Phys. Perspect. 12 (2010) 102–117

1422-6944/10/010102-16
DOI 10.1007/s00016-009-0007-8

Physics in Perspective

Book Reviews

Alisa Bokulich, *Reexamining the Quantum–Classical Relation: Beyond Reductionism and Pluralism.* Cambridge: Cambridge University Press, 2008, x + 195 pages. $80.00 (cloth).

This book is perhaps best characterized as integrative history and philosophy of science; it elucidates historical scientific writings and uses insights gained in doing so to develop new philosophical concepts. It is an exemplary application of the approach. The explanations are clear and the philosophical insights are novel and potentially transformative. The specific focus of the book is on intertheoretic relations between classical mechanics and quantum mechanics having mainly to do with what classical mechanics can reveal about quantum-mechanical systems. Such relations run in the opposite direction of reductionism, which in this case would focus on explaining classical systems in quantum terms. The philosophical themes and viewpoints developed in the book have mainly to do with the foundations of physics; however, they may be generalized to foundational studies that involve other sciences. The writing is semitechnical in character, meaning that some sections contain equations. Readers without a substantial mathematical background may struggle with those sections of the book; however, the equations are typically flanked by good prose that provides a suitable measure of understanding so that such readers can feel comfortable moving on.

The philosophical insights developed in the book are inspired largely by the writings of three of the founders of quantum mechanics: Niels Bohr, Werner Heisenberg, and Paul A. M. Dirac. Publications of recent practitioners of semiclassical mechanics and pertinent philosophical literature are also used to provide important insights. The views that are of particular interest to Bokulich have to do with the nature of intertheoretic relations between classical and quantum mechanics, including those that have been developed thus far and those that are hoped for in the long run. About half of the book is devoted to elaborating the views of the founders, one chapter for each. The most important figure for Bokulich is Dirac; his notion of structural continuity is particularly influential. The second half of the book focuses on intertheoretic relations using recent case studies from semiclassical mechanics. Two innovative philosophical notions are put forth in doing so, *interstructuralism* (a viable alternative to reductionism and pluralism) and *structural model explanations* (a new type of scientific explanation). Bokulich makes clear that she regards her development of these notions in the book as programmatic, meaning that they are to be developed more fully in future publications. One purpose of the book is to provide a proper contextual framing for them through the writings of key figures in the history of quantum physics and pertinent work in philosophy of science and semiclassical physics.

The first chapter is introductory. It provides a concise summary of reductionism and pluralism, which are the two main approaches to intertheoretic relations that have been promoted in the philosophical literature. The term "approach" indicates a variety of related notions rather than a single specific one. Reductionists assume that theories may be arranged hierarchically, with some theories being more fundamental than others and one being more fundamental than the rest; it also assumes that a less fundamental theory is explicable in terms of one that is more so. The term "explanation" is broadly construed; reduction might involve deducing or eliminating or approximating one theory using the other. By contrast, pluralists maintain that theories cannot for the most part be arranged hierarchically and that they are for the most part autonomous with respect to one another, meaning that theories operate in disjoint arenas and cannot be connected together in any extensive or robust manner. After introducing these abstractions, the discussion shifts to a more concrete level that is pertinent to the concerns of the book. Bokulich provides a brief discussion that is both clear and informative of the main approaches to reductively explaining classical mechanics by quantum mechanics. This intertheoretic relation is not the central concern of the book as already indicated, but it is important to have some understanding of what is involved in it to better appreciate the relation that is the central one.

Bokulich first shows that standard textbook approaches to explaining classical mechanics by quantum mechanics—those that involve Ehrenfest's theorem and those that involve a simplistic asymptotic limit, such as Planck's constant (or an appropriate dimensionless constant) approaching zero or as some quantum number approaches infinity—have very limited intertheoretic utility. She then considers a more sophisticated approach that involves *decoherence*, which characterizes what happens to the state of a quantum-mechanical system when it interacts with another such system. The claim that decoherence explains the emergence of classicality is rather contentious when the discussion turns to classically chaotic phenomena; nevertheless, the explanatory approach is regarded as quite promising.

As indicated above, the emphasis of the book is on the ways in which classical mechanics serves to explain quantum mechanics, the inverse of what is considered by reductionists. Some elaboration is now in order. The specific concern is how semiclassical mechanics is used to explain quantum chaos, by contrast with the reductionist concern of how quantum decoherence is used to explain classical chaos. Quantum chaos theory is the study of quantized counterparts to classical chaotic systems. One crucial difference between classical chaotic systems and their quantum counterparts is that the classical ones exhibit sensitive dependence on initial conditions whereas their quantum counterparts do not. More to the point, the use of quantum decoherence to explain the emergence of classical phenomena (including classical chaos) follows a standard reductive mode. The inverse relation requires a more flexible account of intertheoretic relations (interstructuralism) that may involve a new mode of explanation (structural model explanation).

The next three chapters explicate the writings of Heisenberg, Bohr, and Dirac. The central notion of Heisenberg's views on the nature and role of intertheoretic relations is that of a closed theory, a theory for which the physical and mathematical assumptions are no longer susceptible to modification. New developments in physics do not infringe on the validity of a closed theory, only on its scope, which is empirically determined. Moreover, closed theories characterize distinct sectors of physical reality, and developments towards a closed theory for an undertheorized sector requires the abandonment of the concepts of existing closed theories. The upshot is that Heisenberg is a type of pluralist, which turns out not to be so for Bohr and Dirac. Bokulich also draws interesting parallels between the views of Heisenberg and Thomas S. Kuhn, which are based in part on recorded conversations between the two.

Contrary to Heisenberg, Bohr claims that the concepts of classical mechanics are indispensable for quantum mechanics, which he regards as a rational generalization of classical mechanics. That is to say, quantum mechanics is a universal theory that encompasses classical mechanics, and its application requires essential use of classical concepts. Thus, Bohr is neither an advocate of pluralism (in that he recognizes quantum mechanics as a universal theory), nor is he an advocate of eliminative reductionism (in that classical concepts are essential for applying quantum mechanics). The restriction imposed by quantum mechanics on the simultaneous applicability of certain pairs of classical concepts is another aspect of Bohr's rational-generalization thesis, which serves to maintain theoretical continuity as opposed to introducing a discontinuous break. Also worth mentioning is Bokulich's discussion of Bohr's correspondence principle. She clearly shows it to be a selection rule that characterizes allowable quantum transitions such as those that have a corresponding harmonic component in the classical electron orbit. The upshot is that Bohr's correspondence principle differs dramatically from its textbook characterization (as requiring an asymptotic agreement of predictions in some limit). Bokulich notes that Bohr does not oppose such a requirement, which might be taken to suggest that Bohr is at least sympathetic to some form of reductionism other than the eliminative variety.

