History *of* Physics
NEWSLETTER

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News of the Forum:
David C. Cassidy Wins 2014 Abraham Pais Prize

By Gloria B. Lubkin, Chair, 2014 Selection Committee

David C. Cassidy, 2014 Pais Prize recipient.

The 2014 Abraham Pais Prize is awarded to David C. Cassidy "for his foundational studies on the history of quantum mechanics and his nuanced examinations of physics in Germany and the United States with special attention to the scientific work, personalities, and dilemmas of Heisenberg and Oppenheimer."

David Cassidy, a professor at Hofstra University in Hempstead, NY, has written two scientific biographies of Werner Heisenberg. The first, more scholarly volume, *Uncertainty: The Life and Science of Werner Heisenberg* (W. H. Freeman, New York, 1993), grew out of his doctoral dissertation and subsequent years of research in Germany and other countries. That highly celebrated volume won both the 1993 Pfizer Award of the History of Science Society and the AIP Science Writing Prize. The second volume, *Beyond Uncertainty: Heisenberg, Quantum Physics, and the Bomb* (Bellevue Literary Press, New York, 2009) was written for a broader audience, and includes fewer details of the development of quantum mechanics. However, the newer book includes information from the "Farm Hall" transcripts, which were only released in 1993. Cassidy also maintains a website on Heisenberg, *http://www.aip.org/history/heisenberg/p01.htm*.

Cassidy's biography of Oppenheimer, *J. Robert Oppenheimer and the American Century* (Johns Hopkins University Press, Baltimore, 2009) provides a rich sense of the cultural and historical contexts in which Oppenheimer lived. Cassidy also wrote *A Short History of Physics in the American Century* (Harvard University Press, Cambridge, 2011). With Gerald Holton and James Rutherford, Cassidy also wrote the undergraduate textbook *Understanding Physics* (Springer, 2002). Cassidy's play, *Farm Hall*, based on the transcripts made of the captured German nuclear scientists in 1945, received a staged reading at the March 2013 APS meeting.

Cassidy received BA and MS degrees from Rutgers and a PhD in physics from Purdue in 1976 in conjunction with the Department of History of Science at the University of Wisconsin in Madison. He did a postdoc with John L. Heilbron at the University of California, Berkeley and then was a Humboldt Fellow with Armin Hermann at the University of Stuttgart. He then became an assistant professor in history of science at the University of Regensburg. Cassidy was Associate Editor of *The Collected Papers of Albert Einstein,* Volume I ("The Early Years") and Volume II ("The Swiss Years: Writings"). He has been at Hofstra University since 1990, where he is now professor of natural sciences. He has served FHP as secretary-treasurer (1995) and as chair (2008).

In This Issue

From the April 2013 APS Meeting

FHP Chair Don Howard, Past Chair Peter Pesic, and APS Executive Officer President Kate Kirby at the FHP Executive Board meeting.

FHP Chair-Elect Brian Schwartz introducing staged reading of "And the Sun Stood Still," by Dava Sobel, a play about Nicholaus Copernicus.

History *of* Physics NEWSLETTER

The Forum on History of Physics of the American Physical Society publishes this Newsletter biannually at *http://www.aps.org/units/fhp/newsletters/index.cfm*. If you wish to receive a printed version of the Newsletter, please contact the editor. Each 3-year volume consists of six issues.

The articles in this issue represent the views of their authors and are not necessarily those of the Forum or APS.

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Forum on History of Physics

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Upcoming Events at the 2014 APS March and April Meetings

APS Meeting, March 3-5, Denver, Colorado:

Women and the Manhattan Project
Co-sponsored by FHP and CSWP
Monday, March 3, 2014
11:15 AM

Session Chair: Margaret Murnane, University of Colorado, Bolder and JILA

"The Girls of Atomic City: The Untold Story of the Women Who Helped Win World War II," Denise Kiernan, Author, Touchstone/Simon & Schuster

"Women and the Hanford Site," Michele Gerber, Gerber Group Consulting

"After the War: What Happened to the Women Scientists of the Manhattan Project," Ruth Howes, Professor Emerita of Physics and Astronomy, Ball State University

"Preserving the Manhattan Project: Women at Work," Cindy Kelly, Director, Atomic Heritage Foundation

"On the Ordinary Genius of Laura Fermi," Olivia Fermi, On the Neutron Trail

The History of the Communication of Science to the Public
Co-sponsored by FHP and FOEP
Wednesday, March 5, 2014
8:00 AM

Session Chair: Brian Schwartz, Brooklyn College and The Graduate Center, CUNY

"The Establishment of Science Communication for the Public at the Royal Institution" Frank Burnet, Emeritus Professor of Science Communication, University of West of England

"Displaying Science: The Exhibits Revolution in Science and Natural History Museums, 1900-1990,Karen Rader, Virginia Commonwealth University

Women at Oak Ridge gaseous diffusion plant

"The Role of Living History in the Communication of Science to the Public," Susan Marie Frontczak, Storysmith®

"The Historical Role of the *New York Times* in the Communication of Science to the Public," Dennis Overbye, The *New York Times*

Twentieth-Century Chinese Physicists and Physics
Co-sponsored by FHP and FIP
Thursday, March 6, 2014
2:30 PM

Session Chair: Danian Hu, The City College of New York, CUNY

"Chien-Shiung Wu: An Icon of Physicist and Woman Scientist in China," Yuelin Zhu, Harvard University

"Chinese physicists educated in the Great Britain during the first half of the 20th century," Xiaodong Yin, Capital Normal University

"Mao and physics research in China in the 1950s-1960s: the H-bomb project and the Straton model," Tian Yu Cao, Boston University

"Some problems in the competition of high-temperature superconductivity research during the late 1980s," Bing Liu, Tsinghua University

"A brief history of the Institute of Theoretical Physics in the Chinese Academy of Sciences since 1978," Jinyan Liu, Chinese Academy of Sciences

APS Meeting, April 5-8, Savannah, Georgia:

The Many Worlds of Leo Szilard
Co-sponsored by FHP and FPS

"The Many Worlds of Leo Szilard," William Lanouette, author of *Genius in the Shadows: A Biography of Leo Szilard, the Man Behind the Bomb*

"Leo Szilard in Physics and Information," Richard Garwin, IBM

"Leo Szilard: Biologist and Peace-Maker," Matthew S. Meselson, Harvard

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Upcoming Events at the 2013 History of Science Society

THURSDAY, NOVEMBER 21, 2013
3:00–5:00 p.m.

