinery," *Minutes of Proceedings of the Institution of Civil Engineers* 97 (1888–89): 1–79, figure 7 (on 10).

Paralleling Alternators and Hopkinson's Unconvincing "Language of Equations"

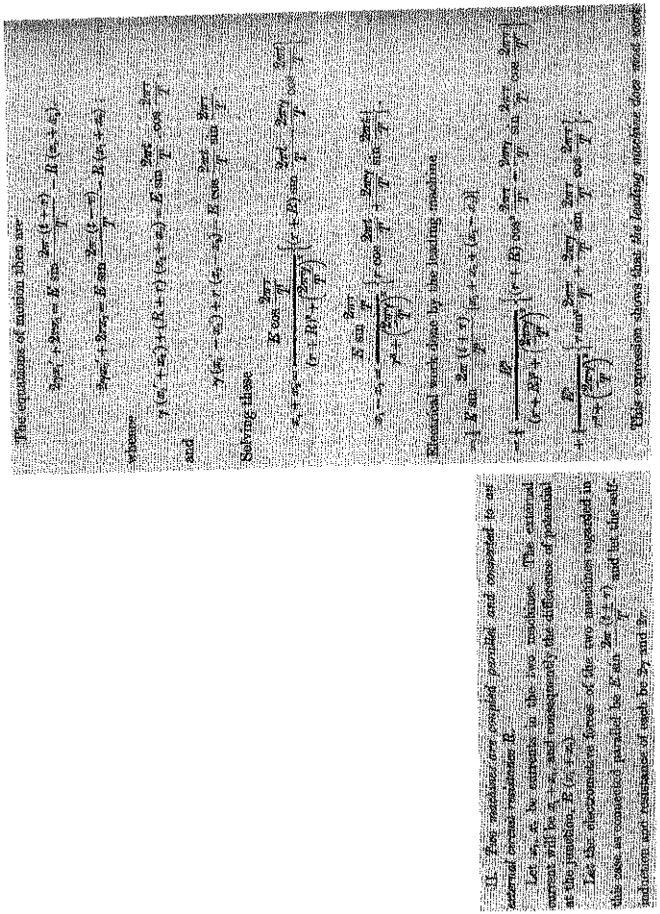
From the advent of electrical lighting in the early 1880s, many problems faced those who sought to harness the behavior of electrical machinery to both financial gain and personal or communal prestige. One central dispute was whether the future lay in d.c. or a.c. technology, especially as the latter offered better business returns for larger urban scales and long-distance supply systems. Yet while d.c. technology proved relatively tractable to mechanical engineers expert in handling steam engines, and to telegraphists (such as Edison) familiar with simple electrical circuits, generating alternating current posed challenges beyond the training and experience of all. In particular while d.c. dynamos could fairly readily be connected in parallel to meet increasing consumer demand on cold winter evenings, this was perplexingly difficult to accomplish for a.c. power—at least not without huge current surges that disastrously burned out the lamps to which they were connected. Although his magisterial survey covers almost every other aspect of the "Battle of the Systems" between a.c. and d.c. from ca. 1885 to 1893, Thomas Hughes does not discuss the critical problem of paralleling alternators.[23]

John Hopkinson's 1883–84 theory of alternator operation has been hailed both by historians and by his son Bertram as definitively showing that it was possible to connect alternators in parallel and derive power from the combination, thereby (allegedly) establishing the practical and commercial viability of a.c. power.[24] Yet few contemporaries readily accepted Hopkinson's theory as proving anything at all about practical engineering; on the contrary, the seemingly false conclusion of his account gave many strong grounds for questioning the very relevance of Cambridge-style theorizing to understanding the effective operation of a.c. technology. At the time it appeared, Hopkinson's theory could not, after all, offer any explanation of one crucial and highly inconvenient empirical fact: no commercially produced alternators could actually be made to run in parallel.[25] This is especially surprising because Hopkinson, as a freelance consultant engineer in 1879, had been among the very first to promote the *empirical* study of Edison's electrical lighting and generating technology at the Institute of Mechanical Engineers.[26] Specifically, in that year Hopkinson developed what was soon known as the "characteristic curve" of a dynamo—a graph plotting the non-linear relationship between current generated and the potential difference between the terminals. This became an invaluable graphical technique in comparing the performance of dynamos for which Hopkinson was thereafter renowned—even when the rest of his theoretical work was under attack for its characteristically Cantabrigian equation-centered approach.

In "Some points in electrical lighting," a lecture given to the Institution of Civil Engineers (ICE) in April 1883, Hopkinson presented a highly a priori model of parallel alternator operation.[27] As a consultant to the English Edison Company, Hopkinson was a d.c. specialist with little experience of a.c. technologies. Without any reference to practical examples of alternator operations, Hopkinson turned immediately to the case of two linked alternators. Taking the conventional view that self-induction was the most important parameter, and drawing on his Wrangler practice of using mechanical analogies to map unfamiliar situations, he likened an alternator's self-induction to the moment of inertia of a rotating physical body. The analogy was viable to the extent that changing the speed of rotation of a spinning body was comparably difficult to changing the current through a metal conductor—in both cases an inertial "reaction" appeared to oppose any imposed change. Hopkinson used this Cambridge analogy to claim that the mathematics was of the same form for mechanical and electrical interpretations—notably that self-induction was as constant a parameter as the moment of inertia. He moved quickly, with typical Wrangler facility, to five characteristic equations, declining to "trouble" his audience with the "simple" derivations involved. He declared that these showed self-induction to be as important as resistance in determining the maximum current in an a.c. circuit and thus the work that the alternators could do—central conclusions that his audience could only accept on his Wrangler authority. Importantly, though, Hopkinson did not use such algebraic legerdemain to resolve the question of whether paralleling was possible. He instead presented phase diagrams of two alternators with voltage varying sinusoidally and examined the outcomes with staged verbal narrative. These showed that whereas a series connection would tend to produce a motor-generator pair doing no net work, a parallel-synchronized combination could in fact do useful work together.[28] Thus, he implied, it was possible for alternators to be coupled usefully together.

In November of the following year, Hopkinson presented a more developed "theory of alternating currents, particularly in reference to two alternate-current machines connected to the same circuit" to the Society of Telegraph Engineers and Electricians (STEE, which also met in the ICE building).[29] Here we see the force of Warwick's point about the pedagogical influence of Cambridge training on Wrangler research presentations (as instanced in the case of Poynting discussed above). For this highly formalized paper, Hopkinson laid out his arguments somewhat in the format of a Mathematics Tripos examination paper, "illustrating" the subject as a series of seven discrete problems. He started with the simplest cases of series and parallel connection in which the self-induction was conveniently assumed to be a constant and mutual induction effects taken to be zero. (See figure 4.2.) He then progressed to trickier variants—like the "riders" in a Tripos problem—of interesting and analytically soluble problems, albeit not closely connected to the main subject of the paper, such as the behavior of an arc lamp connected to an alternator.

Most significantly, what was common to his solutions of all seven problems was the characteristically Cantabrigian representation of the algebraic formalism as concerning "equations of motion"—as if analyzing coupled alternators with self-induction and resistance were self-evidently analogous to coupled frictional oscillators in dynamics.



II. *Two machines are coupled parallel and connected to an external circuit resistance R.*

Let x_1, x_2 be currents in the two machines. The external current will be x_1+x_2, and consequently the difference of potential at the junction, $R(x_1+x_2)$.

Let the electromotive forces of the two machines regarded in this case as connected parallel be $E\sin\frac{2\pi(t\pm\tau)}{T}$ and let the self-induction and resistance of each be 2γ and $2r$.

The equations of motion then are

$$2\gamma x_1' + 2r x_1 = E\sin\frac{2\pi(t+\tau)}{T} - R(x_1+x_2),$$

$$2\gamma x_2' + 2r x_2 = E\sin\frac{2\pi(t-\tau)}{T} - R(x_1+x_2);$$

whence

$$\gamma(x_1'+x_2') + (R+r)(x_1+x_2) = E\sin\frac{2\pi t}{T}\cos\frac{2\pi\tau}{T},$$

and

$$\gamma(x_1'-x_2') + r(x_1-x_2) = E\cos\frac{2\pi t}{T}\sin\frac{2\pi\tau}{T}.$$

Solving these

$$x_1+x_2 = \frac{E\cos\frac{2\pi\tau}{T}}{(r+R)^2+\left(\frac{2\pi\gamma}{T}\right)^2}\left\{(r+R)\sin\frac{2\pi t}{T} - \frac{2\pi\gamma}{T}\cos\frac{2\pi t}{T}\right\},$$

$$x_1-x_2 = \frac{E\sin\frac{2\pi\tau}{T}}{r^2+\left(\frac{2\pi\gamma}{T}\right)^2}\left\{r\cos\frac{2\pi t}{T} + \frac{2\pi\gamma}{T}\sin\frac{2\pi t}{T}\right\}.$$

Electrical work done by the leading machine

$$= \frac{1}{2}E\sin\frac{2\pi(t+\tau)}{T}\{x_1+x_2+(x_1-x_2)\}$$

$$= \frac{1}{4}\frac{E^2}{(r+R)^2+\left(\frac{2\pi\gamma}{T}\right)^2}\left\{(r+R)\cos^2\frac{2\pi\tau}{T} - \frac{2\pi\gamma}{T}\sin\frac{2\pi\tau}{T}\cos\frac{2\pi\tau}{T}\right\}$$

$$+ \frac{1}{4}\frac{E^2}{r^2+\left(\frac{2\pi\gamma}{T}\right)^2}\left\{r\sin^2\frac{2\pi\tau}{T} + \frac{2\pi\gamma}{T}\sin\frac{2\pi\tau}{T}\cos\frac{2\pi\tau}{T}\right\}.$$

This expression shows that *the leading machine does most work*

Figure 4.2
In characteristic Cambridge Mathematics Tripos fashion, John Hopkinson treated the parallel coupling of alternators as a problem in dynamics governed by "equations of motion." From this he concluded that paralleling alternators should be possible. Source: John Hopkinson, "The theory of alternating currents, particularly in reference to two alternate current machines connected to the same circuit," *Journal of the Society of Telegraph Engineers and Electricians* 13 (1884): 496–515, on 503–504.

