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Rethinking 'Classical Physics'

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What is 'classical physics'? Physicists have typically treated it as a useful and unproblematic category to characterize their discipline from Newton until the advent of 'modern physics' in the early twentieth century. But from the historian's point of view, over the last three decades several major interpretive difficulties have become apparent, not least the absence of unequivocal criteria for labelling physicists and their work as 'classical', whether during the nineteenth century or earlier. Some historians have consequently either treated the term as a retrospectively contrived anachronism (such as Olivier Darrigol), or carefully avoided using it in their analyses (such as Jed Buchwald).[1] Nevertheless, current historiographies have not systematically explored the implications of abandoning 'classical physics' as an analytical category. As a result, they arguably overstate the unity of the physics prior to the rise of quantum and relativity theories in the twentieth century. Moreover, many studies into the activities of late nineteenth-century physicists have adopted the perspective of later theoretical developments typically associated with the birth of one type or another of 'modern physics', for example the origins of microphysics and, through special relativity, the history of electrodynamics.[2] This focus on theoretical discontinuities, implicit in the classical/modern distinction, has long diverted attention away from important historical continuities in both experimental practice and the applications of physics. We take these reasons as sufficient motivation for rethinking 'classical physics'.

Our analysis builds upon Richard Staley's recent, systematic attempt (we believe that it is the first such attempt) to enquire historically into the origins of the distinction between 'classical' and 'modern physics'. Staley fruitfully raises the key questions of how and why theoretical physicists invented, accepted, and used a classical/modern dichotomy to represent their practice.[3] But like Russell McCormmach's nostalgic fictional physicist Jakob, we still remain unsure about the meaning of 'classical physics'.[4] We therefore begin by reviewing Staley's contribution, in particular his thesis that 'classical' and 'modern physics' were invented simultaneously–'co-created', in Staley's terminology–by Max Planck at the Solvay conference in 1911.[5] Through an extension of Staley's methodology, we argue instead that the emergence of these notions took place separately over a period that reached as late as the 1930s, and that this process took different forms in different countries. This leads us to regard the apparent unity of 'classical physics' as the post-hoc creation of twentieth century theoretical physicists seeking to consolidate new departures within their discipline. We then explain how this perspective is consistent with present-day physics pedagogy, in which the term 'classical physics' is used to emphasise both continuity and change in theoretical aspects of physics, while making little or no reference to experimental or applied physics.

Our review of Staley's approach reveals the interpretive difficulties associated with maintaining a sharp dichotomy between 'classical' and 'modern physics'. The distinction between 'classical' and 'modern physics' neither helps the historian to draw a contrast between different past approaches to physics, nor to demarcate periods of time that possessed an important unity.[6] In the final three sections, we develop alternative analytical tools to investigate important non-theoretical continuities in the practice of physics between the late-nineteenth and early-twentieth centuries through three illustrative examples: the physics of the ether, the industrial connections of physics, and the national context of French experimental physics. We do not claim that these examples necessarily epitomize physics in the period, nor aim to undermine biographically-focused histories of research traditions in theoretical physics (such as Maxwellian or Hertzian). Instead, we wish to show how the history of physics might be enhanced by identifying and revising latent ahistorical presumptions about the unity of physics presupposed by notions of 'classical' and 'modern physics'.[7]

First, we describe how the identification of the ether as a key feature of classical physics has drawn historians' attention towards its changing metaphysical fortunes during the nineteenth century. We argue that characterising these fortunes in terms of 'belief' and 'non-belief' in the ether distorts the positions taken by practitioners of physics, many of whom took the ether to be irrelevant to their theoretical and/or experimental practice. We draw on historiography employed fruitfully by Jed Buchwald and Andrew Warwick to suggest that a more subtle approach would be to examine the contingent role played by ontological commitments–whether to a form of ether or any other framework–in guiding and grounding the practice of physics.

Second, we explore the connections between physics and industry that are obscured by the theoretical bias of any dichotomy between 'classical' and 'modern physics'. We characterize these connections in terms of an 'industrial nexus' for physics: a significant range of aims, tools, concepts, and expertise were developed symbiotically between physics and industry. Although this applies notably to the domains of electricity and thermodynamics during the nineteenth century, we extend the thesis to relativity and quantum theory during the early twentieth century. From our survey across this period, we reveal close associations between the development of these areas of physics and their practical-commercial applications that further strengthen our emphasis on the disciplinary continuity of ‘industrial physics’.[8]

Finally, we address a particular national approach to physics in the period 1880 to 1930 that likewise resists assimilation into a classical/modern analytical framework. We reveal the longue durée continuity in some characteristic aims and approaches of the discipline of French experimental physics through a synthesis of three comparative case studies of French research, due to Mitchell, Atten and Pestre, and Lelong. Without presupposing any trajectory towards a form of ‘modern physics’, we then explore how these may have changed as a new generation of French physicists began to draw more substantially from the work and approaches of their international peers from the turn of the century onwards. The distinctively anti-metaphysical and heavily empirical character of much French physics, especially in contrast to physics in Britain and Germany, provides further strong grounds to doubt the existence of a monolithic international project for physics in the late nineteenth century, whether labelled ‘classical physics’ or otherwise.[9] We offer this example as an invitation to historians of physics to explore the ways in which other nationally-localized forms of physics also diverged from the model of discontinuous theoretical transition presupposed by a dichotomy between ‘classical’ and ‘modern physics’.

The epithet ‘classical’ and the invention of ‘classical physics’

We begin our review of Staley’s work with a nagging problem in the historical study of classical physics: the difficulty in identifying classical physicists. Historians have struggled to identify them in eras when they ought to have been plentiful, according to common characterisations of ‘classical physics’.[10] For example, in his Metaphysics and Natural Philosophy: the problem of substance in classical physics, Peter Harman employed the term to distinguish ‘the philosophical assumptions of eighteenth- and nineteenth-century physics from the relativistic and indeterministic doctrines of twentieth century or ‘modern physics’.[11] He was forced to reject this distinction as unsuitable because he concluded that the physicists that he studied could not be labelled as ‘classical’. Other historians have reached a similar conclusion. In ironic contrast to McCormmach’s fictional classical physicist Jakob, Thomas Kuhn considered that Max Planck was not at all radical because he initially sought to integrate quantum analysis into an existing thermodynamical framework.[12] Sympathetic to Kuhn’s conclusion, Darrigol has described the diversity of views about Planck’s mooted classicism among supporters and critics of Kuhn’s account. Following Needell, he judged ‘classical’ an unilluminating term for classifying Planck’s work, at least until a highly retrospective interpretation emerged in the 1910s. Unsurprisingly given these difficulties, historians have increasingly abandoned direct reference to ‘classical physics’, and have left it as a term in need of explanation.[13]

This is the point of departure for Staley’s work. In *Einstein’s Generation,* he charts the unexpected complexity in physicists’ divergent uses of the term ‘classical’ between the 1890s and the 1910s. He describes how, until the turn of the century, this term was usually applied to canonical theoretical texts, for example Maxwell’s Treatise on Electricity and Magnetism of 1873.[14] Physicists only began to label discrete domains of physical theory as classical, such as ‘classical mechanics’ and ‘classical thermodynamics’, during the first decade of the twentieth century. This occurred as part of a process to determine methods appropriate for the future development of physics. Unsurprisingly, vehement battles sometimes broke out. Interpretations of ‘classical thermodynamics’ hinged upon particular stances towards atomism; for example, Ludwig Boltzmann argued strongly that the field should be understood to include a statistical hypothesis about the microscopic motion of molecules, and hence he opposed the macroscopic phenomenological form preferred by anti-atomists (such as the early Planck) and proponents of energetics (such as Georg Helm).[15]

Similarly, Staley argues that the perspective of the entire discipline of pre-quantum physics as 'classical' was an attempt by Max Planck, in his address to the 1911 Solvay conference, to articulate a template for future research in 'modern' theoretical physics.[16] This took place soon after physicists, including Einstein, Lorentz, and Planck himself, interpreted Planck's work as implying the quantization of energy.[17] Debates on the status of the equipartition theorem (the theorem that in any mechanical system at thermal equilibrium, on average the kinetic energy is evenly distributed between each degree of freedom) formed the crucial background to these events. Equipartition was the second of Lord Kelvin's 'clouds on the horizon of the dynamical theory of heat and light', and he recommended its rejection altogether. Rayleigh, Jeans, and Einstein were aware of the difficulties in applying the theorem to the specific heats of gases, and consequently regarded their attempts to construct a law of black-body radiation using the theorem as uncertain theoretical departures, rather than unproblematic extensions of an established technique. Although empirically inadequate, during the 1900s the trio regarded laws based on equipartition as possessing clearer theoretical foundations than Planck's law.

Crucially, none of them described the equipartition theorem as 'classical'. To Staley, equipartition only became 'classical' when Planck extended the concept to include statistical mechanics in his Solvay address. Without speculating unduly on whatever Planck may have wished for, Staley seems to interpret him as inventing a tradition in order to endorse the approach of Rayleigh, Jeans and Einstein 'as valued and traditional' on the one hand, whilst reducing 'the search for secure foundations to a contrast between the eras before and after quantum theory' on the other. Indeed Staley insists that 'it is hardly to be doubted that interest in the new quantum theory was considerably heightened as a result.'[18] Staley goes on to imply that Planck's address mobilised a new generation of physicists to fashion a clear trajectory and identity for future theoretical physics when faced with the bewildering results of early quantum physics. This gives rise to Staley's thesis that 'modern physics' and 'classical physics' were co-created.[19]

Whilst we find Staley's analysis of the application of 'classical' to individual branches of physics persuasive, we think that the terms 'classical physics' and 'modern physics' were initially coined separately over a decade later. First, Staley misleadingly identifies Planck's ambiguous invocation of 'classical theory' with 'classical physics'.[20] In his Solvay address, Planck appears to remain concerned with the problems of particular classical branches of physical theory rather than those of 'classical physics' in its entirety: he did not focus explicitly on the demise of a discipline of classical physics. Planck also left open the question of how radically the framework of classical dynamics needed to be modified, and hence did not explicitly proclaim the arrival of 'modern physics':

> The principles of classical mechanics, fructified and extended by electrodynamics, and especially electron theory, have been so satisfactorily confirmed in all those regions of physics to which they had been applied, that it had looked as though even those areas which could only be approached indirectly through statistical forms of consideration would yield to the same principles without essential modification. The development of the kinetic theory of gases seemed to confirm this belief. Today we must say that this hope has proved illusory, and that the framework of classical dynamics appears too

> narrow, even extended through the Lorentz–Einstein principle of relativity, to grasp those phenomena not directly accessible to our crude senses. The first incontestable proof of this view has come through the contradictions opened up between classical theory and observation in the universal laws of the radiation of black bodies.[21]

Our interpretation of Planck's Solvay address is consistent with the views Planck expressed in his Nobel lecture 'The genesis and present state of development of the quantum theory', given upon receipt of the Physics prize in 1918. In this lecture, he did not frame 'classical physics' as a complete domain that had been superseded by the challenges of quantum theory. He emphasized instead the difficulties of accommodating quantum theory into the precepts of classical theories, such as classical electrodynamics and classical mechanics, which he treated as distinct branches of physics. His main concern was that the quantization of energy still proved 'elusive and resistant to all efforts to fit it into the framework of classical theory.'[22]

Neither did the perception that Planck's work on the quanta of radiation carried radical implications lead to the co-creation of 'classical' and 'modern physics' at another time. Niels Bohr considered these implications likely to seem 'monstrous' to a theoretical physicist of the 'older school' and that energy quantization posed a clear and 'pronounced contradiction' to classical theory.[23] When he addressed these issues in his 1922 Nobel prize acceptance speech, he advocated a new approach that broke completely with 'classical physics', but this approach did not yet constitute a 'modern physics':

> It has…been possible to avoid the various difficulties of the electrodynamic theory by introducing concepts borrowed from the so-called quantum theory, which marks a

> complete departure from the ideas that have hitherto been used for the explanation of natural phenomena. This theory was originated by Planck in the year 1900, in his investigations on the law of heat radiation, which, because of its independence of the individual properties of substances, lent itself peculiarly well to a test of the applicability of the laws of classical physics to atomic processes.

At this time the epithet ‘modern’ was still used to refer to specific domains of physics, such as X-rays, radioactivity, and electron theory. Even in the late 1920s there are references to ‘classical physics’ that are not framed as complementary to any new project of ‘modern physics’.[24] The specific and systematic application of the term ‘modern physics’ to quantum and relativistic phenomena did not take place concurrently with the articulation of ‘classical physics’, but some time later.

Second, Staley does not provide evidence to suggest that Solvay conference participants interpreted Planck’s address as setting up a boundary between a ‘classical’ and a ‘modern physics’ or that they returned to their countries and spread this terminology amongst their contemporaries.[25] His argument relies instead on the role of the first Solvay conference in shaping an international agenda for physics. We agree with him that the creation of a new form of conference that concentrated on a single subject–quantum theory–was important in focusing international attention on the specific problems from which ‘modern physics’ emerged, and also with Jungnickel and McCormmach that participants left the conference believing that quantum theory ‘entailed something new and important’. But whilst the beginning of a programme to develop a completely new kind of physics can be retrospectively-identified with the first Solvay conference, we suggest that it took more time to implement (perhaps as long as another two decades) and was only labelled ‘modern physics’ much later.[26]

Finally, on a conceptual level, Staley’s co-creation thesis seeks to explain the origins of an unambiguous present day understanding of ‘classical’ and ‘modern physics’ that maps directly onto the ‘non-quantum/quantum’ theoretical division of physics that emerged following the Solvay Council.[27] But there is no unique understanding of ‘classical’ and ‘modern physics’. Whilst the ‘non-quantum/quantum’ division is a necessary means of achieving the cleavage, it can be applied in different ways to emphasize either continuity or change in the metaphysics or practice of theoretical physics. This results in different versions of ‘classical’ and ‘modern physics’, each with unique weaknesses, ambiguities, or inconsistencies.

Recent textbooks of ‘modern physics’ and its branches routinely assume that the classical/modern distinction represents a sharp historical and conceptual break. They assign a watershed moment in the history of physics to the fin-de-siècle when Planck’s and Einstein’s early work initiated the development of ‘modern physics’, and use this to construct a clear division between groups of theories. Consider the following representative example due to Robert Eisberg:

> Classical physics comprises fields in which the subjects studied are: Newton’s theory of mechanics and the varied phenomena which can be explained in terms of that theory, Maxwell’s theory of electromagnetic phenomena and its applications, thermodynamics, and the kinetic theory of gases. In modern physics the subjects studied are: the theory of relativity and associated phenomena, the quantum theories and quantum phenomena, and,

> in particular, the application of the relativity and quantum theories to the atom and to the nucleus.[28]

Yet whilst claiming that ‘the theories used to explain the phenomena’ with which the fields of modern physics are concerned are ‘startlingly different from the theories that were in existence before 1900’, the same textbook treats these differences as self-evident. Planck’s famous yet modest excursion into quantum analysis the same year is typically assumed to have launched a revolution in the subject. As such, textbooks of modern physics offer no more than an un-illuminating and dubious temporal demarcation to support their classification. Neither can classical physics be treated as encompassing Newtonian mechanics, electrodynamics, and thermodynamics by fiat. Textbooks of classical physics subvert Eisberg’s neat classification by distinguishing between co-existing classical and modern versions of a branch of physics, for example classical and quantum electrodynamics.[29]

Textbook authors who make this finer distinction between typically attempt to emphasise the continuity in theoretical practice between these classical and modern versions.[30] Firstly, they distinguish between ‘classical’ and ‘modern physics’ by subtly identifying the latter with ‘quantum-mechanical’. Passing over metaphysical issues related to quantisation, they then identify mathematical tools shared between ‘modern’, quantum-based branches of physics and their ‘classical’, quantum-free counterparts, then specify mathematically their relevant, mutually-consistent domains of applicability (for example, ‘low/high’ energies or speeds). Hence classical mechanics occupies a central role in the articulation: every other branch of classical physics is established as classical through dependence on mathematical tools that found their initial and canonical applications in classical mechanics (we leave aside the issue of how textbooks of classical physics define ‘classical mechanics’). But how is this consistent with the classical form that can be given to the equations of quantum mechanics, or the status of relativity as part of ‘modern physics’?