Session T2: The Materiality of Words in Chemistry and Pharmacy

FRIDAY NOVEMBER 22, 2013
7:30–8:45 a.m.

Physical Science Forum Business Meeting

FRIDAY NOVEMBER 22, 2013
9:00–11: 45 a.m.
(includes break from 10:00 to 10:15)

Session F6: Rethinking the Cold War Scientist: Advisers, Activists, and Archetypes from Sputnik to Star Wars

Session F13: Fields, Waves, and Particles: Debates in Modern Physics

FRIDAY NOVEMBER 22
12:00–1:15 p.m.

Physical Science Forum Distinguished Lecture: Peter Galison (Harvard University)

FRIDAY NOVEMBER 22
1:30–3:30 p.m.

Session F17: The Fifty-Year Anniversary of the Limited Test Ban Treaty: Origins and Legacies

Session F28: Exploring Space: Politics, Institutions, and Collaborations

FRIDAY NOVEMBER 22
3:45–5:45 p.m.

Session F37: Paleontology as an International Endeavour in the Nineteenth and Twentieth Centuries

Session F40: Natural History and Natural Philosophy in the Eighteenth Century: Two Arts or One?

FRIDAY NOVEMBER 22
8:45–10:00 p.m

Workshop F4: The Past, Present, and Future of the Physical Sciences

SATURDAY NOVEMBER 23
9:00–11: 45 a.m.

Session Sa43: Chemists and Chemistry in the Nineteenth Century: A Session in Honor of Alan J. Rocke

Session Sa46: Interrogating the Cosmos with Mathematical Imaginings and Physical Intuitions, 1880-1965: Bridging Disciplinary and Cultural Practices

SATURDAY NOVEMBER 23
1:30–3:30 p.m.

Session Sa61: Foundations at the Philosophical Turning-Points: Chronicling Conceptual Turns in Theories of Modern Physics

Session Sa67: Chymistry and Life in Early Modern Europe

SATURDAY NOVEMBER 23
3:45–5:45 p.m

Session Sa83: Epistemic Strategies in 20th Century Physics and Cosmology: Reshaping Spaces, Structures, and Styles

SUNDAY NOVEMBER 24
10:00 a.m.–12:00 p.m.

Session Su5: The Order of the Interdisciplinary: How the Materiality of Sociopolitics Shapes Interpretations and Representations in Physics ■

Crowdfunding Effort for a biography of Richard Garwin

By Tony Fainberg, Institute for Defense Analyses

The last newsletter announced the preparation of a crowdfunding effort to be launched soon: the goal is to support a completed, published biography of Dr. Richard Garwin, designer of the first thermonuclear device and arms control advocate for over half a century. Preparations to this end have been made on the indiegogo.com site: a 60-day campaign to raise funds to support this project is planned to "go live" on November 1. Members of the Forum on the History of Physics (along with others, of course) are encouraged to visit the indiegogo site after November 1, to search for the campaign titled "Richard Garwin Biography" and to contribute to this worthy effort, following instructions on the site. More information on the campaign is available there. Any questions or difficulties encountered should be communicated to Tony Fainberg at fainberg666@comcast.net. Thanks to all.

Maria Goeppert Mayer: The 50th Anniversary of Her Nobel Prize

By Paul Halpern

On April 13, 2013, at the APS April Meeting, an invited session was held to mark the 50th anniversary of Maria Goeppert Mayer's Nobel Prize in Physics, an honor she shared with J. Hans D. Jensen and Eugene Wigner. (Half of the prize money in 1963 went to Wigner; the other half was divided between Jensen and Mayer.) Remarkably, Mayer was only the second women to receive the Nobel Prize in Physics. It had been a full 60 years since Marie Curie had been awarded the prize. The session was well-attended and stimulated considerable discussion.

The first speaker at the session was Prof. Steven Moszkowski of UCLA who spoke about "Maria Goeppert Mayer's work on beta-decay and pairing, and its relevance today." Moszkowski pointed out the importance of Mayer's lesser known work in the theory of double beta-decay, in addition to her Nobel Prize winning work on the nuclear shell model and magic numbers. He showed how her theory of double beta-decay derived from her PhD work in double photon emission, along with her understanding of Fermi's beta decay model. Moszkowski brought the discussion up-to-date by commenting on recent experiments looking for double beta decay without neutrino emission—a process that would violate lepton number conservation.

The second speaker was Prof. Karen E. Johnson of St. Lawrence University, who spoke about "Maria Goeppert Mayer and the Nobel Prize." Johnson pointed out that while Marie Curie, the first woman to win the Nobel Prize in physics is well-known, Maria Goeppert Mayer, the second woman, who won the prize in 1963, is much lesser known. She speculated about reasons for this, including that the ceremony for Mayer took place shortly after Kennedy's assassination, which dominated the news. She also pointed to a bias in news stories reporting the three winners that year. Johnson showed how Mayer's father, and later her husband, American chemist Joseph Mayer, were very supportive of her work. Joseph Mayer respected and collaborated with her.

The third speaker was Prof. Elizabeth Baranger of the University of Pittsburgh, who spoke about "Remembrances of Maria Goeppert Mayer and the Nuclear Shell Model." Baranger is the daughter of the late Nobel laureate in chemistry Harold Urey who knew the Mayers well. She spoke fondly about getting to know and being inspired by Maria Mayer when the Ureys and the Mayers lived in the same neighborhood in New Jersey, and then later when both families moved to Chicago. Baranger pointed out how because of "nepotism rules," Maria Mayer was not appointed as a paid professor until later in life. For much of her seminal work, she was listed as "vol." or a "volunteer." Baranger also discussed Mayer's early life in Göttingen, where, because of her father being a professor, she led a life of privilege. Mayer worked with Max Born, who had a great influence on her.