As was typical of Wrangler practice, this strategy allowed Hopkinson at least to formulate the algebraic equations with facile dexterity. For alternators in series he equated the sum of the inductive and resistive components of circuit electro-motive force to the sum of the impressed e.m.f.; for the parallel case he argued similarly with regard to each branch of the circuit. From the latter Hopkinson showed that one alternator will always do more work than the other (in contrast to the series case), leaving readers to infer from this that it should be possible to connect alternators in parallel. This was very surprising news for the electrical engineers who, since his ICE lecture the previous year, had persistently found it impossible to do so in practice.[30]

In making these extraordinary claims, Hopkinson proposed two simplifying assumptions akin to those he had made in his previous year's presentation to the ICE. These were that the alternator's coefficient of self-induction was a constant (which he admitted to be "not exactly" the case) and that there was negligible mutual induction in the circuit (an equally convenient approximation).[31] Hopkinson acknowledged that these were questionable assumptions and that they would have been difficult to defend on the basis of Maxwellian electromagnetic theory; but—as he said some years later—without these simplifying approximations his equations would have been "unmanageable."[32] The manageability of the equations was clearly a central issue for Hopkinson to establish neat analytic solutions as embedded within the conventions of Wrangler protocol. Moreover, to acknowledge the variability of self-induction and of mutual induction as variable would have required Hopkinson to include machine-dependent expressions for these in his equations—making his solutions uninterestingly contingent on the particular construction of a particular alternator. Thus, to accomplish "universal" solutions of a generalized analytic form, Hopkinson followed a Wrangler's *theoretical practice* and not rigorous Maxwellian theory.[33]

Several expert members of his audience not trained in Cambridge mathematics—notably William Ayrton and Silvanus Thompson—found much to challenge in Hopkinson's Wranglerish simplifications concerning self-induction and mutual induction.[34] More importantly for present purposes, at least one young engineer in the audience, Llewellyn Atkinson, was skeptical about the utility of Hopkinson's "language" of equations. Atkinson later recalled hearing Hopkinson's paper as a 19-year-old trainee while studying in Ayrton's Finsbury Technical College laboratory:

> He treated [the problem] simply as the equation of motion of two moving bodies with certain forces between them—it might almost as well have been the sun and the moon. Nobody believed him, because the only alternators at that time that had any practical vogue were the Siemens alternators, and everyone knew it was practically impossible to run them in parallel. Until it had been proved in practice, this language of equations convinced nobody.[35]

Thus, it was daily familiarity with the complexity of alternators—rather than "ignorance" of theory—that made it easy for Atkinson to doubt the ability of highly self-referential Wrangler analysis to capture anything useful about the world of electromagnetic machinery. Indeed, Cambridge-trained mathematicians had much to learn from such practical men to ensure that their mathematical virtuosity engaged the material world of electrical engineering.

Later sections of this essay will cover the ways in which the opacity and disputed efficacy of Cambridge mathematical techniques became the focal point of heated discussion in the engineering community—not least in Hopkinson's own defense of Cambridge mathematical techniques as uniquely cogent in engineering work. To put that episode into perspective, however, we should not see any inevitability in the conflict between Cambridge learning and other sorts of practical engineering wisdom. In the next section I survey the work of Hopkinson's Cambridge-trained contemporary, James Gordon, who showed in 1884–1886 how university learning could be—and indeed had to be—harnessed to artisanal expertise for success in practical a.c. engineering.

J. E. H. Gordon and the Limits of Cambridge Training in a.c. Engineering

> The public who through long winter evenings, and longer London fogs, sit reading by the cool and steady light of their electric lamps, but who are most indignant if by any chance it flickers or fails them, do not realize how intense the struggle has been for those pioneers of electric lighting who have toiled so hard and incessantly to surprise yet one more of Nature's secrets. . . . Many an engineer's wife knows how common it was four or five years ago for their husbands, who had come back late at night worn out and exhausted, to be fetched again by the message that there was "something wrong at the works."
>
> —Mrs. J. E. H. Gordon, *Decorative Electricity,* 1891[36]

Studying James Gordon's work enables us to rebut any fallacious deterministic inference that a Cambridge training in mathematics necessarily led an electrical engineer into confrontation with practical men. The importance of Gordon's case is not just that he was able to abandon the idealizing practices inculcated by the Cambridge Mathematics Tripos and learn to adapt to the recalcitrant conditions of practical engineering in ways not recognized in the formulation of Cambridge Tripos examination questions. More significantly, the contemporary engineering community also recognized, in implicit contrast to Hopkinson, that it was possible for a Cambridge-trained mathematician to make a systematically positive contribution to electrical engineering—despite (rather than because of) their high level of educational attainment. Most tellingly of all, the theories of Hopkinson were of no value to his fellow Cambridge graduate in his practi-

James Edward Henry Gordon was educated in the Cambridge Mathematics Tripos tradition, graduating with rather less distinction than Hopkinson as Junior Optime (3rd class) in 1875 before researching and publishing on electricity and magnetism for three years under James Clerk Maxwell in the Cavendish Laboratory.[37] After some years spent as a textbook writer and an independent researcher, with his wife Alice as chief collaborator, he became the Manager of the Electric Light Department of the Telegraph Construction and Maintenance Company (Telcon) in late 1882. Gordon's work for this company in managing the installation of the first British public a.c. supply for lighting at Paddington railway station (West London) between 1884 and 1886 brought him a minor place in the professional folklore of electrical engineers.[38] In this unprecedentedly grand a.c. project, Gordon found no help in Hopkinson's writing, hardly attempting to run alternators in parallel as the latter claimed ought to be possible. Moreover, Gordon found himself heavily reliant on the practical skills of mechanical engineers to supplement what he had learned in the tutorial supervisions and Cavendish Laboratory exercises of 1870s Cambridge.[39]

On June 22, 1883, Gordon read a paper to the Applied Chemistry and Physics section of the Society of Arts on his plans for a large Telcon lighting station. His main concern was with the huge practical problems to be overcome in providing customers with lighting that was "perfectly steady, perfectly safe and reliable." This was rather harder to accomplish in the contingent chaos of the world outside than in the conveniently controlled small-scale conditions of the scholar's laboratory. Tellingly, he made no appeal to the artificiality of Tripos mathematics to simplify the problem by idealizing what ought to happen if only the British public were truly deferential to his academic expertise:

> You must regard the engines, dynamos, mains etc. all as one complete system, and you must remember that if anything goes wrong, the public will not make excuses for the lamps like we may make in the laboratory. They will not say that it was not the dynamo's fault, but [rather that] it was the engine's fault or the boiler's fault. All that the public would be concerned with was that on this or that occasion the light went out.

Gordon acknowledged that perfecting such light required the coordination of a wide range of skills that were "not often united in the same person." He candidly admitted of his own status as a Cambridge-trained scholar that an "electrician" (meaning an all-round expert in electrical matters) was "very often not a first class engineer." Only by combining the requisite expertise from a team of assistants could the heterogeneous operations of an a.c. system be melded together and anything like perfection achieved in electro-technology.[40] Gordon related with striking candor his painful discovery of the insufficiency of his own skills and the necessity of supportive labor to "perfect" the a.c. technology of lighting. Over the previous three years he had tried, with the help of friends, to build his first industrial-size alternator, but encountered innumerable problems of a constructional nature. His first high-speed model was abandoned when its uncontrollable vibrations produced a "deafening roar in the dynamo room." After working well for a few hours, his second high-speed dynamo shuddered and eventually "flew to pieces with a loud explosion." Sympathetic and wealthy friends donated thousands of pounds for a third project to produce an unprecedentedly large but safely low-speed alternator. The Chief Engineer of Telcon secured for Gordon the crucial assistance of the company's mechanical engineers. After months of labor during 1882, Gordon's team completed the machine only to find that, despite a robust input of steam power, it refused to move. Only after several more days and nights of virtually constant toiling and tinkering by Telcon engineers would it produce any current at all.