By contrast with Heisenberg's views on closed theories, Dirac sees the need to keep successful theories open and promotes their ongoing development, modification, and extension. In doing so, he emphasizes that it is best to bring about gradual theoretical transformations by maintaining a substantial measure of structural continuity while at the same time allowing for substantial differences. The development of Heisenberg's formulation of quantum mechanics is a case in point. It was developed so that many fundamental structures of the Hamiltonian formulation of classical mechanics are preserved. There are of course important structural differences between classical and quantum mechanics such as the one involving the algebra of observables, which is commutative in the classical case and noncommutative in the quantum case; the uncertainty relations in quantum mechanics derive from the noncommutativity. Dirac's use of the Lagrangian formulation of classical mechanics to develop an alternative version of quantum mechanics is another case in point, having key structural analogues and key structural differences. Dirac's theory is noteworthy in that it later influenced Richard Feynman to develop the path-integral formulation of quantum mechanics.

Bokulich makes a good case that Dirac's notion of structural continuity is incompatible with pluralism since it promotes maintaining substantial conceptual connections between a theory and its successors. It also does not sit well with reductionism in that the relation between a theory and its successor is analogical (a partial structural similarity), which is substantially weaker than the more stringent types of explanatory connections that are typically required in reducing one theory to another. In addition, the movement is in the inverse direction, from classical mechanics to quantum mechanics. These features of structural continuity considered together with semiclassical mechanics provide inspiration to Bokulich in developing a more flexible type of intertheoretic relation and an alternative notion of explanation.

After the chapter on Dirac, the discussion turns to semiclassical mechanics, which is best characterized as an approach that combines classical and quantum structures to form a hybrid theory (as opposed to being a specific theory). Dirac would favor such developments, but Heisenberg would not. The old quantum theory is a good starting place for thinking about such theories. That theory encountered serious problems in quantizing systems having nonseparable Hamiltonians, meaning systems whose components substantially interact with one another. Most quantum systems are of that sort with the exception of very simple (which is certainly not to say trivial) quantum systems such as the hydrogen atom. Semiclassical techniques for dealing effectively with systems having nonseparable Hamiltonians were not devised until the 1970s by Martin C. Gutzwiller. The core of his method is the trace formula, which enables one to calculate the quantum density of states from the classical density of orbits even when the classical counterpart is fully chaotic, in which case the Hamiltonian must be nonseparable. One important victory for the trace formula is that it can be used to accurately calculate the spectrum of helium under the assumption that electrons orbit the nucleus following classical trajectories. Helium is a three-body system involving inverse-square forces, which make the Hamiltonian nonseparable. The classical counterpart can exhibit chaos, which serves to explain why the quantization conditions of the old quantum theory were unable to deal effectively with the helium atom. Bokulich draws on this case and others like it, including the anomalous resonances of the Rydberg atom and wave-function scarring in quantum billiards, which have proven extremely problematic for a straight quantum approach, but can be handled effectively using the trace formula. Only the case involving helium is considered below.

Bokulich draws several lessons from the successes of the trace formula. One lesson is that the trace formula uses classical structures (orbital paths) to deduce quantum structures (helium energy levels). More to the point, it exemplifies interstructuralism, a new mode of intertheoretic relation whereby a less fundamental theory provides important insights about a more fundamental theory; specifically, the direction of the deduction is from classical to quantum structure, which is the inverse of what would occur in reduction. Another lesson is that if one were to close off theories in the manner advocated by Heisenberg, then that would serve to discourage rather than promote the development of new quasiclassical theories such as the trace formula and thereby make it more likely that physics would miss out on important scientific insights. Inter-structuralism promotes the development of theories that make use of key structural features of classical mechanics to provide new insights, which is precisely in line with Dirac's view that theories should be open and developed by analogy so as to maintain a substantial degree of structural continuity. Physicists should not regard classical mechanics as a rigidly fixed theory; rather they should regard it as an approach to theorizing having characteristic structures that are subject to substantial modification as long as suitable analogical counterparts can still be identified. Further development of what constitutes a structural feature of a theory, what counts as a suitable structural analogy, and what constitutes a measure of the degree of structural continuity are suggestive lines of research that could flow out of developments in this book.

Bokulich draws another innovative and potentially important lesson from such cases, namely, that there may be a new notion of scientific explanation that is involved in interstructuralist relations. She claims that the trace formula uses classical trajectories to explain the spectrum of helium. This is an example of what she refers to as a structural-model explanation. Classical structure is used to explain quantum structure with demonstrable counterfactual dependency, meaning that the formalism provides answers to what would happen if the setup were to be changed (say by introducing a magnetic field). The sticking point, of course, is that the electrons do not follow classical trajectories. The deep issue is whether a fictional entity or structure can really serve an explanatory role. Certainly, the trace formula uses classical trajectories to enable the derivation of descriptive content and structural information about helium. But such claims fall short of claiming that it serves an explanatory function. Bokulich is right in saying that there is more going on than saving the phenomena, given the counterfactual dependency and the gain in genuine physical insight. However, it is not clear it gets beyond revealing what the structure is to revealing why it has that structure. She is aware of the gaps in the account and explicitly says that the notion of a structural model explanation is to be developed more fully elsewhere. A possible extrapolation of her approach would be to question the actual-fictional dichotomy and allow for a more liberal ontology that permits virtual processes. No doubt there are other approaches. It will be very interesting to see how the dialectic she has initiated plays out.

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Helge Kragh, *The Moon that Wasn't: The Saga of Venus' Spurious Satellite.* Basel: Birkhäuser Verlag, 2008, 199 pages. $59.95 (cloth).

Helge Kragh, the author of this little jewel of a book, is clearly an industrious man. In addition to his appointments at the Institute for Science Studies at the University of Aarhus and on the editorial board of *Physics in Perspective,* Kragh is also, according to Amazon.com, the author or coauthor of more than a dozen books, including a recent scientific biography of Dirac, a history of cosmology, and a social history of chemistry in Europe. Two of Kragh's books—*Entropic Creation: Religious Contexts of Thermodynamics and Cosmology* and (*Science, Technology and Culture, 1700–1945*) and *Science of Denmark: A Thousand-Year History*—came out in hardcover in 2008 within months of *The Moon that Wasn't*.

