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Crisis and the construction of modern theoretical physics

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Abstract. This paper takes up the concept of 'crisis' at both historical and historiographical levels. It proceeds through two examples of periods that have been described by historians of physics using a language of crisis. The first examines an incipient German theoretical-physics community around 1900 and the debates that concerned the so-called 'failure' of the mechanical world view. It is argued, largely on the basis of what is now an extensive body of secondary literature, that there is little evidence for a widespread crisis in this period. Abandoning the term as both description and explanation, one comes to the far more intriguing suggestion that the conflict over foundations was not evidence of a divisive dissonance but rather of collective construction. What has been termed crisis was, in fact, the practice of theoretical physics in the *fin de siècle*. The second example is the period either side of the advent of quantum mechanics around 1925. Different subgroups within the theoretical-physics community viewed the state of the field in dramatically different ways. Those, such as members of the Sommerfeld school in Munich, who saw the task of the physicist as lying in the solution of particular problems, neither saw a crisis nor acknowledged its resolution. On the other hand those, such as several researchers associated with Niels Bohr's institute in Copenhagen, who focused on the creation and adaptation of new principles, openly advocated a crisis even before decisive anomalies arose. They then sought to conceptualize the development of quantum mechanics in terms of crises and the revolutions that followed. Thomas Kuhn's language of crisis, revolution and anomaly, it is concluded, arises from his focus on only one set of theoretical physicists. A closer look at intra-communal differences opens a new vista onto what he termed 'normal' and 'revolutionary' science.

Crisis

Seven years after the publication of *The Structure of Scientific Revolutions*, Thomas Kuhn published a 'Postscript' to the book that, while clarifying some points of confusion, also detailed the ways in which he had modified his position since 1962. A key shift involved the notion of a 'crisis'. Although crises had once been central to the description of the onset of a revolution and of crucial importance in explaining why

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scientists make choices between incommensurable paradigms, Kuhn now played down their significance:

> A number of critics have doubted whether crisis, the common awareness that something has gone wrong, precedes revolutions so invariably as I have implied in my original text. Nothing important to my argument depends, however, on crises' being an absolute prerequisite to revolutions; they need only be the usual prelude, supplying, that is, a self-correcting mechanism which ensures that the rigidity of normal science will not forever go unchallenged. Revolutions may also be induced in other ways, though I think they seldom are.[1]

The casual style serves to elide what is in fact a profound alteration in meaning. Crisis as 'the common awareness that something has gone wrong' is unrecognizable as the crisis of the original edition. It has become a collective comprehension, whereas it had once been an individual experience. A new paradigm, Kuhn had written in 1962, 'emerges all at once, sometimes in the middle of the night, in the mind of a man deeply immersed in crisis'.[2] The weakness of a notion of crisis simply as the 'awareness that something has gone wrong' has lost both its explanatory power and, crucially, its distinction from the modes of thought characteristic of normal science. After all, the processes of change involved in normal and revolutionary science were parallel. Normal science proceeds through problems and their solution, revolutions through crises and their resolution. If crises become so watered down as merely to be the realization that all is not well, it is hard to see them or their causes as anything but quantitatively more difficult puzzles.

To the reader already groaning at the thought of yet another contribution to a long set of critiques of Kuhn's work, here is some immediate reassurance.[3] In spite of the character of the lines above, my aim in this paper is not, in fact, one of philosophical exegesis but rather of historiographical analysis. Perhaps because the use of the term in common parlance long preceded *Structure*, the language of crisis has remained a part of the history of science long past the point where most historians began to avoid any mention of paradigms and incommensurability.[4] In this preface, therefore, Kuhn is

1 T. S. Kuhn, *The Structure of Scientific Revolutions*, 2nd edn, Chicago, 1970, 174–210, 181.

2 T. S. Kuhn, *The Structure of Scientific Revolutions*, Chicago, 1962, 89.

3 For early criticisms of Kuhn's theory and his responses see I. Lakatos and A. Musgrave (eds.), *Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science, London, 1965, Volume 4*, Cambridge, 1970; Frederick Suppe (ed.), *The Structure of Scientific Theories*, 2nd edn, Urbana and Chicago, 1977. On 'epistemological crises' and their relationship with Kuhn's theory see A. Macintyre, 'Epistemological crises, dramatic narrative, and the philosophy of science', in *Paradigms and Revolutions: Appraisals and Applications of Thomas Kuhn's Philosophy of Science* (ed. Gary Gutting), London, 1980, 54–74.

4 In 'Autobiographical notes', written in 1946, Einstein, for example, spoke of two 'fundamental cris[e]s' that faced physics at the end of the nineteenth century: the instability of Lorentz's electron and Planck's law of heat radiation. 'Reflection of this type', he wrote, 'made it clear to me as long ago as shortly after 1900, i.e. shortly after Planck's trailblazing work, that neither mechanics nor thermodynamics could (except in limiting cases) claim exact validity. By and by I despaired of the possibility of discovering the true laws by means of constructive efforts based on known facts. The longer and the more despairingly I tried, the more I came to the conviction that only the discovery of a universal formal principle could lead us to assured results'. Albert Einstein, 'Autobiographical notes (in German, and in English translation)', in *Albert Einstein: Philosopher-Scientist* (ed. Paul Arthur Schilpp), Menasha, WI, 1949, 37, 51, 53. In this account, therefore, crisis and

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examined as a paradigmatic case, if the reader will excuse the expression, one that serves to illuminate a general historiographical tactic that deploys the term 'crisis' in a manner that is intended to be at once descriptive and explanatory. Historical analysis replaces circular argument. When he reappears in the paper's final section, Kuhn will have a different role, as much historical actor as historian. My intent will be to examine the ways that his particular usage of the notion of crisis has fundamentally shaped a particular and one-sided view of the development of modern physics. Finally, while as a historian of physics I have chosen examples solely from my own field, it is my hope that historians of science generally will find strong echoes in their own respective areas.

Kuhn was far more consistent on the issue of crisis in his original text. There revolutions actually required crisis or something very like it.[5] How else can one explain the decision to move between two fundamentally different modes of thought? Originally, the period of crisis had offered a liminal space. A scientist entered a time of crisis and stood, balanced on a knife-edge, between two possible choices. Like an invalid in the throes of a potentially terminal fever, the only possibility for any actor or observer was to wait. Life or death marked the two possible resolutions of the crisis. If one replaces the crisis period with a time of negotiation, as members of each paradigm attempt to convert each other, translating between each of their positions, it is hard to imagine how one can maintain the rupture that is characteristic of revolution. Continuity, to be sure, does not demand crisis, but the ruptures that Kuhn termed revolutions most certainly do.

Two of Kuhn's key critical periods, 'the late nineteenth century crisis in physics that prepared the way for the emergence of relativity theory'[6] (and, more generally, the crisis induced by the so-called 'failure' of mechanics[7]) and 'the crisis in quantum mechanics

despair become the cause of a search for a new approach. Miller, for example, makes use of this retrospective account in reconstructing Einstein's path towards special relativity. A. I. Miller, 'On Einstein's invention of special relativity', in *PSA: Proceedings of the Biennial Meeting of the Philosophy of Science Association 2: Symposia and Invited Papers* (1982), 377–402.

5 See also, originally published in 1961, T. S. Kuhn, 'The function of measurement in modern physical science', in *idem*, *The Essential Tension: Selected Studies in Scientific Tradition and Change*, Chicago and London, 1977, esp. 202–13.

6 Kuhn, op. cit. (1), 72.

7 On the crisis around 1900 see e.g. R. McCormmach, 'Editor's foreword', *Historical Studies in the Physical Sciences* (1970) **2**, xi–xx. J. L. Heilbron, 'Fin-de-siècle physics', in *Science, Technology and Society in the Time of Alfred Nobel* (ed. C. G. Bernhard, E. Crawford and P. Sörbom), Oxford, 1982, 51–73. L. Pyenson, 'The relativity revolution in Germany', in *The Comparative Reception of Relativity* (ed. T. Glick), Dordrecht, 1987, 59–111. Porter notes the historiographical trend, but contests it, arguing that what Heilbron calls 'descriptionism' should be understood as a 'more optimistic, almost exuberant, view of physics', at least for two prominent philosophers of science: E. Mach and K. Pearson. T. M. Porter, 'The death of the object: fin-de-siècle philosophy of physics', in *Modernist Impulses in the Human Sciences, 1870–1930* (ed. Dorothy Ross), Baltimore, 1994, 130. Holton makes a distinction between forces 'outside the laboratory' that spoke of the 'death and decay of science' around 1900 (G. Holton, 'Spengler, Einstein, and the controversy over the end of science', *Physis* (1991), **28**, 543–56, 543, 544) and scientists themselves who were, on the whole, upbeat: 'Some, such as Kelvin and Poincaré, discerned clouds; but the predominant feeling was that of satisfaction, even enthusiasm, with the momentum and direction in which the work was going.' G. Holton, 'Einstein's search for the "Weltbild"', *Proceedings of the American Philosophical Society* (1981) **125**, 1–15, 2. Holton's point is polemical, but not without merit as a historiographical distinction. Many of those,

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in the years immediately before 1925', have been described by many historians of physics in very similar terms.[8] As with Kuhn, there is often in these accounts an elision between the use of the term in a descriptive and an explanatory sense. The crisis induced by particular anomalies (or by 'external factors'[9] or the implications of new theories[10]) is taken as the cause for the search for new and innovative solutions, for radical new modes of description. This is perhaps unproblematic, except for the fact that evidence to bolster the descriptive validity of the terms is often difficult to locate. As Kuhn himself noted: 'explicit recognitions of breakdown are extremely rare'.[11] This direct evidentiary vacuum leads, in the case of *Structure*, to the identification of crises by their effects, since Kuhn claims that these 'do not entirely depend upon [a crisis-]conscious recognition'.[12] As Kuhn lists such effects one realizes how slippery this slope really is. Two effects are 'universal'. The first is the 'blurring of a paradigm and the consequent loosening of the rules for normal research'. Yet how, except in hindsight, are we to tell the difference between the 'blurring of a paradigm' and its creative extension through the practices of normal science, which are, even in Kuhn's grudging estimation, capable of such limited creativity?

The second universal effect is that crises are resolved in one of three ways: by normal science handling the crisis-causing problem, by normal science reserving the problem for a later date, or, finally, by the fact that the crisis causes the emergence of a new paradigm, and the beginning of 'a battle over its acceptance'.[13] Since normal science deals with the first two resolutions (by turning a so-called 'crisis' into a solved problem, or a problem to be solved at some later date), we are left with the case of identifying a crisis by the fact that a revolution succeeds it, thus creating a circularity in our

post-Kuhn, who would argue for a *fin de siècle* crisis of physics would also argue for connections between science and the broader social context, McCormmach, for example, drawing a connection between German physicists and other academics, on the grounds of their collective 'participation in the general crisis mentality of their class'. Talk of this crisis mentality is far less prevalent in his two-volume text on the history of theoretical physics, co-authored with Jungnickel, which largely eschews social and cultural history. C. Jungnickel and R. McCormmach, *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein. Volume 1, The Torch of Mathematics, 1800–1870*, Chicago and London, 1990, 22.