With his team of Telcon assistants, Gordon conducted his first major trial with the new alternator at 9 o'clock one Monday evening in 1883 at Telcon's factory in Greenwich. After the 1,200 lights produced a "blaze of daylight" without a hitch, Gordon was appointed the Manager of Telcon's electrical light department. Gordon nevertheless remained explicitly aware of his dependence in mechanical matters on the skills of expert technicians—especially his chief assistant from 1883 to 1893, Frank Bailey. Originally trained in mechanical engineering, Bailey was employed in mineworks and locomotive construction in Northern England before picking up a knowledge of electricity in evening classes at the City of London College ca. 1882. Bailey's engineering skills in mechanical construction and human management were crucial to the successful execution of Gordon's grandiose installation plans. By the late 1880s, Bailey had acquired a reputation for overcoming "most of the difficulties inseparable from electrical lighting at that time."[41]

When site testing began at Paddington on the three alternators to be used (not in parallel) to power the station, many socio-technical problems arose. These challenged Gordon's ingenuity in ways that went far beyond any difficulty with apparatus that he might have encountered as a researcher in the Cavendish Laboratory. Frank Bailey's later reminiscences, and those of his own chief assistant, A. H. Walton, reveal that Gordon's monstrous devices failed to behave in situ as well as they had in Telcon's laboratories. Overheated armature cores had to be dismantled and redesigned with improved cooling facilities. Equally uncongenial was the irrepressible tendency of shunting locomotives to spray boiling water onto the gutta percha insulation of electrical power cables. One case of insulation breakdown was so serious that the Great West Railway's Board of Directors thought Gordon's plan was essentially unworkable; only by "very hard" pleading on Gordon's behalf did Bailey manage to avert abandonment of the whole scheme.[42] Needless to say, Bailey's case did not appeal to the authority of the 1871 Senior

Wrangler, John Hopkinson, that such a scheme *ought* to work in principle. Similarly, when local residents threatened legal action against the noise of the Paddington Station generators in November 1885, it was the practical skill of Bailey's team in soundproofing that prevented the shutdown of the new a.c. power system.[43]

After the installation began full-scale operations in April 1886, *The Electrician* lavished praise on Gordon as a university-trained scholar. Unusually he had not merely been an inventive "genius," but had also learned to be an effective manager of practical a.c. projects:

> Mr. Gordon must be congratulated upon the success attained. First known to the world as an earnest student and a successful writer, he has now shown the world that he can carry out his ideas into practice.[44]

Yet this leading electrical trade journal did not attribute Gordon's success solely to his Cambridge pedigree: Gordon had been "ably assisted" by Bailey in accomplishing success at Paddington. While the significance and nature of this assistance would probably have been obvious to most readers of *The Electrician*, the journal tactfully passed over the details. Only 35 years later, when Bailey was himself a senior figure in the field, did he gain the opportunity to offer publicly his account of the Paddington scheme's practical success. Bailey contended that it was only by hard continuous labor that he and his team had been able to convert the threatened failure into a practical success. While he paid tribute to Gordon for his "remarkable mathematical abilities, his inventive genius, and the charm of his personality," Bailey diplomatically passed no comment on Gordon's mastery of practical engineering.[45] Also contributing to retrospective discussion at the half-century celebrations of the Institution of Electrical Engineers (IEE), Bailey's assistant A. H. Walton addressed the subject of "Mr. J. E. H. Gordon's system of electric supply." Attaching no direct significance to Gordon's educational pedigree in accounting for success, Walton focused on the many "anxious times" that arose even after Paddington was operational in April 1886, recalling that his team seldom worked fewer than 12–15 hours a day.[46] Although at the time largely unreported by a sympathetic electrical press, these difficulties were manifestly obvious to Alice Gordon, as seen in the epigraph above. In the last chapter of her 1891 book *Decorative Electricity*, she observed how often her husband was called back late at night to advise on problems at Paddington. Yet, at the same time, "Mrs. J. E. H. Gordon" subtly ensured that her readers knew how much *she* had contributed to the Paddington scheme by acting as loyal assistant to her spouse—all without the university education that her husband had enjoyed.[47] Thus we see the perspectival nature of judgments of the Telcon success at Paddington, various participants claiming, with widely differing educational credentials, that their contribution was a necessary (if tacitly insufficient) component of the project.

In none of the diverse accounts of how Paddington was established as Britain's first public a.c. system in 1886 was any mention made of John Hopkinson's mathematical alternator theory, developed two years earlier. Gordon had initially tried the paralleling of alternators, but then abandoned it to operate each machine on a separate circuit (the conventional practice). In a discussion paper presented to the STEE in 1888, Gordon related his attempts to connect alternators in parallel:

> Many of us have tried . . . but they do not work together till they have run for three or four minutes; they will in that time jump, and that jumping will take months of life out of 40,000 lamps. That alone is a rather serious difficulty in coupling machines together, and I think we may take it in practice—I am not speaking about the laboratory or experiments—we do not couple machines.[48]

Even with his training in both Cambridge Tripos mathematics and Maxwellian laboratory physics, and with the constant assistance of the best mechanical engineers available and the unfailing support of Alice, Gordon and his team still could not satisfactorily instantiate Hopkinson's tendentious theoretical conclusions.[49] Indeed, so dissatisfied was Gordon with a.c. technology that in the late 1880s he experimented (as a consultant engineer) with the rather more tractable machinery of *direct current* supply—then seen by a significant lobby (including Edison) as a strong contender for winning the "battle of the systems." Importantly, though, it was not only Gordon who was puzzled by Hopkinson's Wranglerish claims in 1889 to have solved the practical problems of parallel a.c. operation. In the following section, I explain how Hopkinson's claims once again came to the fore of discussion, framed within a typically unflinching mode of Senior Wrangler condescension to fellow electrical engineers.

Parallel Explanations: Unpacking the "Hidden Mathematical Truths" of Coupled Alternators

> The practical dynamo-builder does not care two straws whether the mystery of parallel running of alternators has or has not been packed up in somebody's mathematical equations to be extricated therefrom subsequently as from a conjuror's inexhaustible bag[;] nor does he derive much guidance from the differences of experts, some of whom consider the secret depends upon abundance of self-induction in the armature, and some of whom think it does not. The dynamo designer . . . "wants to know" exactly what are the conditions to be complied with in order to construct alternators which shall successfully perform this act.
>
> —editorial note, "Alternate current working," *Electrician* 24 (1889): 325

During the three-year period of Gordon's highly visible Paddington adventure, Hopkinson remained virtually silent on the problems of paralleling alternators, working instead on applying magnetic circuit theory to enhancing the efficiency of the d.c. dynamo for the English Edison company.[50] As Jordan has shown, Hopkinson's work on d.c. systems was much less controversial, and contemporaries subsequently awarded him joint credit with Gisbert Kapp for developing a viable and reasonably universal theory of d.c. dynamos in 1886.[51] One brief publication in the Royal Society's journal in 1887 did indicate, however, that Hopkinson now had reservations about the rigor of his alternator theory. Although his assumption of unvarying armature self-induction had been a "most useful approximation," he admitted that quantity was "not in general" constant after all—precisely the criticism made by Thompson, Ayrton, and others in 1884.[52] Hopkinson did not, however, go on to consider whether his original conclusion about alternator coupling was vitiated by the error induced in his "most useful approximation"—an error that a Wrangler might have tried to quantify.

The standing of Hopkinson's Cantabrigian theory became moot again in early 1889 when Westinghouse effectively demonstrated the paralleling of its company's alternators in its U.S. central stations. Several practitioners tried to find general accounts of the necessary and sufficient conditions that made it possible to couple alternators *in practice*. In February 1889, Gisbert Kapp presented a paper on a.c. machinery to the Institution of Civil Engineers, using a clock diagram to contend that the crucial condition for successful parallel working was a "sensible" (i.e. substantial) amount of self-induction in the armature circuit.[53] This choice of graphical analysis certainly went down well with one former Cambridge mathematics student in the audience, the freelance engineer George Forbes:

> Hitherto most persons who had taken up the subject had done so by means of analytical formulae. The geometrical method was extremely clear and simple, and the Author had so developed it as to put down all the propositions, which were usually in an analytical form, [and] in a purely geometrical form this was very instructive.[54]

Even so, Kapp was criticized by the aristocratic James Swinburne for assuming the constancy of armature self-induction. Swinburne argued instead that the best way of calculating the behavior of an alternator was to use a more painstaking and rigorous form of graphical analysis. Rather than focusing on self-induction, Swinburne suggested that the way to proceed was to find out "how many lines of force were cutting the circuit at any time. By plotting the field magnets out by this means, and working through a graphic method," he could establish the all-important "armature reaction"—his translation of what others treated as variable self- and mutual induction. This was a long and tedious iterative process, but it produced results of some accuracy.[55] Indeed, the use of armature reaction theory soon came to be a major feature in the graphical analysis techniques used in a.c. textbooks.[56]

Hopkinson soon broke his silence on a.c. matters, claiming a further instance of the commercial paralleling of alternators as a trivial instantiation of his a.c. theory. At a meeting of the Institution of Electrical Engineers in May 1889, William Mordey, manager of the Anglo-American Brush Company in London, announced the successful coupling of newly designed alternators. Mordey's lengthy paper explicitly disputed Kapp's diagnosis of the necessary and sufficient condition by stipulating that Brush-designed alternators were designed to have the *minimum* possible self-induction. For the discussion of Mordey's paper at a subsequent IEE meeting, Hopkinson sent in a haughtily written report claiming that his 1884 theory had been "quite sufficient" to explain all of Mordey's results. Hopkinson's claim amounted to the point that Mordey's design instantiated his 1884 stipulation for maximizing the power output of alternators.[57] Yet Mordey replied sardonically that he had not derived any assistance from Hopkinson's theory, and disagreed that the Senior Wrangler's theory was sufficient to predict all the results he (Mordey) had obtained.[58] *The Electrician* agreed with Mordey and, in a rather exasperated editorial in the issue dated August 16, 1889, declared that it had not "the least desire to impeach Dr. Hopkinson's mathematics," but ventured that the conditions of the problem were "not fully expressed in the formula he has given us."[59] Hopkinson's assertions that all the relevant conditions for parallel running were already locked up in his theory sounded all too much like the mathematician's conjuror's trick of which *The Electrician* had complained two weeks earlier (see epigraph above).