We have identified four conflicting present day pedagogical purposes for which ‘classical physics’ is pressed into service. Firstly, to reveal clearly the (familiar) techniques upon which relativity theory and quantum theory are built; secondly, to focus attention on the rejection of Newtonian absolute space-time and Laplacian determinism; thirdly, to isolate the physics of everyday objects from the physics of more extreme realms (the ‘tiny’, ‘huge’, and ‘very fast’), and to prove its adequacy for many experiments and practical applications; and fourthly, to provide an antonym for 20th and 21st century fields of research in physics that define themselves in terms of their rejection of various common-sense assumptions. Much research remains to be done on how these (and possibly other) purposes may have given rise to different notions of ‘classical physics’, how these notions became part of physics orthodoxy around the world, when and how they were invested with explicit content, and their links to particular visions of ‘modern physics’.

The British context is illustrative of the complexity of this task as well as Staley’s over-emphasis on the Solvay conference. The first British attempt to articulate the meaning of ‘classical physics’ seems to have taken place in 1927. This was by the Quaker pacifist and idealist physicist-philosopher Arthur Eddington, Cambridge University’s Plumian Professor of Astronomy, in his Gifford Lectures on the connections between religion and science. These were public lectures at the University of Edinburgh intended by Eddington to explain the results of recent physics to a general audience.[31] His analysis revealed that it remained unclear precisely what constituted ‘classical physics’ (scare quotes indicated the novelty of the term). But

Eddington still left considerable latitude in its content, conceiving of classical physics as vaguely Newtonian whilst permitting great heterogeneity in the nature of energy, light, and electricity:

> I am not sure that the phrase 'classical physics' has ever been closely defined. But the general idea is that the scheme of natural law developed by Newton in the Principia provided a pattern which all subsequent developments might be expected to follow. Within the four corners of the scheme great changes of outlook were possible; the wave-theory of light supplanted the corpuscular theory; heat was changed from substance (caloric) to energy of motion; electricity from continuous fluid to nuclei of strain in the aether. But this was all allowed for in the elasticity of the original scheme.[32]

Although he had abandoned all previous attempts to hang on to the theories of classical physics (in his sense), Eddington referred to elements of continuity from classical to 'modern physics' and identified key surviving concepts, such as 'waves, kinetic energy, and strain.'

In Britain, Eddington's widely reported lectures probably provided an important stimulus to the shaping of 'classical physics' both inside and outside the physics community. His conception of classical physics, and its relationship to 'modern physics', was not the only one available, however. For example, The *Times'* special science report in 1928 interpreted 'classical physics' as deriving from the work of Maxwell, and opposed it to the 'new physics of quanta and radiation.'[33] Furthermore, some were altogether sceptical of the dichotomy, such as the veteran chemist Henry Armstrong. Writing for The Times in 1930, he questioned the neologism of 'classical physics' and demanded an account of the 'modern' in order to make sense of it. He bemoaned the lack of ‘good English’ in describing the results of scientific workers and grumbled that

> …we are no longer allowed to speak of things as old; they must be ‘classical.’ The old physics–which was termed good general physics–is now termed ‘classical physics’. What is the modern? May it not be dangerous to invite comparison with the classical?[34]

Our re-examination of the co-creation thesis and associated historical enquiries in this section lead us to recommend the extension of Staley’s project over a longer period. His identification of a broad range of conceptions of the epithets ‘classical’ and ‘modern’ has raised issues of etymology and semantics that ought to shape the direction and methodology of further study into the origins and adoption of the categories ‘classical physics’ and ‘modern physics’. We agree with Staley that the ‘rich language in which physicists described the grounds from which the new theory departed should by now have demonstrated the very real possibility that physics might never have been described as classical’. Nevertheless, we suggest that this judgement applies not to classical physics, as Staley proposes, ‘in the major sense we now recognise’, but instead as an actors’ category.[35]

We regard this historical contingency in the meaning of ‘classical’ as a means of establishing broad cultural connections between physics and other knowledge-making enterprises. The range of meanings of ‘classical’–ancient, authoritative, perfected, and exemplary–has allowed physicists to use the term ‘classical’ to bypass thorny epistemological questions surrounding the partially discredited canon from the nineteenth century. A particularly significant claim can be deemed simply ‘classical’: neither true nor false and yet somehow partially both, which enables credit to be assigned to outmoded yet discipline-making predecessors. Close analogies are apparent in the disciplines of sociology, economics and even ethology, each of which, like physics, saw mid-twentieth century revisionists recast their subject's nineteenth century origins in terms of a 'classical' canon rather than a falsified precursor.[36]

Similarly for the 'modern', a key question that emerges from Staley's analysis is whether 'modern' refers to a particular period in time or to a particular canon. In the period studied in this chapter, physics considered 'modern' by contemporaries included ether theory in the 1880s, electron theory and the study of X-rays and radioactivity in the 1890s, relativity theory in the 1900s, and quantum-mechanics by the 1920s. Just as for modern art or modern literature, 'modern physics' came to derive its meaning from a particular set of assumptions and values that 'classical' precursors putatively lacked. 'Modern' could also simply refer to the latest work.

Sensitivity to shifts in meaning between languages, contexts, and over time will be invaluable in understanding cases where what was initially considered 'modern' was later re-designated as 'classical', such as one of the young Einstein's favourite texts: A Föppl's Einführung in die Maxwellsche Theorie der Elektrizität, originally published in 1894. This was an introduction to the Maxwellian theory of electricity that drew heavily upon the work of the English Maxwellian and telecommunication theorist Oliver Heaviside. In 1905, a collaborator, Max Abraham, turned Föppl's book into a broader theory of electricity by adding a second volume on the theory of electromagnetic radiation. Abraham described this theory as 'modernische', an epithet that was dropped by fourth German edition published in 1920. But the eighth German edition of 1930 was translated into English two years later as Max Abraham, The classical theory of electricity.[37] Thus we see one instance of how the boundaries between 'classical' and 'modern' physics could be made, unmade and remade.

Based on Staley's work, we have established that the emergence of 'classical physics' is a topic for the historian of the early 20th century (especially the 1920s and 1930s) and not of the 18th or 19th centuries. This raises the question of how to describe the concepts and practices of physicists during the earlier period. We believe that as a descriptive tool, 'classical' ought to be abandoned altogether. Even if carefully defined, the term is so ingrained amongst historians and philosophers of physics that it is liable to lead to confusion, either with actors' uses or other current uses. It also clear from the cases that we and Staley examine that classical/modern distinctions were drawn by those concerned largely with problems of theory and metaphysics, and that this is still the case. Their distinctions tend to downplay the practical activities and applications of physics. Contemporary critique of notions of 'modern physics' based on its narrow focus on physical theory and exclusion of experimentation constituted an implicit denial that quantum (and relativity) theory were sufficiently important to dominate the agenda of physics.[38]

There is one apparent counterexample, however, that merits special attention because historians generally consider it to encapsulate a major concern of many turn-of-the-century physicists. Famously identified by Sir William Thomson in his Baltimore Lectures of 1884 as a cloud on the prospects of the dynamical theory of heat and light, this is the existence and nature of an all-pervading ether (or aether) of space. We now address whether the alleged abandonment of the notion of the ether might be characterized appropriately in terms of a transition from 'classical' to 'modern physics'.[39]

Is ether theory the essence of classical physics?

Conceived in the late nineteenth century as the medium for communicating many of the forces of nature, including light and other forms of radiation, the ether provided an electromagnetic foundation for the absolute space-time framework of Newtonian (and arguably of Maxwellian) physics. The few who understood Maxwell's foundational Treatise on Electricity and Magnetism dedicated themselves to elaborating the theory of the electromagnetic ether as the basis for a unified physics based on the principles of mechanics and hydrodynamics.[40] The supposed failure of Michelson and Morley to detect this ether has been simplistically identified in some naïve accounts as a crucial experiment that enabled the straightforward adoption of the theory of special relativity during the early twentieth century. Hence belief/disbelief in the ether has, on some interpretations, formed the basis of a distinction between 'classical' and 'modern physics'.[41]

As has long been realized, however, the Michelson-Morley experiment of 1887 had no simple epistemic import. Michelson and Morley themselves suspected that their failure to detect ether drift might have been due to errors in their experimental conduct or the inadequacy of their instrumentation, so both of them independently conducted numerous further experiments, Morley over the following five decades. Theoreticians also introduced multiple hypotheses to explain the null result of the Michelson-Morley experiment: Oliver Lodge's notion of 'ether drag', developed during 1889, supposed that motion relative to the ether was difficult to detect because ether clung to matter.[42] Around the turn of the century, the Lorentz-Fitzgerald contraction hypothesis posited the contraction of electrons–and hence of all (electromagnetically-constituted) matter–in the direction of motion. The idea that the Michelson-Morley experiments spelled the end of the ether is a standard retrospective re-evaluation that emerged only after the development of the theory of special relativity. No experiment ever disproved the existence of the electromagnetic ether, even for Albert Einstein. It simply became marginal to the work of most physicists and engineers.[43]

Nevertheless, the enduring influence of the simplistic interpretation of the 'Michelson-Morley' experiments, bolstered by a particular classical/modern dichotomy, has left an unfortunate tacit imprint on ether historiography. In the so-called 'classical' period–in this case pre-Michelson/Morley–historians have typically focused on historical figures with a positive ontological commitment to the notion of an ether. This is the approach of contributors to the collaborative volume Conceptions of Ether, edited by Cantor and Hodge. The subtitle of this work, 'studies in the history of ether theories', epitomizes the way in which contributors sought to examine the diversity of views and past debates on the nature and properties of the ether, rather than to reconstruct less visible, contemporaneous doubts about its ontological status. One contributor, Dan Siegel, asserts boldly that the ether programme of research 'compelled allegiance through the end of the nineteenth century.'[44] Prominent figures who did not share this conviction (inevitably) received only a passing mention, although as Siegel himself notes, Michael Faraday, a major founder of electromagnetic theory, had serious doubts about the ether's existence.

Similarly, in the 'modern' period that supposedly followed Einstein's key publication in 1905 on special relativity (as it later became known; note the ambiguous classical/modern status of the interim period between 1887 and 1905) historians have picked on supposedly irrational individuals who persisted in believing in the ether well after Einstein's major publications on special and general relativity. The idea that they were hanging on unreasonably to 'classical' physics lurks behind this judgment of irrationalism. Foremost among the accused is Oliver Lodge, whose commitment to spiritualism was so closely tied to the ether as the repository of the spirit world that he could not abandon one without the other.[45] More subtle is the case of physicist and historian E. H. Whittaker, whose mid-career conversion to Roman Catholicism is interpreted by some as underpinning his emphasis on the continuities between nineteenth century ether theory and Einstein's theory of the space-time continuum in general relativity.[46]

The neglect of ether sceptics or their unsympathetic treatment, however, are symptoms of deeper challenges posed by this particular reading of 'classical' and 'modern physics' based on beliefs about the ether (although as the primary inspiration for Maxwell's theory of the electromagnetic field, the neglect of Faraday's position by historians indeed constitutes a significant historiographical gap). These challenges are two-fold: first, a sharp epistemological dichotomy between 'belief' and 'non-belief' is not the best way to differentiate past (metaphysical) stances towards the ether; and secondly, contrary to a common presumption of historical accounts of the development of relativity, there was a wide diversity of views concerning the ontological status of the ether.

We aim to show how the subtle historiography of the interaction between belief and theoretical practice developed by Andrew Warwick (building on Buchwald's work) offers a remedy to the first challenge. To meet the second challenge, we suggest how Buchwald and Warwick's historiography might be adapted to illuminate the ether scepticism of non-practitioners, and applied to the historical investigation of alternative theoretical frameworks that found no role for an ether. We offer these techniques as means of re-evaluating the significance of the ether to the practice of physics during the late-nineteenth and early-twentieth century, as it was displaced by an alternative energy-based ontology.[47]

In From Maxwell to Microphysics, Buchwald studied the rise, re-interpretation and (partial) fall of James Clerk Maxwell's theory of electromagnetism in the last third of the nineteenth century. Maxwell's theory, which was first fully elaborated in his 1873 Treatise on Electricity and Magnetism, was premised on the ether as the universal medium for communicating electromagnetic interactions. For a network of Maxwellians across Britain, Europe, and the USA in the 1880s and 1890s, Buchwald found that their theoretical practices did not depend on certain knowledge of its structure. To be sure, at least some of them believed in the ether, and hoped to determine its structure eventually, but this was not their immediate goal, even amongst specialists in electromagnetism.[48]

As Warwick reveals in his book Masters of Theory, even Maxwellians based at the University of Cambridge–where Maxwell spent his final years until his death in 1879–were not specifically dedicated to securing grounds for belief in the ether. The book is a detailed, localized case study of how a cohort of physicists and mathematicians trained in the Cambridge Mathematics Tripos learned to treat the electromagnetic ether as a universal medium of energy transmission that could be subjected to hydrodynamic calculations.[49] Warwick demonstrates how their metaphysical beliefs in the ether were inseparable from a framework of theoretical practice, and describes the processes of pedagogy and research through which students acquired this framework and maintained these beliefs. These Cambridge Maxwellians used the concept of ether to render the mechanisms of electromagnetic interaction intelligible and to give physical meaning to their theoretical practice. When this proved too difficult, they adapted the concept to suit their practice in ways unlicensed by Maxwell's theory.