In the ten-page preface that precedes the text, Kragh carefully lays out the rationale for and the road map to the book. "This is a book about the non-existing satellite of Venus," (p. viii) a moon that was observed by several professional and amateur astronomers in the 123 years between 1645 and 1768. (Table 3.1 on page 42 lists the thirty-six known observations.) Even though the failure of observations during the transits of Venus of 1761 and 1769 to detect such a satellite led most astronomers to discount its existence, the fictitious moon captured the public imagination much the way black holes and dark energy do today. Readers aware that 2009 marks the fiftieth anniversary of C. P. Snow's *The Two Cultures and the Scientific Revolution* will be especially interested to learn how a scientific non-fact permeated popular culture for such a long time. Who knew, for example, that Frederick the Great of Prussia tried, albeit unsuccessfully, to lure Voltaire to join his new Berlin Academy by comparing him to the satellite of Venus? (Chapter 3, pp. 71–72) Or that in Jules Verne's science fiction novel *Hector Servadac* (1877), the eponymous hero and his team take advantage of a temporary if dangerous change in Earth's orbit, bringing them close to Venus, to verify "that beyond a doubt the planet has no moon or satellite such as Cassini, Short, Montaigne of Limoges, Montbarron, and some other astronomers have imagined to exist"? (Chapter 6, pp. 131–132) By surveying all the references he could find to the Venus satellite in both scientific and popular literature, and including 32 interesting illustrations, Kragh has found "a novel way" to "address the history of planetary astronomy," providing a window "not only into the world of the astronomers but also into the popular literature concerning the planetary system and other aspects of astronomy." (p. viii)

In several chapters Kragh calls our attention to a question that remains unanswered to this day, namely, "if the moon does not exist, why had several astronomers of reputation seen it," (p. ix) including those mentioned by Verne's hero. The final chapter of the book summarizes the main explanations offered over the years to account for these observations of a Venus satellite. The most popular were those proposed by the Jesuit astronomer Maximilian Hell (1720–1792), who attributed the observations to optical illusions, and by Paul Henri Stroobant (1868–1936), director of the Royal Observatory in Brussels, who suggested that faint stars were mistaken for a moon. Although Kragh does not attempt to give his own explanation of why some excellent observers detected a satellite, he does gives examples of other scientists "who have seen things that do not exist, either for psychological, social or instrumental reasons," (p. 147) including Blondot's "discovery" of *N* rays, Thomas Harriot's drawing of lunar features "that almost certainly had their origin in his mind rather than in his telescope," (p. 147) (though we don't necessarily agree with Kragh, given that, unlike Galileo, exactly 400 years ago, Harriot had no Renaissance training in shadows in drawing) as well as Percival Lowell's "sensational discovery claim of canals on Mars," (p. 148) and several sightings of rings around Neptune by astronomers who, like the Council of the Royal Astronomical Society in 1847, believed that "the existence of the ring seems almost certain." (p. 148)

Kragh concludes the book with a tantalizing comparison between the astronomical community's rejection of the existence of a Venus satellite and its acceptance of the moons of Uranus, which had been found by William Herschel but not independently confirmed by other astronomers. "The case for the six moons of Uranus, say about 1820, was not much stronger than the case for Venus' moon had been fifty years earlier, and yet the astronomical community responded very differently in the two cases." (p. 154) Kragh surmises that the disparity can be accounted for by Herschel's impeccable reputation as an observer. Even though it took the space probes of the second half of the twentieth century to bring to an end "the romance of the natural satellite of Venus," (p. 141) astronomers had earlier come to a virtually unanimous decision "that the companionless Cytherean planet was a fact rather than a convincing hypothesis. And facts are not to be questioned." (p. 154)

We were disappointed to see that Kragh seems unaware of the work on the black drop effect done by one of us (JMP) at the 1999 transit of Mercury and the 2004 transit of Venus. In Chapter 3, footnote 3, Kragh says, "On the correct explanation of the black drop effect, see Schaefer 2001." (p. 40) Unfortunately for Kragh (and Schaefer), though, the explanation that Shaefer guessed was incorrect. The correct explanation was found only in the paper one of us (JMP) wrote with Glenn Schneider, inspired by Shaefer's report (the main point of his paper) that a high percentage of books and articles used an incorrect explanation (involving Venus's atmosphere, which is much too thin to provide the observed black drop effect). Schaefer's paper inspired one of us (JMP) to use recent observations, with results reported in articles by Pasachoff, Schneider, and Golub.[1] Making measurements on space observations from NASA's *TRACE* (Transition Region and Coronal Explorer), we and they concluded that the effect results from a combination of solar limb-darkening and telescopic point-spread functions.

Despite this personal disappointment, we found much to admire in the book's wide-ranging coverage. Among the most intriguing topics the book touches on *passim* is cosmic pluralism—the belief in numerous other worlds beyond the Earth, which may possess conditions that would sustain life. While reading, thoughts about SETI—the Search for Extraterrestrial Intelligence—and about pseudoscientific claims of alien abduction came to mind. How interesting, then, to read in Chapter 6 about the assertion in a "recent UFO newsletter" that sightings of the Venus moon in 1740 and 1764 suggest "a powered spacecraft moving out of Venusian orbit." (p. 142) Further down the page we learn about "a somewhat similar suggestion made in 1959 by the brilliant Russian astrophysicist Iosef Shklovskii in connection with Mars' inner moon, Phobos. A pioneer radio astronomer… he was also a pioneer of modern SETI research." Footnote 66 informs us that Shklovskii eventually turned his back on cosmic pluralism. Of course, the spaceship ideas are currently known to be ridiculous.

One of the admirable features of this book, grounded though it is in overlooked historical documents, is the way it brings the story up to date. Chapter 1, for example, not only asserts that "Today we know that Venus does not have a moon" (p. 5) but also modifies that statement in Footnote 8: "Although this is true, it is not quite as true as it was just a few years ago. In 2004 a group of astronomers discovered that Venus has a so-called quasi-satellite…. This object, called 2002 VE68, is… an asteroid orbiting around the Sun, but in such a way that it appears to travel around the Venusian sky about once every Venus year." Since this review was written during a 2008–2009 sabbatical at Caltech, another example of such updating seems particularly satisfying. While acknowledging that theories of how Venus might have lost its satellite during the early history of the solar system has no bearing on the history he is covering in this book, Kragh draws attention in Chapter 6 to research published in 2006 by David Stevenson (JMP's current next-door office neighbor at Caltech) and Alex Alemi (a Caltech undergraduate), suggesting that "the mystery of the missing moon might be explained by assuming two large impacts," one creating a moon and causing Venus to spin counterclockwise, and a second, some millions of years later, reversing the rotational direction and causing the moon and the planet to collide. (p. 143) Stevenson now points out that in recent years, planetary astronomers understand a lot more about the formation of planets.

Although Kragh is basically a fine writer who can charm the reader with wry editorial comments (the powered spacecraft idea "strangely enough has not won the acclaim of either astronomers or historians of science"), English is not his first language, presumably accounting for the occasional infelicities that creep in, such as "felled" for "fallen" (p. 120) and "beyond the point" instead of "beside the point," (p. 131) as well as grammatical errors that also intrude on the reader's pleasure. The editorial process did not even catch the not-so-recent move of their editorial-board member J. Z. Buchwald from Cambridge to Pasadena.