By 1893, when a.c. engineering seemed to have won the battle of the systems, a new view emerged that running alternators in parallel was not a question of armature self-induction after all. Rather, it hinged on mechanical matters: linking the governors of the steam turbines to get stability in rotation, and using synchronization devices to ensure that alternators were exactly in phase before linking them. In a paper on this subject presented to the IEE in February 1893, Mordey explicitly avoided theoretical speculation, addressing only the "practical" principles of machine management that he and his assistants had established for paralleling.[60] While Mordey cited Hopkinson's now-standard 1886 "back-to-back" method of comparing dynamo efficiencies, and extended it to a.c. machines, his single direct reference to Hopkinson's 1884 theory concerned the possibility of making an alternator run as a motor.[61] Indeed, in his own comments on Mordey's paper, Hopkinson himself considered only how Mordey had borrowed his comparative testing techniques. One participant who perhaps misconstrued Mordey's level of deference to Hopkinson was Hugh Erat ["Huge Rat"] Harrison, Principal of "Faraday House" training college in London. Later in the discussion, made this arch comment:

Mr. Mordey acknowledges his indebtedness to Dr. Hopkinson's earlier papers on the subject, and states that his empirical results have there been anticipated. I am afraid that most practical engineers regard these forecasts rather in the light of that statue of great price which is said to lie in the proverbial block of marble, and I doubt if many engineers have ever chipped off the mathematics in which these great truths lay hidden.[62]

In his response, however, Mordey made clear how little benefit he had himself found in such theories. Elaborating on Harrison's metaphor somewhat further, while showing polite deference to Hopkinson (elected three years earlier to the Chair of Electrical Engineering at Kings College, London), Mordey explained:

Mr. Harrison referred to Dr. Hopkinson's paper, and he properly points out that I give Dr. Hopkinson credit for having prophesied some of these things. No doubt I should have been able to get on a great deal better with my work if I had been fully able to benefit by Dr. Hopkinson's papers. I expect a good many of us are not able, to use Mr. Harrison's figure [of speech], to chip off the mathematics and get at the really beautiful statue inside. I have had to work round as if the statue did not exist at all.[63]

Far from being a sine qua non for practical success, Hopkinson's elaborate abstract constructions were superfluous or even chimerical as far as the ordinary engineer was concerned. More brusquely—perhaps even sarcastically—Mordey added that the responsibility for rendering Hopkinson's work more intelligible and usable lay with Hopkinson himself:

We do not often get Dr. Hopkinson here now—I wish we saw him more frequently—but as he is here I may take the opportunity of uttering a wish that when writing the papers which always turn out long afterwards to be so very important, he would remember people like me, and condescend to our level: it would greatly increase the immediate value of his writings.[64]

Mordey's sarcasm was not far below the surface when he addressed another IEE meeting on the same subject in March 1894. For this occasion, the freelance electrical engineer George Forbes had formulated a version of Mordey's account into the algebraic language so cherished by Cambridge graduates while removing the problematic inconstant factor of self-induction from consideration. Responding to this, Mordey noted with relief that it would now be clearer to all concerned that, as he had always maintained, the practical solution to the paralleling problem was "something different" from what Professor Hopkinson had deduced from his equations: "Now that my views have received the consecration of x's and y's, I may dare say that they will be honoured with the tardy approval of those who have hitherto failed to be convinced by the logic as expressed in the vulgar tongue."[65] Mordey's arch characterization of this intra-professional tension as one opposing his own brand of "vulgar" tongue to that of analytic mathematics is very revealing. His resentment of the almost clerical elitism of Cambridge-style mathematics invokes concerns of social class only hinted at in previous discussions of whose expertise could best handle a.c. technology. To see that Mordey's position was far from idiosyncratic, let us consider broader evidence on whether contemporary electrical engineers saw Cambridge Tripos mathematics as a useful or problematic resource for handling the challenges of their subject matter.

The Use and Abuse of Mathematics: Debating the "Cambridge Man"

In the preceding section we saw the grounds of "practical" electrical engineers' antagonism toward the apparent sterility of abstract mathematical analysis and the apparent presumptuousness of the analytical engineers who wielded it. To avoid over-generalizing from just one case study, it is important to note that the debate about Cambridge mathematicians was not confined to the topic of paralleling, or even to electrical engineering. Conversely, it is important to note that, insofar as professional disputes about theory were endemic in electrical engineering, these disputes were not only about the technological applicability of Maxwellian techniques of electromagnetic analysis. Disputes over authority and expertise in electrical engineering also hinged upon the educationally grounded credibility of the notional "Cambridge man."

From this perspective, it is very informative to consider some debates conducted in the spring of 1891 in the correspondence columns of *Industries*, a journal owned by James Swinburne and (until recently) edited by Gisbert Kapp. After a discussion (initiated by a Glaswegian student) about whether thermodynamic theory was of any use to trainee engineers,[66] a secondary debate emerged under the editorial heading "The use and abuse of mathematics." This illustrates the widespread view—hitherto undocumented by historians of electrical engineering—that the alleged ignorance of practical men was far less of a problem for the nascent profession than the hubristic assertions of mathematically trained practitioners, especially the Cambridge-trained variety.

On March 28, 1891, a Leeds "Electrician" wrote to the editor of *Industries* seeking to clarify the long-running "feud between the so-called theorist and the practical man." His concern was that theorists who wielded mathematical learning uninformed by practical knowledge had little dialogue with the practical men who "reviled" mathematics, preferring to rely instead on their inscrutable wisdom. In a normative vein, he argued that the *true* theorist and practical man should be one and the same individual. Nevertheless, he singled out the highly educated group for special criticism:

> The chief real complaint against mathematicians is that they are apt to over-rate the value of mathematical work, and to forget that they are only employing deductive reasoning. Nothing is commoner than to find a simple law assumed, and then followed by pages of calculations which would be useful if the assumed law were true, but are really valueless. This vice is especially common in electrical literature.[67]

In electrical engineering the feud had indeed raged "very bitterly." For example, the study of dynamos had unhelpfully been "retarded for years" by those who wrote page after page showing hypothetically how such machines would behave if the pressure were "this function or that function" of the excitation currents—but with little reference to real engineering machinery. The target of his attack was almost certainly John Hopkinson, author of many Wranglerish treatments of idealized alternators and dynamos in the preceding decade. In ironic tones the Leeds Electrician maintained that Cambridge seemed to set the "fashion" in mathematics: none but a "Cambridge man" was supposed capable of anything in the subject, others being generally doomed to "outer darkness" or "eternal ignorance," especially if they had never attended a university.[68] Notwithstanding the alleged Cantabrigian monopoly of expertise, the correspondent hoped that somebody might yet write a book on electricity that contained the practical mathematics that was required by engineers and nothing more. Perhaps taking a particular potshot at Wrangler faith in algebraic solutions, he emphasized that he did not mean a book of "delusive" simplified explanations. Extended use of graphical methods was the important thing, notwithstanding their inelegance as viewed by the elite mathematician:

> For practical work graphical methods should be much more developed than they are. The Cambridge man looks upon a graphical solution as a kind of foul play, and would as soon think of solving an equation by trial [and error], or by omission of a term, or in any other reasonable way.

The main issue was thus overcoming the Cambridge graduate's adherence to vacuous virtuosity in algebraic manipulation and the associated prejudice against recourse to graphical methods when more appropriate. Although the long-term resolution of the theory-practice feud lay in having the two parties learn from each other, the Leeds Electrician concluded on a partisan note that "a little more modesty on the part of the college man" would be of the greatest advantage to all.[69]

Subsequently published correspondence under the editorial heading "The use and abuse of mathematics" evoked a broadly sympathetic if not uncritical or consensual response. The following week, there appeared a letter credited to "Ne Sutor Supra Crepidam," an archly erudite pseudonym meaning "Let not the shoemaker judge above his shoe." While contending that the complexity of dynamo behavior could only be apprehended by similarly complex and "unpractical" mathematics, he also welcomed the new Cambridge school of engineering as a means of bringing due practicality to mathematics taught at that university. His even-handed conclusion was that the mathematician should become an engineer as much as engineers should become mathematicians.[70] Robert H. Smith of Mason College, Birmingham, wrote in to contend that the best theoreticians and practitioners in engineering had always maintained a "free, friendly and respectful" communication of ideas. Yet all reasonable observers would agree, he claimed, on the "pernicious" effects of combining knowledge of pure mathematics with ignorance of practical matters. Not only did learning unnecessarily abstract techniques waste time; it had the "evil" effect of "perverting" engineers' whole way of thinking and acting. He thus laid out a detailed agenda of the kind of mathematical learning that was appropriate to engineering education, a staged acquisition of geometry, algebra, and elementary calculus that produced no tendency to "specious mathematical showiness" and no inclination to "indulge in imaginative theory not based on fact."[71]

The debate closed with an exchange between two correspondents who differed on the significance of the appointment of a recent Wrangler—among the top twenty in Cambridge—to an electrical engineering consultancy run by Alexander Kennedy, formerly Professor of Mechanical Engineering at University College London. An "Engineering Demonstrator" from London observed this to be part of a growing trend for electrical supply companies to call in "theoretical advisors": the commercial demand for specialists with higher mathematical training was the clearest recognition of their increasing importance.[72] Writing from Bombay on June 5, "Scrutator" replied that such higher mathematical training was useful but still not necessary. Professor Kennedy himself had attained the status of chief consultant electrical engineer without any university training in mathematics, only what he had picked up in the practice of civil engineering. Scrutator added, nevertheless, that it was important for the current generation of trainee engineers to learn the equivalence between analytical, graphical, and mechanical means of calculation. Concurring with the Engineering Demonstrator, Scrutator concluded that by learning the "real" use of mathematics, such students could avoid the worst "abuse" of mathematics—which was not to use it at all.[73]

In the context of such discussions arguing the need for convergence between the two factions, I will now consider how educational plans were developed in the mid 1890s to ensure that future electrical engineers could embody a suitable unification of the "practical man" and the mathematician in one individual.