Following the third talk, there was a lively discussion about the "nepotism rule" that many universities cited when barring couples from taking paid positions at the same university. All in all, it was an enlightened session about a pivotal physicist. ■

2013 Pais Prize Lecture: "The Joy of History"

By Roger H. Stuewer, University of Minnesota

Introduction

Physicists and historians of physics share a common goal, the quest for understanding, but their objects are different: Physicists attempt to understand Nature, while historians attempt to understand the past, and as the novelist L.P. Hartley (1895-1972) famously remarked, "The past is a foreign country: they do things differently there."[1] In 1891 the German polymath Hermann von Helmholtz reflected on his own researches, saying that:

I must compare myself to a mountain climber, who without knowing the way climbs up slowly and laboriously, must often turn around because he can go no farther, discovers new trails sometimes through reflection, sometimes through accident, which again lead him forward a little, and finally, if he reaches his goal, finds to his shame a Royal Road on which he could have traveled up, if he would have been clever enough to find the right starting point.[2]

This Royal Road, this linear, logical route to the summit, is eschewed by historians, who find both the challenge and joy of history in exploring the byways, uncovering the contingencies of historical events and shaping them into a coherent narrative. To illustrate this, I will draw on my own researches, showing how my analyses were based crucially on three different types of documentary evidence, private laboratory notebooks, unpublished personal correspondence, and the published literature.

The Discovery of the Compton Effect

When I began thinking about the discovery of the Compton effect, the first thing I did was read Arthur Holly Compton's classic 1923 paper,[3] and I soon realized that something important was missing. Nowhere in his paper did Compton mention Einstein's light-quantum (or photon) hypothesis or cite his famous 1905 paper, or even mention Einstein's name. I thus asked myself: Was it possible that Compton discovered the Compton effect essentially independently of Einstein's hypothesis? As we will see, I eventually was able to answer this historical question by analyzing Compton's laboratory notebooks.[4]

Compton's Ph.D. research at Princeton and his subsequent research at Minnesota during the academic year 1916-1917 focused on the scattering of X rays from crystals, but he could not pursue it at Westinghouse in Pittsburgh because it had nothing to do with the production of better light bulbs. Nonetheless, he kept abreast of the literature, and in 1917 came across a paper by C.G. Barkla,[5] in which he reported that X rays passing through aluminum had a mass-scattering coefficient smaller than the classical Thomson mass-scattering coefficient. To explain this, Compton eventually concluded that Barkla's X rays were being *diffracted* by *ring electrons* in the aluminum atom, which meant that the diameter of the ring electron, the diffracting obstacle, had to be comparable to the wavelength of the incident X rays, perhaps about 0.1 Ångstrom. That was a very large electron, but Compton showed that X rays incident on it would have a mass-scattering coefficient smaller than the classical Thomson value.

When Compton left Westinghouse to go to the Cavendish Laboratory in 1919, however, he soon discovered that its new Director, the blunt Ernest Rutherford, would have nothing whatsoever to do with this idea. Thus, Compton recalled that Rutherford once introduced him with the words: "This is Dr. Compton, who is with us from the United States to discuss his work on 'The Size of the Electron.' I hope you will listen to him attentively. But you don't have to believe him!"[6] Rutherford's first biographer also recalled that Rutherford once burst out saying, "I will not have an electron as big as a balloon in my Laboratory!"[7]

Compton also rethought his ideas after he carried out *γ-ray* experiments at the Cavendish and found that scattered γ rays became "softer" or of longer wavelength than the primary γ rays, which he eventually explained by assuming that the incident γ rays were striking tiny electron-oscillators in the scatterer and propelling them forward while they emitted a new type of Doppler-shifted *fluorescent γ radiation* of longer wavelength. Later, in the summer of 1920, when Compton left the Cavendish to go to Washington University in St. Louis, he took a Bragg spectrometer along with him, used it to produce a monochromatic beam of X rays, and by April 1921 found that when scattered they also excited a similar type of Doppler-shifted *fluorescent X rays* of longer wavelength.[8]

That fall Compton then used his Bragg spectrometer to compare the primary spectrum of MoK_α X rays of wavelength $\lambda = 0.708$ Ångstrom to their secondary spectrum when scattered through an angle of about 90˚ by pyrex and graphite. And this is precisely where my analysis of Compton's laboratory notebooks was crucial to understanding the further development of Compton's thought. Thus, Compton reported that the wavelength of the scattered X rays was about 35 percent greater than the wavelength of the primary X rays. I could not understand that claim—until I plotted Compton's data directly from his laboratory notebooks. My plots for a pyrex scatterer showed that the prominent line in the secondary spectrum is shifted to a slightly higher wavelength than that line in the primary spectrum. In other words, I knew what I was looking for, but Compton did not, so when he reported a huge 35-percent increase in wavelength it was clear to me that he had taken the primary spectrum to be the prominent high-intensity lines—which he took to be a single line at 0.708 Å—and the secondary spectrum to be the low-intensity lines—which he took to be a single line at 0.95 Å, and which we recognize today as the secondary MoK_α spectrum. To Compton, however, the ratio of the primary to secondary wavelengths was $\lambda/\lambda' = 0.708$ Å$/0.95$ Å $= 0.75$.

But how did Compton explain this huge shift in wavelength? It seemed clear to me that he must have invoked his Doppler-shifted fluorescent-radiation hypothesis, so I carried out the simple calculation that Compton had omitted: As seen at 90˚, the ratio of the primary to the secondary wavelengths is $\lambda/\lambda' = 1 - v/c$, where v is the velocity of the electron-oscillators that were emitting his new fluorescent X rays. But what was the velocity v? By "conservation of energy," that is, by setting $\frac{1}{2}mv^2 = h\nu$, we have that $\lambda/\lambda' = 1 - v/c = 1 - (2h\nu/mc^2)^{1/2} = 1 - [2(.017\ \mathrm{MeV})/(.51\ \mathrm{MeV})]^{1/2} = 1 - 0.26 = 0.74$. Who could ask for better agreement between theory and experiment? I have called this the *first phase* of Compton's *classical-quantum compromise*, which to me it is a splendid historical example of a *false theory* being confirmed by *spurious experimental data.*

One year later, by October 1922, Compton realized that he had misread his experimental data, and that the shift in wavelength between the primary and secondary X-ray spectra was only a few per cent. He now reported that the ratio of the primary to the secondary wavelength was $\lambda/\lambda' = 0.708\ \text{Å}/0.730\ \text{Å} = 0.969$. But how did he now explain this new experimental result? Again by his Doppler-shifted fluorescent-radiation hypothesis. Thus, at a scattering angle of 90˚ we again have that $\lambda/\lambda' = 1 - v/c$, but how did Compton now determine the velocity v of the electron-oscillators? By "conservation of momentum," that is, he set $mv = h/\lambda$ to yield $\lambda/\lambda' = 1 - v/c = 1 - h/mc\lambda = 1 - 0.034 = 0.966$. Again, who could ask for better agreement between theory and experiment? I have called this the *second phase* of Compton's *classical-quantum compromise*, which to me it is a splendid historical example of a *false theory* being confirmed by *good experimental data*.

