The Morals of Measurement

Accuracy, Irony, and Trust in Late Victorian Electrical Practice

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Moralizing Measurement: (Dis)Trust in People, Instruments, and Techniques

> The scientific community is morally superior to every other form of human association since it enforces standards of honesty, trustworthiness and good work against which the moral quality of Christian civilization in general stands condemned.
>
> Rom Harré, *Varieties of Realism*[1]

> The scientific laboratory is also populated by a wide variety of inanimate agents: experimental apparatus, oscilloscopes, measuring instruments, chart recorders and other inscription devices.
>
> At any time, the culture of the laboratory comprises an ordered moral universe of rights and entitlements, obligations and capabilities differentially assigned to the various agents.
>
> Steve Woolgar, *Science: The Very Idea*[2]

Whom and what should people trust or distrust? This question has long been a prominent concern not only in everyday human transactions but also in the most abstruse domains of science, commerce, and technology. Both Steve Shapin and Ted Porter[3] have shown the significance of this question in the complex relationship between trust and quantification. They demonstrate that, to a certain extent, Restoration natural philosophers and nineteenth-century engineers were able to win greater trust for their claims by giving them quantitative expression. At the same time, though, Shapin and Porter map some of the important historical contingencies of the subject. Quantification has not always been achieved to the satisfaction of all, nor has it necessarily made claims uniformly more highly trusted by all parties. Therefore, to avoid facile transhistorical generalizations about the relations between trust and numerical work, the historian has to ask questions rather more socio–historically specific in nature. Why did a *particular* group of practitioners come to trust or distrust particular strategies for quantification? Why did

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[1] Rom Harré, *Varieties of realism: a rationale for the natural sciences*, Oxford: Blackwell, 1986, pp. 1–2, 6–7. See discussion of this passage in Theodore M. Porter, *Trust in numbers: The pursuit of objectivity in science and public life*, Princeton, NJ: Princeton University Press, 1995, p. 218.

[2] Steve Woolgar, *Science: the very idea*, Chichester, England: Horwood, 1988, p. 102.

[3] Steve Shapin, *A social history of truth: Civility and science in seventeenth century England*, Chicago: University of Chicago Press, 1994. T. M. Porter, *Trust in numbers*.

they come to trust or distrust particular means of achieving quantification for certain specific purposes? How did they come to judge the trustworthiness of particular individuals and instruments to quantify faithfully? What standards of honesty and openness were required for quantitative claims to be trusted? Addressing questions in this contextualist vein, this book has a principal aim to explore the particular modalities of trust and distrust that pervaded the tricky and relatively novel enterprise of measuring electricity in the latter part of the nineteenth century.

Whereas Porter and Shapin have focussed on the trust relations between individuals, I extend the exploration of the intricacies of trust into the material culture of quantification. I look back to the development of electrical measurement instruments in the late nineteenth century and consider how considerations of trust were unavoidably part of the complex division of labour in the business of designing, making, and using such devices. Because this was not simply trust in individual humans, a major concern is to show that the evaluation of measurements made with technologies involved considerable indeterminacy about the location and reference of the trust. In the last section of this chapter, I explore how the subject of trust or distrust might be non-human: It could also be the hardware itself, the materials out of which it was made, the techniques used to make or use it, or the theories employed in interpreting its performance.[4] To the extent that judgement of the trustworthiness of measurements was about the trustworthiness of individuals, we shall see in Section 1.4 that such evaluations of trust were only *in part* moral judgements concerning honesty, honour, and fidelity. In preceding sections I argue for the significance of 'trust' as being at least as important as more commonly discussed themes in the history of measurement, namely, 'power' and metrology. Accordingly, I discuss the historical literature on issues of trust, quantification, and electrical measurement to show how my approach both builds upon and goes beyond previous work. Before that, however, I reappraise the significance of William Thomson's well-known claims about the close relation between measurement and knowledge.

1.1. WILLIAM THOMSON AND THE LIMITS OF MEASUREMENT

I often say that when you can measure what you are speaking about and express it in numbers you know something about it; but when you cannot measure it, when

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[4] For a related project on the location of trustworthiness in twentieth-century computing, see Donald Mackenzie, *Mechanizing proof: Computing, risk and trust*, Cambridge, MA: MIT Press, 2001. My perspectives on how practitioners determine the properties of material culture owe much to Mackenzie's piece 'How do we know the properties of artefacts? Applying the sociology of knowledge to artefacts', in Robert Fox (ed.), *Technological change: Methods and themes in the history of technology*, London/Amsterdam: Harwood Academic, 1996, pp. 247–63.

you cannot express it in numbers, your knowledge is of a meagre and unsatisfactory kind.