While reading *The Moon that Wasn't* it occured to us how appropriate it was that we had been selected to review it. Doing research in rare book libraries, picking up books and monographs that have perhaps not been touched in decades, even centuries, and selecting frontispieces and tables from them to use as illustrations in our own work, gives us great pleasure. Nonetheless, a comment Kragh makes about one of the obscure books he has obviously enjoyed thus uncovering struck us as perhaps appropriate for *The Moon that Wasn't.* In assessing *Der Venusmond,* an 1875 publication by German amateur astronomer F. Schorr, Kragh concludes, "Although of no scientific worth, his book is an important if overlooked contribution to the history of planetary astronomy." (p. 114) As such, should Kragh's book be on the "must read" list of every subscriber to this journal? After some deliberation we have concluded that if Kragh has found the time to write more than one book that was published in 2008, even the busiest member of his potential audience should be able to find time to peruse this charming and engrossing, if not terribly consequential, book, especially since, weighing in at only 154 pages of real text—the rest is taken up with ten pages of biographical sketches of "some of the central figures in the saga of the Venus moon" (p. ix) and twenty-five pages of bibliography, "including nearly 400 primary and secondary sources" published "over a period of more than 350 years" (p. viii)—it can easily be devoured in a single session.

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[1] Glenn Schneider, Jay M. Pasachoff, and Leon Golub, "TRACE Observations of the 15 November 1999 Transit of Mercury and the Black Drop Effect: Considerations for the 2004 Transit of Venus," *Icarus* **168** (2004), 249–256; Jay M. Pasachoff, Glenn Schneider, and Leon Golub, "The black-drop effect explained," in D.W. Kurtz and G.E. Bromage, ed., *Transits of Venus: New Views of the Solar System and Galaxy*, IAU Colloquium No. 196 (U.K., 2004), pp. 242–253.

Nicolaas A. Rupke, *Alexander von Humboldt: A Metabiography*. Chicago: University of Chicago Press, 2008, 316 pages. $21.00 (paper).

I once read a metareview of a book written by a friend of mine. It consisted of a review of other reviews of the book. When I received this metabiography, I wondered: Would it consist of a biography of other biographies of the subject? Yes, it would. In fact, as the author relates in a footnote, (p. 215) his book began as an entry to the *Reader's Guide to the History of Science*, which he says has a "metabiographical slant." Rupke writes that his study was originally intended to cover "the entire international body of literature on Humboldt," but that it "eventually narrowed to focus on the German Humboldt corpus, when the extent became apparent to which different nations have produced their own distinct Humboldt representations." (p. 16) In contrast to my friend, who had only to read the book reviews rather than the entire book, Rupke had to read all of the biographies, and this is reflected well in the thorough documentation of his book: there are copious footnotes, and almost a quarter of the book's pages are lists of printed sources.

Roughly the first third of Humboldt's ninety-year life was spent growing up and pursuing a career as an administrator in the Prussian mining industry. The second third began with a five-year exploration of Latin America, followed by a stopover in the United States to meet President Thomas Jefferson, followed by residency in Paris, during which he published writings about his travels (in French). In the last third of his life, Humboldt moved back to his native Berlin. Besides traveling through Russia and Siberia and conducting diplomatic missions, he wrote and lectured about the sciences to make them accessible to the public, under the title *Kosmos*.

In the six chapters of his metabiography, Rupke describes Humboldt as he was portrayed by six periods of German history: (1) the period before German unification; (2) unified Germany before 1933 (the "Wilhemian" and Weimar periods); (3) Germany under Hitler; (4) the post-World War II German Democratic Republic; (5) the post-World War II German Federal Republic; and (6) reunified Germany. Each of these periods brought forth different aspects of Humboldt's life to match the temper of the times. Humboldt's writing about science in his native language endeared him to the German people and helped to unify them. In fact, the quality of his writing was compared to that of Goethe and Schiller. In later periods, his Latin American travels became more significant. Humboldt visited the same territory Charles Darwin would visit later on *H.M.S Beagle*, and Darwin acknowledged reading Humboldt's narratives; this provided the basis for a link between Humboldt and Darwin, although Humboldt died the year Darwin published *On the Origin of Species*. Humboldt's Latin American travels and subsequent encouragement of Simón Bolívar endeared him to Latin America, which led to the establishment of the Alexander von Humboldt Foundation to represent German cultural interests there.

Rupke writes that although nothing could be more foreign to Nazism than Humboldt's philosophy, the Nazis made him one of their own, in a manner that continued almost seamlessly from their Weimar predecessors. They produced an *Ahnentafel* certifying Humboldt's Aryan ancestry and continued to exploit the favorable German influence in Latin America that began with Humboldt's travels there. In fact, this can account for the ease with which Nazis found safe haven in Latin America after World War II.

The Humboldt of the post-World War II German Democratic Republic represented an abrupt change. The East German Communists were the first to invoke Humboldt's career in mining, portraying him as a friend of miners. In addition to emphasizing his support of Bolívar, they depicted him as a "geographer with a social conscience who included the economic and political aspects of human society in his descriptions of the physical world." Rupke writes that, "Whereas the Wilhemian Monists made Humboldt a Darwinist before the *Origin of Species*, the GDR Humboldtians, some fifty years later, made him a Marxist before *Das Kapital* (1867)." (p. 126) Meanwhile, West Germans, seeking to reintegrate Germany into the family of nations and atone for Germany's anti-Semitic atrocities, focused on Humboldt's contributions to geography and developed his love for all groups of people to the point of making him an object of hero worship, as a symbol of "supranational cosmopolitanism." Singled out among the groups befriended by Humboldt were Jews, whom Humboldt in his own lifetime had helped gain acceptance in academia, *without* the requirement that they be baptized.

But, writes Rupke in his final chapter, "in the wake of German reunification… Humboldt-the-Marxist died." He continues: "in spite of the fact that East Germany's Humboldt was the most elaborately worked out of his varied and successive identities and founded on the most extensive basis of primary sources available at the time… the issue of 'the right and authentic political framework' [to quote Humboldt Society President Werner Plarre] was decided not by the strength of the West's Humboldt scholarship but by the success of its free market economy." (pp. 175–176) Because of Humboldt's voluminous correspondence with many people all over the world, "the new buzzwords 'globalization' and 'global information network' entered Humboldt studies." (p. 180) Moreover, Humboldt's early warnings about the impact of deforestation on soil and water also enabled the Humboldt of the 1990s to be portrayed as "green." But not all was adulatory: Rupke notes at the end of this chapter attempts to deconstruct what were now characterized as myths about Humboldt and questions about his sexual orientation.

