History of Physics NEWSLETTER

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2011 Pais Prize Lecture: Shelter Island Revisited

By Silvan S. Schweber

The Shelter Island Conference on Quantum Mechanics was a landmark. It was the first of three small post-World War II conferences on theoretical physics sponsored by the National Academy of Sciences (NAS). The first took place June 2–4, 1947, at the Ram's Head Inn on Shelter Island at the tip of Long Island. The second, the Pocono Conference, was held from March 30 to April 2, 1948, and the third, Oldstone, occurred April 11–14, 1949.

The initial impetus for the Shelter Island Conference came from Duncan MacInnes, a distinguished physical chemist at the Rockefeller Institute, who had suggested to Frank Jewett, then president of the National Academy of Sciences (NAS), that it sponsor a series of small conferences. The participants (see Fig. 1) were primarily theoretical physicists, most of whom had been leaders at the highly successful wartime laboratories: the MIT Radiation Laboratory, Chicago Metallurgical Laboratory, Johns Hopkins Applied Physics Laboratory, and Los Alamos. Also in attendance were several experimental physicists including Willis Lamb and Isidor Rabi, who reported on experiments on the spectrum of hydrogen that had been recently performed at Columbia University, and Bruno Rossi who reported on the results of researches carried out in Rome on the atmospheric absorption of cosmic rays.

The significance of the conference was soon as evident to the participants as it is now in retrospect. Six months later, in January of 1948, APS secretary Darrow wrote his fellow organizer Duncan MacInnes a brief postcard stating, "I must quote [you] . . . the words of warm commendation used yesterday by I. I. Rabi about your Shelter Island meeting—he said that it has proved much more important than it seemed even at the time, and would be remembered as the 1911 Solvay Congress is remembered, for having been the starting-point of remarkable new developments." Similarly, Richard Feynman many years later recalled, "There have been many conferences in the world since, but I've never felt any to be as important as this."

The meeting turned out to be the most seminal conference held right after the end of World War II; its impact was indeed comparable to that of the Solvay Congress of 1911. Just as that meeting set the stage for all the subsequent developments in quantum theory, similarly Shelter Island provided the initial stimulus for the post-World

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Fig. 1. Assembled attendees of the June 1947 Shelter Island Conference on Quantum Mechanics. Left to right: Isidor Rabi, Linus Pauling, John Van Vleck, Willis Lamb, Gregory Breit, Duncan MacInnes, Karl Darrow, George Uhlenbeck, Julian Schwinger, Edward Teller, Bruno Rossi, Arnold Nordsieck, John von Neumann, John Wheeler, Hans Bethe, Robert Serber, Robert Marshak, Abraham Pais, Robert Oppenheimer, David Bohm, Richard Feynman, Victor Weisskopf, Herman Feshbach. Not included here is Hendrik Kramers. Photograph courtesy of AIP Emilio Segrè Visual Archives, Marshak Collection.

In This Issue

Editors' Corner

Fig. 1. My house (transformed into a high-entropy state) on May 24, 2011. I needed to clean out that garage anyway. (Photo by Mark Winslow, of the SNU Physics Department)

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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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The Spring 2011 issue of the FHP *Newsletter* was unfortunately delayed due to inclement weather. On May 24, in a spectacularly violent display of non-equilibrium thermodynamics in a nonlinear system, an EF5 tornado collided with our house, and the houses of our neighbors (see Fig. 1).

My priorities suddenly changed. Thankfully, former Editor Michael Riordan, and the recently appointed next Editor Robert Crease, approached me through an email message aptly entitled "Your Plight," and offered to steer the Spring 2011 issue to completion. Their thoughtful initiative solved a pressing logistical dilemma for me, and foreshadowed a flood of thoughtfulness to come.

I am not writing this to elicit sympathy—we are going to be fine—but to express gratitude. Within hours of this event and ever since, my wife Rhonda and I have received e-mails, telephone calls, and letters from friends and colleagues. I wish to take this opportunity to express our gratitude and appreciation to the Forum on History of Physics members who sent messages of concern, and offered various means of helping us through this challenging time. Throughout this experience we found a force stronger than a mere EF5 tornado: the care and concern of so many kind and generous people. We have so much for which to be grateful.

Having your house suddenly annihilated by a tornado imposes a Janus-like predicament, where you find yourself looking backwards and forward simultaneously. New beginnings can be good. Rhonda always wanted to design her own house, and now she can do so. But looking back forms an indelible part of the experience. Although a tornado makes history, *history lost* must be counted among its casualties. Before going there, let me return to the tornado itself.

The May 24 tornado that honored us with its impersonal visit cut a 65-mile swath across central Oklahoma (the English units stick in the mind from local weather reports). Its rain-wrapped, half-mile-diameter main vortex evidently carried within it at least four tighter vortices. We have reason to think that one of them passed through our den and dining room. That part of the house was cleaned off down to the slab; the rest of the structure collapsed into a pile of twisted rubble. Our roof, doors, appliances, sofas, and dining room table were nowhere to be found, not even as fragments. Tornadoes famously produce bizarre effects—your refrigerator vanishes but you find the tiny wheel of a toy car. We have done the experiment, and can affirm that, for objects having large surfaces, the integral of PdA trumps mg when the wind slams into your house at 300 mph. I wish that high-speed cameras, like those used in the nuclear weapons tests of the 1950s, had been mounted throughout the house. It would have been interesting to see how our Silverado ended up on top of the Highlander. Most of the vehicles on our street landed in a wheat field half a mile from where they had been parked, folded into a right angle or rolled into a ball with wheels sticking out. When the tornado hit, we were on our way home but were turned back twice by the police. Had we been at home we would not have survived. Our new house will have an underground shelter. Assuming that one can dash into a neighbor's shelter now seems too uncertain a strategy. We are thankful to still be among the living. We can still look forward to seeing our grandchildren grow up.