Hopkinson and the Relation of Cambridge Mathematics to Engineering, 1894–1896

If in my lecture I have rather insisted on the uses of the calculus it was because I thought its methods were unduly feared and shunned by engineers, not because I or other Cambridge mathematicians disliked or shunned geometrical methods.

—letter from John Hopkinson to *The Electrician*, May 1894[74]

In the world of electrical engineering, Hopkinson was no diplomat; he was rarely amenable to debating his judgments on technological topics. In contrast to Mordey, Harrison, and the correspondents in *Industries*, Hopkinson was unwilling, in the early 1890s at least, to resolve the theory-practice tension by mutual accommodation; nor was he willing to translate Cambridge mathematics into more widely intelligible graphical forms, as undertaken by Blakesley and Kapp. This much is clear in his 1890 memorandum, written for the University of Cambridge, on the future of engineering education at his alma mater. Hopkinson asserted that while the majority of engineers would be "practical men" executing only a limited class of practical tasks, engineers taking a broader professional view would benefit from a mathematical education closely linked to "physical work." Indeed, he would advise a son who wished to be an engineer not to take early employment in a traditional mechanical workshop but rather to go to university first to study mathematics and laboratory physics.[75]

When the Institution of Civil Engineers (ICE) in London invited Hopkinson to deliver its prestigious James Forrest lecture in early May 1894, the supremacy of such university training was the unstated major premise of "the relation of mathematics to engineering."[76] Speaking in the same venue in which he had first pronounced on alternator theory a decade before, Hopkinson observed that mathematics had been described in that room by engineers as a good servant but a "bad master." Assuming a characteristically Cantabrigian definition of what constituted mathematics, his overt aim was to prove the first part of this proposition, and less explicitly to challenge the latter point by showing engineering's great debt to mathematics. He thus showed how the "higher" mathematics of differential equations and Bessel functions, coupled with some strategic analogies, had served engineers well in examining cases as diverse as stressed beams, compass corrections, and submarine telegraph signaling.

As one might have expected of one tutored in the rigor of the Cambridge Mathematics Tripos, Hopkinson emphasized the practice of solving all problems by first formulating differential equations to capture the material conditions, and solving these analytically to arrive at an all-encompassing algebraic expression. For the case of transmitting a musical sound down a telephone line, he cited the formula which "tells us everything" about the propagation and decay of the signal, asserting it was difficult to see how the problem could be dealt with by any means except solving a differential equation. Hopkinson was as unyieldingly committed to the necessity of Wrangler methods as he was convinced of his own authoritative originality in the complex phenomena of a.c. currents. He thus characteristically reiterated the claim that it was he who had "shown mathematically" that alternators could be run in parallel, even before—he now alleged—the matter had became one of serious practical concern in the electrical industry. Although he catalogued some of the areas in which mistakes had been caused by the "wrong" application of mathematical formulae, he insisted that such errors were only to be cured by a "more abundant supply of more powerful mathematics." It was not appropriate, by implication, to seek recourse in alternative forms of mathematics, such as approximate graphical methods, when difficulties or discrepancies arose.[77]

Some of the anticipated mixed reaction to Hopkinson's lecture can be gauged from the vote of thanks offered by the ICE's president, Alfred Giles. While he recognized this lecture would have been an intellectual treat for those already cognizant with higher mathematics, he hoped at least that younger members not so familiar with it would feel "spurred" to follow Dr. Hopkinson's "brilliant" example in applying mathematics to engineering. As Giles put it, it was not after all very often that the ICE had the opportunity of "receiving good advice from a senior wrangler."[78] Hopkinson's audience, however, had not treated his lecture with the utmost sympathy. The editors of *The Electrician*, who had long been critical of Hopkinson's views on the role of mathematics and mathematical theory in engineering, were not impressed. Reprinting the first half of his lecture in its May 11 issue, the periodical chose also to devote a substantial editorial criticizing the Cantabrigian bias of his lecture, rather pointedly highlighting the parochial nature of Hopkinson's claims with the subtitle "the relation of Cambridge mathematics to engineering."

Taking Hopkinson to be a representative of the Cambridge "school" of mathematics, *The Electrician* reproved him for assuming that Cambridge "algebraic" mathematics was the only useful form of mathematics for engineers.[79] There was of course no doubting the *general* value of such mathematics for science: it would have been "ridiculous" for Lord Rayleigh, for example, to lecture the Royal Society on the relation of algebraic mathematics to physical science—these two fields were nearly identical.[80] Yet, *The Electrician*'s staff writer contended, it was simply false to assert, as Hopkinson had done, that algebraic methods were *necessary* in engineering. Professor Hopkinson had failed to acknowledge that clock-face diagrams could readily solve a.c. problems that became highly complicated when attempts were made to solve them in the formulation of

"algebraical" language. Though graphical methods were less informative and perhaps slower than analytical methods, the economic argument for using them in the engineering business was overwhelming: whatever the problem, it was generally cheaper to employ a draftsman for three hours than a wrangler for three minutes. With no little irony, and with a gesture indicating how Hopkinson might be able to redeem his position, *The Electrician* pointed to a major fertile innovation by Hopkinson in 1880—the empirical "characteristic curve" of dynamo performance—that showed the effectiveness of deploying graphical methods in electrical engineering.[81]

To the editors of *The Electrician*, the problem was particular to the educational culture of a particular prestigious British university. Although geometrical techniques were given appropriate attention at Oxford, the lamentable non-recognition of graphical methods at the higher levels of engineering would continue if Cambridge were allowed to continue to dominate the field of mathematics:

> So far is geometry behind algebra today, that it is difficult to say that the former will ever become so powerful and general a branch of mathematics. Its present condition in this country is largely due to the neglect, and we would even say the ignorance, of geometry in Cambridge.

This situation could hardly change while so many Cambridge-trained schoolmasters taught mathematics of a kind geared toward getting their pupils entrance to Cambridge. A whole political economy of mathematics teaching thus had to be reformed if any beneficial change were to be effected in engineering training.[82]

Hopkinson's response to the scathingly anti-Cambridge editorial moved him to an uncharacteristically public rebuttal. The next week, *The Electrician* published a letter from him complaining that his university had not been treated with "justice." Geometry held a specially protected place in the Cambridge curriculum, and had been used in important ways not just by Maxwell but also by such eminent graduates as Kelvin (in his method of electrical images) and P. G. Tait (in his theory of damped oscillations). Mathematicians, Hopkinson now contended (contradicting his ICE lecture), should be "ambidextrous" in both geometrical and algebraic techniques. The reason his lecture had focused on calculus was not that Cambridge mathematicians disliked geometrical methods but because calculus was "unduly feared and shunned" by engineers. Importantly, Hopkinson conceded that his own work in electrical engineering (the characteristic curve and a.c. theory) was in fact more frequently handled by *other* practitioners using geometrical methods. Nevertheless, he insisted that algebraic methods deserved paramount consideration since they had long been central to making new "discoveries" that were only later translated into geometrical form. Tellingly, Hopkinson acknowledged that this prioritization was rooted in local Cambridge history: before the analytic revolution of the 1820s, a Tripos student who solved a problem by quick algebraic methods had to translate his solution back into geometrical terms to win credit from examiners. With such a legacy, wranglers equipped with powerful analytical techniques were unlikely to capitulate ever again to the reactionary hegemony of geometry.[83]

The historian chronicling these events can hardly fail to see a considerable shift in emphasis between Hopkinson's ICE lecture and the defensive letter he wrote two weeks later in response to *The Electrician*'s review. Without overstating the significance of this embarrassing public assault on both Hopkinson and his university, it is perhaps no coincidence that his subsequent publications on a.c. theory abandoned some of their idealized Wranglerish formalism for which *The Electrician* had so strongly criticized him. One year after his ICE lecture, he produced a revised account of "alternate current dynamo electric machines" with the assistance of his Kings College demonstrators; this was published by the Royal Society in its *Philosophical Transactions*. Hopkinson may have been seeking a more sympathetic audience less dominated by skeptical engineers. Unprecedentedly, he now categorized previous work on alternators specifically as "algebraic" discussions, conceding that such research had adopted unwarranted simplifying assumptions about the invariance of self- and mutual induction. Whereas in 1884 he had strategically ignored the phenomenon of mutual induction and variable self-induction as unduly complicating the formalization of rotating electromagnetic machinery, Hopkinson now explored the effects of both these parameters in determining the perplexing performance of alternators.[84]

In characteristic Wrangler style, Hopkinson formulated the operation of an alternator as a pair of differential equations—one each for the magnetic circuit and the armature circuit. He rendered them soluble by using a standard approximation technique, requiring the assumption that the current in the magnetizing circuit did not vary greatly. These equations produced a result for a single alternator with several correction terms and coefficients, with some unanticipated higher harmonic effects—more complicated than anything in his 1884 account of *coupled* alternators. Recognizing still further the vulnerability of his assumption of constant current in the magnetizing circuit, Hopkinson then devoted the bulk of this paper to empirical studies of the characteristics of standard Siemens alternators undertaken by F. Lydall, his laboratory assistant at Kings. These Hopkinson represented in an elaborate series of *graphical* depictions. Looking at the characteristic curves that emerged, Hopkinson noted it was easily shown that the ordinary theory (implicitly his original 1884 account) "does not fully account for the facts." Hopkinson considered the discrepancies between actual machine performance and his most revisionist formalization in quantitative and qualitative depth—and found the equations unequivocally to be wanting.[85] Here was a clear illustration both of the failure of even quite sophisticated approximation procedures in theorizing alternators and of the contrasting power of graphical methods to capture the contingencies of alternator performance—contingencies alien to the orderly world of the Mathematics Tripos.