Sir William Thomson, 'Electric Units of Measurement',
Lecture to the Institution of Civil Engineers, 1883[5]

Towards the end of the nineteenth century, measurement work had unequivocally become a collective enterprise. It was premised on a shared trust in the efficacy of measurement to capture important characteristics of natural phenomena and machinery. It was also embedded within an ever-diversifying division of labour among designers, makers, and users of measuring instruments, all importantly supported by technicians and assistants. Some individuals nevertheless had a higher profile than others did in this enterprise, and one such was William Thomson, elevated to the peerage as Lord Kelvin in 1892. As Professor of Natural Philosophy at the University of Glasgow from 1846 to 1899, Thomson was uniquely wide ranging in his activity on electrical theory, metrological standards, submarine telegraphy, power generation, electrical lighting, and domestic supply meters.[6] Even when his theories lost favour, respect remained for the instruments he developed in collaboration with the Glasgow instrument-maker James White. Specifically important were the electrical-measurement devices for telegraphic signalling and testing developed from the late 1850s and those for measuring the electrical performance of lighting and power two decades later. When Thomson presented new instruments at the Society of Telegraph Engineers and Electricians (STEE) in spring 1888, his protégé William Ayrton declared criticism of them to be 'out of the question', coming as they did from one revered as if belonging in 'another universe'.[7] Indeed such Thomson–White measurement instruments as the 'current balance' were canonical for standardizing laboratories some decades thereafter (Chapter 4). Yet can we infer from such praise that Thomson's broader pronouncements *about* measurement were equally authoritative or even unproblematic?

Thomson's most famous remark about the epistemological significance of measurement was delivered in a lecture to the Institution of Civil Engineers (ICE) in London in 1883. It amounted to the claim that in order to have a satisfactory knowledge of physical properties it was necessary to be able to measure them. Thomson was doubtless heard with some deference when

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[5] William Thomson, 'Electrical units of measurement', in *Popular lectures*, London: 1891, Vol. 1, pp. 73–76, quotation from p. 73.

[6] Crosbie W. Smith and M. Norton Wise, *Energy and empire*, Cambridge: Cambridge University Press, 1989, pp. 445–94, 649–722; Graeme Gooday, 'Precision measurement and the genesis of physics teaching laboratories', *British Journal for the History of Science* (hereafter *BJHS*), 23 (1990), pp. 25–51, esp. pp. 29–36.

[7] William Ayrton in discussion of W. Thomson, 'On his standard inspectional instruments', *Journal of the Society of Telegraph Engineers and Electricians* (hereafter *JSTEE*), 17 (1888), pp. 540–67, quote on p. 559. There was indeed no criticism voiced against Thomson's instruments at this meeting.

he related how he himself had nearly single-handedly established the importance of 'definite electric measurement' in 1858. He retold the already familiar story of how he had persuaded manufacturers of the first trans-Atlantic cable to take more care in measuring the resistance of copper cables and gutta-percha insulation so as to optimize the performance and profitability of their phenomenally expensive project. His audience was probably not surprised, though, to hear Thomson expounding on the virtues and efficacy of measurement: After all, civil engineers considered submarine telegraphy to belong to *their* professional territory. Given the ubiquity of the standard resistance coil that Thomson himself acknowledged, it is clear that in the intervening quarter century the world had hardly failed to notice the consequences of applying Thomsonian measurement techniques to the telegraph industry. Notably, though, Thomson's 1883 lecture was more than just an autobiographical recapitulation of the pragmatic benefits of quantification. He also forecast that the commercial requirements of electric lighting would bring a similar 'advance' in the practical science of electrical measurement – a point to which I shall return in my concluding chapter.[8]

Ironically, however, Smith and Wise have shown that, at the time of his ICE lecture, Thomson himself was anyway not unequivocally committed to measurement as the only possible grounding for knowledge. Nor was he irrevocably wedded to the view that all practical or epistemological disputes could decisively be solved by a measurement. As is well known, Thomson had challenged the late James Clerk Maxwell's claims for the existence of a 'displacement current' in electromagnetic propagation on the grounds that this theoretically constructed entity was in principle unmeasurable and thus unintelligible. Measurability of itself was not persuasive evidence for Thomson, however, not even the measurable similarity of numbers. When Maxwell pointed out that measurements of the velocity of light were very close to those of the velocity of telegraph signals and the theoretically important ratio of electrostatic to electromagnetic units, Thomson initially doubted this constituted definitive evidence that light was an electromagnetic wave phenomenon.[9] Smith and Wise show, in fact, that physical intelligibility was *more* important to Thomson than measurability. In theorizing the vortex construction of atoms in the ether he drew heavily on phenomenological analogies of steam engines, telegraph lines, and turbine vortices drawn from the Glaswegian landscapes of manufacture and marine technology.[10] Thus

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[8] Thomson, 'Electrical units', pp. 82–6.

[9] Smith and Wise, *Energy and empire*, pp. 445–94. Simon Schaffer, 'Accurate measurement is an English science', in M. N. Wise (ed.), *The values of precision*, Princeton, NJ: Princeton University Press, 1995, pp. 135–72. Note, however, that, in his 1883 lecture, Thomson did concede that the speed of light and the speed of electromagnetic waves were 'probably connected physically'; Thomson, 'Electrical units', p. 90.