As I read Rupke's metabiography, I couldn't help but remember the old TV program, "To Tell the Truth," in which only one of three contestants was telling a true story and, after a question-and-answer period in which panelists sought to identify the "true" contestant, the "true" contestant was asked to stand up. In the Conclusion to his book, Rupke has exactly this game in mind. If all six Humboldts he has portrayed were contestants on "To Tell the Truth," he concludes that "all or none" of them should stand at the end – "all" because all of them are telling the truth but "none" of them is telling all of it. Rupke concludes that "some Humboldts are arguably more genuine than others," but "all Humboldts… are the product of institutional cultures and embedded in changing socio-political contemporaneities. The task of metabiography is primarily to explore the fact and the extent of the ideological embeddedness of biographical portraits, not to settle the issue of authenticity." (p. 215)

Two questions also continued to perk through the back of my mind: "Which historical figures would lend themselves best to metabiography?" and "Why Humboldt?" In his Introduction, Rupke intimates a parallel of his work to that of Albert Schweitzer in *The Quest of the Historical Jesus*, and in his Conclusion he cites Gordon Wood's *The Americanization of Benjamin Franklin* as endowing its subject with "five different identities: gentleman, imperialist, patriot, diplomat, and American." (p. 213) He also cites Darwin as an example of another scientist who has been appropriated by various groups for their own purposes. But he suggests that purported metabiographies of Lavoisier and Newton are "in danger of overextending the genre's meaning." (p. 214)

An answer to "Why Humboldt?" is perhaps less transparent. Although the "Humboldt current" is named after him, and he is one of ten explorers featured in a TV series, *Ten Who Dared*, he seems to have achieved more in making science accessible to the public than in being a first-rate scientist himself, a point that is touched on several times in this book, as is the notion that he is something of a "cult" figure. In fact, although John Lynch's listing of four categories of references to Humboldt in his recent biography of Simón Bolívar clearly points up the importance of Humboldt in the history of Latin America, a survey of German history books in my local library turned up more references to Humboldt's older brother Wilhelm, who is credited by Rupke as being a founder of the University of Berlin. It is clear, however, from Rupke's metabiography, that Alexander von Humboldt was a "man for all seasons" in the distinctly different phases of the history of his native Germany. To the extent that we have different phases in the history of the United States, it seems to me that we are more likely to regard our political leaders as metabiographical subjects.

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Maria Rentetzi, *Trafficking Materials and Gendered Experimental Practices: Radium Research in Early 20th Century Vienna.* New York: Columbia University Press, 2008, xxiii + 291 pages. $60.00 (cloth).

At first sight both the title and the outward appearance of this book seem somewhat puzzling. Does *trafficking* modify only the word *materials*, or does it refer to the phrase *gendered experimental practices* as well? The title's grammatical ambiguity could have been cleared up by the use of italics, a comma, or quotations marks to flag "Trafficking Materials" as a single, albeit neologistic, expression. The book's strangely parsimonious appearance – the generic dust jacket, the total lack of images, the absence of an index – is explained in a prefatory note. The printed volume is not complete, but merely a "partial representation" of a multimedia work published in the "Gutenberg-e online history" series. Indeed, the online version, which is readily accessible at <www.gutenberg-e.org>, is richly illustrated and furnished with a good search engine in place of an index.

What I hold in my hand is thus not really a book, but part of a publishing experiment– and a first for me. It assumes that the reader will consult the electronic version alongside, or instead of, the printed text. Some readers may therefore not have much use for the hard copy, because they habitually read full texts on a computer or a Kindle. Whether this hybrid format will survive the electronic revolution I cannot venture to predict.

Returning to the title, Rentetzi, a professor of science and technology studies at the National Technical University in Athens, Greece, defines a *trafficking material*, in this case mainly radium, as follows: "A material which is traded and bargained as both a commodity of the mundane world and as a scientific object … the vital material around which research communities, and, in the long run, a whole discipline [is] organized." (p. xx) The notion of trafficking materials turns out to be an addition to the vocabulary of the study of material culture. *Gendered experimental practices*, on the other hand, are instances in which "gender actively and constructively shape(s) experimental cultures," rather than being a mere external factor in laboratory life. (p. xviii) As the unambiguous subtitle suggests, the subject of the book is the interplay of these two notions in radium research in early 20th-century Vienna.

"The scene of my historical play," as the author calls it, the Vienna of Boltzmann, Freud, Klimt, Goedel, and eventually the Nazis, is a veritable cauldron of cultural and political ferment. Stories of individual scientists vie with descriptions of experiments, laboratories, university buildings, coffee houses, urban landscapes, and analyses of social movements in a richly textured piece of historical narration. Three dense pages of acknowledgments and thirty-two pages of bibliography provide evidence of Rentetzi's indefatigable scholarship, and give the reader confidence that she has left no stone unturned to document her story. Although I found her writing style a bit dry, and the organization less than straightforward, I enjoyed the book for the intimate feeling it manages to convey of a turbulent period in the history of science in Vienna.

Of her two theoretical innovations, the concepts of trafficking materials and of gendered practices, which wend their way like threads through the fabric of the book, I found the first less compelling than the second. To be sure, radium has unique characteristics that influence the way it impinges on the world, but then so do many materials. (Gold and DDT each have fascinating tales to tell, too.) The significance of radium in the study of women in science is embodied in the larger-than-life person of Marie Curie. The role of radium in the history of Viennese science is determined by the accidental fact that the only readily available source of uranium in Europe was the mine in St. Joachimsthal in Bohemia, which was then part of the Austro-Hungarian Empire. Nevertheless, the use of radium as the link between women, science, and Vienna is less than persuasive.

But to shine a spotlight on the Radium Institute in Vienna in order to learn about the role of women in science is a rewarding exercise. In the first half of the twentieth century women were not only beginning to assert themselves as scientific pioneers (Marie Curie, Lise Meitner, Irène Curie) but were also entering laboratories as rank-and-file researchers. The Radium Institute was ahead of its time in this respect: In the 1920s a third of its staff, from technicians to lead scientists, was female. (p. xvii) In that context Rentetzi pursues what appears to be her principal theme, the effect of gender on experimental practice.

To focus her discussion she turns to an episode concerning the Institute, known as the Cambridge–Vienna Controversy, to which she devotes about a third of her book. For the purpose of understanding her argument I found it most helpful to read the detailed and lucid account of this incident published a quarter of a century ago by Roger H. Stuewer,[1] whom Rentetzi quotes copiously. The scientific question was this: Are alpha particles from Radium C (one of the decay products of radium, now known to be an isotope of bismuth) capable of disintegrating nuclei of carbon and other light elements, ejecting tell-tale protons in the process? Rutherford's lab in Cambridge said no, Vienna's Radium Institute said yes. After several years of public debate, Rutherford sent his colleague James Chadwick to Vienna to try to resolve the issue. The story of his visit makes for high drama, which Stuewer compared to the more famous tale of R.W. Wood's exposé of nonexistent *N* rays twenty years earlier. Spoiler alert: Rutherford and Chadwick were right, carbon nuclei were not smashed, and the Viennese had fooled themselves into seeing protons where there were none.