8 On the crisis around 1925 see e.g. P. Forman, 'Weimar culture, causality, and quantum theory, 1918–1927: adaptation by German physicists and mathematicians to a hostile intellectual environment', *Historical Studies in the Physical Sciences* (1971), **3**, 1–115; O. Darrigol, *From c-Numbers to q-Numbers: The Classical Analogy in the History of Quantum Theory*, Berkeley, Los Angeles and Oxford, 1992, 175–212; H. Kragh, *Quantum Generations: A History of Physics in the Twentieth Century*, Princeton, 1999, 155–60.

9 Forman, op. cit. (8); Heilbron, op. cit. (7).

10 T. S. Kuhn, 'Revisiting Planck', *Historical Studies in the Physical Sciences* (1984), **14**, 231–52.

11 Kuhn, op. cit. (1), 84.

12 According to T. K. Rabb, it is this 'retrospective' identification of crisis through its effects that distinguishes the term from any other word used to denote times of trouble: 'crisis is *always* followed by resolution … To the extent that, as in medicine, a resolution offers the best proof that a "crisis" has occurred, our evidence of a dividing point will derive mainly from what ensues'. T. K. Rabb, *The Struggle for Stability in Early Modern Europe*, New York, 1975, 31.

13 It is worth noting that, although Kuhn claimed that the issue of crisis was relatively unimportant in the structure of his overall argument, the response to crisis formed one of only two areas where he modified the main text of *Structure* for the second edition. The discussion of three responses to crisis replaced the claim of the earlier edition (Kuhn, op. cit. (2), 84) that 'all crises close with the emergence of a new candidate for paradigm and with the subsequent battle over its acceptance'.

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evidentiary problem. Our explanation of behaviour depends upon our demonstrating the existence of a crisis, but that existence is demonstrated by the very behaviour with which we began.

The circularity is vicious enough to lead me to propose the following historical rule: crisis, as a descriptive category for the historian, can only be utilized when it is an actors' category. If actors do not speak of crisis or something provably similar and speak only of problems to be solved or difficulties to be dealt with, we cannot invoke crisis as either an explanation for behaviour or a description of interaction. We require another means of understanding the set of phenomena we have labelled crisis.

From this proviso follows the choice of the two examples examined here. In each case I am less concerned with the question of whether a crisis 'really' occurred, though in the example of *fin de siècle* physics, however, I draw on a number of newer secondary sources that suggest that it did not. I am nevertheless more concerned with the issue of what new questions and perspectives arise if we no longer assume the existence of crisis. For theoretical physics around 1900, the sometimes vitriolic debates around the appropriate methodological and ontological foundations of physics can be reformulated not as crisis, but as a particular form of practice. This provides an answer to a question that has been left unanswered, or unsuccessfully so, in the literature to date: what was the nature of the public discourse of theoretical physics and hence its self-representation as a discipline in the first decades of its development? The second example draws more explicitly on my own work on the history of the 'Sommerfeld school' of theoretical physics. Kuhn was right to identify crisis talk as an integral part of discussions that preceded the so-called revolution accompanying the introduction of wave and matrix mechanics. 'History of science, to my knowledge', he would write of the period, in response to critiques by Imre Lakatos, 'offers no equally clear, detailed and cogent example of the creative functions of normal science and crisis'.[14] As we shall see, however, not all members of the theoretical-physics community recognized the events as part of a crisis. In particular, one of the two great centres for the study of the quantum, Sommerfeld's Munich school, failed to perceive a crisis at all. Nowhere is this more obvious, and at this point the historical and the historiographical issues become inextricably intertwined, than in Kuhn's own interviews with people who had been participants in the 'revolution', as part of the interview project that produced the Archives in the History of Quantum Physics. In spite of leading questions about the existence of a crisis in 1923 and 1924, many of those Kuhn interviewed failed to remember such a sense. Contemporary records back their case. In explaining why some perceived a crisis and some did not, I reject the explanation that Sommerfeld and his students simply 'missed' what was, in fact, a genuinely critical moment. Rather, I seek to relate the very presence of crisis talk to particular modes of understanding the progress of science and to the particular theoretical techniques that follow from and inform such understandings. I seek to demonstrate that different inter-communal

14 T. S. Kuhn, 'Reflections on my critics', in *Criticism and the Growth of Knowledge: Proceedings of the International Colloquium in the Philosophy of Science, London, 1965, Volume 4* (ed. I. Lakatos and A. Musgrave), Cambridge, 1970, 258.

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groups within theoretical physics understood and deployed the notion and the discourse of crisis in very different ways. This more nuanced account results in a reformulation of the origin and meaning of Kuhn's most fundamental and long-lasting distinction, that between normal and revolutionary science.

Case one: crisis and the nature of *fin de siècle* physics

In 1895, during a speech delivered at the unveiling of a memorial to his former teacher Josef Stefan, Ludwig Boltzmann offered an explanation of the designation 'theoretical physicist'. Although Stefan was 'above all else a theoretical physicist', Boltzmann admitted that the definition of the concept came 'not without difficulty':

> But what is a theoretical physicist? Because he must possess a fundamental mathematical knowledge, one often terms his activity mathematical physics. Yet this isn't quite right, since the analysis of complicated physical experiments, indeed even the solution of engineering problems [*technischer Probleme*], can require copious and difficult calculations, but is not to be included in theoretical physics. Theoretical physics has much more to do, as one used to say, with seeking out the root causes of phenomena, or, as one would rather say nowadays, with uniting the experimental results which one has won into unified perspectives, ordering them concisely and describing them as clearly and simply as possible, whereby the compilation of these in their total multiplicity is made easier, indeed actually first made possible. For this reason it is also called natural philosophy in England.[15]

In spite of the initial tentativeness with which it was offered, the explication of his field by the leading German-speaking theoretical physicist was almost deceptively definite. Boltzmann's demarcations would be challenged almost immediately. Arnold Sommerfeld, who took up Boltzmann's former position as professor of theoretical physics at the University of Munich in 1906, spoke out strongly in 1903 against the 'supercilious' physicist and mathematician who held

> himself aloof from the pursuits of engineering [*Technik*], because he perceived a smaller degree of scientific rigour in these branches than in his own particular sphere … We, the theoretical enquirers, record it to our honour if we can assist in the building up of the technical sciences [*technische Wissenschaften*] and we appreciate our good fortune whenever it brings us into active contact with the problems of engineering [*Technik*].[16]

The association of theoretical physics, often considered a German speciality, with English mathematical physics would no doubt have also raised eyebrows.[17] As many

15 L. Boltzmann, 'Josef Stefan', in *idem*, *Populäre Schriften* (Leipzig, 1925), 94. Cf. C. Jungnickel and R. McCormmach, *Intellectual Mastery of Nature: Theoretical Physics from Ohm to Einstein. Volume 2, The Now-Mighty Theoretical Physics*, Chicago and London, 1990, 347.

16 A. Sommerfeld, 'Die naturwissenschaftlichen Ergebnissen und die Ziele der modernen technischen Mechanik', *Physikalische Zeitschrift* (1903), **4**, 773–82, 782. The translation here is altered from A. Sommerfeld, 'The scientific results and aims of modern applied mechanics', *Mathematical Gazette* (1903), **3**, 26–31.

17 On theoretical physics as a German-dominated speciality see P. Forman, J. L. Heilbron and S. Weart, 'Physics circa 1900: Personnel, funding, and productivity of the academic establishments', *Historical Studies in the Physical Sciences* (1974), **5**, 1–185. L. Pyenson and D. Skopp, ''Educating physicists in Germany circa 1900,'' *Social Studies of Science* (1977) **7**, 329–66. Jungnickel and McCormmach, op. cit. (7).

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scholars have noted, British physics (or 'mixed mathematics') took a far more enthusiastic approach to mechanical modelling than did most German proponents.[18] Boltzmann himself would contrast

> the almost ethereally structured and crystal clear though colourless theory of elasticity in Kirchhoff's lectures with the crudely realistic account in Vol. 3 of Thomson's *Mathematical and Physical Papers* which concerns not ideally elastic bodies but steel, rubber, glue; or with the often childlike naiveté of the language of Maxwell, who in the midst of formulae mentions a really effective method for removing fat stains.[19]

Hermann von Helmholtz had offered the term *theoretische Physik* as a translation for the title of the German edition of William Thomson and P. G. Tait's *Treatise on Natural Philosophy* in 1871, perhaps, in part, to avoid any possible parallels with *Naturphilosophie* that a more direct translation might suggest. But such a close association did not seem so evidently reasonable at the turn of the century. Indeed, in an introduction to the German edition of a textbook written by the foremost wrangler-maker in Cambridge University, the Göttingen mathematician Felix Klein would go so far as to suggest that by 1898 one could speak of separate English and German intellectual traditions in mechanics. 'One can fully acknowledge the benefits that our German method may in this respect possess', Klein wrote, 'without overlooking the fact that the English methods possess alongside [ours], as a supplement, so to speak, their extraordinary significance. This in any case is the view from which we most particularly wish to recommend the work of [Edward] Routh to the German public.'[20] Rather than a simple description, Boltzmann's definition must thus be taken as a prescriptive statement, merely one attempt to shape the field in his own image. A similar point can be made about statements by Franz Neumann's acolytes Paul Volkmann and Woldemar Voigt, who in any case, according to Kathryn Olesko, found their 'ethos' in decline by the early years of the twentieth century.[21]

This disagreement over the nature of the field should not seem surprising. Theoretical physics was still a discipline under construction in the 1890s.[22] This disciplinary

18 On the 'Cambridge Programme' see P. M. Harman (ed.), *Wranglers and Physicists: Studies on Cambridge Physics in the Nineteenth Century*, Manchester, 1986. A. Warwick, *Masters of Theory: Cambridge and the Rise of Mathematical Physics*, Chicago, 2003.

19 L. Boltzmann, 'On the methods of theoretical physics (Orig. 1892)', in *Ludwig Boltzmann: Theoretical Physics and Philosophical Problems* (ed. B. McGuiness), Dordrecht, 1974, 11–12, n. 1.

20 'Vorwort' by F. Klein to E. J. Routh, *Die Dynamik der Systeme starrer Körper* (Leipzig, 1898), pp. iii–iv. Cf. D. E. Rowe, 'Klein, Hilbert, and the Göttingen mathematical tradition', *Osiris* (1989), 5, 186–203; Warwick, op. cit. (18), 252–3.