Despite this, or perhaps because of it, Hopkinson's detailed examination of working alternators greatly facilitated his initiation in the commercial production of alternators for the Mather and Platt company. In the fifth (1896) edition of his best-selling book *Dynamo-Electric Machinery*, Silvanus Thompson noted that, despite their high self-induction, Hopkinson's machines were suitable for working in parallel.[86]

Epilogue

Hopkinson's shift toward accommodation of the analytical and practical traditions was characteristic of a wider pattern of endeavor in the later 1890s among electrical engineers from diverse educational backgrounds to communicate with one another using shared assumptions and mathematical techniques. What Gordon had accomplished in melding laboratory and workshop techniques of practical engineering a decade earlier was now undergoing an analogous process in the abstract computational practices of electrical engineering. Silvanus Thompson—ever the irenic Quaker—was, like his colleague at Finsbury Technical College, John Perry, involved in the contemporaneous project of making calculus intelligible to the next generation of student engineers in all the technical colleges. At Finsbury, since the early 1880s, Perry and William Ayrton had developed the pragmatic use of squared paper and mechanical models to teach elementary calculus to engineering students with little prior geometry or algebra. This approach was epitomized in Perry's *Calculus for Engineers* (1896), which went through several editions.[87] Thompson's most famous contribution to this project was his more broadly directed (and initially anonymous) *Calculus Made Easy* (1910). Leading his readers with great jocularity through the preliminary "terrors" of the subject, he encouraged them to adopt the motto "What one fool can do, another can." Such was the long-lasting impact of this remarkably accessible volume (continuously in print since its first publication),that its 1931 edition inspired the 13-year-old Richard Feynman to learn calculus well before his school curriculum required him to do so.[88]

With a certain poignancy, after John Hopkinson's early death in a family mountaineering accident in 1898, his surviving son Bertram—also a Cambridge mathematics graduate—completely reworked and refined his father's alternator theory in the ensuing five years. He analyzed the phenomenon of "hunting" (persistent perturbation around a harmonic oscillation) as a major machinic impediment to successful paralleling.[89] By 1903 a newly comprehensive body of a.c. theory was emerging, cultivated by Cambridge mathematics graduates working in the electrical industry, notably Hopkinson junior and also Alexander Russell in his two-volume *Treatise on the Theory of Alternating Currents* (1904–1906).[90] Despite being grounded in the Cambridge syllabus of the previous century, their new theoretical canon was free of simple Wranglerish mechanical analogies. The authors recognized too that the problems of alternator paralleling were not a matter of failing to understand Cambridge mathematics, but rather were electromagnetic and mechanical problems that needed more than a Wrangler's analytic facility to solve. Most interesting from the point of view of issues raised earlier in this chapter, they represented the analysis of a.c. machines by vector diagrams drawn directly from the "clock-face" graphical analysis long favored by "practical" engineers. The two languages of mathematics were thus amalgamated in a common pedagogy through which trainee electrical engineers learned what had been cherished by both the practical man and the theoretician of the previous generation.

In this context of pedagogical restructuring in the early twentieth century, the assessment of Hopkinson's theories of a.c. technology by his professorial contemporary W. E. Ayrton is highly revealing.[91] Reviewing Hopkinson's collected technical and scientific papers for the journal *Nature* in 1904, Ayrton suggested that, while his theory of the d.c. dynamo had helped more than "crude trial and error" in solving practical commercial problems, such was not the case for his a.c. theories. These theories had evidently not been applicable to all a.c. machines, and had been more important for what they "suggested" than what they actually proved. Indeed, Ayrton concluded that Hopkinson, in making his so-called prediction in 1883 that alternators could be worked in parallel, had been more influenced by his experimental work with commercial machinery than by his "theoretical reasoning."[92]

While revealing of how a fellow academic judged the qualified success of Hopkinson as a theoretical electrical engineer, Ayrton's account does not tell us why the controversies in which Hopkinson became ensnared happened to focus so specifically and critically on his educational credentials as a *Cambridge* graduate. Why indeed was this a controversy construed in primarily educational terms, rather than as a matter of practice versus theory, social class, or gender? The very fact that Hopkinson's much-criticized early theory of alternators persisted as a point of reference in the electrical engineering canon is indicative, I suggest, of the high social status attached to the views of a former Senior Wrangler so cogently portrayed in Warwick's recent account. Even those practical men who had been through rather different masculinity-building ordeals in the engineering workshop and found much to disagree with (in what little they understood of Hopkinson's account) felt obliged to indicate how *little* it had

helped them when debating the subject in the tribunals of electrical engineering. In contrast to the other controversies over the role of self-induction, Hopkinson neither argued a case drawn from Maxwell's *Treatise* nor attracted support from academic Maxwellians committed to a more sophisticated account of self-induction in electrical engineering theory. In such an isolated position, Hopkinson's tactic was to treat challenges from mechanically trained engineers as if they had simply failed to understand the mathematics that was the prerogative of the Senior Wrangler to promote. By not working dextrously with his academic language Hopkinson afforded them no consideration when these engineers disagreed with the premises of his arguments. This reinforced the class prejudices and antipathies of those who had not had the privilege of a Cambridge mathematics education, moving them to represent Cambridge mathematics as part of the problem in a.c. technology rather than part of the solution.

Tellingly, however, the demographic predominance of practitioners who protested against the glibness of his Cambridge methods and the volubility of critical journals eventually forced Hopkinson to abandon at least some of the Wrangler techniques that he had perhaps involuntarily brought to bear on uncongenial problems of electrical engineering. Concurrently, the increasing sophistication of electrical engineering problems and the commercial pressure for solving them effectively called into the marketplace just the kind of high-level theoretical expertise that Cambridge Wranglers offered to the industry. This story, therefore, has not just been one of how economic forces determine the education needed for a "scientific industry." Nor has it been simply a story of a philosophical spat in which quasi-Platonist mathematicians argued for the underlying orderliness of machines, opposed by quasi-Aristotelian practical men arguing for the organicism of technology. Rather, I have shown that certain kinds of education—or its absence—define the very categories in which engineers of the past articulated their understanding about what was at stake in the successful prosecution of a new discipline. And the sheer heterogeneity of the skills required for electrical engineering meant that practitioners had much to learn, after leaving the academy, the college, or the workshop, from those of other educational backgrounds. Thus James Swinburne told each member of the IEE in 1900 that a practitioner had to "not be only an overgrown wireman, a mechanical engineer with a little electrical knowledge, a mathematician, a financier, a lacquered brass and sealing-wax varnish instrument maker, a physicist, or a manager of men." He had to be all these things, but in "different proportions in different men."[93]

Whatever electrical engineers did or did not say about the matter, education is not the only factor for historians to consider in understanding techno-scientific controversies of the past. But when the engineers of the past denigrate rivals specifically in terms of their deficient education, we should not merely be unsurprised; we also should seek to understand what contextual and contingent circumstances led to education's becoming a center-stage issue. After this study, historians might research how differences in practitioners' education are resolved to some extent in making collective sense of the world. After all, in no discipline of science or engineering is there ever an utterly homogeneous workforce: as David Kaiser shows in this volume, the problem of educational localism can persistently afflict even as prestigious a subject as quantum physics after World War II. And while it is hard to conceive of any practitioners claiming that their education made their accomplishments uniquely possible (are there any discoveries transcendentally reserved for the graduates of Cambridge, Harvard, Princeton, or Yale?), the very fact that techno-scientists eventually *stop* talking about the discrepant and error-inducing education of their peers unarguably merits the historian's closest attention.

Notes

1. "The use and abuse of mathematics," letter from Leeds "Electrician" to *Industries*, April 3, 1891.

2. James Swinburne, "Presidential address," *Journal of the Institution of Electrical Engineers* 32 (1902), 13. For more information on James Swinburne and other early British electrical engineers see Graeme Gooday, *The Morals of Measurement: Accuracy, Irony and Trust in Late Victorian Electrical Practice* (Cambridge University Press, 2004).

3. Andrew Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics* (University of Chicago Press, 2003). See also Peter M. Harman, ed., *Wranglers and Physicists: Studies on Cambridge Physics in the Nineteenth Century* (Manchester University Press, 1985).

4. Michael Sanderson, *The Universities and British Industry, 1850–1970* (Routledge and Kegan Paul, 1972); idem, *Educational Opportunity and Social Change in England* (Faber, 1987); 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; idem, "Lies, damned lies and declinism: Lyon Playfair, the Paris 1867 Exhibition and the contested rhetorics of scientific education and industrial performance," in *The Golden Age: Essays in British Social and Economic History, 1850–70*, ed. I. Inkster (Ashgate, 2000), 105–120.