For example, in his detailed study of the commitment of Cambridge mathematical physicists to Joseph Larmor's post-Maxwellian Electronic Theory of Matter (ETM), Warwick shows how Larmor's development of this theory in the early 1890s lay in his preferred mathematical tools and interests, his most prized tool being the Principle of Least Action.[50] Previously, Larmor had invoked an electromagnetic ether to give physical meaning to the concept of an absolute frame of reference.[51] His reading of a paper by Fitzgerald now convinced him that a rotationally elastic ether, due to MacCullagh, could provide a dynamical foundation for electromagnetic phenomena; crucially, Fitzgerald had manipulated MacCullagh's theory using the Principle of Least Action.[52]

A vigorous correspondence with Fitzgerald eventually convinced Larmor to introduce into his mathematical theory discrete charge carriers, or 'electrons', in order to resolve contradictions arising from its physical interpretation.[53] Warwick describes how this constituted a dramatic innovation. Previously, the electromagnetic ether and matter had largely been kept ontologically distinct, which raised unanswered questions about how the two interacted. The Maxwellians worried, for example, about their inability to provide a physical mechanism for conduction, during which electromagnetic energy (in the ether) was converted into heat (in the conductor).[54] Larmor's mature ETM[55] responded to the challenge by reducing matter to mobile discontinuities in the newly reinterpreted ether as 'a sea' populated 'solely by positive and negative electrons'.[56] The development of Larmor's beliefs about the ether thus depended upon his educationally-conditioned selection of mathematical tools and concepts to resolve theoretical challenges posed by electromagnetic phenomena.[57]

Similar conclusions can be drawn from Warwick's analysis of the educationally-induced ontological commitment to ether of Cambridge mathematical physicists trained in the ETM after the turn of the century. Through Larmor's lectures and such textbooks as James Jean's influential Mathematical Theory of Electricity and Magnetism, Cambridge students learned to apply the theory to problems set as part of the mathematical Tripos and thus for them the ether became an 'ontological reality'.[58] Warwick describes how their specific commitments to both an ether-based stationary reference frame and ether-derived mass were sustained through the application of Larmor's techniques to original problems well into the second decade of the twentieth century. In other words, Cambridge mathematicians were not clinging on irrationally to a discarded ontology, but modifying that ontology to suit the application of well-established mathematical techniques as part of an active research tradition. Warwick's approach is especially persuasive because it also accounts for the subtle differences between Larmor and his students in the priority that they placed on particular ontological commitments within the ETM.[59]

One case in point was Ebenezer Cunningham, a student of Larmor's and later his colleague at St. John's College, Cambridge, who altered the properties of the ether to accommodate developments in the mathematical structure of the ETM.[60] Cunningham had noticed a key property that Larmor had overlooked in his 1900 text Aether and Matter, namely that the Lorentz transformations ensured that Maxwell's equations retained the same form in every frame of reference.[61] Warwick explains that, unlike Larmor, Cunningham interpreted this result as implying the impossibility of detecting the Earth's motion through the ether. Further impressed by the mathematical symmetries revealed by Einstein in his famous paper of 1905 'on the electrodynamics of moving bodies', Cunningham dismissed the notion of a unique frame of reference as meaningless. This prompted him to make a radical revision to Larmor's conception of ether which, according to Warwick retained the ether's ontological purpose while rendering it compatible with Einstein's new mathematical formalism.' Hence minor changes in Cunningham's mathematical practice resulted in major ontological implications.[62]

This account of the changing nature of the ether in the ETM shifts our attention to the dynamics of the formation and development of ontological commitments through theoretical practice. In the remainder of this section, we draw on this historiography to reframe scepticism about the ether in terms of independence from, or involvement in, traditions of broader scientific practice. We firstly explain why a 'sceptical agnosticism' was attractive to informed commentators through the example of the electrical researcher and Conservative Prime Minister Lord Salisbury, whose views have been described by Cantor and Hodge as 'symbolic of the attitudes of many'.[63] We then analyse the role of energy conservation in electrical engineering through the example of one of its leading figures, William Preece, in order to illuminate his dismissal of the ether in favour of energy-based ontologies of electromagnetic action. This points the way towards a symmetrical treatment of nineteenth and early-twentieth century ontologies through their role in licensing specific practices within distinct traditions.

As a Fellow of the Royal Society and Chancellor of the University of Oxford, Lord Salisbury was invited to become the President of the BAAS when it met at Oxford in 1894. Following a narrow defeat in the 1892 general election, Salisbury had found time to pursue his reflections on the problems of contemporary science. This led him to select the atom and the ether as 'two instances of the obscurity that still hangs over problems which the highest scientific intellects have been investigating for several generations'. He expressed a position that might be called 'sceptical agnosticism' regarding the existence of the ether:

> The ether occupies a highly anomalous position in the world of science. It may be described as a half-discovered entity. I dare not use any less pedantic word than entity to designate it, for it would be a great exaggeration of our knowledge if I were to speak of it as a body or even as a substance... For more than two generations the main, if not the only, function of the word ether has been to furnish a nominative case to the verb "to undulate".

Salisbury went on to describe the 'most brilliant' theoretical connection established by Maxwell between the speed of light and 'the multiplier required to change the measure of static or passive electricity into that of dynamic or active electricity', which he considered a 'notable extension' to conceptions of the ether. Alluding to Hertz's illustration of 'the electric vibrations of the ether', he interpreted Maxwell's result as establishing with probability that 'light and the electric impulse' were propagated in the same medium. He nevertheless cautioned that 'the mystery of the ether, though it has been made more fascinating by these discoveries, remains even more inscrutable than before' because the interactions between ether and matter remained unknown. Hence Salisbury concluded: 'of this all-pervading entity we know absolutely nothing except this one fact, that it can be made to undulate.'[64]

In their otherwise sensitive survey of 'opposition to ethers', Cantor and Hodge dismiss Salisbury's opinion as 'uninformed'. Yet Salisbury was an Oxford mathematics graduate, possessed all three editions of Maxwell's Treatise in his personal library, and had published research from his private laboratory.[65] But whilst his views were grounded in empirical familiarity with the electro-technical practice of domestic lighting, power, and telephony,[66] they were 'uninformed' by mathematical practice. This led Salisbury to pose metaphysical questions about the nature of the ether independently of the role it played in mathematical practice, and thereby to reach understandably sceptical conclusions.

In a solely metaphysical context divorced from mathematical practice, Salisbury's complaint that 'even its solitary function of undulating ether performs in an abnormal fashion which has caused infinite perplexity'. By the time Maxwell wrote his Treatise, he had abandoned the attempt to determine the actual physical structure of the ether. He sought instead to explain electromagnetic processes in terms of the flow of energy through the ether, and only assumed that it was capable of storing kinetic and potential energy, and that the total energy was conserved.[67] The physical structure of the ether had proved difficult to elaborate owing to apparently contradictory mechanical requirements: it had to be both a rigid medium to transmit electromagnetic waves, yet also a non-viscous 'jelly' to allow the unimpeded passage of the planets.[68] Put simply, post-Maxwellian ether-theorists were unable to deliver an internally consistent, ether-based ontology. Furthermore, prior to Larmor's introduction of the electron into the ETM, the mechanism of interaction between ether and matter was rarely discussed.[69]

Although there were exceptions like Ambrose Fleming, electrical practitioners did not entertain questions about the nature of the ether because it played no productive role in their daily practice.[70] Consider the case of William Preece, an informal pupil of Faraday, who rose to prominence in the telegraph industry during the 1880s as the Chief Electrician of the UK Post Office. He came into direct conflict with the Maxwellians, who argued at this time that the 'empty' space (i.e. the ether) around the wire governed the speed and character of electromagnetic signals. Preece and other apprenticeship-trained telegraph engineers and electricians focused on electrical activity within the wires and their insulation without recourse to the surrounding space. A major battle over the operation of lightning rods during 1886-8 with the Maxwellian Oliver Lodge exemplifies these differences in practice. Preece insisted that the key factor in constructing effective lighting rods was to maximize their conductivity by minimizing their resistance to rapid lightning discharges. He thought that the rods behaved basically like a wide drainpipe for direct current, and hence that the discharges travelled through them.[71] Lodge argued instead that lightning discharges were very high frequency current oscillations, and that the lightning rod dissipated the destructive energy through the ethereal space around it. He accordingly recommended minimising the rod's coefficient of self-induction. Preece fought back against these kinds of advanced theoretical and metaphysical speculations by emphasizing the tangibility of energy, which engineers measured and manipulated routinely.

> The engineer regards electricity, like heat, light, and sound, as a definite form of energy, something that he can generate and destroy, something that he can play with and utilise, something that he can measure and apply..., something which he can manufacture and sell, and something which the unphilosophic [sic] and ordinary member of society can buy and use.[72]

This energy-based ontology emerged from a framework of physical concepts essential to Preece's electrical practice. For Preece, the engineer's notions of work and power were the keystones to the 'conception of the character of those great sources of power in nature'.[73] His pragmatic, metaphysical reasoning about electricity never strayed from these notions; he explained that 'the definition of energy is capacity for doing work... electricity is something which has a capacity for doing work; [therefore] it is a form of energy'. This parsimonious logic licensed Preece to dismiss laboratory physics as a 'little world' of unintelligible speculation and fictional ethers:

> The physicists–at least some physicists, for it is difficult to find any two physicists that completely agree with one another–regard electricity as a peculiar form of matter pervading all space as well as all substances together with the luminiferous ether which it permeates like a jelly or a sponge... The practical man, with his eye and his mind trained by the stern

> realities of daily experience, on a scale vast with that of the little world of the laboratory, revolts from such wild hypotheses, such unnecessary and inconceivable conceptions, such a travesty of the beautiful simplicity of nature.

Untroubled by the relative motion of the earth through space or the arcane nature of Maxwell's theory, Preece found that the overarching theory of energy conservation provided a sufficient framework to explain all electrical phenomena: 'no single electrical effect can be adduced that is not the result of work done, and is not the equivalent of energy absorbed.'[74] Preece's disgust at the metaphysical extravagance of the ether was echoed in the response of early-twentieth century experimentalists to the ETM, which they found 'overly speculative and irrelevant' to their practice, according to Warwick.[75]

Generalising from Preece's example, we suggest that a fruitful way to investigate alternative metaphysical frameworks to the ether is to consider how they were developed and grounded through unique forms of practice, particularly experimental. In nineteenth-century electrical science, for example, studies in this vein might treat the persistence of electrical fluid theories–either the two-fluid version promoted by John Tyndall or the one-fluid version promoted by some telegraph engineers–in the same way that Warwick treated the persistence of the ETM. During this time there was no widespread metaphysical consensus on the nature of electricity, and so by abandoning a classical/modern dichotomy based on the ether, historians will open up the conceptual space to locate it appropriately amongst the discarded ontologies of late-nineteenth and early twentieth-century science, as well as the rising quantitative ontology of energy physics. This shared much in common with the industrial physics of transforming and inter-converting the powers of nature, to which we now turn.

The disciplinary continuity of industrial physics

The claim that physics and industry shared a deep interdependence is well-worn. Many historians have realised that the growth of industry simultaneously gave rise to, and benefited from, much innovative physics. Bruce Hunt has recently identified and described the connections between thermodynamics and steam power during the eighteenth and early nineteenth centuries, and between electromagnetism, telecommunications, and electrical generation during the late nineteenth and early twentieth centuries. Robert Fox and Anna Guagnini have likewise echoed Hunt's emphasis on the industrical connections of thermal and electrical physics.[76] In this section, we similarly develop, under the notion of 'industrial physics', the theoretical, experimental, and instrumental connections between physics and industry during the late-nineteenth and early-twentieth centuries. Following Hunt, we recommend a synthetic investigation of nineteenth and early twentieth century physics rooted firmly in industrial developments.

As we indicate below, historians still sometimes underestimate the importance and range of the industrial connections of physics (for example, the industrial origin of the problem of black-body radiation). So although we have chosen to focus primarily on the influence of industry on physics, our historiographical proposal is more far-reaching. We suggest that interactions between physics and industry also characterize aspects of relativity and quantum physics, and hence the cross the boundary between a retrospective classical/modern dichotomy and its associated periodization. We propose three specific ways in which industry influenced the nature and trajectory of physics.[77] First, key material resources for the practice of physics were either produced by (or shared with) industry; secondly, industry set some of the key theoretical problems in physics and provided theoretical resources to tackle them; and thirdly, it supplied practical expertise towards the solution of problems in experimental physics.

Victorians publicly endorsed the symbiosis between physics and industry. Even Thomas Huxley, a keen promoter of the efficacy of 'pure science', asserted the common identity of the 'interests' of science and industry in his 1887 jubilee survey of Queen Victoria's reign. He explained that while 'science cannot make a step forward' without, sooner or later, 'opening up new channels for industry,' every industrial advance 'facilitates those experimental investigations, upon which the growth of science depends'.[78]

The Times obituary of Lord Kelvin in December 1907 reported that all Kelvin's scientific enquiries had been pursued with a 'keen eye for practical application'.[79] His career provides a paragon of the symbiosis between physics and industry during the late 19th century and early 20th century. It is representative of the rise of a type of industrial physicist named the 'scientist-engineer' by Sungook Hong. The careers of scientist-engineers offer a means of investigating how the three distinct components of industrial physics that we have identified are entwined. They moved readily between the domains of physics and electrical industry, theorised about the machinery of power and lighting in the lecture theatre and the patent office, and employed electrical apparatus derived from industry in their physical experiments. Prominent scientist-engineers included Ambrose Fleming, Oliver Lodge, Silvanus Thompson, John Hopkinson, and William Ayrton in Britain, Irving Langmuir and Henry Rowland in the United States, Marcel Deprez in France, and Karl Ferdinand Braun in Germany.

While there was considerable heterogeneity in their theoretical practices (the Maxwellians Lodge and Fleming, for example, used ether theory overtly) scientist-engineers all brought crucial expertise to the formulation and solution of the numerous theoretical problems raised by technical or industrial uses of electricity. The research of Lodge, Thomson, and Rowland combined advanced theorizing on electromagnetism with the concurrent patenting of practical applications of this theory in telecommunications technologies.[80] This iterative process often resulted in the extension and refinement of existing theories of physics. Scientist-engineers also shared a common hardware of electrical measuring instruments and experimental devices. The experiments of Maxwellians on Hertzian waves were based on generically similar electromagnetic measurement devices to those employed by Marie and Pierre Curie to detect alpha and beta radiation, and by German physicists at the PTR in Berlin to investigate radiation phenomena in quantum physics.[81]

The careers of Hong's scientist-engineers testify to the emergence of a disciplinary structure in the second half of the nineteenth century that brought natural philosophy into close contact with the electrical engineering of power generation, lighting, and wireless telegraphy. But there are also earlier figures whose work exemplifies the fruitful interactions between practical and theoretical expertise in physics and industry. For example, Hunt has described how Michael Faraday used the phenomenon of retardation in long-distance submarine telegraphy during the 1850s to demonstrate the efficacy of his developing theory of the electrical field. Unlike continental 'action-at-a-distance' theories, Faraday's ideas helped telegraph engineers to reconceptualise telegraph cables as giant capacitors, which enabled them to explain and to some extent to mitigate practical problems of transmission posed by the retardation and distortion of cable signals.[82] In the opposite direction, industrially-acquired practical expertise could also serve as the starting point for physical theorising. Otto Sibum has shown how James Joule's brewery expertise in delicate temperature measurement enabled him to determine a mechanical equivalent of heat in the 1840s. Joule was among those uniquely placed to perform the kind of thermometric work that would later provide compelling evidence for (what Joule conceived as) the exact inter-convertibility of heat and mechanical work. Although doubted at first by William Thomson among others, Joule's conclusions were eventually accepted as evidence in favour of both the dynamical theory of heat and the principle of the conservation of energy.[83]

Other cases enable the individual components of industrial physics to be examined separately. Industrial processes gave rise to the material apparatus for experimental physics, such as the thermometric and electrical equipment of 1880s physics laboratories. For example, techniques of fault-finding in long-distance submarine telegraphy in the 1860s motivated and facilitated the economic production of material standards of electrical resistance. Similarly, the rise of the electrical lighting and power industry generated the ammeters and voltmeters that came to be widely used for high-speed, low-skill physical measurements.[84] According to Huxley, this was how industry had 'largely repaid' its 'heavy debt' to physics. In his 1887 survey, he reflected on the broader dependency of progress in physics on material resources supplied by industry:

> It is a curious speculation to think what would have become of modern physical science if glass and alcohol had not been easily obtainable; and if the gradual perfection of mechanical skill for industrial ends had not enabled investigators to obtain, at comparatively little cost, microscopes, telescopes, and all the exquisitely delicate apparatus for determining weight and measure and for estimating the lapse of time with exactness, which they now command.[85]

Electrical devices found important applications in established branches of physics as well as for investigating radioactivity and quantum phenomena. For example, in 1898 Marie Curie used electromagnets and electromagnetic measurement devices to determine the charge and constitution of the rays emanating from her samples of polonium and radium.[86] Between 1880 and 1883, Thomas Edison and his team investigated how an asymmetrical deposit of carbon on the glass in his patented light bulbs related to their eventual breakdown.[87] Edison's British associate Ambrose Fleming eventually explained the phenomenon in terms of the tendency of the ionized carbon emanating from a hot electrical filament to flow across the evacuated valve in a unique direction, determined by its negative charge. Fleming then used this 'Edison effect' to produce amplifying 'valves' that became crucial parts of radio receivers and early electronic computers. Soon afterwards, Owen Richardson, the British Nobel prize-winning physicist, and Walter Schottky, a senior German physicist employed by Siemens (and Planck's erstwhile doctoral student), made independent demonstrations of the quantum wave behaviour of electrons based on similar thermionic emission phenomena. This was how the culture of experimentation on incandescent light bulbs (along with Crookes vacuum tubes) contributed to both the development of quantum electron physics and to the inception of semiconductor technology.[88]

There are many examples of industrial problems shaping the theoretical agenda of physics, regardless of whether this agenda might be labelled 'classical' or 'modern'. The exemplary case during the nineteenth century was the problem of the efficiency–or rather inefficiency–of the steam engine. The dissipation or 'waste' of useful power posed a pressing economic concern in industrial Britain and France. Smith and Wise have revealed how William Thomson and his brother James drew upon expertise from Glasgow steam shipyards to frame the problem in the context of the general irreversibility of energy processes. Until this point these kind of asymmetrical processes had not been of much concern to nineteenth-century physicists preoccupied with conservation laws. The macroscopic study of the steam engine's inefficiency enabled them to recognise, however, that whilst energy was always conserved, the generation of mechanical work (to propel machinery) invariably led to the running down of the available 'useful' energy in the universe. Over the following decade James and William arrived at a more precise expression for this approximate statement of the second law of thermodynamics.[89]

Electrical technologists involved in large scale projects of electrical lighting and telephonic communication, such as Einstein in his later years, shared the challenges of applying electromagnetic theory to quantifying and representing the cyclical and resonant behaviour of alternating currents in large systems. These theoretical applications became crucial both for long-distance power supply and later for high frequency forms of wireless transmission. Electromagnetic theory itself also benefited. In the United States, the German émigré mathematician-engineer, Charles Steinmetz, brought vectors and complex numbers into regular everyday usage in American electro-technical laboratories and classrooms, and hence into the mainstream activities of physics. Oliver Heaviside's early training in telegraphy framed his interpretation of Maxwell's Treatise on Electricity and Magnetism, which led to two highly productive decades of refinement and re-articulation of Maxwell's theory of electromagnetic waves to deal with the problems of distortion and volume loss in long-distance telephone lines.[90] Although understated by historians of physics, Heaviside tended to treat these projects as integrally inter-related. Indeed, without the stimulus of these practical challenges, why would Heaviside have dedicated so much effort to transforming 'Maxwell's equations' from their original, unwieldy form into one readily usable by his peers?