21 K. M. Olesko, *Physics as a Calling: Discipline and Practice in the Königsberg Seminar for Physics*, Cornell History of Science Series, Ithaca, 1991, 366–450.

22 The novelty of theoretical physics as a discipline, once a key part of the literature, has been challenged by the periodization offered in what is, on most issues, the definitive account, C. Jungnickel and R. McCormmach's *Intellectual Mastery of Nature*. Their defence of their dating of the beginning of the field to 1800, however, is weak, and depends on a problematic elision between theoretical physics and physical theory. If the two are assumed to mean the same thing, one wonders why one should stop in 1800, rather than with Newton's natural philosophy, an earlier contribution to 'physical theory'. The authors write, 'Although there had been a body of physical theories for centuries, in Ohm's day, the early nineteenth century, theoretical physics was not a specialized field of study, and it was not to emerge as one for a good part of the

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novelty, however, should shape the way in which the early twentieth-century field is to be understood. Rather than viewing the actions of men like Planck, Sommerfeld, Lorentz, Boltzmann, Volkmann and Voigt as a continuation of existing traditions or as the ongoing manifestation of institutional forms, their work must be seen as part of a process of creating these very traditions and of sculpting these very forms. Their efforts are to be interpreted in terms of the active, multiple and often conflicting constructions of content, discourse, method and style in theoretical physics in the two decades after 1900. Yet to speak of theoretical physics when it was in such a state of disciplinary, intellectual and institutional ambiguity presents an immediate problem. It is not clear to what one is referring when describing the field. The range of subjects pursued by major practitioners, from crystal physics (Voigt) to electrodynamics (Lorentz), from thermodynamics (Planck) to kinetic theory (Boltzmann), makes the field's perceived boundaries unclear. The fundamental disagreement over world views, whether mechanical, energetic or electromagnetic, reveals a near total lack of consensus about basic questions of content. One is left wondering whether describing 'theoretical physics' merely divides into the problem of characterizing each form of the subject as studied by each individual practitioner.[23]

century, until nearly the time of Einstein. *So, for much of the period covered by our study, our subject, strictly speaking, did not exist.* But of course it did, in a practical sense, right from the start. Major contributions to physical theory were made by Ohm and his contemporaries and by their successors throughout the nineteenth century. In the second half of the century, teaching positions were gradually created for theoretical physics … and these positions laid the foundation for the partial separation of physics into the fields of experimental physics and theoretical physics.' Jungnickel and McCormmach, op. cit. (7), 16; added emphasis. Forman has noted this peculiarity of the two-volume history in P. Forman, 'Review: intellectual mastery of nature', *Philosophy of Science* (1991), **58**, 130: 'volume 1 never does arrive at the announced subject, the history of *theoretical* physics, which scarcely existed as a separate intellectual, let alone institutional, enterprise before 1870'; original emphasis. E. Garber makes a related point, arguing that it is ahistorical to label G. S. Ohm's work 'physics'. She writes, 'To label Ohm as a physicist and his mathematical work as physics was to miss the point of what he was actually trying to accomplish. … Ohm's experimental results were physically important but his goal was not to produce a physical interpretation of those results in mathematical form.' E. Garber, *Language of Physics: The Calculus and the Development of Theoretical Physics in Europe, 1750–1914*, Boston, 1999, 159. In general, however, it should be stressed that Garber's desire to draw a sharp line between mathematical and theoretical physics seems overly dogmatic. In particular, her claim that no one 'in Germany used the terms mathematical and theoretical physics interchangeably or simultaneously' (167) is belied by the naming of chairs alone. Note, to take merely one example, that the chair created in 1883 for Woldemar Voigt at Göttingen was in 'theoretical (mathematical) physics'. See, more generally, K. M. Olesko, 'The emergence of theoretical physics in Germany: Franz Neumann and the Königsberg School of Physics, 1830–1890', Cornell University dissertation, 1980.

23 The ambiguity in terms of dominant and shared research areas would not last long. After 1900 theoretical physics can be increasingly defined by the interests of the growing number of its practitioners in what Buchwald has called 'microphysics'. J. Z. Buchwald, *From Maxwell to Microphysics: Aspects of Electromagnetic Theory in the Last Quarter of the Nineteenth Century*, Chicago and London, 1985; J. Z. Buchwald and A. Warwick, *Histories of the Electron: The Birth of Microphysics*, Cambridge, MA, 2001. Sommerfeld, for example, would be hired to the Munich chair in 1906 – after Lorentz and Boltzmann had both declined it – in large part due to his work on what he described in December of that year as the 'burning questions of electrons'. Sommerfeld to Lorentz, 12 December 1906, reproduced in M. Eckert and K. Märker (eds.), *Arnold Sommerfeld: Wissenschaftlicher Briefwechsel*, 2 vols., Berlin, Diepholz and München, 2000–4, i, 257–8. See also M. Eckert and W. Pricha, 'Boltzmann, Sommerfeld und die Berufungen auf die Lehrstühle für theoretische Physik in München und Wien, 1890–1914', *Mitteilungen der Österreichischen Gesellschaft für Geschichte der Naturwissenschaften* (1984), **4**, 101–19.

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A quest for commonality in terms of practices seems at first to lead to the same splintering. The distinction to be discussed in part two of this paper, between a 'physics of principles' and a 'physics of problems', will make it clear that different ways of *thinking about* theoretical physics have tied to them different ways of *doing* theoretical physics. Yet at a social level of practice, as opposed to the level of individual manual or intellectual practices, one can begin to discern a factor that does tie holders of chairs in theoretical physics not only to each other but also to a wider physics community. Perhaps paradoxically, it is precisely the fact that these practitioners of 'theoretical physics' did disagree with one another that expresses a specific form of social commonality. For these disagreements were public, carried out in articles, essays, lectures and debates. What was shared was a participation in an ongoing discussion over what it meant to do theoretical physics and over the character of the fundamental constituents of the physical world, the tools of their trade. This discourse chacterizes the discipline.

In a number of cases, these disagreements have been taken as evidence for the existence of a crisis. 'World-view antagonisms', Russell McCormmach has argued, 'reflected [physicists'] participation in the general crisis mentality of their class'.[24] My alternative explanation, that such antagonisms provide evidence for the social practice of theoretical physics, thus depends on whether crisis really existed. This question is considered below, largely by drawing on what is now a significant body of secondary literature on the subject, before discussing in more detail the implications of my proposed reimagining. The question is perhaps best broken into three. Was there, first, a crisis of the Kuhnian kind that preceded either or both of the quantum and relativity 'revolutions'? Second, in the absence of any specific subsequent revolution, was there nonetheless a 'crisis' of the mechanical world view in the face of substantial critique? Third, if evidence for such an intellectual crisis is lacking, can one nonetheless speak of a 'crisis' mentality induced by 'external' factors?

Arguments against a 'quantum crisis' prior to 1900 are of long standing. While acknowledging in 1962 the popular perception that such a crisis had existed, Martin Klein explicitly rejected the notion: 'As a matter of fact, there was no such crisis, or perhaps one should say there was no awareness of such a crisis.'[25] Kuhn himself argued for the crisis, but his conclusion was both anaemic and peculiar in terms of timing: 'The preceding crisis, to the extent that there was one, resulted from the difficulties in reconciling Planck's derivation with the tenets of classical physics.'[26] As a result, it will be noted, Kuhn seems to put the cart before the horse, by having Planck's 'revolutionary' step (however 'classical' it may have been in motivation) precede the

24 McCormmach, op. cit. (7), 16. For an excellent discussion of these debates see Jungnickel and McCormmach, op. cit. (15), 211–53.

25 M. J. Klein, 'Max Planck and the Beginnings of the Quantum Theory', *Archive for History of the Exact Sciences* (1962), **1**, 459–79, 459. Klein shows that Planck paid little attention to a speech Lord Kelvin delivered in 1900 on 'Nineteenth century clouds over the dynamical theory of heat and light', once taken to be evidence of a widespread crisis-sensibility over the questions raised by the equipartition theory and the problems of the ether. Proponents of the electromagnetic view of nature never believed in the applicability of this mechanical principle to what they saw as a fundamentally electromagnetic ether.

26 Kuhn, op. cit. (10), 245.

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crisis.[27] A more recent article has pushed the date for the 'crisis' even later, to around 1910.[28] The argument for a pre-relativity crisis has received some more support from historians. Pyenson, for example, has argued that the structure of the 'relativity revolution' is compatible with Kuhn's position, including 'a period of intense disagreement and confusion – the revolutionary break – before the establishment of a new scientific regime'.[29] Pyenson's argument, based on the prevalence of a discourse of revolution, is considered more from the standpoint of social history than technical content. At this latter level, Darrigol decisively dismisses the notion of a widespread feeling of crisis due to problems in electromagnetic theory or mechanics, limiting its application to Poincaré alone:

> The physicists who shared his endeavour to dematerialise the ether, for example Drude and Cohn, felt free to violate the general principles of mechanics. Those who, on the contrary, maintained a variety of mechanical reductionism, for example, Boltzmann and (to a lesser extent) Lorentz, treated the ether as part of the mechanical system and did not expect mechanical principles to apply to matter alone. Poincaré's was the only one to conjugate the general principles of mechanics and a mechanically irrelevant ether. At the turn of the century, he was the only one to detect a crisis and predict a major alteration of Lorentz's views. Other theorists saw nothing very wrong with Lorentz's theory and some of them imagined new promising developments.[30]

In this description, Poincaré is an isolated figure precisely because of his belief in crisis, while those around him either expected no consistency or, like proponents of the electromagnetic world view, saw the opportunity to proceed in novel directions, pursuing a programmatic commitment to found physics solely on electromagnetic concepts and principles. Wilhelm Wien's announcement of the programme in 1900 shows none of the nervousness about the future to be found in Poincaré's public statements.[31]

A similar argument can be made for the crisis of the mechanical world view. Viktor Jakob, Russell McCormmach's central character in *Night Thoughts of a Classical Physicist*, may have thought he remembered those years before the turn of the century

27 For a discussion of Kuhn's book on black-body theory see M. J. Klein, A. Shimony and T. J. Pinch, 'Paradigm lost? A review symposium', *Isis* (1979), **70**, 429–40.

28 J. Büttner, J. Renn and M. Schemmel, 'Exploring the limits of classical physics: Planck, Einstein, and the structure of a scientific revolution', *Max Planck Institute for the History of Science, Preprint Series* (2000), **150**.