Within a month, Compton put everything together.[9] He set up the correct vector diagram for conservation of momentum, invoked both conservation of energy and conservation of momentum, used the correct relativistic expression for the mass of the electron, and derived a formula equivalent to his famous expression for the change in wavelength, which at a scattering angle of 90˚ reduces to $\Delta\lambda = \lambda' - \lambda = h/mc = 0.024$ Å, which he compared to his experimental value of 0.022 Å. This, however, was precisely *the same experimental data that he had reported one month earlier, in October 1922*—he merely changed his formulas by substituting his new theoretical expression for his old one. Every physicist knows that good experimental data lasts forever, while theories come and go.

In sum, Compton followed no Royal Road to his discovery of the Compton effect. His thought evolved—over a period of six years—only as fast as his own experimental and theoretical researches progressed. His motivation never was to carry out an experiment to test or confirm Einstein's light-quantum (or photon) hypothesis, so we now can understand why Compton never cited Einstein's 1905 paper or even mentioned Einstein's name in his 1923 paper.

In complete contrast, Peter Debye at the Federal Institute of Technology (*Eidgenössische Technische Hochscule*, ETH) in Zurich, Switzerland, did follow a Royal Road by directly adopting Einstein's light-quantum hypothesis and discovering the same effect, seemingly virtually simultaneously. That, however, illustrates the contingency of the publication process. Compton submitted his paper to *The Physical Review* on December 13, 1922, where it was published in May 1923, while Debye submitted his paper to the *Physikalische Zeitschrift* on March 14, 1923, where it was published on April 15, 1923, that is, one month *before* Compton's paper was published in *The Physical Review*. Fortunately for Compton, Arnold Sommerfeld was just then a visiting professor at the University of Wisconsin, and when he returned home to Munich he spread the word that Compton had priority in both theory and experiment. Debye himself later insisted that it should be called the Compton effect, and not the Compton-Debye effect, because, he said, the person who did most of the work should get the name.

Millikan and the Photoelectric-Effect

No physicist to my knowledge was more concerned with trying to establish his priority than Robert A. Millikan. Thus, in his *Autobiography*, which he published in 1950 at the age of 82, he included a chapter entitled "The Experimental Proof of the Existence of the Photon," in which he wrote that at the April 1915 meeting of the American Physical Society in Washington, D.C., his experiments on the photoelectric effect constituted a "complete verification of the validity of Einstein's equation," adding that:

> This seemed to me, as it did to many others, a matter of very great importance, for it ... proved simply and irrefutably I thought, *that the emitted electron that escapes with the energy hν gets that energy by the direct transfer of hν units of energy from the light to the electron* [Millikan's italics] and hence scarcely permits of any other interpretation than that which Einstein had originally suggested, namely that of the semi-corpuscular or photon theory of light itself.[10]

In other words, *he*, not Compton, had first established the validity of Einstein's light-quantum (or photon) hypothesis. It seems, however, that Millikan never dreamed that someday some historian might actually read his 1915 paper. Thus, in 1915, at age 47, he had indeed established the validity of Einstein's equation,[11] which achievement, however, he clarified in his 1917 book, *The Electron*, writing that:

> Despite...the apparently complete success of the Einstein equation, the physical theory of which it was designed to be the symbolic expression is found so untenable that ... we are in the position of having built a very perfect structure and then knocked out entirely the underpinning without causing the building to fall. It [Einstein's equation] stands complete and apparently well tested, but without any visible means of support.... Experiment has outrun theory, or better, *guided by erroneous theory* [my italics], it has discovered relationships which seem to be of the greatest interest and importance, but the reasons for them are as yet not at all understood.[12]

Millikan's quest for priority thus led him to completely revise history. By then, however, he already had had some experience with that. Thus, in 1899 J.J. Thomson was photographed sitting in his study in Cambridge, England, in a chair that once had belonged to James Clerk Maxwell.[13] Seven years later, in

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1906, Millikan and Henry G. Gale published their textbook, *A First Course in Physics,* in which Millikan reproduced this picture of J.J. Thomson, but with a noticeable difference: He etched out the cigarette in J.J.'s left hand.[14] He evidently wanted his students to admire J.J. as a great experimentalist, but not to mimic his bad habit. I confess that I found some satisfaction, even a little joy, in catching Millikan out here. In any case, these two episodes beautifully illustrate what I like to call Millikan's philosophy of history: If the facts don't fit your theory, change the facts.

The Cambridge-Vienna Controversy on Artificial Nuclear Disintegration

In the late 1970s, I turned to the history of nuclear physics, building on my graduate research in nuclear physics and on the proceedings of a symposium I organized on the history of nuclear physics in the 1930s.[15] In one of my studies, I focused on a long controversy between Ernest Rutherford and James Chadwick at the Cavendish Laboratory in Cambridge and Hans Pettersson and Gerhard Kirsch at the Institute for Radium Research (*Institut für Radiumforschung*) in Vienna.[16] As we will see, its resolution would have remained unknown if I had not uncovered crucial correspondence between the protagonists.

A few months before moving from Manchester to Cambridge in the middle of 1919, Rutherford discovered that RaC ($_{83}Bi^{214}$) alpha particles could disintegrate a nitrogen nucleus, expelling protons that produced scintillations—tiny flashes of light—on a "scintillation screen." Rutherford knew from long experience that the observation of such scintillations was difficult and tedious, and also depended on the observer's optical system, training, experience, physical health, and psychological state.[17]

Rutherford and his right-hand man Chadwick found by 1921 that RaC alpha particles could disintegrate the nuclei of various light elements. In 1923, however, Pettersson and Kirsch in Vienna reported that RaC alpha particles could expel protons from many more nuclei. Moreover, they also challenged Rutherford's interpretation of the disintegration process. Rutherford and Chadwick, however, were undeterred. In 1924 they published a bar graph showing that protons could be expelled from many light elements, but not from carbon or oxygen, contrary to what Pettersson and Kirsch had claimed.