An unfamiliar instance of the industrial influence on physical theory is the material constitution of magnetism. Familiar for centuries as lodestones and compass needles, the polar attractive and repulsive behaviour of magnets was easy to harness but difficult to explain. Maxwell and his followers offered no theoretical account of the existence of permanent magnets (or magnetisation) because this polar behaviour proved uncongenial to a theory that only addressed the properties of fluctuating magnetic fields. Pierre Duhem judged that the very existence of permanent magnets constituted a conspicuous problem for Maxwell's theory, since it was unable (by itself) to account for the existence of an unvarying magnetism fixed in matter.[91] The widespread use of so-called 'permanent' magnets in alternating current machinery from the 1880s soon revealed the variability of their magnetic strength. Following an interaction with neighbouring magnetic fields, magnets did not return (elastically) to their original state but retained persistent effects from the encounter, a phenomenon labelled at that time as magnetic 'memory'.[92] Matthias Dörries has described how the engineer Alfred Ewing and his Japanese team in Tokyo (and also the physicist Emil Warburg in Berlin) developed a theory of molecular 'hysteresis' to explain how this memory constituted the 'permanence' of magnets.[93] A full theoretical explanation of magnetism emerged through the work of the electrical engineer John Hopkinson and instrument maker Sydney Evershed. While their research ultimately became canonical examples of the physics of molecular magnetism, historical accounts have obscured its industrial origins by focusing instead on the contribution of quantum physics to magnetic theory.[94]

Peter Galison has described how the distinct routes to Einstein and Poincaré's formulations of relativity were embedded in the interconnected material, technological, and epistemological culture of the Patent Office and telegraphic communication.[95] In particular, the problem of simultaneity was initially posed by the growth of transnational railways systems and global telegraphic networks: how could the timing of practical commercial activities at remote locations be synchronised? Galison observes that Einstein was ‘not only surrounded by the technology of co-ordinated clocks’, but also located in one of ‘the great centres for the invention, production and patenting of this burgeoning technology’. Another source of demand for co-ordinated clocks was provided by the electrical power industry, which needed a reliable commercial means of measuring the consumption of electricity. Einstein was the son and nephew of electrical engineers who made domestic electricity meters. These meters incorporated two initially-synchronized clocks running at different speeds, which became a motif in Einstein’s illustrations of the time-dilating effects of near-light-speed travel. Poincaré likewise pursued research into the laws of electromagnetism and the nature of simultaneity in the midst of a vast trans-European effort to synchronize clocks.

Finally, industrial problems and developments gave rise to the problem of black-body radiation addressed in Planck’s early theoretical work on quantum theory. A black body was an idealized object that could absorb all electromagnetic radiation (and hence was black), which made it necessarily the most efficient radiator. David Cahan has described how Planck’s research was undertaken between 1893 and 1901 at the Physikalische-Technische Reichsanstalt in Berlin with the financial support of the German state and the Prussian industrialist Werner von Siemens. This support was premised on the study’s relevance to the design of efficient electrical incandescent lighting, but ended up producing unanticipated insights into thermodynamics. The large-scale institutional resources made available by the vast PTR enterprise provided Planck with the mass of observational data that posed such a challenge to conventional views of radiation. While the observed variation of radiated intensity with frequency agreed well with the predictions of Wilhelm Wien's law of 1896 for high frequencies, this law completely failed for low frequencies. In order to resolve this 'ultra-violet catastrophe', Planck postulated that light was only emitted in discrete rather than continuously-variable quantities. Hence quantum theory initially took root in the industrial pursuit of efficiency prosecuted with resources at that time available only in German state research.[96]

The examples of James and William Thomson, Steinmetz, Heaviside, Ewing, Hopkinson and Evershed, Einstein, and Planck–figures associated with 'classical' and 'modern physics' alike–serve to establish the ongoing influence of industrial concerns in both posing theoretical problems and resolving them through new techniques. In our final section, we use the example of French experimental physics during the same period to exhibit the fertility of a disciplinary approach to the history of physics based on the continuity of experimental practice. We show that the temporal continuity of French physicists' distinctive experimental practices undermines first, the radical disjuncture in late nineteenth and early twentieth century physics implied by a classical/modern dichotomy, and secondly, the trans-national unity presupposed by the notion of 'classical physics'.

French physics and the continuity of experimental practice

Science in France from the late-nineteenth to the early-twentieth centuries has long posed historiographical challenges that undermine the notion of a unified body of knowledge that might be labelled as 'classical'. Many historians have recognised that the activities of French scientists during the nineteenth century, to quote Elisabeth Garber, do not 'easily fall into line with the work of scientists in the same fields in either Britain or Germany.'[97] Robert Fox, Mary Jo Nye, and Dominique Pestre have all suggested that French scientific research evinced a distinctive style that was responsible for the choices of many French scientists that, as Nye provocatively claims, 'placed them outside what was to become the mainstream of research'.[98] They agree that, whilst there was not a decline in standards in an absolute sense, France's comparative position amongst scientific nations worsened. One of us has summarised (and implicitly endorsed) this consensus as the claim that 'French physics only appeared to decline because the French national style lost out during the transition from 'classical' to 'modern physics''. Hence French physics has been evaluated unhelpfully through the prism of an emerging 'modern physics' to which it supposedly failed to contribute.[99]

The historiographical problems posed by nineteenth-century French physics therefore derive at least in part from historians' implicit acceptance of a transition from 'classical' to 'modern physics' that apparently left France 'behind'. As we mentioned in our introduction, this has led historians to focus on the development of key theoretical aspects of 'modern physics' rather than on the experimental interests, aims, and approaches of late nineteenth and early twentieth century physicists that were sustained throughout the imagined transition. Their historiography represents the interests and objectives of physicists during the period as orientated towards theory. Hence the relative lack of recent historical interest in physics in France is explicable because, unlike physics in Britain and Germany, it did not experience the emergence of specialisms loosely defined by theory, but instead remained dominated by experiment until well into the twentieth century.[100]

In this section, we offer a critical synthesis of three case studies, due to Mitchell, Atten and Pestre, and Lelong, in terms of the continuity of one key aspect of experimental practice in French physics. Each of these studies compares French with British or German research into the same physical phenomenon: either electrocapillarity, electromagnetic waves, or x-rays. We reveal striking under-acknowledged differences in approaches to experiment, and in interpretations of the relationship between experiment and theory, that would be obscured by any concept of 'classical physics'. Our synthesis reveals continuities in French physicists' distinctive, shared experimental aims and approaches for areas of physics that might fall retrospectively on either side of a classical/modern divide (as in the previous section). We thereby identify a national context for physics for which this periodization is particularly inappropriate.[101]

Although our historiography obviously draws directly from the theme of a French 'national style' articulated clearly by Fox, Nye, Garber, and Pestre, there are three main reasons why we have chosen instead to talk about 'shared approaches'. First, it is unlikely that all of the elements of a national style will be present in a single research project. Faced with this challenge, the pluralist notion of 'shared approaches' grants the historian more flexibility. For example, it might be useful to separate approaches to measurement from approaches to experiment. Secondly, although the community under study may mostly be located in a single country, this need not be the case for all its members. Indeed, in one of our case studies, we treat two Swiss experimentalists as 'French experimental physicists'. And the reverse also applies: we would not expect every French experimental physicist to fit the mould we describe, nor to have adopted all possible shared approaches in a given research context. Finally, a 'national style' is liable to be inferred from scientific products rather than practices. Adopting the notion of a 'shared approach' directs attention towards practitioners' shared practices and goals.[102]

Following the demise of Laplacian physics in the first few decades of the nineteenth century, (the attempt to explain all phenomena in terms of short-range molecular forces), a nationally distinctive institutional and intellectual division between mathematics and experimental physics became widespread in France. Until the 1930s this remained barely affected by the rise of late nineteenth century specialisation in theoretical physics in Britain and Germany (see below).[103] Work in French physics with an explicit theoretical orientation was typically undertaken either by mathematicians holding chairs of mathematical physics, such as Henri Poincaré, or outside a university setting altogether. In the post-Laplacian period, French experimental physicists ('physiciens') believed that the fundamental nature of phenomena could never be known, and hence that natural knowledge should be based directly on observation and precise measurement. They aimed primarily to discover empirical laws, which they expressed in simple mathematical relationships between macroscopic variables.[104]

Programmatic statements to this effect are easy to find. In a lecture on the nature of electricity given at the Collège de France, the professor of physics of the Ecole Normale, Pierre Bertin-Mourot, described the 'true domain of experimental physics' as 'the attentive observation of phenomena and the experimental investigation of their laws'.[105] The founder of the French Physical Society, Jean-Charles d'Almeida, likewise claimed in his Cours de Physique (co-authored with Augustin Boutan) that 'the principal goal followed by the scientist in his research is the discovery of general laws relating phenomena'.[106] Similarly, Josep Simon's detailed study of Adolphe Ganot's textbooks reveals a pervasive focus on laws in the context of what Simon labels as an 'experimental and instrumental inductivist' approach.[107]

Unlike their British and German counterparts, French experimental physicists drew a sharp conceptual separation between facts and laws (supposedly) induced from experiment, and explanations or interpretations. These consisted of attempts to synthesise (descriptive) observations, and they were often framed as provisional because it was possible that these observations might be consistent with many different, mutually-incompatible syntheses. Furthermore, causal explanations that required commitment to a specific ontology were considered sufficiently uncertain to be labelled ‘hypotheses’. D’Almeida and Boutan summarised a widespread French consensus on atoms:

> The simplest interpretation of the laws that govern chemical combinations would lead us to suppose that bodies consist of indivisible particles or atoms that withstand all chemical and physical attempts to split them. But this is purely a mental projection that is impossible to actually demonstrate, and against which we should be careful not to attribute the same degree of truth to a fact or a physical law.

Ganot likewise demonstrated sensitivity to the inferior ontological status of ‘hypotheses’, particularly about the nature of electricity. In fact, he railed against causal fluid-based theories of physical phenomena in general, whereas d’Almeida and Boutan merely observed and regretted that the fundamental experimental laws of electrostatics unavoidably expressed some form of fluid hypothesis.[108]

Whilst the role of ‘hypotheses’ in motivating and guiding research in both French experimental and mathematical physics merits closer examination, it seems that imponderable fluids, micro-molecular mechanisms, and other such inventions were usually invoked for heuristic purposes. Nearly fifty years later, a French inspector of public instruction and a researcher at the physical research laboratory at the Sorbonne, Lucien Poincaré, dealt with the ontological status of the ether on exactly this basis. For him, it was not necessary to know whether the ether had an objective existence in order to ‘utilize’ it because ‘[i]n its ideal properties we find the means of determining the form of equations which are valid.’ He then went on to claim that 'for scholars free from "all metaphysical prepossession", that was the essential point.'[109]

An influential exemplar of the French search for experimental laws is Gabriel Lippmann's doctoral study of electrocapillarity. This was the name given by Lippmann to the phenomenon of the variation in the surface tension of mercury with the electrical charge at its surface.[110] Lippmann claimed to have established a simple relationship between these variables by measuring the variation in height of a mercury column in a capillary tube (which was directly proportional to the surface tension) with an applied voltage at its surface. His research gained a hugely positive reception amongst French contemporaries. Lippmann's French biographers also praised electrocapillarity, describing it as 'a masterstroke' and 'one of his principal glorious achievements' because it fully realized the French law-based approach to experimental physics. One of them, his friend and later a fellow professor of physics at the Sorbonne, Edmond Bouty, framed the achievement in terms of distinctive national characteristics. Bouty recalled that when he had first read a paper that Lippmann had published on electrocapillarity in the German periodical Poggendorff's Annalen, he had initially been 'fooled by the name of the author' and had told himself: 'here is a German who possess all the unique qualities of our race: he deserves to be French.'[111]

Lippmann's findings directly challenged the earlier work of the German physics professor Georg Hermann Quincke. The approaches of the two investigators were diametrically opposed. Whilst attempting to measure the surface tension of various mercury interfaces, Quincke had observed small random perturbations in the height of a mercury column and a gradual decrease in this height over time. He identified the cause as the contamination of the mercury with invisible impurities. Lippmann claimed, however, that the perturbations disappeared whenever the mercury formed part of a closed circuit, and suggested that Quincke had failed to observe the disappearance because all his experiments were performed with an open circuit. The disappearance of the perturbations allowed Lippmann to assign precise values of surface tension and charge to the mercury interface. In the introduction to his thesis, which was published in the Annales de Chimie et de Physique in 1875, Lippmann totally rejected Quincke's causal approach by signalling explicitly how premature 'explanations' impaired the search for laws:

> We would have undoubtedly thought of relating these two physical properties of contact surface to each other, [the electrical charge] and surface tension, and of seeking a fixed relationship between them, if we were not so used to considering this latter quantity as a variable, and explaining its variations by the presence of invisible impurities.[112]

Quincke, on the other hand, interpreted Lippmann' s work in the context of his own search for the micro-molecular causes of a much wider range of phenomena generated by mercury interfaces. When he read Lippmann's Annalen paper, he latched onto the alternative interpretation that Lippmann had given for the open-circuit behaviour of the mercury column, in terms of the gradual loss of charge from the mercury surface. Instead of attempting to directly refute Lippmann's law, Quincke designed experiments to challenge his interpretation. By attributing any discrepancies in their observations to the contamination of the mercury by invisible impurities, Quincke reasserted his own causal hypothesis.