29 Pyenson, op. cit. (7), 84.

30 O. Darrigol, 'The electrodynamic origins of relativity theory', *Historical Studies in the Physical and Biological Sciences* (1996), **26**, 241–312, 276–7. See also O. Darrigol, 'Henri Poincaré's criticism of fin-de-siècle electrodynamics', *Studies in History and Philosophy of Modern Physics* (1995), **26**, 1–44, 23, where he writes, 'at the close of the century Poincaré, and to a lesser extent Wien, seem to have been the only experts to worry about the fate of Newton's third law. In any case, Poincaré was the only one to perceive a major crisis in electromagnetic theory and to expect major modifications'.

31 W. Wien, 'Über die Möglichkeit einer elektromagnetischen Begründung der Mechanik', *Annalen der Physik* (1901), **5**, 501–13. On the electromagnetic world view see T. Hirosige, 'Origins of Lorentz' theory of electrons and the concept of the electromagnetic field', *Historical Studies in the Physical Sciences* (1969), **1**, 151–209; R. McCormmach, 'H. A. Lorentz and the electromagnetic view of nature', *Isis* (1970), **61**, 459–97; R. McCormmach, 'Einstein, Lorentz, and the electron theory', *Historical Studies in the Physical Sciences* (1970), **2**, 41–87; S. Seth, 'Quantum theory and the electromagnetic world-view', *Historical Studies in the Physical and Biological Sciences* (2004), **35**, 67–93.

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as 'bewildering' for physicists but neither he, nor most others, fictional or not, left much evidence of it.[32] Rather than the confusion and negativity of crisis, Pyenson points to 'a climate of revolutionary optimism'.[33] Many of those who supported the mechanical *Weltanschauung*, like Gustav Kirchhoff and Heinrich Hertz, responded to critiques not with confusion or dismay but rather by reformulating mechanics along the lines of what John Heilbron has called 'descriptionism'. This advocated a 'tolerance, even encouragement, of diverse, partial or complementary approaches to physical theory', and recommended 'the withdrawal from big questions and relaxations of claims to knowledge of truth'.[34] Others, like Planck, who eschewed descriptionism but also reacted vehemently against the implication drawn from Boltzmann's statistical mechanics that the second law of thermodynamics was statistical in character, began increasingly to advocate the foundation of physics on principles understood as independent of any material substrate.[35]

The only figure I can find who spoke of something like a crisis of mechanics prior to 1900, Ludwig Boltzmann, would also argue for his own isolation. In an address to the meeting of natural scientists in Munich, 'On the development of the methods of theoretical physics in recent times', Boltzmann told his audience that when he began his own career in the field 'the task of physics seemed confined for ever to ascertaining the law of action of the force acting at a distance between any two atoms and then to integrating the equations that followed from all these interactions under appropriate initial conditions'.

> How many things have changed since then! Indeed, when I look back on all these developments and revolutions I feel like a monument of ancient scientific memories. I would go further and say I am the only one left who still grasped the old doctrines with unreserved enthusiasm – at any rate, I am the only one who still fights for them as far as he can …
>
> I therefore present myself to you as a reactionary, one who has stayed behind and remains enthusiastic for the old classical doctrines as against the men of today …[36]

32 R. McCormmach, *Night Thoughts of a Classical Physicist*, Cambridge, MA, 1991.

33 Pyenson, op. cit. (7), 65.

34 Heilbron, op. cit. (7), 51. The question of whether descriptionism can be understood as a response to crisis is considered below. Porter (op. cit. (7)) makes the argument for seeing descriptionism as a positive and liberating move, not merely a defensive one. On reformulations of mechanics in the late nineteenth century see e.g. M. J. Klein, 'Mechanical explanation at the end of the nineteenth century', *Centaurus* (1972), **17**, 58–82.

35 M. Planck, 'Antrittsrede zur Aufnahme in die Akademie vom 28. Juni 1894', in *Physikalische Abhandlungen und Vorträge*, Braunschweig, 1958, 1–5. M. Planck, 'Das Einheit des physikalischen Weltbildes (Lecture, 9 December 1908, at the University of Leiden)', *Physikalische Zeitschrift* (1909), **10**, 62–75. J. L. Heilbron, *The Dilemmas of an Upright Man: Max Planck as Spokesman for German Science*, Berkeley, 1986.

36 L. Boltzmann, 'On the development of the methods of modern theoretical physics in recent times (Orig. 1899)', in *Ludwig Boltzmann: Theoretical Physics and Philosophical Problems* (ed. B. McGuiness), Dordrecht, 1974, 82. Cf. Pyenson, op. cit. (7), 64–5. For a discussion of Boltzmann's use of the term 'classical' see Richard Staley, 'On the co-creation of classical and modern physics', *Isis* (2005), **96**, 530–58. On Boltzmann see E. Broda, *Ludwig Boltzmann: Man, Physicist, Philosopher* (tr. L. Gay and E. Broda), Woodbridge, CT, 1983; J. Blackmore (ed.), *Ludwig Boltzmann: His Later Life and Philosophy, 1900–1906*, 2 vols., Dordrecht, Boston and London, 1995, i; A. D. Wilson, 'Mental representation and scientific knowledge: Boltzmann's *Bild* theory of knowledge in historical context', *Physis* (1991) **28**, 769–95. For a recent description of Boltzmann's life and philosophy (upon which a sizeable literature now exists) see the discussion

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As with the case of Poincaré, whose devotion to a 'physics of principles' Darrigol uses to explain an apparently idiosyncratic belief in crisis, Boltzmann's sense of embattlement flowed from elements peculiar to his intellectual outlook. In particular, few other defenders of the mechanical *Weltanschauung* saw it as so all-encompassing a world view as did Boltzmann, or at least expressed themselves as openly as he did on this matter. Many might agree with the statement in his 1900 inaugural lecture at the University of Leipzig that analytical mechanics was the 'entrance gate' through which one steps into the 'vast and imposing edifice of theoretical physics',[37] and even possibly agree with his description of Darwin's theory of natural selection as 'that most splendid mechanical theory in the field of biology'.[38] Yet one suspects that few would be as enthusiastic about the claim that the behaviour of crowds could be explained through the 'mechanics of psychology', or as convinced that the student who wondered at the peculiarities of the 'countless rules of propriety and forms of politeness that seem, in part, so unnatural and forced that they seem absurd and ridiculous to that unprejudiced view often called reason' had simply forgotten that even social arrangements are controlled by mechanical laws.[39] As for Boltzmann's suggestion that even the power structures of imperial Germany could be justified in mechanical terms, one begins to understand that the title of Ostwald's critique of mechanics and defence of energetics at Lübeck in 1895, 'The overthrow of scientific materialism', was perhaps not overstated.[40] 'Bismarck', Boltzmann wrote, 'could see through his political opponents' thinking as clearly as a mechanical engineer sees through the gears of his machine, so that he knew exactly how to move them to act as he wished just as the machinist knows what lever to push'. He continued,

> The enthusiastic love of freedom of men like Cato, Brutus, and Verrina arises from feelings that had grown in their souls by purely mechanical causes and we again can explain mechanically that we live contentedly in a well-ordered monarchical state and yet like to see our sons reading Plutarch and Schiller and draw inspiration from the words and deeds of enthusiastic republicans. This too we cannot alter, but we learn to understand and bear it. The god by whose grace kings rule is the fundamental law of mechanics.[41]

With a world understood so completely in mechanistic terms, one can begin to sympathize with Boltzmann as even his supporters appeared to turn away or sought to turn explanation into description.[42]

and references in C. Cercignani, *Ludwig Boltzmann: The Man Who Trusted Atoms*, Oxford, New York and Melbourne, 1998.

37 L. Boltzmann, 'On the principles of mechanics (Orig. 1900)', in *Ludwig Boltzmann: Theoretical Physics and Philosophical Problems* (ed. B. McGuiness), Dordrecht, 1974, 129.

38 In *Ludwig Boltzmann: Theoretical Physics and Philosophical Problems* (ed. B. McGuiness), Dordrecht, 1974, 133.

39 In *Ludwig Boltzmann: Theoretical Physics and Philosophical Problems* (ed. B. McGuiness), Dordrecht, 1974, 139.

40 W. Ostwald, 'Die Überwindung des Wissenschaftlichen Materialismus', in *idem*, *Abhandlungen und Vorträge; allgemeinen Inhaltes, 1887–1903*, Leipzig, 1904.

41 Boltzmann, op. cit. (37), 136–7.

42 Boltzmann's journey into the realm of the sociopolitical in this last statement may have been a response to Ernst Mach's critique of mechanics, written in a similar register. In an essay on the history of the principle

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If intellectual arguments internal to physics seem to have been inadequate to engender a sense of crisis at a broad level within the German physics community, one might nonetheless expect that certain 'external' factors might help in producing the requisite feelings of dismay, bafflement and confusion. Such an externalist argument is offered by Heilbron in his article on 'Fin-de-siècle physics':

> The increasing tempo of civilized life and the squalor of the cities in which it was played out caused legitimate concern about uncontrolled technological progress and reservations about the sciences that were supposed to be responsible for it. *To respond to these worries, and to fit their new prominence, physicists adjusted the image and even the substance of their discipline.* Adjustment also appeared necessary to accommodate both recent advances and persistent difficulties in physical theory. The physicists of the *fin-de-siècle* sought to redefine their professional objectives so as simultaneously to achieve internal consensus and to secure their place in the wider society.[43]

The change in the 'image and even the substance' of physics to which Heilbron alludes turns out to be the growth of a 'wide descriptionist consensus' by around 1900. Rather than seeing debates about the methodological and ontological status of mechanics as themselves evidence of crisis, therefore, Heilbron's claim is that descriptionist methodologies are the response to a crisis induced by factors outside the discipline. In particular, he emphasizes the fact that while the *discourse* of descriptionism was strong, few scientists carried this anti-realist mode of thought into their research. That is to say, the rhetoric and the practices of science did not match. While Heilbron admits that part of this disjunction was due to difficulties with the mechanical world view, he rejects the notion that this could be responsible for all the 'constant sermonizing in favour of descriptionism'.[44] The cause, then, must be sought not within physics, but in matters 'external' to it.

The idea that Western society was in a state of decline, of 'degeneracy', was endemic to Europe at the close of the century.[45] From a cultural historical standpoint, there is a

of energy, written in 1872, Mach noted that two things had puzzled him since childhood. 'In the first place, I did not understand how people could like letting themselves be ruled by a king even for a minute. The second difficulty was that which Lessing so deliciously put into an epigram, which may be roughly rendered: "One thing I've often thought is queer,"/Said Jack to Ted, "the which is/That wealthy folk upon our sphere,/Alone possess the riches." The many fruitless attempts of my mother to help me over these two problems must have led her to form a very poor opinion of my intelligence'. E. Mach, *History and Root of the Principle of the Conservation of Energy* (tr. P. E. B. Jourdain), Chicago, 1911, 15, 46. The relation between these questions and the intellectual hegemony of mechanics in the contemporary world was provided by Mach's assertion that both the social situations that would puzzle us in youth and the propositions (like those of mechanics) taught us in school could only fully be understood in historical terms. No God guarantees the truth of our particular beliefs in the make-up of the physical (and, by implication, the social) world. Our understandings, as Mach would repeat like a mantra through the text, are 'purely historical, accidental, and conventional. Neither the mechanical world-view nor the rule of kings could be justified in any manner other than by an appeal to convention'.