[10] Smith and Wise, *Energy and empire*, pp. 396–444. Smith and Wise have emphasized that Thomson's interest in quantification was strongly driven by both a pragmatic secular

it would be unhelpful to treat measurement as the unique key to unravelling Thomson's endeavours, and indeed Smith and Wise carefully avoid this reductionist trap in their wide-ranging study of his career.

Even if he was ambivalent about the definitive power of measurement, Thomson was certainly highly critical about the alternative(s): Any claim to knowledge that could *not* be expressed in numerical form was 'meagre' and unsatisfactory. Although Thomson named no specific targets for this criticism, his audience might have discerned two possible candidates. One was the plethora of semi-popular and educational books then being published which engaged in unresolved speculations about the nature of electricity; these described many venerable traditional experiments without quantitative interpretation.[11] Another possible allusion was to the showy public demonstrations of these or other qualitative electrical experiments by well-known public lecturers such as John Tyndall and Henry Pepper. They perpetuated an older tradition of entertaining audiences with spectacular demonstrations of electrical effects. As Iwan Morus has shown, William Sturgeon and others in the early to the middle part of the century survived on fees earned from such theatrical display[12]; and this qualitative culture of electricity was only gradually displaced from the 1860s when transoceanic telegraphy made manifest the more lucrative value of rigorously quantifying electrical performance. Because, on Smith's and Wise's account, Thomson

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concern for financial economy and a personal moral imperative to minimize waste of divinely endowed resources. Ibid., 248–9, 255–6, and esp. p. 684. For further discussion, see Crosbie Smith, *The science of energy: A cultural history of energy physics in Victorian Britain*, London: Athlone, 1998.

[11] Examples of an entirely qualitative experimental treatment of electricity are to be found in such textbooks as Edmund Atkinson, *Natural philosophy for general readers and young persons*, London: 1872 (adapted from Ganot's *Cours Elementaire de physique*); John Angell, *Elements of magnetism and electricity*, London/Glasgow: 1879; Frederick Guthrie, *Magnetism and electricity*, 1876. Note: Guthrie's revised edition of 1884 included a supplementary chapter (constituting about 15% of this edition) by his assistant, Charles Vernon Boys, which covered electrical machines and measurements.

[12] For a discussion of John Tyndall see William H. Brock, N. D. McMillan, and R. C. Mollan (eds.), *John Tyndall: Essays on a natural philosopher*, Dublin: Royal Dublin Society, 1981. On Pepper, see Kenneth Chew and Anthony Wilson, *Victorian science and engineering portrayed in the Illustrated London News*, Stroud, England: Sutton/Science Museum, 1993, pp. 11, 95, 97. For a detailed discussion of the culture of electrical display see Iwan Morus, *Frankenstein's children: Electricity, exhibition and experiment in early-nineteenth century London*, Princeton, NJ: Princeton University Press, 1998; idem, 'Currents from the underworld: Electricity and the technology of display in early Victorian England', *Isis*, 84 (1993), pp. 50–69; idem, 'Telegraphy and the technology of display: The electricians and Samuel Morse', *History of Technology*, 13 (1991), pp. 20–40; and David Gooding, 'In nature's school' in David Gooding and Frank A. J. L. James (eds.), *Faraday rediscovered*, Basingstoke, England: Macmillan, 1985, pp. 105–36. I am grateful to Richard Noakes for pointing out that William Crookes was almost certainly not the target of Thomson's criticism. For details of Henry Pepper's career, see J. A. Secord, 'Quick and magic shaper of science', *Science* 297 (2002), pp. 1648–9.

avowed a divine obligation to maximize efficiency, we might infer that he discouraged qualitative speculation and entertainment because they incurred the expenditure of resources without necessarily solving important technological problems.[13]

A notable irony throughout this book, though, is that measurement techniques borrowed from Thomson were not always sufficient to furnish unproblematic means of quantifying the performance of electrical technology. An injunction to measure in order to acquire a better knowledge about electricity was of itself simply not sufficient for these scientific communities to know how to proceed. There was no single obvious answer for Thomson's contemporaries to the question of what even constituted a measurement. As we shall see in Chapter 2, the execution of a measurement could be construed in three distinct ways that were not self-evidently equivalent. Measuring a quantity could involve a *comparison* with a standard pre-calibrated unit of the same kind until equality or balance was reached. Alternatively, it could involve a *reduction* of the unknown quantity to be determined into (absolute) determinations of length displacement and or mass, with a theory-laden calculation used to produce a final result. Then again, in the mid-1880s, the electrical lighting fraternity rather radically extended the meaning of the term 'measurement' to a high-speed practice that involved neither direct comparison nor simple reduction. Instead, this new approach used complex electromagnetic–mechanical techniques to deflect a needle or light-spot over a certain length of a dial pre-calibrated in the relevant units, so that users could take 'readings' in volts or amperes at an instantaneous glance. This new 'direct-reading' technology embodied the industrialization of measuring instruments. To achieve a faster (if somewhat fallible) result, 'automatic' apparatus replaced both the human labour hitherto required in experimental manipulation and the human skill formerly used in theoretical interpretation of instrumental action. Importantly, Thomson embraced this latter approach in his own electrical engineering work alongside the two more traditional approaches to measurement. Unlike some contemporaries in natural philosophy, Thomson did not publicly attack this new approach as constituting a practice that was *less* than authentic measurement.[14]