What made the incident useful for Rentetzi's purposes was the circumstance that almost without exception the Cambridge observers were men, those in Vienna women. She gives Stuewer credit for understanding the role of the Viennese women, but claims that by leaving them on the periphery of his story – which indeed he did – he allowed other scholars to misinterpret the dynamics of research at the Radium Institute. Thus, for example, the editors of the essay collection which included Stuewer's article wrote in a very brief general preface: "…Chadwick successfully challenged the performance of the women employed as scintillation counters by the rival Viennese group. Such an attempt to defuse a dispute over an experiment was possible only because a division of labor had separated the eyes of the observers from the minds of the experimenters, so that the former could be evaluated in the light of technical performance without calling into question the scientific integrity of the latter."[2]

This passage must have made Rentetzi see red! She demonstrates convincingly that the women in Vienna were more than merely "employed as scintillation counters," that they had not only eyes, but minds as well, that their technical performance was part and parcel of their scientific competence, and that editors Achinstein and Hannaway failed to understand the true division of labor in Vienna. It is important to stress that neither they, nor Chadwick and Rutherford, nor any later commentator, ever accused any of the members of the Radium Institute of dishonesty. Like countless experiments in countless laboratories throughout the world, this one went wrong, and the truth ultimately triumphed. Nevertheless, it is ironic, and perhaps surprising, that Rentetzi chose this particular example as the centerpiece of her study of women in science.

The book's final chapter recounts the subsequent fates of several of the Vienna "radioactivists," with special emphasis on Marietta Blau (1894–1970), who is credited with pioneering work on the use of photographic emulsions in nuclear physics. Like her fellow Jewish woman scientist Lise Meitner she was forced to flee from the Nazis, and like Meitner she was nominated for the Nobel Prize. In spite of Einstein's fruitless intercession it took her several tries, interrupted by temporary employment in Mexico, to enter the United States. When she finally succeeded she began a peripatetic journey through academic and industrial institutions, ending up at the University of Miami. In 1960, ill and disillusioned, she left without retirement rights or health benefits and moved back to Vienna where she died ten years later "lonely and unknown to the international physics community." Her name deserves to be added to those of Marie Curie, Lise Meitner, and Irène Curie.

In the end, this book's principal strength is also its main weakness. The wealth of details in their complex interactions, which makes the book so interesting, obscures its message and overwhelms the reader. A single topic, such as a history of the Radium Institute, or the story of radium, or a revision of Stuewer's essay, or a biography of Blau, might be a more effective vehicle for making Rentetzi's points. Even a leaner title would help.

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[1] Roger H. Stuewer, "Artificial Disintegration and the Cambridge-Vienna Controversy," in Peter Achinstein and Owen Hannaway, ed., *Observation, Experiment, and Hypothesis in Modern Physical Science* (Cambridge, Mass.: MIT Press, 1985), pp. 239–307.
[2] *Ibid.*, p. x.

Deborah R. Coen, *Vienna in the Age of Uncertainty: Science, Liberalism, and Private Life.* Chicago and London: The University of Chicago Press, 2007, xi + 380 pages. $45.00 (cloth).

It's an intriguing title. To a physicist the word "uncertainty" conjures up the uncertainty principle, especially when coupled with the name of the city where Erwin Schrödinger was born and got his start. For the author of *Vienna in the Age of Uncertainty* its meaning is, however, quite different.

The book is only peripherally concerned with 20th-century developments. Its ambitious aim is to recast the scientific and cultural history of 19th-century Vienna.

It uses the device of building its story around a single family, the Exners, whom the author describes as having "their hands in projects spanning science, politics, and the arts." With its ten professors in Austrian universities over three generations Coen calls them an ideal guide and says that "their scientific accomplishments were celebrated by contemporaries … and … remain relevant today."

The book is primarily about Franz Exner (1802–1853) and his five children, and among them predominantly about his younger sons, the physicist (Franz) Serafin Exner and the physiologist Sigmund.

To talk about the 19th century as a time of uncertainty seems strange. The reign of the emperor Franz Joseph in Austria (1848–1916) was even longer than that of Queen Victoria at about the same time, and defined the age there much as hers did for the British Empire. To his subjects in the Austrian capital it may well have seemed as if the dynasty would continue indefinitely.

Where then is the link to uncertainty? It turns out to be associated with the liberalism of the subtitle, and within it with the study of probability. The Exners are described as being in the vanguard of the liberal movement of their time. We are here, however, far from anything that a modern liberal would recognize. There is none of the openness across races and classes, and the attitude toward women is barely a step beyond the medieval, with its emphasis on the "womanly virtues" of a wife and mother. Instead, what distinguishes the 19th-century Austrian liberal is the struggle against the authoritarianism of the imperial regime and against the dogmatism of the Catholic state religion.

The academic atmosphere in the Vienna of that time is illustrated by the fate of one of Franz Exner's teachers, who was charged with deviation from the prescribed textbooks and, refusing to submit his lecture notes to the government for inspection, Franz was the beneficiary when he was asked to take over the deviant's duties. By the time he was thirty he was Professor of Philosophy at the University of Prague.

During his time there he met and married his wife Charlotte, the daughter of Jews who had converted to Catholicism. Her origins played no part in their lives and were never mentioned, until much later when they haunted the family after the *Anschluss*, the annexation of Austria by Hitler's Germany in 1938.

A glimpse of their relationship and his personality can be seen from a letter that Franz wrote to Charlotte after she complained of the discomforts of her first pregnancy. He tells her that she lives for the power and the glory of the fatherland. "We want to raise our boys as future soldiers…. Our girls, though, shall become nothing but mothers, who in turn have such sons." Women were considered to be intellectually inferior. Even Sigmund's wife, Emilie, who is called "an activist for academic education of women," agreed and asked her readers to acknowledge their own limitations.

In Prague Franz became known for his theatrical lectures, and eventually for his espousal of approximate, empirical laws, in contrast to dogmatic "absolute" knowledge. This, then, is the meaning of the word "uncertainty" in the book's title: the battle against dogmatism and oppressive absolutism was to be won by engaging in "probabilistic reasoning."

Exner had his chance for lasting influence when he became an advisor to the Imperial education ministry and submitted a plan for reform of the curriculum of the *Gymnasium*, the elite secondary school that was the path to a university education. Coen considers this plan to be Exner's legacy, and calls it "the standard, the ideal against which Austrians judged the success and failure of the Empire's schools for over half a century." The plan made the cultivation of a "noble character" the goal of education, and did so by involving the students more actively, and emphasizing science and mathematics.

Coen says that "Exner had become convinced that liberal education would have to begin by training the students to resist the scourge of determinism…. As a component of the new

Gymnasium curriculum, exercises in probabilistic reasoning would encourage students to view absolute claims skeptically."