By July 1924, Rutherford, who was not famous for his patience, let off some steam to his friend Niels Bohr, writing, in the first of many letters I found in the Cambridge University Library, that:

> He [Pettersson] seems a clever and ingenious fellow, but with a terrible capacity for getting hold of the wrong end of the stick. From our experiments, Chadwick and I are convinced that nearly all his work published hitherto is either demonstrably wrong or wrongly interpreted.... It is a very great pity that he and his collaborators are making such a mess of things, for it is only making confusion in the subject.[18]

Here was Rutherford, by far the most revered experimental physicist of the period, placing Pettersson, a novice, on notice, as well as Pettersson's boss and Rutherford's friend, Stefan Meyer, Director of the Institute for Radium Research.

The battle lines therefore were drawn. By the summer of 1925 the tone of the controversy can be judged from a letter that Chadwick wrote to Rutherford while he and his wife were on an extended trip home to New Zealand and Australia:

> Our friend Kirsch has now let himself loose in the Physikalische Zeitschrift. His tone is really impudent to put it very mildly.... Kirsch & Pettersson seem to be rather above themselves. A good kick from behind would do them a lot of good. The name on the paper is that of Kirsch but the voice is the familiar bleat of Pettersson. I don't know which is the boss but as Mr. Johnson said there is no settling a point of precedence between a louse and a flea.[19]

The controversy was clearly heating up. In February 1926 Rutherford proposed an explanation of his and Chadwick's observational differences with the Vienna team in another letter to Bohr, writing that:

> The idea that you can discriminate between slow α particles and H particles [protons] by the intensity of the scintillation is probably the cause of their going wrong.... [Such] a discrimination by eye is terribly dangerous.[20]

In other words, a low-energy scattered alpha particle could produce a scintillation just as bright as a high-energy disintegration proton.

Only one avenue remained open to resolve the controversy, namely, an exchange of visits between the two laboratories. Pettersson visited the Cavendish first, in May 1927, each evening reporting his experiences to Meyer in long letters I found in the Institute for Radium Research. He told Meyer that Rutherford and Chadwick was treating him well, but nothing convinced him that their observations were correct.

Then the time came for the return visit. Rutherford was far too busy to make the trip, so he dispatched Chadwick to Vienna, where he and his wife arrived on Wednesday, December 7, 1927, and stayed in the comfortable Hotel Regina, about a ten-minute walk from the Institute for Radium Research. Chadwick told Rutherford in his first letter from Vienna that after unpacking he went to the institute and talked with Meyer and "Pettersson's people."[21] He made no progress in resolving the controversy, however, nor did he on Thursday, because it "was a holy day and no work could be done without danger to our future in the world to come." Friday, December 9, however, was very different. Chadwick told Rutherford that he and Pettersson "ended up with a fierce and very loud discussion." Nor would Pettersson agree to allow Chadwick to test whether carbon could be disintegrated, which "precipitated a most fiery outburst...." Chadwick wrote that Meyer and others "with no direct interest in the question [were] exceedingly pleasant and friendly but the younger ones [stood] around stifflegged and with bristling hair."

The atmosphere thus was extremely tense. As Elisabeth Rona, one of Pettersson's assistants, recalled:

> The impression made on us by Chadwick...was not favorable. He seemed to us to be cold, unfriendly, and completely lacking in a sense of humor. Probably he was just as uncomfortable in the role of judge as we

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were in that of the judged.[22]

Rona added that she later understood that Chadwick's "ordeal" when he was incarcerated in Berlin during the Great War "had much to do with his behavior" now that he again was in a German-speaking city.

On Monday, December 12, everything changed. Rona recalled that:

> All of us sat in a dark room [in the laboratory] for half an hour to adapt to the darkness. There was no conversation; the only noise was the rattling of Chadwick's keys. There was nothing in the situation to quiet our nerves or make us comfortable.[23]

Chadwick reported to Rutherford, that:

> I arranged that the girls should count and that I should determine the order of the counts. I made no change whatever in the apparatus, but I ran them up and down the scale like a cat on a piano—but no more drastically than I would in our own experiments if I suspected any bias.[24]

The result was that the counters found "no evidence" of disintegration protons from carbon. Chadwick added that he could see "no reason why the counters should be off colour."[25]

The Vienna scintillation counters, in fact, came as a great surprise to Chadwick. In Cambridge he and Rutherford regularly participated in the scintillation counting, but in Vienna, Chadwick told Rutherford,

> Not one of the men does any counting. It is all done by 3 young women. Pettersson says the men get too bored with routine work and finally cannot see anything, while women can go on forever.[26]

Chadwick also later recalled in an interview that Pettersson said he believed that women were more reliable than men as scintillation counters because they would not be thinking while observing, and that Pettersson preferred women of "Slavic descent" as counters because he believed that Slavs had superior eyesight.[27] We do not know in what tone of voice Pettersson made these remarks. We do know, however, that he respected his women scintillation counters, and they him, and that they in fact were remarkably talented scientists with outstanding careers ahead of them.*

Chadwick emphasized that the Vienna scintillation counters were not being dishonest; he suspected no cheating. Rather, they were deluding themselves. They knew that their bosses, Pettersson and Kirsch, believed that scattered alpha particles could be distinguished from disintegration protons by the brightness of their scintillations, while Rutherford and Chadwick knew that was impossible. Moreover, the Vienna scintillation counters were informed of the nature of the experiments, and they knew that Pettersson and Kirsch believed that carbon could be disintegrated. They therefore saw what they were expected to see. They had fallen prey to a psychological effect, much as in the famous earlier case of René Blondlot and his N Rays.

When Chadwick confronted Pettersson with these troubling results he become "very angry indeed,"[28] and when they met in Meyer's office on Wednesday morning, December 14, Meyer became "very upset indeed" and offered to do anything necessary to set the record straight, such as make a public retraction. Chadwick refused this suggestion, however, because he knew that Rutherford was adamantly opposed to public controversy, and also that Rutherford would not wish to do anything that might cause his friend Meyer pain, which a public retraction certainly would. Chadwick therefore told Meyer that the Vienna experiments should simply be dropped, and nothing further should be said about them.