There were further questions left open by Thomson's public exhortation to engineers to measure, answers to which required shared commitments to value-laden decisions of measurement practice: *What* should they

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[13] The original electrician on the 1858 Atlantic cable expedition, Wildman Whitehouse, was certainly soon marginalized when he refused to acquiesce in the new regimes of quantitative instrumentation; see Bruce Hunt, 'Scientists, engineers and Wildman Whitehouse: Measurement and credibility in early cable telegraphy', *BJHS*, 29 (1996), pp. 155–69.

[14] See Chapters 1 and 2 of this book, and Graeme Gooday, 'The morals of energy metering: Constructing and deconstructing the precision of the electrical engineer's ammeter and voltmeter', in M. Norton Wise (ed.), *The values of precision*, Princeton, NJ: Princeton University Press, 1995, pp. 239–82.

measure? How should they conduct their measurements? With what sorts of instruments? To what degree of accuracy? How should they judge the reliability of measurements made by others, and how should they interpret the outcome of their own measurement activities? Thomson's 1883 lecture offered little explicit advice on all but the first of these questions. Even on that subject he raised significant ambiguities about what should constitute the working ontology of electrical measurement. Should measurements of electrical current represent metals fundamentally as conductors of electricity or as inclined to resist such conduction? For Thomson the question had a pragmatic 'instrumental' solution. He suggested, for example, that those working on lighting installations ought to measure not the resistance of circuit elements but its reciprocal – conductivity – as that was far more algebraically useful in quantifying the performance of Edison – Swan filament lamps connected in parallel.[15] Thomson's proposal for the 'mho' as a unit of conductivity as the reciprocal of the 'ohm', did not gain wide currency, however until the 20th century. In Chapters 3–4 and 5 we shall see that there were several major debates on the contentious points just raised about measurement practice that not even Thomson's techniques, nor even personal interventions, could resolve.

To be more specific, there were several important difficulties in extending Thomson's quantifying practices and imperatives into the enigmatic new domains of electrical technology. Thomson's support for a plan by the London chemist Augustus Matthiessen to use a stable metal alloy in constructing resistance standards was insufficient to defeat Werner von Siemens' rival arguments for the trustworthiness of mercury (Chapter 3). The legitimacy of extending Thomson's 'mirror' techniques of galvanometry to instruments used for dynamo testing was strongly challenged by the aristocratic mechanical engineer James Swinburne in the 1890s (Chapter 4). The most determined advocates of a Thomsonian agenda to render measurable electrical parameters hitherto difficult or impossible to quantify were his Glasgow protégés, William Ayrton and John Perry, whose fertile collaboration from 1875 to 1889 produced a number of novel instruments, discussed in Chapters 4 and 5. But the attempt of Ayrton and Perry to develop a 'secohmmeter' to give direct-readings of self-induction faltered when even its developers acknowledged that even this commercially significant and undeniably 'real' quantity could not in principle be measured *at all* when the machinery was in motion. This was a distinctly limiting blow to the Thomsonian programme of equating the measurability of electrical phenomena with their 'reality'.

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[15] Thomson, 'Electrical units', pp. 133–4. The 'Siemens' was first proposed as the unit of electrical conductance in 1933 but was not formally adapted as an SI unit until 1971. H. G. Jerrard and D. B. McNeill, *A Dictionary of Scientific Units*, London/New York, 4th edition, 1980, pp. 127–8.

The writ of Thomson's agenda was limited not merely in scope, but also in time. When he stood down from his Glasgow chair in 1899, new preoccupations were already diverting the attention of many researchers and students in physics and natural philosophy away from a narrow programme of measurement. Thomson's successor at Glasgow was his former student Andrew Gray, and we can perhaps gauge something of this broader transition away from measurement from the successive editions of Gray's *Absolute Measurements in Electricity and Magnetism*. In the first edition of 1884, Gray echoed in a celebratory tone a Thomsonian account of how telegraphy had stimulated the interest and expertise in measurement since the 1850s. But in his greatly reworked edition of 1921, Gray complained bitterly that, such was his fellow physicists' obsession with X-rays and radioactivity, they were now only interested in measurements that pertained to the telegraph's ethereal successor:

> ... if it were not for the needs of Wireless Telegraphy, I question whether the theory and practice of absolute measurements would at the present time command serious attention ... As it is, we now have an army of students and others talking glibly of Einstein and of quantum theory, whose attention to the fundamentals of dynamics and physics has been wo[e]fully slight.[16]

Gray nevertheless resigned himself to addressing a large part of his physics textbook to the theory and practice of measuring the constants of coils used in wireless work: A 'difficult and thankless task', he opined melancholically.[17] As one of only a small minority of physicists still interested in electrical measurement per se, Gray thus acknowledged the persistent reliance of this specialist group on the activities of electrical engineers to provide the problems, techniques, instruments, and audiences for their research. Accordingly, I devote most of my attention to those in the engineering community whose work made electrical measurement both possible and important – both before physicists were greatly interested in the subject and indeed after most of them had lost interest in it. In the next section I thus consider the role of physicists and engineers in the early development of standardized technologies for measurement.