Lippmann responded by retreating to epistemological territory that his French contemporaries found more certain. He simply dropped all such 'explanations' entirely and boasted in his thesis that 'no hypotheses...have been invoked... [in] the present work; it was in order not to introduce them that I refrained from giving a physical theory, [that is to say] an explanation of the properties that have been observed.'[113] In other words, he This enabled Lippmann to dismiss Quincke's criticism entirely in a review of the German experimenter's latest research for the Journal de Physique. He based his evaluation on the French pursuit of experimental laws, and hence judged the wide variation in conditions investigated by Quincke to be an incoherent way of proceeding. Lippmann's victory in France was total. Quincke's work was rarely, if ever, discussed alongside his own in French textbooks.[114]

Michel Atten and Dominique Pestre have described a similar polarisation between the responses of French experimental physicists and British Maxwellians to Hertz's 1887 researches on the transmission of electromagnetic waves. As with elsewhere in Europe, the importance of Hertz's work was recognised swiftly in France. The French Academy of Sciences awarded him its prestigious Lacaze prize following successful public replications by Jules Joubert and Guillebot de Nerville at the 1889 Electrical Congress in Paris. Two members of the Society of Physics and Natural History of Geneva, Édouard Sarasin and Lucien de la Rive, were among those who followed up on Hertz's experiments. First, they sought to create a stationary electromagnetic wave in air through the interference of an incident wave with its reflection (as in optics). They then moved on to investigate 'the regularity and the stability of the [standing wave] phenomenon… by varying as many parameters as possible.'[115] These included the sizes of the wave generator (primary oscillator) and the wave detector (circular resonator).

According to Atten and Pestre, the Swiss pair claimed to have employed a 'purely experimental logic by avoiding calculation and any reference to theory'.[116] To their surprise, de la Rive and Sarasin discovered that resonators of different dimensions all respond to the oscillator, albeit with different intensities that fall off with difference from the primary oscillatory frequency. By expressing this discovery in terms of experimental laws, and by respecting the epistemic primacy of these laws over their tentative interpretations, de la Rive and Sarasin followed two key 'French' approaches to physics. They supplemented the qualitative law that 'the distance between the nodes only depends on the detector and is independent of the generator' with other quantitative laws, such as 'the distance between the nodes for a circular resonator is noticeably proportional to its diameter.' De la Rive and Sarasin then offered an interpretation in terms of the 'selection' of one wavelength by the detector from the multiple wavelengths or spectral band emitted by the generator. They named this phenomenon 'multiple resonance'.[117]

By assuming that the generator emitted a unique frequency, Hertz had shown that his measurements of the distances between the zeroes (nodes) of the stationary wave produced a speed of propagation equivalent to the speed of light. Multiple resonance provoked scepticism in France about this conclusion, which Alfred Cornu voiced cogently in a commentary that followed his presentation of de la Rive and Sarasin's findings before the French Academy of Sciences on 13th January 1890. Whereas Maxwellians such as Fitzgerald and Lodge had enthusiastically embraced Hertz's work as a triumphant confirmation of Maxwell's electromagnetic theory of light, Cornu took the experimental laws established by de la Rive and Sarasin to undermine Hertz's calculations of the speed of propagation of the waves. He offered this implication as a cautionary tale to the predominantly British Maxwellians about the proper way of doing science: 'you'll see that it's very prudent to go about things in the manner of MM. Sarasin and de la Rive... [by] scrutinising carefully the interesting experimental method devised by M. Hertz before thinking about presenting it as a demonstration of the identity of electricity and light.'[118]

Unsurprisingly, the Maxwellians did not draw the same moral as Cornu. Atten and Pestre describe how the search for a decisive experimental test between Maxwell's theory and its action-at-a-distance counterparts led Fitzgerald and his colleague at University College Dublin, Frederick Trouton, to improve (in their view) upon Hertz's experimental set-up, basically by employing a fixed detector and a mobile mirror. Their 'replications' sought to confirm Maxwell's theory rather than to establish and interpret experimental laws. In a short commentary published in Nature on 30th January 1890, Trouton claimed to have already discovered 'multiple resonance'. To safeguard Hertz's calculation of the speed of electromagnetic waves, he contended that Hertz's choice of wave generator ensured that the detector registered the 'central' frequency of the spectral band. In other words, Trouton avoided Cornu's sceptical conclusion by committing to an alternative and more favourable theoretical interpretation of de la Rive and Sarasin's experimental findings.[119]

Even when members of the French Physical Society debated the nature of X-rays the epistemic boundary between 'facts' and explanatory 'hypotheses' demarcated by d'Almeida and Boutan remained remarkably stable. The debates focused on the puzzling experimental 'fact' that X-rays could discharge an insulated electrical conductor without contact. This was demonstrated in France in 1896 by Paul Langevin and Jean Perrin at the Ecole Normale and Louis Benoist and Dragomir Hurmuzescu at the Physical Research Laboratory of the Faculty of Sciences (of which Lippmann was Director). The two teams proposed contradictory mechanisms for the discharge (Perrin favoured ionization of the gas, Benoist molecular convection) but, in Benoit Lelong's judgement, 'the protagonists shared a common implicit definition of the boundary between facts and hypotheses. Atoms, ions, and molecules were not experimental facts. "Facts" were in fact mathematical laws between observable parameters. Atomist vocabulary was implicitly forbidden in factual statements, but could be used in interpretations.'[120] Lelong stresses how even Perrin, 'the future champion of molecular reality', had to take pains to reassure readers that statements about 'electric charges' presupposed no hypotheses, and even then Perrin only introduced hypotheses and theories to explain facts and laws.[121]

No such Gallic boundary between facts and hypotheses was recognized by members of Cavendish group at the University of Cambridge working under J. J. Thomson from 1884. They too were investigating the discharge of electricity by X-rays, but with strong ontological commitments to atomism; as Lelong puts it, 'in Cambridge experimental facts were statements about ions.'[122] Unlike Perrin, who resisted the temptation to mobilise experimental facts in support of theories, Thomson had no metaphysical scruples about using Perrin's experimental research for this purpose. He shared the view with Perrin that moving ions created by X-rays were responsible for the conductivity of gases. The Cambridge transformation of Perrin's tentative 'interpretations' into 'factual' knowledge provided an important step towards establishing Thomson's theories. Conversely, when Langevin, who studied the phenomenon of the conductivity of gases at the Cavendish between October 1897 and June 1898, presented Thomson's ideas before the French Physical Society, he was careful to purge them of 'hypotheses' by reformulating Thomson's statements about ions in terms of moving charges.[123]

Our synthesis of these three case studies of French research–into electrocapillarity, electromagnetic waves, and X-rays–reveals that, unlike their British and German counterparts, French experimental physicists typically sought to research experimental laws and approached explanations, especially those based on specific (often micro-molecular) ontologies, with greater epistemological caution. This distinctive difference in the perceived relationship between theory and experiment further undermines the notion of a unified 'classical' approach to physics during the late nineteenth century. This finding leaves open the question of the nature and extent of the transformations undergone by French physics during the first few decades of the twentieth century, and how best to describe them without presupposing some kind of trajectory towards 'modern physics'.

Dominique Pestre's detailed study of physicists and physics in France between 1918 and 1940 suggests that the characteristic aims and approaches exemplified in this section remained recognizable until at least the 1930s. He describes how French textbooks of physics retained a 'historico-inductive' form inherited from the mid-nineteenth century, which consisted of a logical reconstruction of the historical sequence of discoveries and the ways of thinking that resulted in laws of nature. This narrative choice, according to Pestre, was consistent with a view of science that progresses from an ever-increasing stock of immutable facts, 'a solid block, definitively established'. Pestre also explains that the same textbooks integrated new theories and techniques into a Gallic division of physics that was well-established by the mid-nineteenth century: thermodynamics, optics, electricity, and mechanics/acoustics. From the point of view of a British or German commentator, they provided no systematic treatment of the study of matter (for example atomic and nuclear structure) or the emerging quantum physics.

Moreover, Pestre reveals that the handful of French chairs in theoretical physics were often occupied by experimentalists and that only a small percentage of physics students took up the few programmes offered in theoretical physics. In Paris, a Chair of Theoretical and Celestial Physics was created in 1920 following the retirement of Joseph Boussinesq from the Chair of Probability Theory and Mathematical Physics. Of its three occupants until 1937, none of them pursued research in theoretical physics, and only one of them, Eugène Bloch, offered a course in the subject (on quantum theory). In the Provinces, only Edmond Bauer occupied a post in theoretical physics (a lectureship at Strasbourg). Duhem was never replaced as Professor of Theoretical Physics at Bordeaux following his death during the First World War. Between 1918 and 1928, Pestre identifies Louis de Broglie and Léon Brillouin as the only theoretical physicists in France. So it appears that the distinctive institutional characteristics of French physics were reformed quite gradually during the first half of the twentieth century.[124]

Nonetheless, French experimental physicists were beginning to soften their hardline epistemological stance against explanatory theories and to diversify their aims and approaches in response to broader international developments. As Lelong has shown, although Perrin and Langevin reluctantly addressed their French colleagues from the viewpoint of a framework that privileged experimental facts and laws over micro-molecular explanations, their work ultimately helped to revise it. Perrin's early twentieth century experimental proofs of the existence of atoms, for example, are not easily explicable with reference to Pestre's account of early twentieth century French physics.[125] And from 1903 onward, when Langevin was appointed professor at the Collège de France, he built a research school in 'a new subfield of experimental microphysics'. Once scientists connected to Langevin (and the Curies) and sympathetic to his approach took over and transformed the editorship of the journal Le Radium in 1905, young researchers favourably disposed to ionic physics, such as Paul Villard, found a ready outlet for publication.[126]

To reach a balanced picture of twentieth-century French experimental physics, it will be important to weigh studies of established traditions against the emergence of new ones. Indeed, this will determine whether 'French experimental physics' remains a useful analytical category for historians of physics. It would be misleading to only examine the impact of key turn-of-the-century discoveries and theoretical developments, even if these are likely sources of change, because French experimental physicists often favoured dedicating time and resources to quite different research topics than their foreign counterparts. For this reason, it will be important to identify carefully those areas of physics in which French physicists were actively engaged in research. This would help to guard against inappropriately privileging say, radioactivity over optical metrology, or cathode-ray discharge over the standardisation of electrical units.

Jed Buchwald and Sungook Hong have identified six areas with which physicists throughout Europe and America, as well as Japan, were particularly concerned circa 1900: the nature of X-rays; the character and behaviour of electrons; the properties of the ether; the statistical description of gases; liquids, and solids; the phenomenon of radioactivity; and the long wavelength regime of electromagnetic waves. These only partially overlap with those areas of physics that Lucien Poincaré identified in 1906 as belonging to a distinctively French version of 'modern physics' (and which a modern-day physicist may be tempted to label as 'classical'). In a book aimed at a wide audience entitled La physique moderne, son évolution, he included sections on precision measurement and metrology, and experimental research into the statics of fluids, and omitted kinetic theory in favour of a section on physical principles.[127] This selection reveals a clear bias towards areas of physics for which the discovery of laws and the production of experimental 'facts' were particularly integral, and to which French researchers had recently made key contributions.

For each area of French physics, we recommend tracking continuity or change in the epistemological status of facts and laws in comparison to causal explanations. Established traditions may have encountered new or minority approaches, whereas established approaches may have been applied to new areas of enquiry. Research into piezoelectricity and radioactivity offer illustrative examples. During the nineteenth century, French experimental physicists took great interest in the macroscopic manifestations of the conversion of energy from one form to another–so called ‘complex macroscopic effects’, for example magneto-optical, electro-mechanical, thermo-electric, and so on. Shaul Katzir has described how, early in their career, Pierre and his brother Jacques discovered and investigated piezoelectricity–‘the relations between elastic forces and electric fields in crystals’. In typical French fashion, they pursued ‘systematic quantitative experiments to reveal the rules that governed the development of charge by pressure.’ Yet the brothers also constructed a micro-molecular mechanical theory based on William Thomson’s hypothesis of permanent electrical polarization within bodies, which explained the production of electricity in both pyro- and piezoelectricity in terms of ‘mechanical changes of distance between polarized molecules’, and defended it from criticism.[128]

In contrast, Pierre Curie and André Debierne pursued investigations of radioactivity in line with the conservative French law-based approach exemplified by the three case studies discussed in this section. Marjorie Malley has argued that their unwillingness to commit to a specific theory hampered their research. Unlike Rutherford, who pursued fully the experimental consequences of his hypothesis of a material ‘emanation’ from radioactive substances, Curie drew upon thermodynamical analogies based on the transfer of energy to avoid commitment to specific ontological hypotheses. During investigations into the secondary activity excited upon other substances by the radioactive emanation from radium, Curie and Debierne offered only cautious, general speculation on the excited activity and the nature of this emanation (‘a radioactive gas’). Instead, they focused on establishing observable properties of the excited activity, such as its quantitative variation under different circumstances (radium as a solid or in solution, in a closed glass vessel or the open air). As far as Rutherford’s transmutation theory of radioactivity was concerned, ‘they thought he had strayed beyond the established facts by endowing his working hypothesis with the status of reality’. [129]

Our synthesis of studies into late nineteenth century French experimental physics could be extended to include those that tackle other distinctive French approaches. Studies into Lippmann’s determination of the ohm by Mitchell and into piezoelectricity by Katzir indicate that we could have drawn similar conclusions about the distinctiveness of late nineteenth century French experimental physics by focusing on an experimental approach adopted by Henri Victor Regnault. Regnault aimed to secure a purely empirical foundation for science through the ‘direct’ measurement of physical quantities, which entailed eliminating unwanted physical effects through the experimental design, rather than by (subsequently) correcting measurements using theory. An important buttress for this approach fell away as soon as French experimental physicists began to commit themselves to specific ontologies and lose their aversion towards theory.[130]

In this way, local studies of individuals or research schools might be related to broader trends in physics. We would expect further comparative studies in the mould of Lelong to reveal other routes apart from Cambridge ion physics through which new experimental aims, approaches, and results were introduced into France, and how these were transformed in the process. On the other hand, we would not be surprised to discover, at least until an older generation died out, the persistence of the shared approaches described in this section until the 1920s or even 30s, especially in areas of traditional French strength, for example optics, statics of fluids, and precision measurement. Perrin’s biographer Mary Jo Nye follows one of his students in portraying him as ‘a figure of compromise between differing viewpoints and traditions’, one of which is the mainstream French tradition described in this section. Similar remarks also apply to

Langevin, who occupied an intermediate position between Cambridge and Parisian physics, according to Lelong. The coexistence of the old and the new, whether in France or elsewhere, would demonstrate clearly the redundancy of a sharp dichotomy between ‘classical’ and ‘modern physics’.[131]

Conclusion

So what is ‘classical physics’? It cannot refer to a discipline as practised and understood during the nineteenth century since no concept of ‘classical physics’ gained general currency until the early decades of the twentieth century. We suggest instead that the notion was developed by theoreticians during this later period who sought to preserve a restricted role for established theory and techniques whilst setting forth a future research programme based on new forms of theorizing. It is only in this limited sense that classical physics ever existed. Any references to ‘classical physics’ prior to 1900, therefore, implicitly adopt an anachronistic perspective that was created to legitimize the new foundations for physics proposed within relativity and quantum theory.[132]

As an antidote to this anachronism, we showcased three more fruitful historiographies of late-nineteenth and early-twentieth century physics. Firstly, following our careful examination of the work of Buchwald and Warwick, we proposed that questions about the ontological status of the ether, whether before or after the Michelson-Morley experiment, should be reframed in terms of its role in sustaining research practices. This focus seems appropriate for studying other discarded theoretical entities during the late-nineteenth and early-twentieth centuries. Secondly, we complemented existing historiographies of research traditions in physics by introducing the notion of ‘industrial physics’ to capture the intimate connection between industry and physical theorising, experimentation, and instrumentation and apparatus during the same timeframe. We anticipate similarly important connections between physics and chemistry, medicine, geography, and astronomy. Finally, the case of French experimental physics demonstrates that whilst there were clear longue durée continuities in physicists' aims and their laboratory practice, these might vary considerably between countries and sites. This undermines any lingering hope of retrieving a transnational 'classical physics'. An especially important task for historians is establishing the importance of such continuities in ensuring either the coherence or fragmentation of physics as a discipline. In order to ascertain the extent of French engagement with British and German approaches to physics during the early nineteenth century, we suggested investigating the changing epistemological status of facts and laws in comparison to causal explanations and (often micro-molecular) hypotheses within French physics.