In sum, the progress and resolution of the Cambridge-Vienna controversy would never have been known if the above correspondence had not been preserved. In fact, its resolution was kept so secret that not even those close to it but outside of the innermost circle permitted to know about it. Elisabeth Rona, for example, later wrote that, "As far as I know, the discrepancies between the two laboratories were never resolved."[29]

The Meitner-Frisch Interpretation of Nuclear Fission

My final story began when I asked myself, How did it happen that Lise Meitner and her nephew, Otto Robert Frisch, were able to propose their novel and correct interpretation of nuclear fission when they met in Kungälv, Sweden, near Göteborg, over the Christmas holidays of 1938?[30] The answer, I found, emerged from a careful analysis of the published literature and supports, I believe, Arthur Koestler's analysis of the act of creation, namely that:

> the more original a discovery the more obvious it seems afterwards. The creative act is not an act of creation in the sense of the Old Testament. It does not create something out of nothing; it uncovers, selects, re-shuffles, combines, synthesizes already existing facts, ideas, faculties, skills.[31]

To Koestler the creative act thus constitutes the synthesis of what he called two previously unrelated "matrices of thought."[32]

I knew that to understand Meitner and Frisch's creative act I had to understand the origin of the liquid-drop model of the nucleus, which, contrary to what many physicists seem to believe, was not invented by Niels Bohr in 1936 but was invented by George Gamow eight years earlier, at the end of 1928, while he was at Bohr's institute in Copenhagen.

Gamow imagined that the nucleus consists of a collection of alpha particles with short-range attractive forces between them that balance their Coulomb repulsion, and that they exert an outward pressure owing to their kinetic and potential energy but are held inside the nucleus by its "surface tension." He

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* *Elisabeth Rona (1890-1981)* was born in Budapest, immigrated to America in 1941, worked first at Argonne National Laboratory after the war, and became a Senior Scientist at Oak Ridge Associated Universities in 1950. *Marietta Blau (1894-1970)* was born in Vienna, invented the emulsion technique for detecting charged particles in Vienna in 1925, immigrated first to Mexico in 1939 and then to America in 1944 where she worked at Columbia University, Brookhaven National Laboratory, and the University of Miami before returning to Vienna in 1960. *Elizabeth Kara-Michailova (1897-1968)* was born in Vienna, moved with her family to Sophia, Bulgaria, in 1909, returned to Vienna from 1922-1935, and returned to Bulgaria in 1939 where she became the first woman to be elected to the Bulgarian Academy of Sciences. *Berta Karlik (1904-1990)* was born in Vienna, became the first woman to be appointed as full professor at the University of Vienna in 1956 and the first woman to be elected to the Austrian Academy of Sciences in 1973.

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calculated the total "drop energy" of the nucleus in terms of the number of alpha particles in it, and found that a plot of the resulting mass-defect curve has a distinct minimum in it. Five years later, in 1933, after Chadwick's discovery of the neutron, Werner Heisenberg recalculated the total energy in terms of the number of neutrons and protons in the nucleus and again found that the mass-defect curve has a distinct minimum in it. Then, in 1935, Heisenberg's student, C.F. von Weizsäcker, extended his mentor's work, introducing his famous semi-empirical mass formula, and again finding the same distinct minimum in the mass-defect curve. His result thus constituted the culmination of the line of development that Gamow had inaugurated in 1928.

A second phase in the history of liquid-drop model began in February 1936 when Niels Bohr published his theory of the compound nucleus. He argued that a neutron incident on a heavy nucleus interacts with many neutrons and protons in it, producing an excited, long-lived compound nucleus, which then decays by the emission of a proton, neutron, gamma ray, or by any process consistent with conservation of energy. Bohr went on to claim that if the energy of the incident neutron were increased more and more, even up to 1000 MeV, then many charged or uncharged particles would be expelled and the entire nucleus would eventually explode. Otto Robert Frisch drew a picture for Bohr that illustrated the early stage of this process, showing an incident neutron transferring energy to the target nucleus, causing the excited compound nucleus to first heat up, and then to cool down by the evaporation of a single particle from its surface. Bohr and his assistant, Fritz Kalckar, developed this idea further in 1937.

We therefore see that the liquid-drop model of the nucleus developed in two stages, first from 1928-1935 with the work of Gamow, Heisenberg, and von Weizsäcker, who calculated the *nuclear mass-defect curve* by focusing on *static* features of the model, and second, from 1936-1937 with the work of Bohr and Kalckar, who calculated *nuclear excitations* by focusing on *dynamic* features of the model. I showed that the first stage persisted in Berlin into 1938, and that the second stage persisted in Copenhagen, also into 1938. And just at that time, in the middle of 1938, Lise Meitner, who had been thoroughly embedded in the Berlin tradition, was spirited out of Berlin and eventually made her way Stockholm, while her nephew, Otto Robert Frisch, had been thoroughly embedded in the Copenhagen tradition since 1934 while working in Bohr's institute.

Based on Frisch's published recollections, I concluded that the Berlin and Copenhagen traditions merged in his and Meitner's minds during their memorable walk in the snow in Kungälv, Sweden, over the Christmas holidays of 1938. I reconstructed their conversation as follows: First, Meitner rejected the idea that Otto Hahn had made a mistake when he and Fritz Strassmann reported finding barium, an element of intermediate atomic weight, when neutrons bombarded uranium. Both Meitner and Frisch then sensed that this could not be explained by a chipping off or cracking up of the uranium nucleus. Meitner, it seems, then thought of the liquid-drop model of the nucleus in this connection, while Frisch probably suggested the possibility that an incident neutron would induce oscillations in it, since he had sketched just such a picture for Bohr. Meitner then drew a large circle with a smaller circle inside it, which Frisch immediately interpreted as an end-on view of a dumbbell—as an elongated liquid drop with a constriction between its two halves. Meitner, whom Frisch recalled "had the mass-defect curve pretty well in her head," then estimated from it that about 200 MeV—an enormous amount of energy—would be released if the heavy uranium nucleus were split up into two nuclei at the middle of the periodic table. Meanwhile, Frisch had realized that the repulsive surface charge of a heavy nucleus like the uranium nucleus of atomic number about 100 would offset its attractive surface tension. He also calculated that two nuclei in the middle of the periodic table, if initially in contact, would fly apart under their mutual Coulomb repulsion with an energy of about 200 MeV—in agreement with Meitner's figure. As Frisch said, "We put our different kinds of knowledge together." I think Koestler would have said that a synthesis of their two different "matrices of thought" occurred.