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[16] Andrew Gray, *Absolute measurements in electricity and magnetism*, 2nd edition, Glasgow: 1921. Notwithstanding its abbreviated title, this was an updated version of Gray's *The theory and practice of absolute measurement in electricity and magnetism*, Parts 1 and 2 (issued in 3 volumes), London: 1888–1893. Gray published an earlier volume titled *Absolute measurements in electricity and magnetism*, London/Glasgow: 1884, 2nd edition 1889, and in the preface to that Gray presented an upbeat Thomsonian historiography of the telegraphic origins of electrical-measurement practice.

[17] Gray, *Absolute measurements*, 1921, p. v–vi.

1.2. THE METROLOGICAL FALLACY – OR WHAT THE HISTORY OF MEASUREMENT IS NOT

> But more is necessary to complete the science of measurement in any department; and that is the fixing on something absolutely definite as the unit of reckoning . . . The great house of Siemens [has . . .] worked upon this subject in the most thorough and powerful way – the measurement of resistances in terms of the specific resistance of mercury – in such a manner as to give us a standard which shall be reproducible at any time and place, with no other instrument of measurement at hand than the metre measure.
>
> Thomson, 'Electrical Units of Measurement,' 1883[18]

Thomson and his fellow electrotechnologists paid much attention to the topic of metrology. They were undeniably preoccupied with promoting the universal adoption of measurement units and developing material standards in which to embody and reproduce those units at any place and time. In their analyses of nineteenth-century electrical-measurement practice, Schaffer, Hunt, Olesko, and others rightly emphasize the epistemological significance of metrology in attempts to attain objective universal knowledge through measurement. Proper units and standards were considered important means for ensuring – or trying to ensure – that measurement produced universally valid numbers that represented authentic properties of nature or technology in ways unpolluted by material contingency or cultural subjectivity. Accordingly these historians show how electrical standards committees and individuals in both Europe and the USA expended much effort towards such ends from the 1860s until well into the twentieth century. These efforts were directed to arguing for the merits of one system of measurement units against rivals and to securing a definitive form of ultimate standard [*Urmaass*] that reliably embodied fundamental units of electrical measurement and the means of copying it into easily manageable everyday form that could perform consistently at all sites and over the *longue durée*.[19] The general explanandum of such accounts is the way in which any suitably skilled

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[18] Thomson, 'Electrical units', pp. 87, 94.

[19] Simon Schaffer, 'Late Victorian metrology and its instrumentation: A manufactory of ohms', in Robert Bud and Susan E. Cozzens (eds.), *Invisible connections: Instruments, institutions, and science*, Vol. IS09 of the Society of Photo-Optical Instrumentation Engineers (hereafter SPIE) Institute Series, Bellingham, WA: SPIE, pp. 24–55; Kathy Olesko, 'Precision, tolerance, and consensus: Local cultures in German and British resistance standards', Dordrecht, Boston/The Netherlands/London: Kluwer, 1996, pp. 117–56; Bruce Hunt, 'The ohm is where the art is: British telegraph engineers and the development of electrical standards', *Osiris*, 9 (1994), pp. 48–63; Arnold C. Lynch, 'History of the electrical units and early standards', *Proceedings of the Institution of Electrical Engineers* (hereafter *PIEE*), 132A (1985), pp. 564–73, see esp. p. 568; Larry Lagerstrom, 'Universalizing units: The rise and fall of the international electrical congress, 1881–1904', unpublished manuscript. Personal communication, November 1994.

person could use such units and standards to replicate any given measurement result. By implication, this replication would be to within a tolerable degree of uncertainty: Quite what constituted a tolerable degree of uncertainty for the late Victorians will be discussed in my account of the context dependence of 'accuracy' in the next chapter.

Nevertheless, historians of electrical metrology have also highlighted two themes that undercut any simplistic interpretation of it as a pure enterprise of epistemological universalization. With no small irony, they note that putatively universal systems for calibrating measurements drew on very particular culturally embedded values[20] to lend their metrological schemes meaning and legitimacy. Olesko has argued that, when Werner von Siemens promoted a unit of resistance measurement grounded on a metre column of mercury of cross section 1 mm^2 in the 1860s, he did so with a view to its congruence with, and easy integration into, existing length-based metrological schemes in the Germanic states. By contrast, (mostly) British Association (BA) physicists followed the agenda of Wilhelm Weber and William Thomson in articulating all units of electrical measurement in dimensions of mass, length, and time in the 'absolute' universal framework of energy transference articulated in the new thermodynamics. Accordingly they promoted the rival absolute unit of resistance which had the (somewhat counterintuitive) dimension of 'velocity'.[21] Whilst BA lobbyists thus criticized Siemens' resistance unit for its lack of intrinsic connection to other electrical units, the Prussian replied caustically that only a tiny constituency of energy-obsessed physicists needed to use the absolute unit, which was in any case very hard to realize in practice (Chapter 3). Siemens' important point was that most users of such resistance standards worked in the telegraphic sphere and would use them primarily for fault diagnosis and quality control rather than to make laboratory claims about the universal nature of electricity. I extend this theme by showing how similarly *localized* technological purposes stimulated the development of commercial units and standards for current (Chapter 4) and self-induction