In summary, this chapter has contributed to two distinct projects: to investigate how and why physicists invented, accepted, and used a classical/modern dichotomy in physics, and to develop alternative historical tools to reveal important continuities in the practice of physics between the late-nineteenth and the early-twentieth centuries. Our criticism of Staley's thesis that 'classical' and 'modern physics' were co-created in 1911 should not distract from the methodological importance of his analysis. Nonetheless, we do not subscribe to Staley's (or any other) claim that past notions of classical or modern physics have ever converged on a putative single meaning. We suggest alternatively that the complex history first revealed by Staley behind different attributions of 'classical' and 'modern' to individual branches of physics is recapitulated in the emergence of the distinction between 'classical physics' and 'modern physics' as entire disciplines. We have suggested when and by whom the terms 'classical physics' and 'modern physics' were first used, and that whilst the inventions of the terms appear to be connected, they were by no means synchronous.

We are therefore now in a position to pose some fundamental questions about 'classical' and 'modern physics'. What factors shaped the interpretation of these terms, and to what extent did different interpretations inspire controversies and broader debates? And why were the terms taken up in the first place? These questions open up the possibility of studying the emergence of 'classical' sciences as part of a broader cultural history of scientific disciplines. The 'transition' from 'classical' to 'modern' was not unique to physics: the need to establish some form of strategically-ambiguous, Janus-faced connection between the past and the present has parallels with other disciplines in the natural and human sciences.[133] In fact, the persistent, widespread use of classical/modern dichotomies serves as an ironic rejoinder to Kuhn's claim that scientific revolutions are rendered invisible by subsequent textbook treatments written from the perspective of the new paradigm.[134] On the contrary, 'modern physics' drew credibility from its continuous emergence from well-established, older techniques that continued to prove fruitful in restricted contexts. So far from committing a form of Kuhnian patricide, physicists actively constructed a 'classical' identity for the work of previous generations in order to highlight the origins, nature, and pedigree of their own work. The tensions between continuity and change that Planck, Bohr, Eddington, and others grappled with remain very much alive.

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[1] Sources cited in Richard Staley, *Einstein's Generation: The Origins of the Relativity Revolution* (Chicago: University of Chicago Press, 2008) include: Allan A. Needell, 'Irreversibility and the Failure of Classical Dynamics: Max Planck's Work on the Quantum Theory, 1900–1915' (Yale University: Unpublished Ph.D. Diss., 1980); Allan A. Needell, 'Introduction,' in Max Planck,

The Theory of Heat Radiation (Los Angeles: Tomash; New York: American Institute of Physics, 1988), pp. xi–xlv; Olivier Darrigol, From c-Numbers to q-Numbers: The Classical Analogy in the History of Quantum Theory (Berkeley/Los Angeles: University of California Press, 1992); Olivier Darrigol, 'The Historians' Disagreements over the Meaning of Planck's Quantum,' Centaurus, 43 (2001), pp. 219–239.

[2] Two of the most substantial contributions are Jed Z. Buchwald, From Maxwell to Microphysics: Aspects of electromagnetic theory in the last quarter of the nineteenth century (Chicago; London: University of Chicago Press, 1985) and Oliver Darrigol, Electrodynamics from Ampère to Einstein (Oxford: Oxford University Press, 2000). C.f. Terry Quinn's recent book (published as this volume went to press) on the creation of the Bureau International des Poids et Mesures and its role in the development of standards of measurement, which, despite the title, does not resuppose a transition from 'classical' to 'modern physics' in its historiography. Terry Quinn, From Artefacts to Atoms. The BIPM and the Search for Ultimate Measurement Standards (Oxford: Oxford University Press, 2012).

[3] Staley's historiographical stance towards the terms 'classical' and 'modern physics' is closely analogous to Suman Seth's approach to the 'crisis' often associated with early twentieth century theoretical physics. Seth rejects 'crisis' as an analytical tool on the grounds of its circularity, and instead investigates the use of the term as an actors' category. Seth explains that the 'dominant discourse of the new discipline' came from the 'physics of principles' practised by Planck, Einstein, and Bohr, rather than the 'physics of problems' associated with Sommerfeld's school. Suman Seth, 'Crisis and the Construction of Modern Theoretical Physics', British Journal for the History of Science, 40:1 (2007), 25-51 (41-2).

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[4] Russell McCormmach, Night Thoughts of a Classical Physicist (Cambridge, MA; London: Harvard University Press, 1982), p. 10. This is a fictional autobiography about how, late in his career during World War One, a German physicist called Jakob experienced with grave concern the threat of military defeat and the revolutionary developments implied by Planck's quantum theory.

[5] Richard Staley, *Einstein's Generation* (ref. 1).

[6] C.f. Heilbron's analogous basis for evaluating the term 'Scientific Revolution' (SR) in chapter ? of this volume, pp. 1-3, esp. p. 2, where he writes: 'whether as a period or as a metaphor, SR is an historian's category, an analytical tool, and must be judged by its utility.' The SR survives Heilbron's judgement on both counts.

[7] See, for example Jed Z. Buchwald, The Creation of Scientific Effects. Heinrich Hertz and Electric Waves, (Chicago: Chicago University Press, 1994); Peter Harman, The Natural Philosophy of James Clerk Maxwell, (Cambridge University Press: Cambridge, 1998).

[8] Andrew Warwick, reveals intimate connections between physics and 'applied' mathematics in Masters of Theory: Cambridge and the Rise of Mathematical Physics (Chicago: University of Chicago Press, 2003). The authors in Robert Fox and Graeme Gooday (eds.), Physics in Oxford 1839-1939: laboratories, learning and college life (Oxford: Oxford University Press, 2005) explore the connections between physics, physical chemistry, and mechanics. For more general discussion see Graeme Gooday '"Vague and Artificial": The Historically Elusive Distinction between Pure and Applied Science" forthcoming in Isis December 2012.

[9] For an interpretation of nineteenth century physics as a unified discipline see Jed Buchwald and Sungook Hong, 'Physics,' in David Cahan (ed.), From natural philosophy to the sciences (Chicago: University of Chicago Press, 2003), pp. 163-195 (p. 165).

[10] Staley, *Einstein's Generation* (ref. 1), notes that Boltzmann was the only figure in 19th century physics known to have described himself as a 'classical physicist' (c.1899), pp.364-365; but Boltzmann's notion of 'classical physics' derived from the early-nineteenth century French tradition of mathematical physics.

[11] Peter Harman, Metaphysics & Natural Philosophy: the problem of substance in classical physics (Brighton: Harvester, 1982), p.4.

[12] Thomas Kuhn, Black-body theory and the quantum discontinuity, 1894-1912 (Chicago; London: University of Chicago Press, 1987).

[13] Darrigol, 'The Historians' Disagreements' (ref. 1). Nevertheless, some historians of popular science continue to use the category literally to describe physics during the nineteenth century without these historiographical scruples. For an orthodox view of classical physics as an identifiable historical entity, see David Knight, Public Understanding of Science: A history of communicating scientific ideas (London: Routledge, 2006), Chapter 12: 'Classical Physics.' Knight argues that the origins of classical physics did not derive directly from Newton but more specifically from the work of French mathematical theorists at the turn of the nineteenth century, thereby echoing Boltzmann's interpretation (discussed in Staley, *Einstein's Generation*, see ref. 8).

[14] Staley, *Einstein's Generation* (ref. 1), does not emphasize sufficiently perhaps the longevity of this original narrower author-specific usage of 'classical'. For example, see the reference to the

'classical researches' of Dulong and Petit in Leonard Hill, O. W. Griffith and Martin Flack, 'The Measurement of the Rate of Heat-Loss at Body Temperature by Convection, Radiation, and Evaporation,' Philosophical Transactions of the Royal Society of London. Series B, 207 (1916), pp. 183-220 (p.188).

[15] Staley, *Einstein's G*eneration (ref. 1), pp. 355-357 (thermodynamics), pp. 357-360 (mechanics).

[16] David Cahan, An Institute for an Empire: the Physikalisch-Technische Reichsanstalt, 1871-1918 (Cambridge: Cambridge University Press, 1988); and Diana Barkan, Walther Nernst and the transition to modern physical science (Cambridge: Cambridge University Press, 1999) had both previously identified the Solvay conference as a key event in the formulation of 'modern physics', although with less specificity than Staley.

[17] Staley, Einstei*n's Generation* (ref. 1), pp. 375-396, 411-413. This reconceptualisation of the Rayleigh-Jeans law as classical obscures Jean's initial interpretation that it represented a final, unreachable state of nature. See pp. 380-383.

[18] Staley, *Einstein's Generation* (ref. 1), pp. 412, 414. On the search for secure foundations for physics, see Christa Jungnickel and Russell McCormmach, Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein. Volume 2: the now mighty theoretical physics (Chicago; London: University of Chicago Press, 1986), Chapter 24.

[19] Staley, *Einstein's Generation* (ref. 1), pp. 397-422.

[20] Likewise, Staley also occasionally alternates between 'classical physics' and 'classical theory'. See *Einstein's Generation* (ref. 1), pp. 394, 398, 417. On the possible meanings of 'classical

theory' in the Annalen der Physik for 1911 and 1912, see Jungnickel and McCormmach, Intellectual Mastery of Nature (ref. 16), p. 313.

[21] Max Planck, [Laws of radiation, and the hypothesis of elementary energy quanta], in A. Eucken (ed.), Die Theorie der Strahlung und der Quanten: Verhandlungen auf einer von e. Solvay einberufenen Zusammenkunft, 30. Oktober bis 3. November 1911 (Berlin: Verlag Chemie GMBH, 1913), pp. 77–94 (p. 77), trans. Richard Staley. Staley, *Einstein's Generation* (ref. 1), p.412.

[22] See Max Planck's Nobel lecture reproduced in Nobel Lectures, Physics 1901-1921 (Amsterdam: Elsevier Publishing Company, 1967). [Online] 'Max Planck – Nobel Lecture'. Available: http://nobelprize.org/nobel_prizes/physics/laureates/1918/planck-lecture.html. [Accessed 12 Nov. 2010].

[23] Niels Bohr, 'The Structure of the Atom' [Nobel Prize address 1922], trans. Frank C. Hoyt, Nature, 112 (1923), pp. 29-44 (p.31). This is the British journal Nature's first mention of the term 'classical physics'.

[24] For example, see discussion of Levi-Cevita's work on the ballistic theory of light in 'Societies and Academies,' Nature, 118 (1926), p. 432.

[25] Staley's use of Poincaré as an example proves only that developments in quantum theory had revised participants' interpretation of classical mechanics. Poincaré's assent to this interpretation does not imply that he adopted a clear dichotomy between 'classical' and 'modern physics' based on the quantisation of energy. See Staley (ref. 1), pp. 416-417.

[26] Staley, *Einstein's Generation* (ref. 1), pp. 399-409; Jungnickel and McCormmach, Intellectual Mastery of Nature (ref. 16), pp. 318-21 (p. 320).

[27] See Staley, *Einstein's Generation* (ref. 1), pp. 352, 360, 364, 398, 417, and esp. pp. 375, 392.

[28] Robert M. Eisberg, Fundamentals of Modern Physics (New York: Chichester: Wiley, 1961), p.1.

[29] The notion of a classical field theory, which cuts across electrodynamics and gravitation, introduces further complexity.

[30] As their prefaces generally indicate, the same authors also seem to associate this continuity of practice with an epistemological continuity. Their rhetoric cannot be construed as motivated entirely by pedagogical convenience.

[31] Eisberg, Fundamentals of Modern Physics (ref. 26), p. 1; Kenneth Krane, Modern Physics, 2nd edn. (New York: Wiley, 1996), pp. 2-3.

[32] Arthur Eddington, 'Chapter I: The Downfall Of Classical Physics,' in The Nature Of The Physical World [Gifford Lectures, Edinburgh University, 1927] (Cambridge: Cambridge University Press, 1928); Matthew Stanley, Practical mystic: religion, science, and A.S. Eddington (Chicago: University of Chicago Press, 2007); Steven French, 'Scribbling on the blank sheet: Eddington's structuralist conception of objects,' Studies in History and Philosophy of Modern Physics, 34 (2003), pp. 227–259. Commenting on the recurrent themes of Eddington's publications, French notes: 'I have argued that one of the things that Eddington saw as irredeemably 'classical' was the individuality or non-indistinguishability of the particles, and this is only exposed and then replaced in 1927, so in a sense it is not until then that the classical regime finally ends.' [personal communication to Graeme Gooday]. We thank Steven French for pointing out that the project of structural realism in philosophy of physics complements our

project by exploring the mathematical continuities in theoretical physics from the nineteenth to the twentieth century.

[33] The journalist noted that ‘there is a hint of reconciliation of the present antithesis of waves and particles, the antithesis between the classical physics of Clerk Maxwell and the new physics of quanta and radiation’. [‘Scientific Correspondent’], ‘The Progress Of Science: New ideas in physics’, Times (19 Nov. 1928), p. 8E.

[34] Henry E. Armstrong, ‘Good English’, Times (22 Aug. 1930), p. 11F.

[35] Staley, *Einstein’s Generation* (ref. 1), pp. 396, 417-418.

[36] J. M. Keynes claimed that it was Karl Marx who coined the term ‘classical economics’ to refer to the works of David Ricardo, James Mill and their precursors, with whom he was critically engaged. Keynes further extended the scope of ‘classical economics’ to include John Stuart Mill and several of his contemporaries. See: John Maynard Keynes, The General Theory of Employment, Interest and Money (London: Macmillan & Co., 1936), Chapter 1, Footnote 1. Twentieth century sociologists have likewise categorized the work of Marx, Weber, Durkheim as ‘classical sociology’. For the origins of the term ‘classical ethology’, see Gregory Radick, ‘Essay Review: The Ethologist’s World,’ Journal of the History of Biology, 40 (2007), pp. 565–575.

[37] M. Abraham & A. Föppl, Theorie der Elektrizität: Einführung in die Maxwellsche Theorie, Vol. 1, Max Abraham, Elektromagnetische Theorie der Strahlung, Vol. 2 (Leipzig: Teubner, 1904-5). Max Abraham [trans. Richard Becker; authorized trans. John Dougall], The classical theory of electricity and magnetism (London: Blackie & Son, 1932). Another example of contingent re-interpretations of ‘modern’ physics through translation is a work by Henri Poincaré’s cousin Lucien Poincaré, La physique moderne, son evolution (Paris: E. Flammarion,

1906), which we refer to below. This was translated into English as The new physics and its evolution (London: Kegan Paul, 1907).

[38] See F.K Richtmyer, Introduction to Modern Physics (New York; London: McGraw Hill, 1928); and review by L. F. B., 'Modern Physics', Nature, 123 (1929), pp. 198-199.

[39] William Thomson, Baltimore Lectures…. Robert Kargon, *Kelvin's Baltimore Lecture and* Modern Theoretical Physics (Cambridge, MA: The MIT Press, 1987).

[40] Bruce Hunt, The Maxwellians (Ithaca, NY; London: Cornell University Press, 2005). Hunt treats Heaviside, Larmor, Lodge, and Fitzgerald as the primary Maxwellians. Buchwald identifies a larger international constituency of Maxwellians. Buchwald, From Maxwell to Microphysics, 73-74.