43 Heilbron, op. cit. (7), 51. My italics.

44 Heilbron, op. cit. (7), 57.

45 The most famous contemporary chronicler of the 'fact' of European degeneracy was Max Nordau, whose book *Entartung*, Berlin, 1892, was dedicated to the criminal psychologist and physiologist Caesar Lombroso. In the dedication Nordau notes that 'Degenerates are not always criminals, prostitutes, anarchists, and pronounced lunatics; they are often authors and artists. These, however, manifest the same mental

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kind of logic in assuming that science, as a social construct, should participate in this collective *mal de siècle* especially since, according to Heilbron, it was judged a prime contributor both to spiritual and physical degeneration. Such an assumption seems to be borne out in France where several scholars have described a discourse around the 'bankruptcy of science', its failure to deliver on the promises of Comtean positivism.[46] Yet in general German physicists had been loath to embrace the excesses of Comte's philosophy, so one would expect minimal backlash on these grounds. Such an expectation is confirmed by Hiebert, who focuses more on the German case and describes the *fin de siècle*, in terms diametrically opposed to Heilbron's, as exhibiting a 'riptide of enthusiasm for the pursuit of science in the midst of perceived and, one might add, fabricated *fin-de-siècle* degeneration in morality, ethics, belief systems, and literature'.[47] Further to emphasize the difference between the German and French cases, one may add that France had lost the war against Prussia in 1870; this might offer a parallel with the example offered by the Forman thesis, which Heilbron draws upon as a model. Far from the situation in which the physical sciences were blamed for Germany's disastrous defeat after 1918, in the years after imperial unification physics was seen as directly responsible for the nation's military and economic power. The force of the backlash against physics that Forman describes indicates how closely the physical and mathematical sciences were seen to be linked in the minds of the public to a German military–industrial framework. External arguments would seem to suggest that around 1900 German physicists would be exuberant, rather than crisis-ridden.[48]

The advantage gained by abandoning the notion of a general crisis in *fin de siècle* physics is far more than simply the correction of the historical record. In the extensive arguments around the question of the appropriate foundations for physics that occurred in this period one is no longer left with debates 'symbolic' of some deeper yet future transition, but rather with discussions indicative of contemporary communal interaction. Take as example Sommerfeld's description of the debates in Lübeck in 1895

characteristics, and for the most part the same somatic features, as the members of the above-mentioned anthropological family who satisfy their unhealthy impulses with the knife of the assassin or the bomb of the dynamiter, instead of which pen and pencil.' M. Nordau, *Degeneration*, Lincoln, NE, 1993, p. v. Nonetheless, though almost everyone gets called a degenerate at some point by Nordau, scientists are, in general, excluded from his opprobrium; ibid., 105–11.

46 H. W. Paul, 'The debate over the bankruptcy of science in 1895', *French Historical Studies* (1968), 5, 299–329, 299–300: 'some intellectuals, infatuated with positivism, which in its extreme form tackled even problems connected with ultimate origins and final ends, promised too many results, especially in moral, social and religious areas'. See also, on the 'social-idealist' reaction in France to the 'bankruptcy of science', M. J. Nye, *Molecular Reality: A Perspective on the Scientific Work of Jean Perrin*, New York, 1972.

47 E. N. Hiebert, 'The transformation of physics', in *Fin-de-siècle and its Legacy* (ed. Mikulás Teich and Roy Porter), Cambridge, 1990, 235–53, 243.

48 Certainly funding allocations would add to this feeling. In the years from 1882 to 1908, when Friedrich Althoff was in control of universities within the ministry, regular budget allocations to Prussian universities climbed from 5.6 to 12.25 million marks. F. K. Ringer, *The Decline of the German Mandarins: The German Academic Community, 1890–1933*, Cambridge, MA, 1969, 51. From 1872 to 1915 twenty-three physics institutes were built, of which more than half were established in the 1880s and 1890s, and the half-century from the mid-1860s to the beginning of the Great War marked an astonishing tenfold increase in the number of students studying the natural sciences. D. Cahan, *An Institute for an Empire: The Physikalisch-Technische Reichsanstalt, 1871–1918*, Cambridge and New York, 1989.

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over 'energetics', the notion that all of physics could be explained not in mechanical terms, but in terms of the exchanges of energy:

> The paper on 'Energetik' was given by Helm from Dresden; behind him stood Wilhelm Ostwald, behind both the philosophy of Ernst Mach, who was not present. The opponent was Boltzmann, seconded by Felix Klein. Both externally and internally, the battle between Boltzmann and Ostwald resembled the battle of the bull with the supple fighter. However, this time the bull was victorious over the torero in spite of the latter's artful combat. The arguments of Boltzmann carried the day. We, the young mathematicians of the time, were all on the side of Boltzmann; it was entirely obvious to us that one could not possibly deduce the equation of motion for even a single mass-point – let alone for a system with many degrees of freedom – from the single energy equation ...[49]

What is significant here is less the specific validity of Sommerfeld's account written almost fifty years after the events, more the insight it offers into the community-wide discussion concerning the appropriate foundations for physical knowledge. If the discussion of problems with the mechanical world view may seem at times like an episode in the history of ideas, with abstracted thinkers engaging with the arguments of philosophical foes, the 'stiff fight' (as Helm put it) at Lübeck makes clear that the engagement was in fact both personal and communal. The papers themselves were public and widely attended. The list of participants reads like a *Who's Who* of those later to be deeply involved with the development of theoretical physics in the early twentieth century. Arguments over the validity of phenomenology or of the atomic theory involved a similarly large cast of characters with a similar level of engagement. Contest over world views became so common that questions over the validity of the term itself became part of the discourse. While Paul Volkmann sought to make a sharp distinction between a legitimate scientific term, *Weltbild* ('world picture'), and a loaded philosophical one, *Weltanschauung* ('world view'), Planck would be explicit that world pictures must include both scientific and extra-scientific elements. Woldemar Voigt similarly argued that the world views of physicists were strongly akin to the working hypotheses employed by philosophers and theologians.[50]

That these debates represent not crisis but interaction is not entirely surprising. As Porter has phrased it, it is 'more plausible to point to an efflorescence of competing conceptions of the proper foundations of physics than to a despair about finding foundations at all'. But one may advance from this reading to suggest how to understand the significance of this 'efflorescence', in a way that goes beyond the philosophy of physics and towards its sociology. Porter claims that such debates were 'scarcely unprecedented', arguing that 'descriptionism ... surfaced repeatedly in physics since

49 Sommerfeld quoted in Abraham Pais, '*Subtle is the Lord ...' The Science and the Life of Albert Einstein*, Oxford, 1982, 83. On the energetics debate see E. N. Hiebert, 'The energetics controversy and the new thermodynamics', in *Perspectives in the History of Science and Technology* (ed. D. H. D. Roller), Norman, OK, 1971, 67–92; R. J. Deltete, 'The energetics controversy in late nineteenth-century Germany: Helm, Ostwald, and their critics', Yale University dissertation, 1983; A. Leegwater, 'The development of Wilhelm Ostwald's chemical energetics', *Centaurus* (1986), **29**, 314–37. C. Hakfoort, 'Wilhelm Ostwald's energeticist world-view and the history of scientism', *Annals of Science* (1992), **49**, 525–44. Jungnickel and McCormmach, op. cit. (15), 217–27; R. J. Deltete, 'Gibbs and the energeticists', in *No Truth Except in the Details* (ed. A. J. Kox and D. M. Siegel), Dordrecht, 1995, 135–69.

50 R. McCormmach, 'Editor's foreword', *Historical Studies in the Physical Sciences* (1975), **6**, 11–14.

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the Middle Ages'.[51] Yet there was more to the debates than descriptionism. Arguments phrased in terms of competing world views are of more recent provenance. According to Heidegger they are emblematic of the modern age itself.[52] Several considerations suggest that at the social level these *fin de siècle* debates were more than business as usual. First, it was not a particular position, whether descriptionist or realist, electromagnetic or mechanical, that achieved majority status in 1900, but the presence of the debates themselves. It was not for example simply phenomenology itself but debates about phenomenology that made up communal practice for a significant proportion of the physics community. Perhaps more importantly, disagreements over the issue were not boundary-making but internal to the community. A plurality of positions shaped this community; it did not splinter, nor was consensus compelled. Furthermore, these debates and closely related ones would continue through at least the first three decades of the twentieth century, implicated in the birth and reception both of relativity and of matrix mechanics. Last, such debates are contemporaneous with the rise of the new discipline of theoretical physics, a discipline where these arguments would increasingly be conducted after 1900. Whereas before 1900 theoreticians made up a significant proportion of those involved in arguments over appropriate methodological and ontological foundations for physics, they would come to dominate such discussions only a decade later. When Planck again took up cudgels against Mach in 1908, Boltzmann's former foe was now deemed insufficiently knowledgeable of aspects of heat theory or mechanics to make anything more than a philosophical contribution.[53] It is hard to think of any experimentalist or member of another discipline who could rival Planck or Einstein for their authority on foundational questions by 1911, when Adolf von Harnack identified them as twin refutations of the charge that the present generation had no philosophers.[54]

It follows that before 1900 the communal social practice of theoretical physics, the feature its practitioners held in common, was at the discursive level nothing other than the questioning of the foundations of physics. To engage in the debate was not to enter into a period of crisis, it was to interact with one's colleagues and practise one's craft. The fact that debates around foundations continue within the field at particular moments throughout much of the twentieth century should thus be seen as drawing upon and responding to an aspect of the discipline that nearly defines its being. Where crisis is invoked as part of these debates our task is not to take this as simply descriptive but to ask why this foundational questioning is constructed as more dire than any other.[55]

51 Porter, op. cit. (7), 129, 128.

52 M. Heidegger, 'The age of the world-picture', in *The Question Concerning Technology and Other Essays* (ed. W. Lovitt), New York, 1977.

53 Planck, 'Das Einheit', op. cit. (35). M. Planck, 'Zur Machschen Theorie der physikalischen Erkenntnis: Eine Erwiderung', *Physikalische Zeitschrift* (1910), **11**, 1186–90.

54 Heilbron, op. cit. (35), 59–60.

55 For two cases where crisis has already been interrogated in this way see Richard Staley's analysis of Planck's invocation of the 'relativity revolution' as an analogue of the Copernican revolution. R. Staley, 'On the histories of relativity: the propagation and elaboration of relativity theory in participant histories in Germany, 1905–1911', *Isis* (1998), **89**, 285–90; T. Pinch, *Confronting Nature: The Sociology of Solar-Neutrino Detection*, Dordrecht, 1986, 146–51.