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[20] Jan Golinski, *Making natural knowledge*: *Constructivism and the history of science*, Cambridge: Cambridge University Press, 1998, pp. 173–7; Schaffer, 'Late Victorian metrology', p. 27; Olesko, 'Precision, tolerance, and consensus', pp. 117–56; Hunt, 'The ohm is where the art is', pp. 48–63; Lynch, 'History of the electrical units, p. 568.

[21] (The acronyms BA and BAAS refer to the same body: The British Association for the Advancement of Science. They are used interchangeably.) Within the BA's favoured 'electromagnetic' (vis-à-vis electrostatic) system of absolute units, resistance had the dimensions of 'velocity' rather than of length and was thus not straightforward to embody in a permanent material form. In this system the British Association for the Advancement of Science unit figured as 10^9 cm/s and, according to the most recent data available to Thomson in 1883, the Siemens unit was 9.413×10^8 cm/s. For Thomson's elaborate efforts to explain how resistance could be interpreted as velocity see Thomson, 'Electrical units', pp. 97, 130–3. On the unification of physics through considerations of energy, see Gooday, 'Precision measurement', pp. 36–7.

(Chapter 5); the grounding of electrical measurement in absolute energetic considerations became broadly significant *only* with the commercial advent of domestically metered supply in the 1890s (Chapter 6).

Insofar as historians have (rightly) focussed on the importance, problems, and ironies of developing such standards and units, they have written histories of *metrology* – what Thomson called the 'science of measurement'. These are not, however, strictly the same as the histories of measurement, for in a sense 'measurement' encompasses and yet also goes far beyond metrological topics. This differentiation has sometimes been obscured by use of the word 'metrology' as a loose synonym for measurement.[22] More unhelpfully, this elision has perhaps tempted some to come close to committing what I call the 'metrological fallacy'. This is the view that well-defined universal standards and units are somehow necessary and sufficient to facilitate the practice of measurement and thus that the history of measurement consists in explaining how past measurers overcome the lack thereof. Although no single historian has argued explicitly for this position, undertones of it are arguably apparent in some of the attempts to locate the development of standards and units at the forefront of narratives on the history of measurement.[23] I contend, by contrast, that the historiography of measurement cannot be simply about standards of measurement. This is because historical evidence shows that universally agreed standards of measurement are neither necessary nor sufficient for a particular quantification to be judged to be a proper measurement by expert commentators.

First I put the case that units and standards were not of themselves sufficient resources for making reliable electrical measurements. Performing an electrical measurement required much more than having access to a reliable and universal set of standards and units. Also needed were the relevant skills, measuring technologies, and discretionary (decision-laden) practices, and these by no means inevitably accompanied the use of such standards. As Harry Collins argued some time ago, and Otto Sibum has recently reiterated, the mere fact that two different experimenters possess identically calibrated and otherwise standardized apparatus does not generally enable them to attain quantitatively identical results. Much tacit skill and 'gestural knowledge' is also required for accomplishing even similar results, and this can normally be learned only face-to-face from skilled practitioners.[24] Even with such direct-emulation practical knowledge, it is entirely normal to find slightly discrepant results between prima facie identical measurements.

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[22] This is a tendency for which I have been guilty – see Gooday, 'The morals of energy metering', p. 240, where I spoke misleadingly about 'metrological communities'.

[23] Golinski, *Making natural knowledge*, pp. 186–206.

[24] Harry Collins, *Changing order: Replication and induction in scientific practice*, London: Sage, 1985; Otto Sibum, 'Reworking the mechanical value of heat: Instruments of precision and gestures of accuracy in early Victorian England', *Studies in History and Philosophy of Science*, 26 (1995), pp. 73–106.

Indeed, ironically, slight discrepancies between experimenters' results elicit *greater* communal trust than claims for an exact digit-for-digit consonance between them: The latter is widely presumed to be the hallmark of fraud.[25] As Thomas Kuhn pointed out over three decades ago, experimenters habitually *expect* errors to arise; if the spread of results falls within their expectations of unavoidable error, this mild discordance is deemed to lie within the limits of 'reasonable agreement'. By appealing to such canonical standards for reasonable numerical (dis)agreement, non-identical outcomes of the 'same' measurement need not *necessarily* be seen to be in mutual conflict. By recovering the ways in which late Victorian practitioners arrived at and deployed thresholds for what constituted 'sufficient accuracy' or 'sufficient degree of accuracy', we will see in Chapter 2 how they coped with the insufficiency of metrological standards to guarantee agreement between otherwise identical measurements.