5. Warwick, *Masters of Theory*, 205–207. For discussion of Hopkinson as epitomizing some key features of Cambridge Mathematics Tripos culture, see ibid., 187, 216–217.

6. Warwick, *Masters of Theory*.

7. The key original text was James Clerk Maxwell, *Treatise on Electricity and Magnetism* (Clarendon, 1873), 2 volumes; for early readings of this in Cambridge University see Warwick, *Masters of Theory*, 286–356. For accounts of the Maxwellian community see Jed Buchwald, *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century* (University of Chicago Press, 1985); Bruce Hunt, *The Maxwellians* (Cornell University Press, 1991). Buchwald's broad categorization includes Hopkinson as a Maxwellian, but Hunt focuses on a much smaller subgroup. Although Hopkinson's last year in Cambridge (1871–72) overlapped with Maxwell's first year as Professor of Experimental Physics, Hopkinson only became closely familiar with Maxwell's work in 1876 after Maxwell's refereeing of a paper submitted by Hopkinson to the Royal Society. See Peter Harman, ed., *The Scientific Letters and Papers of James Clerk Maxwell*, 3 volumes (Cambridge University Press, 2002), volume 3, 324–329.

8. Bruce Hunt, "'Practice vs. theory': The British electrical debate, 1888–91," *Isis* 74 (1983): 341–355.

9. D. W. Jordan, "The adoption of self-induction by telephony, 1886–89," *Annals of Science* 39 (1982): 433–461. See also idem, "D. E. Hughes, self-induction and the skin-effect," *Centaurus* 26 (1982): 123–153.

10. Hunt, *The Maxwellians*; Buchwald, *From Maxwell to Microphysics*.

11. R. Kline, "Science and engineering theory in the invention and development of the induction motor, 1880–1900," *Technology and Culture* 28 (1987): 283–313. For Pupin's 1922 reminiscences of his Cambridge education ca.1884–85, see Michael Pupin, *From Immigrant to Inventor* (Scribner, 1960), 167–210.

12. In recent historiography the various different meanings of the term "practice" cannot easily be demarcated from "theory." Whether one considers a "practice" in techno-science to be the macroscopic domain of professional activity or a localized, individuated technique, both forms of practice are obviously theory-laden in some sense. See Graeme Gooday, "Practice," in *The Reader's Guide to The History of Science*, ed. Arne Hessenbruch (FitzRoy Dearborn, 2000), 589–591.

13. I. Yavetz, "Oliver Heaviside and the significance of the British electrical debate," *Annals of Science* 50 (1993): 135–173, on 136 and 153.

14. Jordan, "The adoption of self-induction by telephony."

15. A. Warwick, "The laboratory of theory," in *The Values of Precision*, ed. M. Norton Wise (Princeton University Press, 1995), 311–351; idem, "Cambridge mathematics and Cavendish physics: Cunningham, Campbell and Einstein's relativity," *Studies in History and Philosophy of Science* 23 (1992): 625–656 (part 1), and 24 (1993): 1–25 (part 2); idem, *Masters of Theory*.

16. Warwick, *Masters of Theory*, 252–254.

17. Kuhn described normal science as a "strenuous attempt to force nature into the conceptual boxes supplied by professional education." Thomas Kuhn, *The Structure of Scientific Revolutions* (University of Chicago Press, 1962), 5. Joseph Rouse has noted the existence of two types of Kuhn: one focused on practices, and the other on global worldviews; it is the former upon which I focus here. See Joseph Rouse, "Science as practice: Two readings of Thomas Kuhn," in Rouse, *Knowledge and Power: Toward a Political Philosophy of Science* (Cornell University Press, 1987), 26–40. Many thanks to David Kaiser for bringing Rouse's piece to my attention.

18. On open-ended practice, see Andrew Pickering and Adam Stephanides, "Constructing quaternions: On the analysis of conceptual practice," in *Science as Practice and Culture*, ed. Andrew Pickering (University of Chicago Press, 1992), 139–167.

19. Warwick, *Masters of Theory*, 18–26.

20. Ibid., 273–275.

21. Thomas Blakesley, *[Papers on] Alternating Currents of Electricity*, third edition (Whittaker/ Bell, 1891), 4–6.

22. See R. Kline, *Steinmetz: Engineer and Socialist* (Johns Hopkins University Press, 1992), 38–40, 324. Graphical methods were especially congenial for engineers whose formal mathematical schooling had consisted of Euclid and elementary algebra, and whose daily working life consisted in working from scaled geometrical designs or plans. Even academic engineers who took college degrees would be trained in geometrical methods, e.g. Karl Pearson's teaching of mathematics to engineering students at University College London in the late 1880s. I thank Eileen Magnello for pointing out this aspect of Pearson's teaching.

23. Thomas Hughes, *Networks of Power* (Johns Hopkins University Press, 1983).

24. Percy Dunsheath, *A History of Electrical Engineering* (Faber and Faber, 1962); James Greig, *John Hopkinson, Electrical Engineer* (Her Majesty's Stationery Office, 1970). Brian Bowers repeated this claim in a public lecture at the IEE (October 1998).

25. A week after delivering his paper on November 13, 1884, Hopkinson learned that Henry Wilde had published on an instance of "synchronizing control" between linked alternators in the *Philosophical Magazine* for January 1869. Hitherto unaware of this article, Hopkinson apologized to the (notoriously litigious) Wilde in a footnote at the start of his 1884 publication for not having given him "the honour which was his due." J. Hopkinson, "On the theory of alternating currents, particularly in reference to two alternate current machines connected to the same circuit," *Journal of the Society of Telegraph Engineers* 13 (1884): 496–515, discussion 528–558, quotation on 496. This paper was reproduced (minus discussion) in *Original Papers by the Late John Hopkinson*, ed. B. Hopkinson (Cambridge University Press, 1901), volume 1, 133–151, quotation on 133.

26. J. Hopkinson, "On electric lighting [first paper, 1879]," in Hopkinson, *Original Papers*, volume 1, 33–46, esp. 44; idem, "On electric lighting [second paper, 1880]," in ibid., 47–56, esp. 47 , 49, and 55.

27. J. Hopkinson, "Some points in electric lighting [1883]," in Hopkinson, *Original Papers*, volume 1, 57–83, esp. 59–64.

28. Hopkinson, "Some points in electric lighting," 66–69.

29. J. Hopkinson, "On the theory of alternating currents, particularly in reference to two alternate current machines connected to the same circuit," *Journal of the Society of Telegraph Engineers* 13 (1884): 496–515, discussion 528–558.

30. Hopkinson, "The theory of alternating currents," 503–504.

31. ". . . neglecting the effect of currents other than those in the copper wire" (Hopkinson, "On the theory of alternating currents," 501).

32. See Hopkinson's discussion in the Mordey 1889 IEE paper discussed below.

33. Note that in problems V and VI Hopkinson considered results for an individual alternator with mutual induction, but importantly not the case for two connected in parallel ("The theory of alternating currents," 509–512).

34. Ibid., discussion on 531 and 538–554. See discussion of self-induction and mutual induction in Sylvanus Thompson, *Dynamo-Electric Machinery: A Manual for Students of Electrotechnics* (E. and F. Spon, 1884), esp. 387–388.

35. Llewellyn Atkinson, "Proceedings of the Commemoration Meetings," *Journal of the Institute of Electrical Engineers* 60 (1922), 441–443, on 443. For an obituary of Atkinson, see *Journal of the Institution of Electrical Engineers* 85 (1939): 769–770.

36. Mrs. J. E. H. Gordon, *Decorative Electricity* (Sampson & Low, 1891), 153–154.

37. For an obituary of Gordon, see *Electrician* 30 (1893): 117–118. On Gordon's researches, especially in the Maxwellian area of electro-optical rotation, see J. E. H. Gordon, *A Physical Treatise on Electricity and Magnetism* (S. Low, Marsten, Searle, and Rivington, 1880); the second edition (revised) was published in 1883.

38. Gordon's a.c. endeavors were overshadowed by the endeavors of the longer-lived Sebastian Ziani Ferranti (Dunsheath, *A History of Electrical Engineering*, 160–161).

39. For a discussion of the symptomatic historical erasure of technical assistants from accounts of scientific practice, see Steven Shapin, "The invisible technician," *American Scientist* 77 (1989): 554–563.

40. J. E. H. Gordon, "The development of electric lighting," *Journal of the Society of Arts* 31 (1883): 778–787, discussion 787–791, esp. 781–782.

41. Obituary of Frank Bailey, *Journal of the Institution of Electrical Engineers* 69 (1931): 1318.

42. See *The Electrician*, May 28, 1886, 51. Also see the reminiscences of A. H. Walton and of Frank Bailey in "Proceedings of the Commemoration Meetings," *Journal of the Institution of Electrical Engineers* 60 (1922): 402–406 and 416–420 (Bailey quotation on 416).

43. "A complaint," *Electrician* 16 (1885): 25. Quotations from Bailey and Walton in *Journal of the Institution of Electrical Engineers* 60 (1922): 417–418 and 405, respectively. The litigation continued later; see "The Paddington installation," *Electrician* 18 (1886–87): 500–501. Cf. Robert H. Parsons, *The Early Days of The Power Station Industry* (Cambridge University Press, 1940), 42. The Paddington installation served until 1907.

44. [Anon. editorial], *Electrician* 17 (1886), April: 51–56, quotation on 56.