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Case two: the older quantum theory

The first example given here was of a crisis that was not in fact a crisis except for a few essentially isolated individuals. The second is now of a crisis that was only such for part of a community, albeit a significant part. The two are connected. This section takes up the story of German theoretical physics after a significant hiatus, more than two decades during which the field reached a certain maturity. Much of my own work has hitherto been focused on this intervening period.[56] I have characterized aspects of the development of German theoretical physics in this period in terms of a dichotomy between two 'kinds' of theoretical physics, distinguished by their methods, world views, discourse and techniques: 'the physics of principles' and 'the physics of problems'. The physics of principles, which had as its most prominent proponents Poincaré, Planck, Einstein and Bohr, can be seen as the most significant continuation of and response to *fin de siècle* debates about the foundations of physics, offering in place of any particular materialist ontology a physics based on generalized principles.[57] The physics of problems both was newer, beginning essentially with Sommerfeld's move to Munich in 1906, and largely avoided the questions of foundations. Sommerfeld once quipped to Einstein that 'I can only further the engineering of the quantum [*die Technik der Quanten*]. You would have to make its philosophy'.[58] Others had the same

56 S. Seth, 'Principles and problems: constructions of theoretical physics in Germany, 1890–1918', Princeton, NJ, 2003.

57 On Poincaré's 'physics of principles' see e.g. J. Giedymin, 'The physics of the principles and its philosophy: Hamilton, Poincaré and Ramsey', in *idem, Science and Convention: Essays on Henri Poincaré's Philosophy of Science and the Conventionalist Tradition*, Oxford, 1982, 42–89; Darrigol, 'Henri Poincaré's criticism', op. cit. (30). On Planck's philosophy and the importance of his engagement with Mach see S. Goldberg, 'Max Planck's philosophy of nature and his elaboration of the special theory of relativity', *Historical Studies in the Physical Sciences* (1976), **7**, 125–60. E. N. Hiebert, 'The conception of thermodynamics in the scientific thought of Mach and Planck, in *Wissenschaftlicher Bericht Nr. 5/68*. Heilbron, op. cit. (35). There exists a vast literature on the importance of principles, and of Mach's philosophy, to Einstein. For an introduction see G. Holton, 'Mach, Einstein, and the search for reality', in *idem*, *Thematic Origins of Scientific Thought*, Cambridge, MA, 1973; J. Stachel, *Einstein from 'B' to 'Z'*, Boston, Basel, Berlin, 2002; and the first two volumes of P. Galison, M. Gordin and D. Kaiser (eds.), *Science and Society: The History of Modern Physical Science in the Twentieth Century*, 4 vols., New York and London, 2001. Einstein's first explicit enunciation of his famous principle theory/constructive theory distinction is to be found in Albert Einstein, 'What is the theory of relativity?' in *The Collected Papers of Albert Einstein: English Translation*, Princeton, NJ and Oxford, 2002, 100–5. For the importance of the Danish philosopher Harald Høffding's critique of late nineteenth-century methodologies of science on Bohr's philosophy of physics see J. Faye, 'The influence of Harald Høffding's philosophy on Niels Bohr's interpretation of quantum mechanics', *Danish Yearbook of Philosophy* (1979), **16**, 37–72. M. Norton Wise, 'How do sums count? On the cultural origins of statistical causality', in *The Probabilistic Revolution: Ideas in History* (ed. L. Krüger, L. J. Daston and M. Heidelberger), Cambridge, MA, 1989, 395–425. J. Faye, *Niels Bohr: His Heritage and Legacy*, Dordrecht, 1991. D. Kaiser, 'More roots of complementarity: Kantian aspects and influences', *Studies in History and Philosophy of Science* (1992), **23**, 213–39.

58 Sommerfeld to Einstein, 11 January 1922, document 50 in M. Eckert and K. Märker (eds.), *Arnold Sommerfeld: Wissenschaftlicher Briefwechsel*, 2 vols., Berlin, Diepholz and München, 2000–4, ii, 110–11. For biographical data on Sommerfeld see A. Sommerfeld, 'Autobiographische Skizze [Incl. a short addendum by Fritz Bopp]', in *Arnold Sommerfeld: Gesammelte Schriften* (ed. F. Sauter), 4 vols., Braunschweig, 1968, iv, 672–82, and M. Born, 'Arnold Johannes Wilhelm Sommerfeld', *Obituary Notices of the Fellows of the Royal Society* (1952), **8**, 275–96. Most other short accounts tend to rely on these for information about Sommerfeld's early life. Of book-length biographies see U. Benz, *Arnold Sommerfeld: Lehrer und Forscher an der Schwelle*

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impression. The Oxford physicist Frederick Lindemann wrote to Einstein in 1933 that 'I have the impression that anyone trained by Sommerfeld is the sort of man who can work out a problem and get an answer, which is what we really need at Oxford, rather than the more abstract type who would spend his time disputing with the philosophers'.[59]

While Planck, for example, promoted a practice of theoretical physics devoted to abstract, de-anthropomorphized, de-historicized, 'pure' principles, Sommerfeld focused on specific problems, drawing these from a variety of sources including six years spent teaching at an engineering college (*technische Hochschule*), often emphasizing questions of economic or technological benefit.[60] Where the physics of principles provided, and indeed provides, the dominant discourse of the new discipline, Sommerfeld's newer theoretical physics would supply the lion's share of its younger practitioners. He trained two generations of students, including at least eight Nobel Prize-winners. Indicative of the fact that theoretical physics had begun to coalesce around these two figures is a letter from the publisher S. Hirzel to Sommerfeld in 1909 telling him of recent travels which brought Hirzel into contact with 'various physicists at universities and *technische Hochschulen*'.[61] From all sides, Hirzel claimed, he had heard talk of 'the desire and the need' for a 'short, new, modern textbook of theoretical physics'.[62] Since producing such a text would be too time-consuming for one man, the publisher suggested sharing the task between two. He proposed that Sommerfeld take up the authorship together with Planck. The latter had just written to Hirzel agreeing that a 'new textbook written in a modern theoretical physical spirit' was thoroughly necessary. He had reacted with enthusiasm to the prospect of working together with Sommerfeld and expressed his dismay when Sommerfeld declined the offer. 'It is certainly a pity', wrote Planck, 'that you do not want to collaborate on a textbook

zum Atomzeitalter, 1868–1951, Stuttgart, 1975. More recent and detailed is M. Eckert, *Die Atomphysiker: Eine Geschichte der theoretischen Physik am Beispiel der Sommerfeldschule*, Braunschweig, 1993. See also the articles in both volumes of Eckert and Märker, op. cit. (23).

59 Cited in W. Moore, *Schrödinger: Life and Thought*, Cambridge, 1992, 269. My thanks to David Kaiser for bringing this quotation to my attention.

60 On Sommerfeld's work during his time in Aachen see (as well as the references above) A. Hermann, 'Der Brückenschlag zwischen Mathematik und Technik', *Physikalische Blätter* (1967), **23**, 442–9; M. Eckert, 'Propaganda in science: Sommerfeld and the spread of the electron theory of metals', *Historical Studies in the Physical and Biological Sciences* (1986), **17**, 191–233, 192–6.

61 S. Hirzel to A. Sommerfeld, 15 February 1909, in Eckert and Märker, op. cit. (23), i, 353–4.

62 In asking for a short, new and especially modern textbook, Hirzel may have had in mind the relative failure of a textbook written by the Göttingen theoretician Woldemar Voigt, in the mid-1890s. Jungnickel and McCormmach, op. cit. (15), 123–4, note, 'In 1895 and 1896, Voigt published in two volumes his *Kompendium der theoretischen Physik*, the express purpose of which was to present a unified view of the whole field of theoretical physics with a physical, as opposed to a mathematical emphasis throughout. There was no German textbook like it at the time.' Yet Voigt was soon to regret all his hard work. 'Voigt's compendium was difficult, dense, and, in Voigt's eyes, something of a failure. He came to see that he had tried to put too much in two volumes, to present and connect the whole of theoretical physics "*at one stroke*". He had composed the book too fast, which resulted in errors and in a case of nervous exhaustion. Then the timing could not have been worse; he soon considered revising the text, for it had come out just before the penetration into physics of the electron theory, vector analysis, and the Zeeman effect, which influenced great parts of optics. "So the book was obsolete almost with its appearance! – and the great work and joy I have invested in it almost make me sorry now"'.

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for theoretical physics. I had just been thinking that we would, in a certain sense, complement each other well.'[63]

If the Sommerfeld school, as it became known, constitutes a reasonably coherent locus of research, differences in the usage of the term 'principle' among Planck, Einstein and especially Bohr may seem to disallow any common description, in spite of overall similarities. Yet contemporaries did not see matters this way, as von Harnack's description would imply. In 1918 Max Born suggested that both Planck and Einstein were deserving of the Nobel Prize, noting their shared efforts to deepen the foundations of physics:

> Besides an immense experimental progress, contemporary physics shows a clear attempt towards deepening foundations, to acquire universal knowledge [*universeller Erkenntnisse*]. The two main representatives of this direction, which could be described as philosophical, are Albert Einstein and Max Planck. Einstein's theory of relativity and Planck's quantum theory signify radical changes in the domain of science that have rarely taken place in a more colossal and promising fashion.[64]

Continuing with the modest claim that he could not speak to the question of which of the two discoveries was the greater or the more important, Born nonetheless promoted Planck as the more deserving for that year, 'as the older of the two researchers and as the one whose results have been of the greatest importance for the progress in experiments'.[65]

Born was perhaps not wrong to emphasize Planck's age. Sixty-two when he received the delayed 1918 Nobel Prize in 1920, his years of greatest productivity were behind him. In 1926 he announced his retirement; the faculty nominated Sommerfeld as his successor. After Sommerfeld declined, the call was accepted by Erwin Schrödinger, second on the list. Yet it might be said that even prior to his official retirement Planck had already been replaced. The years after the war saw him maintain and even cement his socio-disciplinary position as 'the dean and definition of theoretical physics in Germany', in Heilbron's felicitous words.[66] But in terms of his role in the development of the quantum theory it was Bohr who came ever more to stand as the principal alternative to what remained Sommerfeld's powerful Munich school. As a shift highly symbolic of the passing of a disciplinary baton, one may note the manner in which Born's understanding of the field of theoretical physics changed in the decade after 1918. Maintaining his belief in the existence of a 'philosophical' direction in the development of modern physics in a speech delivered in 1928 on the occasion of Sommerfeld's sixtieth birthday, Born now contrasted this approach directly with Sommerfeld's own. At the same time one of Born's twin exemplars of philosopher–physicists had been replaced. 'Physics as a field of application for philosophical principles', he wrote,

63 Max Planck to Arnold Sommerfeld, 24 February 1909 in Eckert and Märker, op. cit. (58), i, 355.

64 Born cited in S. Sigurdsson, 'Hermann Weyl, mathematics and physics, 1900–1927', Harvard University dissertation, 1991, 80.