The insufficiency of putative standards to bring 'agreement' between measurements is most apparent when the criteria for reasonable agreement require measurements of a reliability or sensitivity that is close to the limits of what can be accomplished at the most sophisticated levels of contemporary practice. At such levels there can be a suspension of trust in innovative practices and techniques that makes the use of even the most precisely constructed standards uncompelling grounds for the acceptance of results. This was notably the case with attempts to standardize readings of current-reading devices and domestic meters during the 1880s and 1890s in which trustworthiness was only cautiously and selectively attributed to new devices (Chapters 4 and 6). The problem was more serious still when there was an outright breakdown of trust between practitioners engaged in measurements who used nominally the same standards. When practitioners did not trust the reports, techniques, or integrity of rivals engaged in measurements who used the same physical standards, the rendering of numerical results became unavoidably partisan. In the 1860s Augustus Matthiessen and Werner von Siemens (as well as their respective allies) repeatedly challenged each others' claims to be able to replicate their respective material resistance standards to within 0.1%. Matthiessen's vitriolic attacks on Siemens' claims regarding the trustworthiness of mercury columns were matched by Siemens' challenges to the trustworthiness of Matthiessen's alloy resistances; the reconciliation of these claims took decades to accomplish.

The use of widely distributed standards was thus insufficient for the consensual conduct of measurement with them. Following the lead given by Shapin and Porter, I contend rather that any discrepancies in the use of standards can become problematic if there is significant distrust among the

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[25] This is arguably an obvious extension of what Thomas Kuhn, dubbed the 'fourth law of thermodynamics'; Thomas Kuhn, 'The function of measurement in modern science' in *The essential tension: Selected studies in scientific tradition and change*, Chicago: University of Chicago Press, 1977, pp. 178–224.

practitioners concerned. Such subtleties of trust in measurement equipment and in those who design, make, or use them have perhaps been understated in previous historical accounts. These subtleties arise from a complex of personal and commercial issues. For example, when individuals were trusted to undertake certain tasks competently and to report them honestly, this was not, by convention, a point that needed to be mentioned explicitly. Moreover, certain kinds of skill and specific dedicated characters seem to have been tacitly accorded particularly high levels of trust in this regard, most notably Lord Rayleigh and his collaborators, Arthur Schuster and Eleanor Sidgwick, in the early 1880s. Nobody contested their conclusion that the hitherto widely used BA 1865 unit was a full 1.3% adrift from its ideal absolute value.[26] And once they had vindicated Werner von Siemens' long-held claim that the resistance of his standard mercury column could be replicated to within 0.1% when 'necessary' precautions were taken, William Thomson at least contended that no further evidence was required for proving the point. Yet, as we shall see in Chapter 3, not all British practitioners accepted that mercury standards could *by themselves* be trusted to provide such a level of reasonable agreement, especially when used by those with rather less expertise than Rayleigh and Sidgwick.[27]

My next point is that universally sanctioned metrological standards were in other contexts not even strictly *necessary* for attaining trustworthy measurement. This was particularly so for everyday transactional purposes in which toleration of 'error' was much greater and or in which the results were merely local in import. The ubiquity of this localism is an important counter to an extreme view of the metrology dependence of society in Keith Ellis's popular treatise *Man and Measurement*. With confident counterfactuality he maintains that if societies did not use widely shared and uniformly regulated standards, everyday commercial transactions would become so contentious and prolonged that civilization would give way to anarchy as starving people took to the streets and rioted.[28] Yet clearly this apocalyptic vision is not borne out by historical evidence. Indeed, as Witold Kula and

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[26] Thomson, 'Electrical units', pp. 93–7, referring to Lord Rayleigh, and Arthur Schuster, 'On the determination of the ohm in absolute measure', *Proceedings of the Royal Society*, 34 (1883), pp. 104–41, and to more recent results announced in 1882 by Lord Rayleigh and Eleanor Sidgwick, published as 'Experiments by the method of LORENZ, for the further determination of the absolute value of the British Association unit of resistance', summarized in *Proceedings of the Royal Society*, 34 (1883), pp. 438–9; the complete version was published in *Philosophical Transactions of the Royal Society*, 174 (1884), pp. 295–322.

[27] One might present this as an alternative to Harry Collins' point that controversies in matters of replication are ended by the fiat of a 'core set' of practitioners exercising sufficient power to decree what must be the definitive answer to a contested research question; Collins, *Changing order*, pp. 142–9. Judging by the response of fellow practitioners in the case of resistance standards, though, this core set did not simply or even necessarily have to be powerful: It had to be *trusted* by those over whom it claimed jurisdiction. See subsequent discussion of the relationship between trust and power.

[28] Keith Ellis, *Man and measurement*, London: Priory, 1973, pp. 1–2.