When Frisch returned to Copenhagen and told Bohr about his and Meitner's interpretation, Bohr burst out, saying,"Oh, what fools we have been! We ought to have seen that before." Bohr now immediately saw that he had missed this Royal Road to the summit, while by exploring its byways I saw that he had been missed it, at least in part, by proposing a very different picture, that a neutron of increasing energy incident on a heavy nucleus would eventually cause it to explode. Meitner and Frisch, by contrast, had climbed to the summit by following two different routes that were contingent on their different personal trajectories and scientific knowledge.

The Human Factor

In closing, I note that besides sharing the common goal of understanding, both physics and history of physics are human endeavors, and nothing has given me more joy over the years than working with many physicists, historians, and others in a variety of activities, for example, in founding and directing Minnesota's Program in History of Science and Technology, in organizing the 1977 Minnesota symposium on the history of nuclear physics in the 1930s and the annual Seven Pines Symposia, in editing the Resource Letters of the *American Journal of Physics* and co-editing *Physics in Perspective*, and in contributing to the professional activities of the History of Science Society, American Physical Society, American Institute of Physics, and American Association of Physics Teachers. I am deeply grateful to my many students, colleagues, and friends who have joined and supported me in these richly rewarding endeavors.

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April Sessions cont'd

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History of G-2: Experiment and Theory
Co-sponsored by FHP and DPF

Session Organizer and Chair:
Robert P. Crease, Stony Brook University

"Study of Lepton G-2 from 1947 to Present," Toichiro Kinoshita, Cornell

"The First CERN Muon G-2 Experiment by G. Charpak, F. J. M. Farley, R. L. Garwin, T. Mueller, J. C. Sens, and A. Zichichi," Richard Garwin, IBM

"The BNL Muon G-2 Experiment and Beyond," Yannis Semertzidis, BNL

Gaining Inspiration From Galileo, Einstein, and Oppenheimer

Session Organizer and Chair:
Catherine Westfall, Michigan State University

"Galileo As An Intellectual Heretic And Why That Is Important," Paolo Palmieri, University of Pittsburgh

"Walking in the Footsteps of Einstein: Why History of Physics Aids Physics Education," Gerd Kortemeyer, Michigan State University

"Using the History of Physics to Enrich Your Teaching," B. Cameron Reed, Alma College

Journeys in the History of Physics: Pais Prize Session in Honor of David Cassidy

Session Organizer and Chair:
Catherine Westfall, Michigan State University

"Physics History, and Biography," David Cassidy, Hofstra University

"Towards A Rethinking Of The Relativity Revolution," Daniel M. Siegel, University of Wisconsin, Madison

"How a Research Physicist Got Involved With History of Science," Brian Schwartz, Brooklyn College and The Graduate Center, CUNY ■

Einstein and Szilard

Pais Prize cont'd

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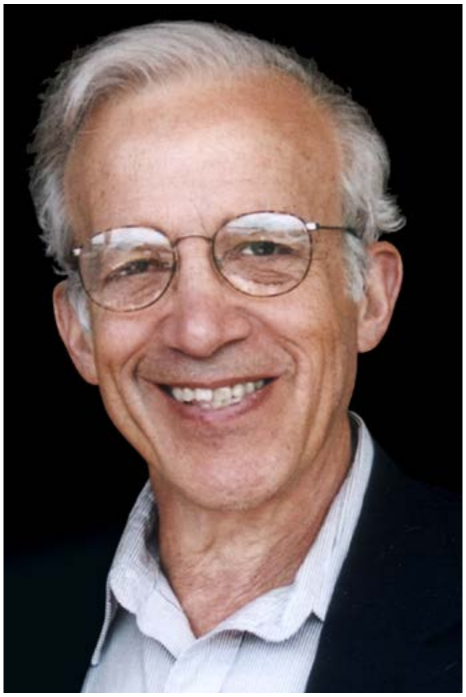
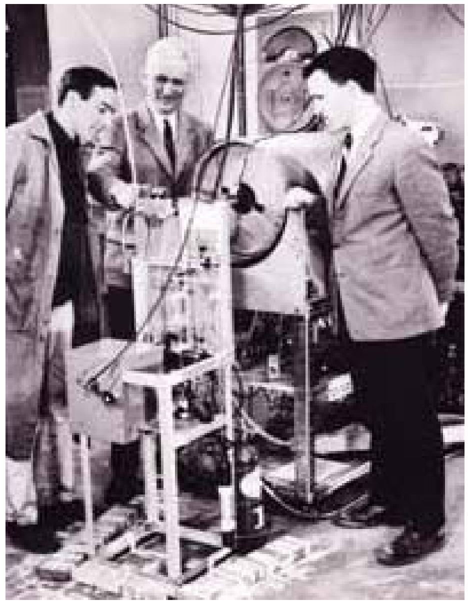
Daniel Kleppner Awarded Prestigious Benjamin Franklin Medal of the Franklin Institute

By Thomas M. Miller



FHP Past Chair, Daniel Kleppner



L-R: S. Cramton, N. Ramsey, D. Kleppner. The H-maser at Harvard University. Credit: AIP Emilio Segrè Visual Archives, Ramsey Collection.

A recent Chair of FHP, Daniel Kleppner, has been awarded the prestigious Benjamin Franklin Medal of the Franklin Institute. Kleppner is Lester Wolfe Professor of Physics, Emeritus, at MIT and co-director of the NSF MIT-Harvard Center for Ultracold Atoms. The citation for the medal reads, "The 2014 Benjamin Franklin Medal in Physics is awarded to Daniel Kleppner for many pioneering contributions to discoveries of novel quantum phenomena involving the interaction of atoms with electromagnetic fields and the behavior of atoms at ultra-low temperatures."