65 Sigurdsson, op. cit. (64).

66 Heilbron, op. cit. (35), 7. Heilbron's biography is, in general, the best source for material on Planck's professional and intellectual activities post-1918.

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> as Einstein and Bohr promote [*treiben*] it, is foreign to Sommerfeld's basic nature; if in spite of this several of his most recent and most significant students have achieved great things in the study of foundations, this must, in so far as, in general, an external impetus should be sought, be due to later influences, particularly to contact with Bohr.[67]

The opposition between Bohr's and Sommerfeld's approaches to physics, between general principles and specific problems, would be a defining aspect of work on the 'older quantum theory'. This intra-communal distinction between sets of physicists who would describe the progress of their field in terms either of the formulation of principles or of the solving of problems is key for understanding the different ways in which crisis was or was not mobilized by each group.

As the name would suggest, 'the older quantum theory' is a *post hoc* ascription, a somewhat artificial designation for the period after the introduction of Bohr's atomic model in 1913 and before the invention of the two new forms of quantum mechanics in 1925 and 1926. Heisenberg, Born and Jordan's matrix mechanics and Schrödinger's wave mechanics offered a largely self-consistent set of techniques to replace what Jammer perhaps somewhat unjustly called 'a lamentable hodgepodge of hypotheses, principles, theorems, and computational recipes'.[68] In particular, wave mechanics, with its far more familiar mathematical methods, was taken up with astonishing rapidity. Hans Bethe, Sommerfeld's student in the mid-1920s, remembered the enthusiasm with which members of the Munich school adopted what they saw as an almost miraculous new means of solving previously intractable problems:

> In the beginning they were just interested that you could now do all this, and one of the fascinating things was the Stark effect – that one could *do* the Stark effect and *do* the intensities ... I think this first seminar was simply fascinated by the fact that it now worked.[69]

By contrast matrix mechanics offered perhaps a greater conceptual clarity but was ill-suited to the rapid solution of problems. As Sommerfeld wrote in 1928,

> We need here only to consider the Schrödinger form of the new theory, because it has the closer relationship with experiment and is more suitable for practical calculational manipulation. But we want to emphasize that the Heisenberg theory factually agrees with that of Schrödinger and that it can claim a high knowledge-theoretical interest.[70]

Bethe put the same point more pithily: 'Sommerfeld said, "Well, of course we really believe that Heisenberg knows better about the physics, but we calculate with Schrödinger."'[71]

67 Max Born, 'Sommerfeld als Begründer einer Schule, *Naturwissenschaften* (1928), **16**, 1035–6.

68 M. Jammer, *The Conceptual Development of Quantum Mechanics*, New York, St Louis, San Francisco, Toronto, London and Sydney, 1966, 196.

69 T. S. Kuhn, 'Interview with Hans Bethe, 01/17/1964', Archive for the History of Quantum Physics (hereafter AHQP), 9; original emphasis.

70 A. Sommerfeld, 'Zur Frage nach der Bedeutung der Atommodelle', *Zeitschrift für Elektrochemie und angewandte physikalische Chemie* (1928), **34**, 426–30. Reproduced in *Arnold Sommerfeld: Gesammelte Schriften* (ed. F. Sauter), 4 vols., Braunschweig, 1968, iii, 845–9, 847.

71 Kuhn, op. cit. (69), 9. Cf. Eckert, op. cit. (60), 205–8.

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If the end-points of the old quantum theory are relatively clear, characterizations of intervening moments are less so. Particularly fraught is the question of how to understand the years immediately prior to 1925. The fact that the invention of quantum mechanics has been cast as a scientific revolution has tended to shape depictions of events both before and after. Revolutions cannot proceed entirely smoothly but are defined as much by those who deny as by those who support them, hence a historical narrative in which major figures hold out against the most radical suggestions of the new theories. Einstein's refusal to admit that God plays dice is constantly cited; the story of Schrödinger driven to his sickbed by arguments with Bohr becomes a narrative set-piece. Revolutions also cannot be spontaneous events. They happen when situations reach their most dire condition and must function both as a break with as well as a solution to the problems of the past. Revolutions must also be resolutions.

The period just before the rise of matrix mechanics has thus been labelled a time of crisis. In one sense, the label is less problematic here than for the period around 1900. Few physicists spoke of a crisis in the *fin de siècle*. A great many, as Paul Forman showed, spoke of crisis during the early years of the Weimar Republic.[72] There was a self-identification of the problems that caused such a critical outlook. In early 1923 Born and Heisenberg completed a systematic treatment of the helium spectrum on the basis of perturbation theory, a treatment that revealed basic contradictions with experimental data and with predictions made on the basis on Bohr's correspondence principle. Born called the result a 'catastrophe', while Bohr claimed it as 'evidence of the inadequacy of the present basis of the quantum theory'.[73] Pauli, who had written to Sommerfeld in 1924 concerning the 'fundamental crisis' in which ideas of atomic models found themselves, located another of the principal fault lines of the modern physics in the persistent inability to explain the anomalous Zeeman effect. It was there, he noted, that one could see 'how deep-seated is the failure of the theoretical principles known till now'.[74] The problems with helium and the Zeeman effect thus stood for many as markers of present techniques' inability to deal with the intricacies of spectral data.[75]

72 'This notion of a crisis in or of learning, although it had roots running back into the previous century, emerged as a universally cogent cliché only in the aftermath of Germany's defeat.' Forman, op. cit. (8), 27.

73 Darrigol, op. cit. (8), 177.

74 Pauli quoted in A. Pais, *Niels Bohr's Times: In Physics, Philosophy, and Polity*, Oxford, 1991, 199. Pauli also provides Kuhn, op. cit. (1), 83–4, with an example of the crisis before the advent of matrix mechanics.

75 Paul Forman, 'The doublet riddle and atomic physics circa 1924', *Isis* (1968), **59**, 156–74; P. Forman, 'Alfred Landé and the anomalous Zeeman effect, 1919–1921', *Historical Studies in the Physical Sciences* (1970), **2**, 153–261; Darrigol, op. cit. (8); D. Serwer, "*Unmechanischer Zwang*: Pauli, Heisenberg, and the rejection of the mechanical atom, 1923–25', *Historical Studies in the Physical Sciences* (1977), **8**, 189–256. As Helge Kragh has noted, however, the hydrogen atom – the subject of Sommerfeld's famous work on fine structure – was not implicated in the growing dissatisfaction with standard explanations: 'In the process that eventually transformed the old quantum theory into quantum mechanics experimental anomalies contributed strongly, but there was no feeling of crisis at all as far as the hydrogen atom was concerned.' H. Kragh, 'The fine structure of hydrogen and the gross structure of the physics community, 1916–26," *Historical Studies in the Physical Sciences* (1985), **15**(2), 67–125, 84.

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Many, but not all. We have already noted the historical question of how to distinguish between difficult questions and critical paradoxes, between problems within a theory and problems for a theory. After a revolution is said to have occurred it is easy to go back and identify certain research problems as having been signs of the moment of crisis for the older theory. Whether they were seen as such at the time is another question. Retrospective comments made by participants during interviews as part of the Archive for the History of Quantum Physics make it apparent that not every atomic theorist expected the fundamental failure of the older quantum theory. While questions by Thomas Kuhn, in particular, often attempt to elucidate the point at which the crisis was recognized, several of those interviewed simply deny the applicability of the term. Consider the following exchange between Kuhn and Paul Dirac:

> TSK: Was there also that sense which again people speak of on the continent that something fundamental now had to come to get around these problems that were just not responding. That there was something fundamentally the matter?
>
> D: I am not sure that that is so. They had the Bohr–Sommerfeld method of quantization and they thought it would have to be extended in some way … I don't think people suspected that one would need such a complete revolution … It rather came as a surprise to me when Heisenberg's ideas came out.[76]

In these interviews, Kuhn's questions on the issue of crisis often appear leading. It is never the existence but only the nature and the timing of the crisis that is a matter of investigation. For example, he stated in an interview with Werner Heisenberg in 1963 that by 1923,

> at least around Copenhagen – I'm not sure now it was then in Munich – it's perfectly clear that what I will call a 'crisis' exists and is recognized with respect to the problems in quantum mechanics. It's clear in things that Bohr said, it's clear at least by what Bohr thinks Born said, it's clear for many people in that 1923 Bohr Heft of Naturwiss. and in other places. But it is by no means clear to me when that strong attitude came, how it developed, and more particularly, where it developed.[77]

This perspective is perhaps to be expected from a man whose philosophy of science depended so completely on notions of crisis and revolutions. But it is clear that many of those interviewed had imbibed the same notions about the period just prior to the advent of quantum mechanics. It was clearly inconceivable to Linus Pauling, for example, that Sommerfeld should not have emphasized the deep-seated problems in which quantum theory had found itself, or rather in which quantum theory must have found itself, since these problems were then resolved by the quantum

76 T. S. Kuhn, 'Interview with P. A. M. Dirac, 04/01/1962', AHQP, 14. Dirac seems to have had a similar response to Bohr's worries, in 1929, about the need for a 'drastic alteration' to quantum mechanics as it then stood. 'I cannot see any reason for thinking that quantum mechanics has already reached the limit of its development. I think it will undergo a number of small changes, namely with regard to its method of application, and by those means most of the difficulties now confronting the theory will be removed. If any of the concepts now used (e.g. potentials at a point) are found to be incapable of having an exact meaning, one will have to replace them by something a little more general, rather than make some drastic alteration in the whole theory.' Quoted in Silvan S. Schweber, *QED and the Men Who Made It: Dyson, Feynman, Schwinger, and Tomonaga*, Princeton, NJ, 1994, 65.