The eight years of *Gymnasium* followed four years of elementary school and ended in the *Matura*, the examination that was the gateway to university entrance. It was truly an elite institution, enrolling 0.2% of the population in 1850. A teacher was addressed as Professor, and usually had a Ph.D. I'm not sure how much it had changed by the time I was in one for three years before leaving Vienna. By that time I had had three years of Latin, one year of Greek, and one year of physics. Let me illustrate the atmosphere, the methods, and the subject matter by a memory from my physics class. In the sadistic fashion of the institution each student was from time to time, unpredictably, called to the blackboard to demonstrate his knowledge to the teacher and the class. Eventually my time came and I was asked to describe the workings of the internal combustion engine. I started my exposition, confident that I knew the topic well. I still don't know why at one point the professor interrupted with *Setzen! Nicht genügend* (Sit down! Not sufficient), the fourth and failing grade in the sequence of very good, good, sufficient, and not sufficient. No wonder that my father had nightmares about his *Gymnasium* examinations throughout his life. It wasn't always pleasant, but the curriculum was rigorous enough for me to need just two more years to graduate from high school in Canada.

A significant part of the book is about the summer house in the village of Brunnwinkl that the family acquired in 1882. It became not only a gathering place for them and their friends, but it also shaped their attitudes. They came to admire those who lived "close to the soil." The farmer and the hunter seemed to have a way of life and pursuits that represented to them a kind of purity not to be found in the city. We are here presumably referring to the German word *Bauer*, which only partially represents the modern farmer. For that time at least, it implies a life largely confined to the family land, far from the cosmopolitan, diverse, and intellectual life of the city. The Exners romanticized the rural simplicity that they claimed to see, and began to think of themselves as part of the local culture. When the region later became a popular summer destination they deplored the changes, and one member of the family called the newcomers "noisy, pushy, foreign in the worst sense," code words to describe the intruders, who were often Jewish.

From there it is only a step to the racial purity sought by the Nazis, and it is no surprise that members of the third generation of the family became members of Hitler's party or sympathizers. They were shocked when the long-denied Jewish origin of their grandmother caused some of them to lose their academic positions.

What can we say about the Exner's contributions to physics? (Franz) Serafin Exner is mentioned in "Vienna: A Random Walk in Science" by Wolfgang L. Reiter in "The Physical Tourist" series in this journal.[1] It shows his memorial plaque in the arcades of the University of Vienna and mentions his various interests, including color theory and atmospheric electricity. There is a good deal about color in Coen's book, including a discussion of different uses of color by artists and craftsmen, but it deals primarily with perception rather than physics.

Coen calls Exner "the father of the next generation of physicists." One of his students was Erwin Schrödinger, and Coen tries hard to link the famous student with the less famous mentor. This is made more difficult because Schrödinger did not accept the probabilistic interpretation of his own work. Instead she sees the connection in Schrödinger's early participation in "alpine physics" during a summer spent in Exner's group on atmospheric measurements near Brunnwinkl. Because the work was done in the outdoor setting of the mountains it was endowed with near-mystical significance by the Exners.

Making measurements outdoors, other than in the controlled surroundings of the laboratory, was not easy and led to variations in the results. Coen describes Exner and his students as using "error to designate an ineradicable variability rooted in nature itself," and sees here the early indication of the indeterminacy of quantum mechanics. She calls the Heisenberg uncertainty principle "a probabilistic interpretation of atomic physics." In both cases she misses its meaning as a fundamental statement about what can be known and measured.

Coen talks about "imperial Austria's profound creativity in the sciences." In view of the size and importance of the empire I am not sure that this is an appropriate description. She sees the sources of this presumed success in probabilistic reasoning. She suggests that this and the work on hydrodynamics in which the Exners participated led directly to the later development of quantum mechanics.

Coen calls Sigmund's son Felix "the father of a statistical meteorology," and says that he "made what was apparently the first attempt to model weather patterns from basic physical principles on a large geographic scale." Her description of him in one breath with Schrödinger is, however, far from convincing. In fact, it is hard to escape the impression that the contributions, at least to physics, of what she calls "Vienna's foremost scientific dynasty" were less than fundamental.

Finally, the book attempts to recast the basic pattern of the intellectual history of Vienna near the turn of the 19th century. The prevalent view is that this was a time of revolutionary ferment in the arts and sciences, as described most famously by Carl Schorske in his book, *Fin-de-Siècle Vienna*.[2] Instead she believes that the achievements of the 19th century, as exemplified by the Exners, demonstrate a continuity of progress.

Coen spends some time with the innovations in art, and the enrollment of two young women of the third Exner generation in the Arts and Crafts School. Their elders, steeped in classical realism, were appalled. Franz Exner's daughter Marie, writing about her niece Nora, mentions her "crazy colors" and that she "raged like a savage in the paint box," with "her taste … spoiled to the core."

Gustav Klimt was given a commission to make ceiling paintings at the University of Vienna to celebrate its various faculties. The first to be installed, in 1900, was *Philosophie*. Its dreamlike atmosphere with naked bodies floating in murky space caused widespread consternation. Sigmund and (Franz) Serafin Exner were among the protesting professors asking that it be removed. Strangely, in view of the Exners' opposition, it forms the frontispiece of Coen's book. Schorske devotes several pages to its description. Only a black and white photograph, shown in both books, survives.

I will leave it to you to decide whether the book's thesis is persuasive, and whether you find the story of the Exner family an appropriate vehicle for the history of their time.

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[1] Wolfgang L. Reiter, "Vienna: A Random Walk in Science," *Physics in Perspective* **3** (2001), 462–489.
[2] Carl E. Schorske, *Fin-de-Siècle Vienna: Politics and Culture* (New York: Knopf, 1980).

Martijn Eickhoff, *In the name of science? P.J.W. Debye and his career in Nazi Germany.* Translated by Peter Mason. Amsterdam: Aksant, 2008, 184 pages. €24.90 (paper).

In 2006 the physics community in the Netherlands was rocked by a bitter debate that centered on the Dutch physicist Peter Debye (1884–1966). A remarkably versatile scientist, Debye's name is known for the Debye–Scherrer method for X-ray diffraction of powdered crystals, the Debye–Hückel equation for ionic solutions, the *debye* unit for molecular dipole moments and a great deal more, and in 1936 he was awarded the Nobel Prize for Chemistry. Although Debye spent most of his career in Germany and lived in the United States from 1940 on, he has for many years been a major figure in the scientific pantheon of the Netherlands. Over the years some raised questions but no one looked very closely into the seven-year period that Debye lived and worked in National Socialist Germany.