Earlier in 2013, Kleppner had been honored at an international symposium in Brazil on the occasion of being named Emeritus Professor of the Institute of Physics of São Carlos of the University of São Paulo. Upon the many accolades, Kleppner reported that "I felt that I had been canonized." The term is appropriate because many of the present miracles in atomic, molecular, and optical physics are based on work carried out by Kleppner. He laid the groundwork for research with trapped cold atoms, leading to the experimental attainment of Bose-Einstein condensation in 1995.

Kleppner was a PhD student of Norman Ramsey at Harvard in the 1950s, and Kleppner remained at Harvard into the 1960s before moving to MIT. His research at Harvard is noted for the invention with Ramsey of the hydrogen maser, which was the basis for early generations of atomic clocks.

Kleppner is a member of the National Academy of Sciences and has served both NAS and APS for decades on various committees. He was Chair of DAMOP in 1983/84 and Chair of FHP in 2010/11. During the controversy over the proper wording of an APS statement on climate change, Kleppner was chosen to come up with a correct version. Among Kleppner's honors are the Frederic Ives Medal (2007), the National Medal of Science (2006), the Wolf Prize in Physics (2005), the Lilienfeld and Davisson-Germer Prizes of the APS (1990 and 1985).

Pais Prize cont'd

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24. Chadwick to Rutherford, December 12, [1927], RC.
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27. Interview of James Chadwick by Charles Weiner on April 17, 1969, Session III, Niels Bohr Library and Archives, American Institute of Phyiscs, College Park, MD USA, p. 10 of 37.
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New Books *of* Note

String Theory and the Scientific Method

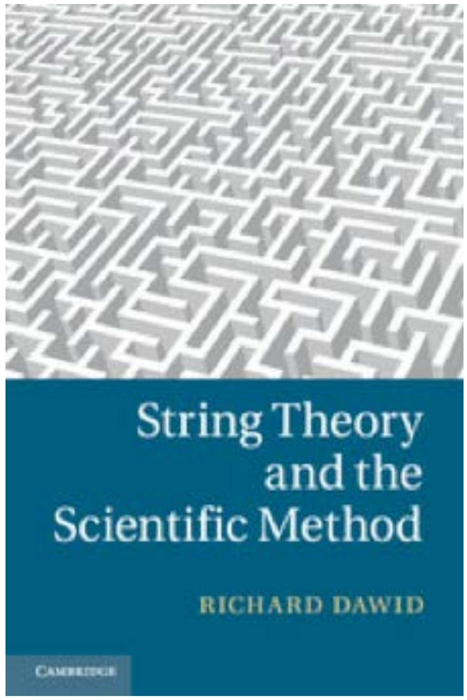
By Richard Dawid | *Cambridge: Cambridge University Press, 2013, 210 pp., $95 (hardback)*

Reviewed by Robert P. Crease

String theory is not only provocative within physics, argues Richard Dawid, a physicist and philosopher of science at the University of Vienna in this noteworthy new book, it also poses an important challenge to philosophy.

In what Dawid calls the "classical paradigm of theory assessment" of traditional Anglo-American philosophy of science, the scientific process can be neatly parsed into a core body of confirmed knowledge, speculations, and testing of such speculations. String theory, however, is notoriously empirically untestable at present, and almost certainly will continue to be so for decades. According to classical philosophy of science, therefore, it should have negligible scientific status. For precisely that reason, critics such as Lee Smolin and Peter Woit have condemned its influence. Yet as Dawid writes, other theorists regard string theory as "well-established and authoritative." Theoretical physicists seem schizophrenic on the subject; while many find string theory "too good to be false," others see it as evidence of groupthink. Dawid's book addresses precisely this discrepancy: "the serious mismatch between the status one would have to attribute to string theory based on the canonical paradigm of theory assessment and the status the theory actually enjoys." The classical paradigm is wrong, Dawid concludes, and he seeks to correct it by arguing for the relevance of "non-empirical theory assessment."

Thomas Kuhn effectively invoked non-empirical theory assessment in his Structure of Scientific Revolutions (1962), but confined it to the choice between paradigms in revolutionary science. In Progress and its Problems (1977), Larry Lauden saw a role for non-empirical theory assessment in more everyday scientific practice. Dawid begins his argument with "scientific



underdetermination," or the fact that when one promising theory points to a certain empirical outcome, a host of other less promising ones will do likewise, meaning that other factors must be in play to account for the theory's desirability. The scientific underdetermination, Dawid concludes, is "limited." Dawid proposes three such limitations to account for the promise of string theory: the "No Alternatives Argument" (NAA), or the fact that string theory is the only available option for constructing a unified theory of quantum interactions and gravity; the Unexpected Explanatory coherence Argument (UEA), or the fact that string theory has provided unexpected deeper explanations of seemingly disparate concepts; and the Meta-Inductive Argument (MIA), based on an analogy between string theory and the earlier research program that led to the Standard Model.

Dawid concludes that the "precarious empirical status" of contemporary physics stems in large part from "the general mechanism" that has driven physics for a half-century; namely, steadily rising collision energies in experimentation and the increasing role of gauge symmetries in theory-building. He calls the outcome the "marginalization of the phenomena." He also argues that string theory is a candidate for a truly final theory, for it implies, by its structure, a limit to new physics.

In making the extended argument that the "canonical reconstruction of scientific theory assessment in physics is inadequately narrow," Dawid keeps his feet firmly planted within the narrow confines of the Anglo-American philosophical tradition, which is focused on developing a logic of science. He does not, for instance, draw from Science, Technology, and Society (STS) studies, whose starting point is the actual practice of science. Nor does Dawid consider Continental approaches (such as Husserl's) that do not begin by attempting to tell scientists how they work, logically speaking, but by examining meaning-formation in scientific experience. Dawid's "brief excursion" into palaeontology would have been more profitable had it recognized that paleontologists make progress less by speculating about and confirming underdetermined theories than by understanding the worlds inhabited by dinosaurs, a much different process.

String Theory and the Scientific Method is clearly written and well argued, one of the clearest expositions of string theory accessible to a non-physicist that I have read. Though narrowly conceived, and yearning to be informed by a broader philosophical perspective, it is an important contribution to traditional Anglo-American philosophy of science insofar as its initial inspiration is not the urge to develop the logic of science for its own sake, but the mismatch between the inherited picture of that logic and the actual experience of scientists.■