77 T. S. Kuhn, 'Interview with Werner Heisenberg, 02/11/1963', AHQP, 7.

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mechanical revolution. In an interview with John Heilbron he began by remembering that Sommerfeld had talked a great deal about a particular 'anomaly' during a visit to the United States. That memory was then completely reversed upon closer reflection:

> P: But while Sommerfeld was here, he emphasised very strongly the anomaly of the inner quantum number and the outer quantum number – well at least he talked about the inner quantum number and the outer quantum number.
>
> [to Heilbron's question, Pauling gives a detailed answer describing the meaning of the outer quantum number.]
>
> P: So that they introduce an azimuthal quantum number; in fact they introduce two azimuthal quantum numbers and then in talking about the s, p, d separation you use one and in talking about the fine structure splitting you use the other. And the question is how can you have two quantum numbers that describe the eccentricity of the orbit? It only has one eccentricity. So this was brought out, and my memory – *I have to change my memory*. Sommerfeld just talked along glibly about the inner azimuthal quantum number without giving anybody, any auditor, any impression that there was anything funny about it.[78]

Sommerfeld and the members of his school did not register a sense of crisis. Indeed, Sommerfeld's attitude, expressed in a letter to Einstein in 1922, seems characteristic of his approach to most of the problems of the quantum theory: 'Everything works, but remains at the deepest level unclear.'[79] The Munich school did not speak of the existence of paradoxical and insurmountable anomalies, nor, crucially, about the occurrence of a revolution. In looking back in 1929 at the events of recent years Sommerfeld was quite explicit: 'The new development does not signify a revolution [*Umsturz*], but a joyful advancement of what was already in existence, with many fundamental clarifications and sharpenings.'[80] Students' descriptions of the reception of some of the major new results of the new quantum mechanics confirm the sense that what were seen as conceptual breakthroughs elsewhere were viewed as useful tools in Munich. Thus to Kuhn's question as to the reception of Heisenberg's uncertainty-principle paper, Hans Bethe replied somewhat laconically that it was 'Not very deep'.

> TSK: Was it interesting? Had these problems bothered people?
>
> B: No, these problems had not bothered people; at least it was not evident that they had bothered people. The paper was discussed, so that was it; this was a 'fine conclusion'.
>
> TSK: But not a conclusion to anything that had really upset people previously?
>
> B: No. It really should have been discussed as the thing which now finally solved the paradox, but it wasn't.[81]

Sommerfeld's focus was in another direction entirely, as Bethe makes clear: 'It was all concentrated on solving the problems of the atom; then the molecule, then the solid

78 J. L. Heilbron, 'Interview with Linus Pauling, 03/27/1964', AHQP, 17. My italics.

79 Sommerfeld to Einstein, 11 January 1922, document 50 in Eckert and Märker, op. cit. (58), ii, 110–11.

80 Arnold Sommerfeld, *Atombau und Spektrallinien: Wellenmechanischer Ergänzungsband*, 1st edn, Braunschweig, 1929.

81 Kuhn, op. cit. (69), 12.

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state and not finding out more about the foundations.'[82] A comment by Pauling makes a similar point with regard to the Munich response to the Schrödinger equation: 'My memory is that everyone was so excited about the possibilities of solving problems, answering questions, the mechanism provided by the new quantum mechanics, that there was little discussion of those details of interpretation.'[83] For the physics of problems, in other words, there were not anomalies, crises and revolutions, but problems and their methods of solution. To cite Kuhn and Bethe once more:

> TSK: Were there great puzzles?
>
> B: No, there was no great puzzle and I think this is the greatest characteristic of the Sommerfeld group; we were not made aware of great puzzles. We were given the impression that here was a wonderful tool. Now you could do things, now you could solve all these interesting problems like all the complicated atoms and so on and chemistry, but fundamental problems, no.[84]

An immediate response to these descriptions might be simply to argue that Sommerfeld, hence his school, had merely failed to see paradoxical anomalies that led to a real crisis. Yet such a claim would be tricky. When does a problem that one cannot yet solve become a problem that cannot be solved? Like Dirac above, Sommerfeld saw the situation in the early 1920s as difficult but not dire. The foundations and the philosophy of the new field may have been shrouded in darkness, but one could still proceed with the appropriate and appropriately modified techniques. By contrast, a point too little emphasized by those who would point to the importance of the helium and Zeeman crises as causes for the search for new bases for the quantum theory, both Bohr and Born had called for a fundamental reworking of classical mechanics long before 1924. As Paul Forman noted, crisis talk considerably preceded the identification of the particular crises that could be said to have provoked a quantum revolution.[85]

The point here is not that Kuhn or others were 'wrong' in describing this period in terms of crisis. Rather, in seeing crisis as an aspect of the community as a whole, scholars have given up the possibility of using the presence or absence of crisis talk as a marker for more fundamental divisions. Explaining why some saw a crisis and a revolution and some did not requires a deep rethinking of our understanding of these terms. Although Kuhn drew much of his terminology from extant materials, often the language of scientists themselves, his use of a particular vocabulary has profoundly shaped our perceptions of the processes of scientific change. It is thus to the ideas of crisis and revolution in *Structure* that we again return.

82 Kuhn, op. cit. (69), 12. Bethe would note that 'looking back on this period from five years later I had the impression that I had really missed the development'. A much more recent interview, however, would suggest that his approach to physics was very close indeed to that of his former teacher. 'I'm not searching that much', he said, 'In fact, I've never tried to go into the really deep fundamentals of physics, into the philosophical part of physics. I'm much more interested in phenomena that you can observe. Now, I am searching for solution for some equations also, but that's not the kind of thing that Bohr was doing.' Judith Goodstein, 'A conversation with Hans Bethe', *Physics in Perspective* (1999), **1**, 253–81, 279.

83 Heilbron, op. cit. (78). Cf. Eckert, op. cit. (60), 206.

84 Kuhn, op. cit. (69), 12.

85 Forman termed this, pejoratively, a 'craving for crises'. Forman, op. cit. (8), 58–63.

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The most fundamental distinction in *Structure* was that between normal and revolutionary science. The first involved puzzle-solving within a given paradigm, the second involved a dramatic change of the paradigm itself. The two are temporally related by the fact that a revolutionary change ushers in a new paradigm and hence a new regime of normal science:

> Successive paradigms tell us different things about the population of the universe and about that population's behavior. They differ, that is, about such questions as the existence of subatomic particles, the materiality of light, and the conservation of heat or energy. These are the substantive differences between successive paradigms and they require no further illustration. But paradigms differ in more than substance, for they are directed not only to nature but also back upon the science that produced them. They are the source of the methods, problem-field and standards of solution accepted by any mature scientific community at any given time … The normal scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before.[86]

In spite of the connection between normal (paradigm-preserving and -extending) and revolutionary (paradigm-replacing) science, a basic asymmetry nonetheless remains: a change in normal science does not necessarily lead to a revolutionary change but a revolution does completely overturn the modes of normal science. The asymmetry is occasionally minimized by the fact that certain puzzles that arise in the day-to-day practice of normal science can present such difficulties that they rise into anomalies; enough anomalies and a crisis develops, followed by a revolution. Yet no puzzle or set of puzzles is sufficient to cause a revolution. All they can induce is the search for, perhaps the creation of, an alternative paradigm. Revolutions then proceed as the choice among competing paradigms. That choice is made on a substantive basis and then and only then has an effect on the practices of normal science. Scientific revolutions are revolutions of conceptual foundations, not of puzzle-solving techniques. Science sees revolutions of principles, not of problems.

Kuhn's book was among the first to draw historians' attention away from either ideas or individuals and towards scientific groupings. Yet Kuhn's analysis breaks down at what might be called the 'mesoscopic' level that distinguishes between small groups within a community like those designated in terms of a theoretical physics of principles and of problems.[87] Communities are either homogeneous, defined by a shared allegiance to a common paradigm, or the collection of individuals whose thoughts Kuhn tracked with care and subtlety. But the fact that such a substantial part of the quantum physics community, the hugely important Sommerfeld school, should be so completely out of alignment with otherwise similar members on the fundamental question of whether their shared field had undergone a revolution suggests that at

86 Kuhn, op. cit. (2), 102.

87 Which explains in part, of course, the turn in the 1970s towards histories of research schools. See G. Geison, *Michael Foster and the Cambridge School of Physiology: The Scientific Enterprise in Late Victorian Society*, Princeton, NJ, 1978; G. Geison, 'Scientific change, emerging specialties, and research schools', *History of Science* (1981), **19**, 20–40; G. Geison and F. L. Holmes (eds.), 'Research schools: historical reappraisals', *Osiris* (1993), **8**; J. B. Morrell, 'The chemist breeders: the research schools of Liebig and Thomas Thomson', *Ambix* (1972), **19**, 1–46.

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the mesoscopic scale one must reconsider the basic distinction between normal and revolutionary science. Rather than imagining a homogeneous community that solves puzzles the vast majority of the time, then contemplates revolutions at select moments, one can, as it were, imagine the temporal axis of scientific change rotated to line up with that of intra-disciplinary structure. The result is depicted below: a scientific community made up in the majority of those who solve problems and eschew the pursuance of revolutions, and a numerically smaller group whose focus on principles and foundations means that the only change that counts is a revolutionary and fundamental one:[88]

What must not be transferred in this rotation of axes is the value judgement explicit in Kuhn's book. There is no suggestion here that the physics of problems is, like Kuhn's 'normal science', basically conservative, unimaginative, blinkered and averse to the calling forth of 'new sorts of phenomena'.[89] In terms of the use of quantum mechanics' new methods there were probably few places more enthusiastic than Munich. The Sommerfeld school was one of the centres of innovation in German theoretical physics for more than a quarter of a century. Amnesia about these innovations may be due to the fact that given a choice between a story of similar events told in terms of crises and foundational change and one told in terms of a succession of puzzles solved, however similar the end-points, historians have opted for the romance of revolutions.

Conclusion

The aim of this essay has been to criticize and explore the historiographical notion of 'crisis' in order to open new perspectives in the history of theoretical physics. Taking up as a first example the question of whether a general crisis existed within the German theoretical-physics community, a negative answer leads to a novel understanding of the nature of theoretical physics in general. Debates about the material foundations of the science become not evidence of nearly terminal divisions but rather of a constitutive element of a field in the making. In the second example, crisis talk is used as an indicator of particular understandings of the nature of fundamental scientific change for groups within the theoretical-physics community in the mid-1920s. While one group,

88 S. S. Schweber has described a similar situation with regard to the development of quantum electrodynamics. Where 'leaders of the discipline' like Bohr, Pauli, Heisenberg and Dirac were insistent on the need for a 'revolution', the actual solution of the divergence difficulties was provided by those who looked rather for incremental, technical solutions to particular problems, like Kramers, Schwinger, Tomonaga, Feynman, Dyson and Bethe. Schweber, op. cit. (76), 25–6. Bethe's significant contribution provides a certain continuity in what could be described as another round in a debate about principles/revolutions versus problems/incremental technical development.

89 Kuhn, op. cit. (2), 24.

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designated by their belief in a 'physics of principles', saw a crisis and called for its resolution through a revolution in advance of problems that were later seen to be decisive, the 'physicists of problems' neither saw a crisis nor acknowledged in the aftermath of matrix and wave mechanics that a revolution had occurred. This tells us something crucial about the relationship between the deployment of particular intellectual practices and historical actors' particular understandings of the meanings of history. For historians, it should lead to a significant alteration of our own conceptual categories if we are not to privilege one perspective over the other.

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