[41] The radical implications of the theory of special relativity are emphasized in Percy W Bridgman, The logic of modern physics (London: Macmillan, 1928), pp. 1-25. See also George David Birkhoff and Rudolph Ernest Langer, Relativity and modern physics (Cambridge, MA: Harvard University Press, 1923).

[42] Harry Collins and Trevor Pinch, 'Two experiments that 'proved' relativity,' in The Golem: what everyone should know about science (Cambridge: Cambridge University Press, 1993), pp. 27-55.

[43] Geoffrey Cantor and Jonathan Hodge, 'Introduction: major themes in the development of ether theories from the ancients to 1900,' in Geoffrey Cantor and Jon Hodge (eds), Conceptions of Ether: Studies in the History of Ether Theories 1740-1900 (Cambridge: Cambridge University Press, 1981), pp. 53-54. The empirical falsification of the non-existence of an entity is impossible, as Popper pointed out. Even Einstein granted a place to a rehabilitated form of ether in his theory of general relativity as the form of space-time manifold. Albert Einstein, Sidelights

on Relativity (London: Methuen, 1922), pp. 3-24. For a more detailed discussion of Einstein's relationship to the ether, see Ludwik Kostro, Einstein and the ether (Montreal: Apeiron, 2000). See also below for Whittaker's ether-based interpretation of the theory of relativity.

[44] Daniel M. Siegel, 'Thomson, Maxwell, and the universal ether in Victorian physics,' in Cantor and Hodge (eds.), Conceptions of Ether (ref. 41), pp. 239-268 (pp. 239-240).

[45] Bruce Hunt, 'Experimenting on the Ether: Oliver J. Lodge and the Great Whirling Machine,' Historical Studies in the Physical and Biological Sciences, 16 (1986), pp. 111-134.

[46] H. Dingle, 'Edmund T Whittaker, mathematician and historian,' Science, 124 (1956), pp. 208-209; G.F.J. Temple, 'Edmund Taylor Whittaker,' Biographical Memoirs of Fellows of the Royal Society of London, 2 (1956), pp. 299-325; Edmund T Whittaker, A History of the theories of aether and electricity, 1st edn. (London: Longman, Green and Co, 1910); 2nd edn. revised and enlarged, Vol. 1: The classical theories, Vol. 2: The modern theories 1900–1926 (London: Nelson, 1951-1953). Whittaker's revised edition aroused controversy by adopting the accretionist approach of crediting the development of special relativity to Henri Poincaré and Hendrik Lorentz, rather than to Albert Einstein.

[47] To emphasize often overlooked continuities between this period and later forms of physics, Buchwald emphasizes the role of energy as a unifying theme in Maxwellian theorizing. Buchwald, From Maxwell to Microphysics.

[48] Buchwald, From Maxwell to Microphysics, pp.20, 73-74.

[49] See Warwick, Masters of Theory (ref. 6).

[50] Larmor considered this 'the clearest, most compact, and most general means of expressing any physical problem'. Warwick, Masters of Theory (ref. 6), p. 367. Warwick goes on to state that 'Larmor was convinced that this form of expression best revealed the formal mathematical

connections that existed between the "different departments" of mathematical physics, and so facilitated the analogical solution of a wide range of problems.'

[51] This was required to ensure that the electrostatic potential ascribed to an open circuit possessed a unique, 'true' value (Maxwell had shown that this potential varied according to the chosen frame of reference

[52] More specifically, Fitzgerald 'replaced the mechanical symbols in MacCullagh's theory with appropriate electromagnetic symbols and [applied] the Principle of Least Action to the resulting Lagrangian'. Warwick, Masters of Theory (ref. 6), p. 368.

[53] Hunt, The Maxwellians (ref. 38), pp. 209-222, esp. 217-222.

[54] Buchwald, From Maxwell to Microphysics, pp.127-86. Hunt, The Maxwellians (ref. 38), pp. 29, 35-36, 209-210; Warwick, Masters of Theory (ref. 6), pp. 295-296, 368. Hunt points out on pp. 209-210 that the Maxwellians Heaviside and Fitzgerald often referred to Maxwell's theory as a theory of one medium (the ether), which drew attention simultaneously to both its strengths and weaknesses.

[55] According to Howard Stein, this constituted a version of the 'classical theory of electrons'. H. Stein, ''Subtler forms of matter' in the period following Maxwell,' in Cantor and Hodge (eds.), Conceptions of Ether (ref. 41), pp. 309-340, (p. 323).

[56] The inertial mass of matter was produced by the acceleration of electrons with respect to the ether. And in Larmor's own words, the 'material molecule is entirely formed of ether and of nothing else' Warwick, Masters of Theory (ref. 6), p. 369.

[57] Warwick, Masters of Theory (ref. 6), pp. 363-76.

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[58] Warwick argued that this 'leant meaning both to the idea of an ultimate reference system and to the application of dynamical concepts to electromagnetic theory.'Warwick, Masters of Theory (ref. 6), pp. 396-7.

[59] Warwick, Masters of Theory (ref. 6), pp. 376-398. These differences derived from Larmor's route to the ETM via the physical micro-structure of the ether, which his students had to consider only in a more limited sense. Warwick explains that 'from an ontological perspective… they needed to know only that the universe consisted of positive and negative electrons in a sea of ether, the application of the ETM then following as a largely mathematical exercise based on Maxwell's equations and Larmor's electrodynamics of moving bodies' (p. 380).

[60] 'The ontological status of Cunningham's ether reflected the mathematical practice inherent in his electrodynamics.' Warwick, Masters of Theory (ref. 6), p. 425.

[61] Larmor initially thought that they transformed Maxwell's equations only to the second order of v/c. Larmor later noticed his error but continued his earlier practice of approximating to the second order because, unlike Cunningham, he believed the transformations to be unjustified physically at higher orders. See Warwick, Masters of Theory (ref. 6), pp. 374, 411-413.

[62] Warwick, Masters of Theory (ref. 6), pp. 402-403, 409-428 (p. 426). See pp. 404-409 for Warwick's analysis of historiographical problems relating to the 'adoption' of the 'theory of relativity' during the early twentieth century. Warwick's explication of Cunningham's ether is confusing. He appears to suggest that Cunningham introduced a 'plurality of ethers' but quotes Cunningham as saying: 'the aether is in fact, not a medium with an objective reality, but a mental image'. See pp. 424-428; and E. Cunningham, 'The Structure of the Ether,' Nature, 76 (1907), p. 222.

[63] Cantor and Hodge, Conceptions of Ether (ref. 41), p. 33.

[64] 'Inaugural Address by the Most Hon. The Marquis [sic] of Salisbury, K.G., D.C.L., F.R.S., Chancellor of the University of Oxford, President of the British Association,' printed in Report of the Sixty-Fourth Meeting of the British Association for the Advancement of Science Held at Oxford in August 1894 (1894), pp. 3-15; and also appearing in Nature, 50 (1894), pp. 339-343. 'Unsolved Problems of Science,' in Popular Science Monthly, 46 (1894), pp. 33-47. Quotations from pp. 39-41. Pace Cantor and Hodge, Salisbury's scepticism about claims concerning the ether were considered sufficiently important by some Maxwellians that they to referred to them long afterwards. See, for example, Oliver Lodge, 'The Ether of Space,' The North American Review, 187 (1908), pp.724-736, esp. p.727.

[65] In his BAAS address Salisbury nonetheless admittedly referred to himself as 'no mathematician.' Salisbury's library is part of the University of Liverpool's Special Collections; the three volumes of Maxwell's Treatise are unmarked, however. See Gooday's discussion of Salisbury's electrical ventures, both practical and otherwise, in Graeme Gooday, Domesticating Electricity: technology, uncertainty and gender, 1880-1914 (London: Pickering & Chatto, 2008), Chapter 3. Robert Cecil (3rd Marquis of Salisbury), 'On Spectral Lines of Low Temperature', Philosophical Magazine, 4th series, 45 (1873), 241–5. Salisbury's assistant regularly encouraged him to publish more of his private researches: Herbert McLeod 'The Marquis of Salisbury, K. G. 1830 –1903', Proceedings of the Royal Society, 75 (1905), pp. 319–25.

[66] For Salisbury's practical involvement with electrification, see Gooday, Domesticating Electricity (ref. 59).

[67] Warwick, Masters of Theory (ref. 6), p. 295.

[68] Cantor and Hodge (eds.), Conceptions of Ether (ref. 41); J. Larmor, Aether and Matter (Cambridge: Cambridge University Press, 1900).

[69] Salisbury, 'Unsolved Problems of Science' (ref. 58), p. 40; Warwick, Masters of Theory (ref. 6), p. 368. Although Salisbury explicitly aired concerns about the ontological inconsistencies associated with the ether, Larmor's account attained significantly greater levels of technical refinement and clarity.

[70] By 1903, however, even Ambrose Fleming's account of wireless telegraphy moved from a strictly Maxwellian focus on the ether as the mediator of electromagnetic waves, to Larmor's electron based account; that focussed attention on the loops of strain produced by the motion of the electron as the key factor in generating the radiation of electromagnetic waves. See Sungook Hong, *Wireless: from Marconi's Blackbox to the Audion* (Cambridge, MA: The MIT Press, 2001), pp. 193-197. By the 1920s reference to the ether had largely become superfluous to the electronic theory of wireless telegraphy, and thus was little discussed in texts by radio engineers.

[71] See discussion of the debate between Preece and Lodge in Oliver Lodge, Lighting Conductors and Lightning Guards (London, Whittaker and Co., 1892), p.118. Hunt describes Preece's account as the 'drainpipe' theory of lightning conductors. See Bruce Hunt, '"Practice vs. Theory": The British Electrical Debate, 1888-1891', Isis, 74 (1983), pp. 341-355 (p.346).

[72] William Preece, 'Address to BAAS Section G (Mechanical Science),' B.A.A.S. Report, Part II (1888), pp. 790-791.

[73] Maxwellian physicists – notably John Henry Poynting at the University of Birmingham – were reframing electrodynamics in terms of the directional flow of energy, rather than any particular mechanism of the ether'. Buchwald, From Maxwell to Microphysics pp. 38-53.

[74] Preece, 'Address to BAAS Section G' (ref. 66), pp. 790-791.

[75] Warwick, Masters of Theory (ref. 6), pp. 377, 385.

[76] Bruce Hunt, Pursuing Power and Light: Technology and Physics from James Watt to Albert Einstein (Baltimore: Johns Hopkins University Press, 2010); Robert Fox and Anna Guagnini, Laboratories, Workshops, and Sites. Concepts and Practices of Research in Industrial Europe, 1800-1914 (Berkeley: Office for History of Science and Technology, University of California, 1999), pp. 35-40.

[77] For a discussion of Boris Hessen's attempt to present the full Marxist case, see Gideon Freudenthal and Peter McLaughlin, The social and economic roots of the scientific revolution: texts by Boris Hessen and Henryk Grossmann (Dordrecht: Springer, 2009).

[78] T.H. Huxley, 'Science,' in Thomas Humphry Ward (ed.), The Reign of Queen Victoria: a survey of fifty years of progress (London: Smith, Elder & Co., 1887), pp. 330-31. This was republished in the USA as The Advance of Science in the Last Half-Century (New York D Appleton & Co., 1888). The essay appears as 'The Progress of Science', in T.H. Huxley, Collected Essays, Vol. 1 (London: Macmillan, 1893), pp. 42-129.

[79] See discussion of Kelvin's legacy in Christine MacLeod, Heroes of Invention: Technology, Liberalism and British Identity, 1750-1914 (Cambridge: Cambridge University Press 2007), pp. 66-67.

[80] See Stathis Arapostathis and Graeme Gooday, Patently Contestable (The MIT Press, 2012, forthcoming); and Stathis Arapostathis and Graeme Gooday, 'Electrical technoscience and physics in transition, 1880-1920,' Studies in History & Philosophy of Science (2012, forthcoming).

[81] On scientist-engineers, see Hong, Wireless (ref. 64). For use of large industrial machinery in experimentation see Bruce Hunt, 'Experimenting on the ether' (ref. 43).

[82] Bruce J. Hunt, 'Michael Faraday, cable telegraphy and the rise of field theory,' History of Technology, 13 (1991), pp. 1-19.

[83] Heinz 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. There were other paths in Europe that led to a similar conclusion, albeit for rather different reasons and with different forms of evidential base, not all of which were industrial. See Thomas Kuhn, 'Energy Conservation as an Example of Simultaneous Discovery,' in The Essential Tension: Selected Studies in Scientific Tradition and Change (Chicago; London: University of Chicago Press, 1977), pp. 66-104.

[84] Graeme Gooday, The Morals of Measurement: accuracy, irony and trust in late Victorian electrical practice (New York; Cambridge: Cambridge University Press, 2004); Bruce Hunt, 'The Ohm is Where the Art is: British Telegraph Engineers and the Development of Electrical Standards', Osiris, 9 (1994), pp. 48-63; Simon Schaffer, 'Late Victorian Metrology and its instrumentation: a manufactory of ohms,' in R.Bud and S.Cozzens (eds.), Invisible Connections: Instruments, Institutions and Science (Bellingham, WA: SPIE Optical Engineering Press, 1992), pp. 24 -55.

[85] Huxley, 'Science' (ref. 72), pp. 330-331.

[86] Marie Curie, 'Rayons Émis per les Composés de l'uranium et du Thorium' (1898), in Oeuvres de Marie Sklodowska Curie (Warsaw: Państwowe Wydawnictwo Naukowe, 1954), pp. 43-45.

[87] For a clear, concise account, see http://www.ieeeghn.org/wiki/index.php/Edison_Effect.

[88] Graeme Gooday, 'The Questionable Matter of Electricity: The reception of J.J. Thomson's "corpuscle" among electrical theorists and technologists,' in Jed Z .Buchwald & Andrew Warwick, Histories of The Electron: the birth of microphysics (Cambridge, MA; London: The MIT Press, 2001), pp.101-134. See Owen Richardson's Nobel prize lecture, 'Thermionic phenomena and the laws which govern them,' reproduced in Nobel Lectures: Physics 1922-1941 (Amsterdam: Elsevier Publishing Company, 1965). For Schottky's career, see Reinhard W. Serchinger, Walter Schottky, Atomtheoretiker und Elektrotechniker: Sein Leben und Werk bis ins Jahr 1941. (Diepholz: GNT Verlag, 2008).

[89] Smith and Wise, Energy and Empire: A biography of Lord Kelvin (Cambridge: Cambridge University Press, 1989).

[90] Paul Nahin, Oliver Heaviside: The Life, Work, and Times of an Electrical Genius of the Victorian Age, 2nd edn. (Baltimore: The Johns Hopkins University Press, 2002); and Arapostathis & Gooday, Patently Contestable (ref. 74).

[91] In his patriotic attack on 'German science' in 1915, Duhem criticized those practitioners who, like German physicists, treated Maxwell's equations as 'orders' whilst using permanent magnets in their experiments. They thereby invoked 'a doctrine whose axioms made the existence of such [magnetic] bodies absurd.' See the reproduction of Duhem's essay 'La Science Allemande' in Roger Ariew & Peter Barker (eds.), Pierre Duhem: Essays in the history and philosophy of science (Indianapolis: Hackett Pub. Co., 1996), quotations from pp. 268, 270.

[92] See discussion in James Swinburne, Practical Electrical Measurement (London: Alabaster & Gatehouse, 1888), pp. 25-56; and Gooday, The Morals of Measurement (ref. 78), p.35.