45. See *Journal of the Institution of Electrical Engineers* 60 (1922): 377–500. Bailey's remarks are on 416–418.

46. A. H. Walton, "Mr. J. E. H. Gordon's system of electric supply, Great Western Railway, Paddington, 1884," in "Proceedings of the Commemoration Meetings," 405.

47. Gordon, *Decorative Electricity*, 153.

48. See Gordon's contribution to the discussion of Rookes E. B. Crompton, "Central station lighting: Transformers vs. accumulators," *Journal of the Society of Telegraph Engineers and Electricians* 17 (1888): 195–196. For a variant reporting of the discussion by *Electrician* staff, see *Electrician* 20 (1887–88): 634–637, 655–656, discussion on 656–657, 694–698, and 749–752; and *Electrician* 21 (1888): 88–93. Gordon's Paddington installation was undertaken on behalf of the Telegraph Maintenance and Construction Company. When Telcon decided to discontinue its electrical lighting projects in 1887–88, Gordon became chief engineer to the newly founded Metropolitan Supply Company: [Anon.], "Obituary: James Edward Henry Gordon," *Electrician* 30 (1893): 417–418.

49. For details of Ferranti's work at the Grosvenor Gallery in 1885–86, see J. F. Wilson, *Ferranti and the British Electrical Industry* (Manchester University Press, 1988); A. Ridding, *S. Z. de Ferranti: Pioneer of electric power* (Her Majesty's Stationery Office, 1964), 7. The heroic focus on Ferranti is sustained in Hughes, *Networks of Power*, 97–99, 238.

50. Greig, *John Hopkinson*, 17–22. See John and Edward Hopkinson, "Dynamo-electric machinery," *Philosophical Transactions of the Royal Society* 177 (1887): 331–358.

51. See D. Jordan, "The magnetic circuit model, 1850–1890: The resisted flow image in magnetostatics," *British Journal for the History of Science* 23 (1990): 131–173.

52. J. Hopkinson, "Note on the theory of the alternate current dynamo," *Proceedings of the Royal Society* 42 (1887): 167–170.

53. G. Kapp, "Alternate current machinery," *Minutes of Proceedings of the Institution of Civil Engineers* 97 (1889): 1–79, esp. 1–21. Note that Kapp's approach assumed the constancy of self-induction, but in the graphical method, this assumption could be avoided by finding a non-circular trajectory for the self-inductive "vector" indicator.

54. Kapp, "Alternate current machinery," discussion on 48.

55. Ibid., 62–63.

56. For a brief period in 1889, Swinburne acted as a supporter of Hopkinson's theory, claiming to have translated this "painfully" mathematical theory into a form intelligible to engineers. For more on Swinburne see Graeme Gooday, *The Morals of Measurement: Accuracy, Irony and Trust in Late Victorian Electrical Practice* (Cambridge University Press, 2004).

57. Several auxiliary assumptions seem to be necessary for this to be a "consequence" of the 1884 theory.

58. W. M. Mordey, "Alternate current working," *Journal of the Institution of Electrical Engineers* 18 (1889): 583–613, discussion on 613–688.

59. [Editorial], "Alternate current working," *Electrician* 24 (1889): 325, and notes in ibid., 367–368. See correspondence from a practitioner, W. B. Sayer, to whom it seemed that residual adherence to the Hopkinson theory was attributable to "parental affection" so strong that it was "too terrible" for the theorists to part with their "cherished offspring." Ibid., 387–388.

60. W. M. Mordey, "On testing and working alternators," *Journal of the Institution of Electrical Engineers*, 22 (1893): 117–134, discussion on 134–194.

61. Ibid., 117, 129.

62. Ibid., 174.

63. Ibid., discussion on 191. See Gooday, *The Morals of Measurement*, chapter 5.

64. Mordey, "On testing and working alternators," discussion on 191.

65. William Mordey, "On parallel working with special reference to long lines," *Journal of the Institution of Electrical Engineers* 23 (1894): 260–314, on 301–304, 312.

66. "Is the study of thermodynamics of use to engineers?" (letter to the editor from "Student"), January 3, 1891, *Industries* 10 (1891): 39. For further correspondence concerning this topic, see ibid., 87, 136, 160, 183, 207.

67. "The use and abuse of mathematics," letter from Leeds "Electrician" to *Industries* 10 (1891): 328.

68. In a further deconstructive vein, he noted that the introduction of women to the Cambridge examination system would beneficially prove that "many an ordinary person" might have become Senior Wrangler given a few years "free from care, work or anxiety" and tutors to help them to "do nothing but read mathematics in a regular and systematic way." Ibid.

69. Ibid.

70. [Anon.], *Industries* 10 (1891): 374.

71. Robert H. Smith, letter of April 21, 1891, *Industries* 10 (1891): 399.

72. "Engineering Demonstrator," letter of May 2, 1891, as published in *Industries* 10 (1891): 447.

73. "Scrutator," letter of June 5, 1891, as published in *Industries* 10 (1891): 616.

74. John Hopkinson, "The relations of Cambridge mathematics to engineering," May 18, 1894, as published in *Electrician* 33 (1894): 85.

75. John Hopkinson, "Memorandum on engineering education [written for a Cambridge University syndicate in 1890]," in Hopkinson, *Original Papers*, volume 1, lxiii–vi, quotation on lxivv.

76. J. Hopkinson, "The relation of mathematics to engineering," reproduced in *Electrician* 33 (1894): 41–43, 78–79, and in Hopkinson, *Original Papers*, volume 1, 269–288.

77. Ibid., 79.

78. Ibid.

79. [Editorial], "The relation of Cambridge mathematics to engineering," *Electrician* 33 (1894): 44–46.

80. Conversely, according to *The Electrician,* James Clerk Maxwell's admirable promotion of graphical methods to addressing engineering problems almost "disqualified" him from the Cambridge school. See Maxwell's use of geometrical methods to expound the theory of electrical images in his *Treatise on Electricity and Magnetism,* third edition (Clarendon, 1891), volume 1, 244–269, 280, and unpaginated appendices at the end of the volume mapping equipotential lines for various electrostatic configurations.

81. This reprises a prominent theme in the debate in *Industries* of 1891: mathematical practices in engineering theory should be adapted to the commercial conditions of engineering, not vice versa.

82. Anon., "The relations of Cambridge mathematics to engineering," *Electrician* 33 (1894): 45–46. The editorial characterized the more general problem in the following sardonic terms: "Cambridge mathematics is the result of a vicious circle to which we have alluded on a previous occasion: school-boy, scholarship, wrangler, school-master. Another smaller circle, whose viciousness is inversely as its diameter, touches this wide one, and is: examinee, wrangler, examiner." Ibid. On the influence of the Cambridge Mathematics Tripos on (private) school curricula, see Warwick, *Masters of Theory,* 254–263.

83. Hopkinson, "The relations of Cambridge mathematics to engineering," 85.

84. John Hopkinson and Ernest Wilson, "Alternate current dynamo electric machines," *Philosophical Transactions* Series A 187 (1897): 229–252, reproduced in Hopkinson, *Original Papers,* volume 1, 156–182; quotation on 157.

85. Ibid., 163.

86. Significantly, however, this was immediately qualified by the vaguely Hopkinsonian recommendation that "it is sufficient if *R* is something of the order of *pL*," *p* being the angular frequency at which the alternator was run. See Thompson, *Dynamo-Electric Machinery,* fifth edition (1896), 600–613.

87. As Professor of Mathematics and Mechanics from 1896 to 1913 at the Royal College of Science and School of Mines in London (part of Imperial College from 1907), Perry persistently fought the prevailing view that a mastery of Euclid was essential for all students, contending rather that for non-mathematicians, the subject should be taught primarily with a view to its "utility." See the short volume he edited for the British Association on the Advancement of Science in 1901, *Discussion on the Teaching of Mathematics,* and Graeme Gooday, "John Perry," in *Oxford Dictionary of National Biography* (Oxford University Press, 2004).

88. Jagdish Mehra, *The Beat of a Different Drum: The Life and Science of Richard Feynman* (Oxford University Press, 1994), chapter 2.

89. Bertram Hopkinson, "On the parallel working of alternators," read at Section G of the British Association for the Advancement of Science in 1903, reproduced in *Electrician* 51 (1903): 886; idem, "The 'hunting' of alternating current machines," *Proceedings of the Royal Society of London* 72 (1903): 233–252.

90. Alexander Russell, *A Treatise on the Theory of Alternating Currents,* 2 volumes (Cambridge University Press, 1904–1906). Formerly an assistant lecturer at Gonville and Caius College, Cambridge, Russell wrote as Lecturer in Applied Mathematics and Superintendent of the Testing Department at Faraday House (electrical engineering college) in London.

91. Hopkinson and three of his children died in an alpine mountaineering accident in 1898. See Greig, *John Hopkinson,* 39–40.

92. W. E. Ayrton, "The life work of a scientific engineer" [review of John Hopkinson's *Original Papers*], *Nature* 70 (1904): 169–172, quotations from 169–170.

93. James Swinburne, "Presidential address," 13. At the time of Swinburne's speech there was only one full female member of the IEE, Hertha Ayrton: educated in the Cambridge Mathematics Tripos she had been elected in 1899. See Evelyn Sharp, *Hertha Ayrton, 1854–1923: A Memoir* (Arnold, 1926); Joan Mason, "Hertha Ayrton (1854–1923) and the Admission of Women to the Royal Society of London," *Notes and Records of the Royal Society of London* 45 (1991): 201–220.