That changed abruptly in January 2006 with the publication of *Einstein in Nederland,* by the historian of science Sybe Rispens. In exploring Albert Einstein's ties to a number of Dutch physicists, Rispens depicted Debye as an opportunist, not close to Einstein, and all-too-accommodating to Nazi racial policies. The Dutch scientific establishment responded at warp speed. On February 16, the University of Utrecht announced that it would change the name of the Debye Institute for Physics and Chemistry of Nanomaterials and Interfaces, and on the same day in Maastricht, Debye's birthplace, the University announced that it would remove Debye's name from its Peter Debye Prize. That launched a furious controversy, "a classic good/bad debate in the Netherlands, in which the various participants – moralists versus rehabilitators – became progressively entrenched in their positions." (p. 8) On June 26 the Dutch Ministry of Education, Culture, and Science commissioned the Netherlands Institute for War Documentation (NIOD) to investigate Debye's role and position during the Nazi period. The NIOD report, which was originally published in November 2007 and translated into English in 2008, is the book under review here. Its author, historian Martijn Eickhoff, provides a solidly documented and often fascinating account of Debye's German career, with insight into his personality, attitude, and the practice of science in the Third Reich.

One is struck by Debye's remarkable success during these years. At a time of acute racial injustice in Germany and economic distress worldwide, Debye clearly inhabited a parallel universe. Soon after the Nazis came to power in 1933, he was tapped for the position of director of the Kaiser Wilhelm Institute (KWI) for Physics, with responsibility for the construction of a large new institute in Berlin. The building was generously funded by the Rockefeller Foundation, which otherwise was wary of funding such large projects in Nazi Germany, and Debye himself entered into the most privileged techno-scientific circles of industry, the military, and the state. In 1935 a visiting Rockefeller officer reported that Debye was "the only undepressed person" he had talked to in Germany; indeed Debye had achieved an extraordinary degree of scientific autonomy, professional influence, and worldwide prominence. An important factor, Eickhoff suggests, was his Dutch nationality. It gave him a "halo of independence" (p. 19) that inspired trust abroad and raised his status within Germany where his international stature helped restore the prestige and maintain the quality of German science, which had been damaged by the dismissals of Jewish scientists and other anti-Semitic measures.

One of the aims of the NIOD report is to determine Debye's attitude and reaction to Nazi racial policies, a matter that Eickhoff judges to be particularly relevant because Debye's anti-Semitism is a matter of record. This volume includes citations from letters written by Debye in 1912 and 1921 that are more explicitly anti-Semitic and (I would add) uglier in tone than anything I have seen in the correspondence of other scientists from that time. No comparable documents have been found for the years after 1933, and Debye is known to have assisted several people with their emigration, including two Jewish scientists: he helped his student Heinrich Sack find a position abroad in 1933 and he acted as a go-between for the Dutch physicists who helped Lise Meitner escape from Germany in 1938. Debye's concern for these individuals was undoubtedly genuine but, Eickhoff points out, not inconsistent with a general acceptance of policies of racial segregation and forced emigration. In fact, Debye's advancement during this time was inseparable from such policies: the directorship of the KWI for Physics was originally slated not for him but for the Jewish physicist James Franck, who emigrated from Germany in 1933.

Debye emerges from this study as a person with a compartmentalized moral outlook, a man who regarded science as an absolute good, a Dutch national who considered himself apolitical while complying with the policies of the Nazi state, a pragmatic scientific organizer who was quite comfortable in Nazi Germany as long as he was able to advance his career and retain his independence. As a scientist he occasionally took a stand – against the pseudo-scientific "German Physics" (*Deutsche Physik)*, for example, and the politicization of professional appointments – but as an individual he did not object to the ongoing process of excluding Jews from every aspect of public and professional life. In this respect, a telling episode took place in December 1938 when

Debye, in his position as chair of the German Physical Society (DPG), addressed a letter to its members stating that the remaining Jewish members who lived in Germany must resign. This letter was the focus of particular controversy in the Netherlands in 2006, the moralists regarding it as evidence of Debye's complicity with Nazi racial policies while the defenders claimed that the regime had forced him to write the letter, giving him no choice. In the past, historians have mostly been among the defenders, noting that the expulsion of Jewish members was inevitable, that the tone of the letter was unenthusiastic, and that his conduct was not very different from most other scientists in Germany. Examining the available sources in a wider historical context, Eickhoff shows that Debye was not in fact forced to write the letter; he concludes that Debye took the initiative at that moment in order to head off interference from the regime in the workings of the DPG and to prevent activist Nazis in the society from ousting him as chair. Further, Debye, unlike most other scientists in Germany, had options: he could have resigned as chair of the DPG rather than write the letter, he was insulated by his international standing, he had offers to work abroad. Taken together, Eickhoff regards this episode as a turning point for Debye, for by formally involving himself in a domestic political process that he understood was unjust, he vacated his outsider status and demonstrated his solidarity with his German colleagues. Eickhoff leaves open the question of whether he acted primarily in the name of science or in his own interest.

In September 1939, soon after World War II began, Debye was informed that only war-related research would be funded and that he himself must accept German citizenship or step aside as director of the institute; a month later the KWI for Physics was taken over by the Army for research into the military potential of nuclear fission. Debye took a paid leave of absence, accepted a longstanding offer from Cornell University, and left for the United States in early 1940. There several scientists, including Einstein, expressed their distrust of him, but he was welcomed at Cornell, eventually cleared by the FBI, and recruited for war work. He became an American citizen in 1946 and remained in Ithaca until his death 20 years later, greatly honored in the United States, Germany, and the Netherlands.

In many biographical narratives, including his own, Debye's voluntary emigration and his refusal to accept German citizenship were construed as a form of principled opposition to National Socialism. From this study it appears indisputable, however, that he left primarily because the takeover of his institute and research program was an intolerable assault on his scientific autonomy. Debye's recollections of his years in Nazi Germany were sparse; he did not publicly reflect on the injustice and suffering he had witnessed or his own position during that time.

The NIOD report is a valuable contribution, carefully documented and transparently argued, that will undoubtedly be the definitive historical basis for further studies of Peter Debye. More generally, it can be read by scientists and others for the essential dilemmas it presents regarding the possibility of individual and collective responsibility in immoral, unjust, and coercive situations.

Meanwhile, the Debye controversy of 2006 seems to have run its course. Debye's name is still attached to a professorship and a lecture series at Cornell, and the American Chemical Society's Peter Debye Award in Physical Chemistry is unchanged. These decisions were taken before the NIOD report appeared; after its publication in 2007, a special commission in the Netherlands recommended that Debye's name be restored to the institute in Utrecht and to the prize awarded by the University of Maastricht. Utrecht agreed, Maastricht refused. An underlying question remains: What does it mean when a scientist's name is made permanently visible for future generations? Is it recognition for truly exceptional scientific achievement, or does it imply that this was an exemplary life as well?

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