[93] Matthias Dörries, 'Prior History and After-effects: hysteresis and nachwirkung in l9th-century physics,' Historical Studies in the Physical and Biological Sciences, 22 (1991), pp. 25-55. J.A. Ewing, 'On effects of retentiveness in the magnetisation or iron and steel,' Proceedings of the Royal Society, 34 (1882), pp. 39-45; 'Experimental Researches in Magnetism,' Philosophical Transactions of the Royal Society, 176 (1885), pp. 523-640. Graeme Gooday, 'Domesticating the Magnet: Secularity, Secrecy and 'Permanency' as Epistemic Boundaries in Marie Curie's Early Work,' Spontaneous Generations: A Journal for the History and Philosophy of Science, 3 (2009). [Online] Available: http://spontaneousgenerations.library.utoronto.ca/index.php/SpontaneousGenerations/article/view/10615. [Accessed 10 Nov. 2011]. The term 'hysteresis' is used to characterise systems whose present properties depend in a path-dependent fashion on preceding states. So in this case the distribution of magnetism at any given time depended on the historical sequence of distributions, in other words on previous magnetic influences.

[94] John Hopkinson, 'Magnetisation of Iron,' Philosophical Transactions of the Royal Society, Part II (1885), pp. 455-69; reproduced in Bertram Hopkinson (ed.), Original Papers by the late John Hopkinson - Vol. 2: Scientific papers (Cambridge: Cambridge University Press, 1901), pp. 154-177. For Langevin's quantum mechanical studies of magnetism, see Dominique Pestre, Physique et physiciens en France, 1918-1940 (Paris: Édition Archives Contemporaines, 1984).

[95] Peter Galison, *Einstein's Clocks and Poincaré's Maps* (London: Sceptre, 2003), p. 31.

[96] Cahan, An Institute for An Empire (ref. 14), pp. 145-157. Traditional accounts of the Planck blackbody story do not mention the PTR or the electrical industry. See for example, Martin Klein, 'Max Planck and the Beginnings of Quantum Theory', Archive for History of Exact

Sciences, 1 (1962), pp. 459-479; and Kuhn, Black-body theory and the quantum discontinuity (ref. 10). In the context of understanding the roots of quantum physics in an industrial setting, one may note Staley's discussion, in *Einstein's Generation* (ref. 1), of Solvay's investment in the 1911 conference and Cahan's discussion, in An Institute for an Empire, of Werner Siemens' financial support for the PTR. See Gooday, The Morals of Measurement (ref. 78), Chapter 6, for a study of how quantization in energy also arose in the methods used by the electrical supply industry for billing its customers.

[97] Elizabeth Garber, The Language of Physics: The calculus and development of theoretical physics in Europe 1750-1914 (Boston: Birkhauser, 1999), p. 312.

[98] Robert Fox and George Weisz (eds.), 'The institutional basis of French science in the nineteenth century,' in The Organisation of Science and Technology in France 1808-1914 (Cambridge: Cambridge University Press, 1980), pp. 1-28 (p. 26); Mary Jo Nye, 'Scientific Decline: is quantitative evaluation enough?,' Isis, 75 (1984), pp. 697-708 (pp. 705–8); Dominique Pestre, 'Sur la science en France 1860-1940: à propos de deux ouvrages récents de Mary Jo Nye et Harry W. Paul,' *Révue d'histoire des sciences*, 41 (1988), pp. 75-83 (pp. 81–82).

[99] Daniel Jon Mitchell, 'Gabriel Lippmann's Approach to French Experimental Physics' (University of Oxford: Unpublished D.Phil thesis, 2010), p. 3, also c.f. pp. 234-5. The decline thesis still persists among some historians, however. See Ivan Grattan-Guiness (ed.), 'France', in Companion Encyclopeadia of the History and Philosophy of the Mathematical Sciences, Vol. 2 (London: Routledge, 1994), pp. 1430-1442.

[100] Matthias Dörries, 'Vicious circles, or the pitfalls of experimental virtuosity,' in Michael Heidelberger and Friedrich Steinle (eds.), Experimental Essays–Versuche zum Experiment

(Baden-Baden: Nomos, 1998); Charlotte Bigg, 'Behind the Lines. Spectroscopic enterprises in early twentieth century Europe' (University of Cambridge: Unpublished Ph.D. Thesis, 2001), pp. 17–18.

[101] More specifically to the historiography of French physics, by eliminating the notion of 'modern physics' from our analysis, we invite a re-evaluation of the 'decline' of French physics during the nineteenth century. Whilst French physicists may have marginalised themselves from major theoretical developments (with some notable exceptions), the importance of their practical and experimental contributions to physics remains open to enquiry.

[102] We admit that these reasons need further elaboration and defence (and even that the notion of 'approach' remains unsatisfactorily vague), but we believe that 'shared approaches' are no less fit for purpose than 'national style'.

[103] Pestre, Physique et Physiciens en France 1918-1940 (ref. 87), pp. 104-108.

[104] Jed Buchwald has raised the question of how optics fits into this picture, especially given that the French maintained strength in both its mathematical and experimental aspects, partly through a shared institutional basis with astronomy. Further studies are required to determine the extent to which the Fresnelian mathematical tradition may have set an agenda for experimental work in optics during this period, and whether this made a difference to its character. For now, we simply note that the transverse wave nature of light was generally regarded as a 'hypothesis' in France, which explains the excitement generated by Otto Wiener's purported photographic proof in 1891.

[105] Pierre Bertin, 'Léçon d'ouverture: l'électricité; sa nature,' Revue des Cours Scientifiques de la France et de *l'Etranger,* 4 (1866), pp. 41-45.

[106] Jean-Charles d'Almeida and Augustin Boutan, Cours Elementaire de Physique, 2nd edn. (Paris: Dunod, 1863), p. 2.

[107] Josep Simon, Communicating physics in nineteenth century France and England. The *production, distribution, and use of Ganot's textbooks,* unpublished PhD thesis (University of Leeds, 2009), pp. 200, 205, 215, 221.

[108] Mitchell, 'Gabriel Lippmann's Approach' (ref. 92), p. 19; Simon, ibid., pp. 205, 215-7. We thank Jed Buchwald for raising Mascart and Joubert's 1882 Le*çons sur l'électricité et le* magnétisme as an exception. Yet whilst the examples cited in the main text were amongst the most highly regarded textbooks of physics in France, Mascart and Joubert's Leçons attracted strong criticism. In a long, savage review for the La Lumière Electrique, E. Mercadier pronounced the separation into two volumes of 'pure theory' and 'the examination of phenomena and methods of measurement' as 'dangerous'. In terms consistent with the orthodox French approach to experimental physics, he explained that 'as a result one ends up forgetting that physics, and even mechanics…are experimental sciences, that's to say that the depend on principles that are in no way a priori axioms, but facts of experience that one generalises through [subsequent] induction'. E. Mercadier, 'Leçons sur l'électricité et le magnétisme, par MM. Mascart et Joubert', La Lumière Electrique, 7:51 (1882), 619-22 (620).

[109] Lucien Poincaré, The New physics and its evolution (ref. 35).

[110] Some of the terms in this exposition of Lippmann's work are anachronistic in order to make the explanation more readily intelligible to the general reader.

[111] Daniel Berthelot, 'Gabriel Lippmann: la vie d'un savant,' Revue des Deux Mondes, 10 (1922), pp. 19-46 (p. 24); Edmond Bouty, 'Gabriel Lippmann', Annales de Chimie et de Physique, 16 (1921), pp.156-164 (pp. 156, 158).

[112] Gabriel Lippmann, 'Relations entre les phénomènes électriques et capillaries,' Annales de Chimie et de Physique, 5 (1875), pp. 494-549 (p. 495).

[113] Lippmann, 'Relations entre les phénomènes électriques et capillaries' (ref. 101), p. 547.

[114] Gabriel Lippmann, 'G. Quincke: Ueber elektrische Ströme bei ungleichzeitigern Eintauchen zweier Quecksilberelektroden in verschiedene Flüssigkeiten,' Journal de Physique Théorique et Appliquée, 4 (1875), pp. 248-251.

[115] Michel Atten and Dominique Pestre, *Heinrich Hertz. L'Administration de la Preuve* (Paris: Presses Universitaires de France, 2002), p. 81. Their apparatus was actually one that Hertz took up following its recommendation by Maxwellian theoreticians and its use by Thomson. See pp. 80-81.

[116] Atten and Pestre, Heinrich Hertz (ref. 104), p. 80. We agree with Pestre's subtle analysis, pp. 83-84, of the approach taken by de la Rive and Sarasin and their minimal invocation of theory.

[117] Atten and Pestre, Heinrich Hertz (ref. 104), pp. 79-83; Édouard Sarasin and Lucien de la Rive, 'Résonance multiple des ondulations électriques de M. Hertz', Comptes Rendus Hebdomadaires de *l'Académie des Sciences*, 110 (1890), pp. 72-5. Nonetheless, given the theoretical uncertainty surrounding the production of novel experimental phenomena, we would not be surprised if these 'French' characteristics appear more widespread in such cases than they actually are, despite the example of Fitzgerald and Trouton (see below). Jed Buchwald has

indicated similarities with other German replications of Hertz's experiments, for example. Jed Buchwald, private communication.

[118] Alfred Cornu, 'Remarques à propos de la communication de MM. Sarasin et de la Rive', Comptes Rendus Hebdomadaires *de l'Académie des Sciences*, 110 (1890), pp. 75-76; quoted in Atten and Pestre, Heinrich Hertz (ref. 104), p. 87. Atten and Pestre, pp. 30-35, 85-90, give one other example of French doubts, due to the Director of the Bureau of Weights and Mesures, Charles-Édouard Guillaume. He cautioned that the enthusiasm of the moment had resulted in the suppression of practical difficulties associated with the replication of Hertz's experiment, and gave examples from the Parisian replication of Joubert and de Nerville. In his opinion, new experiments were required to determine the validity of particular techniques and results. Nonetheless, the French reception of de la Rive and Sarasin's experiments was not entirely uniform. The professor of physics at the Ecole Normale, Marcel Brillouin, downplayed their significance.

[119] Atten and Pestre, Heinrich Hertz (ref. 104), pp. 43-49, 90-91.

[120] Benoit Lelong, 'Ions, electrometers, and physical constants: Paul Langevin's work on gas discharges 1896-1903', Historical Studies in the Physical Sciences, 36 (2005), pp. 93-130 (p. 96).

[121] Lelong, 'Ions, electrometers, and physical constants' (ref. 109), pp. 94-7; Benoit Lelong, 'Paul Villard, J. J. Thomson, and the Composition of Cathode Rays,' in Jed Z. Buchwald and Andrew Warwick (eds.), Histories of the Electron (ref. 81), pp. 135-167, esp. pp. 136-149 (p. 142).

[122] Lelong, 'Ions, electrometers, and physical constants' (ref. 109), pp. 98-100 (p. 99).

[123] Lelong, 'Ions, electrometers, and physical constants' (ref. 109), pp. 110-113; Olivier Darrigol, Electrodynamics from Ampère to Einstein (ref. 2), pp. 300-10, esp. pp. 305-6.

[124] See Pestre, Physique et Physiciens en France 1918-1940 (ref. 87), pp. 16-22, 31-65, 104-26, quotation at p. 45.

[125] See Mary Jo Nye, Molecular Reality (New York: Elsevier, 1972), pp. 145-6, 152-3, 156-7, 161, 165-7 for the French reception of Perrin's experimental proofs of molecular reality, and pp. 52, 58-64 for the roots of Perrin's favourable ontological stance towards molecular 'hypotheses', in particular the influence of his teacher at the Ecole Normale, Marcel Brillouin.

[126] Lelong has described the changing approach of the Villard's investigations into the properties of cathode rays. During the late 1890s, Villard's publications tended to enumerate these properties and he 'preferred the production and description of experimental facts' to theorizing about the nature of the rays. After 1906, Villard increasingly articulated theories and made experimental predictions before presenting his experimental results, framed in the language of ions and corpuscles, in support of these theories. Benoit Lelong, 'Paul Villard, J. J. Thomson, and the Composition of Cathode Rays,' in Jed Z. Buchwald and Andrew Warwick (eds.), Histories of the Electron (ref. 81), pp. 135-167, esp. pp. 141-2, 154-6 (p. 142).

[127] Buchwald and Hong, 'Physics' (ref. 7), p. 164; Poincaré, The New Physics (ref. 35), pp. ix-xi.

[128] Shaul Katzir, 'The discovery of the piezoelectric effect', Archive for History of Exact Sciences 57 (2003), 61-91 (63-71, 75-8), quotations at pp. 61, 66; 'From explanation to description: molecular and phenomenological theories of piezoelectricity', Historical Studies in the Physical Sciences 34 (2003), 69-94 (71-5), quotations at p. 72. Katzir's account leaves open the question of how Curie brothers' perceived the epistemological status of their micro-

molecular theory in comparison to the empirical laws. But he offers a clue by explaining that Jacques Curie and Charles Friedel attempted to reinterpret some contradictory empirical findings, due to the German physicist Wilhelm Hankel, in accordance with the Curie brothers' molecular theory.

[129] See Marjorie Malley, 'The discovery of atomic transmutation: scientific styles and philosophies in France and Britain,' Isis, 70 (1979), pp. 213-23, esp. pp. 217-8 (217). We prefer to attribute Curie's metaphysical caution to an experimental approach shared by French experimental physicists (as Malley hints in her conclusion) rather than a 'positivist' philosophy of science. We wonder to what extent Pierre Curie's apparent change in attitude towards hypotheses was influenced by the replacement of molecular theories by phenomenological ones in piezoelectricity. See Shaul Katzir, 'From explanation to description: molecular and phenomenological theories of piezoelectricity', Historical Studies in the Physical Sciences, 34 (2003), 69-94 (93). Katzir concludes 'since both molecular and phenomenological realms had potential benefits, advancing simultaneously in both was a reasonable strategy as long as one recognized the hypothetical character of the molecular against the firmer ground of the phenomenological.'

[130] Mitchell, 'Gabriel Lippmann's Approach' (ref. 92), pp. 33-7, 78; and 'Measurement in French experimental physics from Regnault to Lippmann', Annals of Science, forthcoming; Shaul Katzir, 'Piezoelectricity and comparative styles,' Studies in History and Philosophy of Modern Physics, 34 (2003), pp. 579-606; and The Beginnings of Piezoelectricity. A study in mundane physics (Dordrecht: Springer, 2006). On Regnault and 'direct' measurement, see Jean-Baptiste Dumas, 'Victor Regnault,' in Discours et Eloges Académiques (Paris: Gauthier-Villars, 1885),

pp. 174–5; Hasok Chang, Inventing Temperature. Measurement and Scientific Progress (Oxford: Oxford University Press, 2004), p. 76.

[131] Lelong, 'Langevin's work on gas discharges', pp. 123-4, 127-30 (130); Mary Jo Nye, Molecular Reality (New York: Elsevier, 1972), p. 170.

[132] A useful analogy to our case is the common anachronistic use of the word 'science' (vis-à-vis 'natural philosophy') to describe the study of nature during the sixteenth and seventeenth centuries. See Peter Dear, 'What Is the History of Science the History Of? Early Modern Roots of the Ideology of Modern Science,' Isis, 96 (2005), pp. 390–406.

[133] For an example of how other fields were starting to formulate a rejection of a 'classical' canon, see Karl Pearson's 1916 account of the mathematization of evolutionary theory in which he complained of the recurrent 'evil of implicit reliance on a classical theory' in the field of mathematical statistics. Karl Pearson, 'Mathematical Contributions to the Theory of Evolution. XIX. Second Supplement to a Memoir on Skew Variation,' Philosophical Transactions of the Royal Society of London, Series A, 216 (1916), pp. 429-457.

[134] Thomas Kuhn, The Structure of Scientific Revolutions, 3rd ed. (Chicago: University of Chicago Press, 1996), 'Chapter 11: The Invisibility of Revolutions', p.136 ff. Our thanks to Adrian Wilson for informed discussion on this point. Staley points out that Kuhn's views on this topic were considerably refined in his later volume on blackbody radiation, which describes how Planck's views on quantum theory evolved slowly over a long period. Staley, *Einstein's* Generation (ref. 1), pp. 349, 375.