Sally Dugan[29] have shown, ordinary consumers at different locations have long used their highly localized standards of length and weight and interconverted their quantitative results in commercial transactions as easily as they could exchange foreign currencies – if not always without squabbles, challenges, or occasional fraud. Thus a locally reproducible standard could be *sufficiently* effective for many purposes, irrespective of its relationship to other sorts of measurement for other purposes and quantities. Although its integrity and universality might be challenged, societies did not permanently collapse into disorder as a consequence. If such disruption broke out in the electrical world – because of discordant experiments, failed cables, or extinguished lighting – many other possible causes might be suspected than a slight uncertainty in the value or definition of an electrical standard.

Much electrical activity in the nineteenth century proceeded with a proliferation of localized and fallible standards and units without major disaster ensuing. For example, in Chapter 3 we shall see that, even into the 1870s, telegraph companies still used a variety of different proprietary standards in the testing and laying of landlines. And despite the palpable variations between many resistance coils allegedly representing the *same* unit, submarine cables were tested, laid, and retested without calamitous inefficiencies or financial strain. In his 1883 ICE lecture, Thomson noted that, although Rayleigh and Sidgwick had shown the BA 1864–5 resistance unit to be 1.3% adrift from its absolute value, thousands of commercial coils copied from it were used with considerable efficacy in telegraphy and lighting as all copies agreed *with each other* to within 1/10%.[30] Given also that converting results to alternative systems was a relatively straightforward procedure, ‘arbitrary units’ of electrical resistance in miles of iron or copper wire were effective for many purposes. After all, throughout the period covered by this book, the quantitative value of standards was being contested. Yet the everyday measurement work of telegraph clerks, lighting engineers, and experimental physicists continued almost entirely unperturbed by the endless wrangling among national or international standards committees. The only significant aggravation they endured was when they were obliged to give up their cherished standards and either purchase new forms with fractionally different values or perform extra correctional calculations.

Thomson himself significantly sanctioned a pragmatic pluralism about measurement standards. He advised the assembled engineers at the ICE in 1883 that they could usefully express electrical resistance of a conductor in *either* the BA unit and the Siemens mercury unit. Notwithstanding his vocal advocacy of the absolute BA unit for the preceding two decades, Thomson

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[29] Witold Kula (trans. R. Szreter), *Measures and men*, Princeton, NJ: Princeton University Press, 1986; Sally Dugan, *Measure for measure: Fascinating facts about length, weight, time and temperature*, London: BBC Books, 1993.

[30] Thomson, ‘Electrical Units’, pp. 93–8.

took particular pains to praise the Siemens product. This is not surprising given that, at the Paris international conference for the determination of units in October the previous year, form of the Siemens unit had been adapted to become the material embodiment of the theoretically absolute universal 'ohm' standard.[31] Although this form of standard was technically ratified as an internationally agreed form by all the major industrial nations represented at the meeting, this did not bring the end of metrological pluralism. As O'Connell has noted, this apparent unification in a universal system of measurement was largely rhetorical: Several European nations explicitly avoided upholding this compromise. Whilst France and Germany continued to use the mercury definition without explicit reference to the absolute system, Britain persistently avoided the use of mercury in its metallic standards – preferring a solid metal form instead (Chapter 3). As Lagerstrom has shown, the international committees that adjudicated universal standards of electrical measurement from the 1860s to the 1910s tried repeatedly and rarely with success to enact many diplomatic compromises – especially in the naming of units – to prevent their enterprise disintegrating into nationalistic factionalism.[32]

Given this sustained socio–cultural resistance to universal definitions, the principal explanandum of a history of measurement in the nineteenth century *cannot* be the universal adoption and implementation of a single unified system of units and standards. Whilst many of those involved indeed *aimed* for this universalization, to tell the story of electrical units and standards with the focus on only this narrative theme would be to tell an unacceptably teleological tale directed to explaining what happened only later (if at all) in the twentieth century. Thus, although my approach draws upon the rich extant literature on the history of metrology, I focus on the questions of trust in measurement that are quite distinct from those of defining a standard metrology. What sort of materials, methods, instruments, and people were trustworthy enough to employ in the task of making reproducible quantitative measurements? It is these sorts of quotidian issues in the *practice* of measurement that I shall pursue in later chapters.[33] In the next section I

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[31] Ibid., pp. 93–8.

[32] Lagerstrom, 'Universalizing units'; J. O'Connell, 'Metrology: The creation of universality by the circulation of particulars', *Social Studies of Science*, 23 (1993), pp. 129–73; Golinski, *Making natural knowledge*, pp. 173–77.

[33] Golinski, *Making natural knowledge*, p. 9; Joseph Rouse, *Knowledge and power: Toward a political philosophy of science*, Ithaca, NY/London: Cornell University Press, 1987; Joseph Rouse, *Engaging science: How to understand its practices philosophically*, Ithaca, NY/London: Cornell University Press, 1996; Jan Golinski, 'The theory of practice and the practice of theory', *Isis*, 81 (1990), 492–505; Andrew Pickering (ed.), *Science as practice and culture*, Chicago/London: University of Chicago Press, 1992; Jed. Z. Buchwald (ed.), *Scientific practice: Theories and stories of physics*, Chicago/London: University of Chicago Press, 1995.