Not everyone was so fortunate. Although no person on our street perished, this particular twister killed ten people in Oklahoma that day. On our street eight horses lost their lives, including our own dear Annie, my wife's equine companion

Fig. 2. Your outgoing editor hard at work a few days after May 24. (Photo by Daryl Cox of the SNU Chemistry Department)

for eleven years. Our two weenie dogs, Heidi and Maggie, amazingly survived unscathed. Their steel wire kennel was found wedged between two heavy pieces of furniture buried beneath rubble at the far end of the house. They were probably the only witnesses above ground who emerged unhurt. I wish they could tell us what they saw and heard as the roof lifted off, and the walls crashed around them in a turbulent blizzard of sheetrock fragments, 2x4s, and muddy insulation.

A house is a state of matter that exists in the transition between the Home Depot and landfill phases. The morning after the tornado my son Charlie said to me, "Dad, you and Mom don't have a house right now. But you are not homeless." That was a perceptive distinction. Garrison Keillor once said that "Home is where they know your story." History is the knowing of the story. Without our story, we do not know who we are.

Once all lives were accounted for, an early priority included salvaging whatever could be found that was worth saving. In looking backwards this way, we were not trying to salvage *things;* we were trying to salvage *stories*; we were trying to preserve our *history.* Some items have value for what they *are,* and can be replaced with mere money. But other artifacts hold deeper value; they are irreplaceable because of what they *mean.* The wedding photos, the baby pictures, images of family vacations, artifacts handed down from one's ancestors…

When my wife looks at her grandma's pedal-operated sewing machine (which survived with repairable damage), she sees more than an obsolete technology for sewing clothes. She also knows the story of how in 1956 her mother made her wedding dress, with *her* mother's help, on this very machine. The memory of Grandma Coatney and those events are held within its cabinet.

A creation myth of the pueblo-dwelling Native Americans tells how the People came up from the mysterious nether regions below. The sanctity of a timeless cultural custom was explained by saying "it came up with us."[1] In our salvage efforts, artifacts that were found too seriously damaged to keep were set aside with surprising reluctance. They came through the tornado with us.

A few days after the tornado, Rhonda sadly reflected that "Although our old house had many flaws, I am going to miss it because Charlie and Steven [our sons] will not be able to show Teegon and Sophie [our grandchildren, too young to remember these events] the house in which they were raised." Of course, we will make new memories in the new house. But my wife is right: a part of personal history was a casualty of the May 24 tornado. Grief and gratitude wash over us together, in abundance.

Before the tornado, hanging in my shop was the very chair that my great-grandfather Ed "borrowed" from a hotel front porch in Junction City, Kansas, sometime in the 1890s. He and my great-grandma Grace, as young people not yet married, were on a hayride with their friends. All the hay bales were occupied, so as they rode by a hotel Ed hopped off the wagon and grabbed the chair so Grace would not have to sit on the wagon floor. My father remembers that chair from the summer of 1940 he spent with his grandparents. Seventy years after that, hanging in my shop, it was more than a rickety old chair: it was a tribute to Great-Grandpa Ed's rascally sense of humor. That wobbly green wooden chair is now gone. Telling my grandchildren about their colorful, ornery, fun-loving great-great-great grandpa became more difficult after May 24.

When an artifact has vanished, the telling of the story is diminished. One cannot save everything, but one must save *some* things, for history. Seeing with you own eyes Galileo's telescope or Marie Curie's letters, Albert Einstein's pipe or one of Niels Bohr's notebooks, brings the story (and the physics) to life. The real thing has no substitute. It came up with us.

The stories of the people and places behind the physics are more complicated than the body of physics knowledge itself, forming incredibly complex webs of relationships that stretch across time and cultures. But without its stories, physics becomes homeless, and loses its identity. The Forum on History of Physics carries an awesome responsibility as a keeper of stories for the physics community.

This is the last time I have the privilege of writing an "Editor's Corner" for this splendid newsletter that others started, and with whose care I have been entrusted for a season. This is not the way I wanted my scheduled term as editor to end (Fig. 2).

But I am grateful to Bob and Michael for voluntarily offering to finish what I was unable to finish myself. I will remain an enthusiastic member of the FHP, and hope to be an occasional contributor to these pages.

So many of you have looked after my wife and me with caring concern during these recent events. I knew you as colleagues in APS meetings, sometimes sitting with you at committee tables. When you heard that the world line of the May 24 tornado had abruptly intersected ours, you went beyond roles and duties. You did what the Russell-Einstein Manifesto of 1955 exhorted us to do in another context: "remember your humanity." For your kind gestures, for your friendship, and for all that I have learned from you, I will always be grateful. Thank you. ■

— Dwight E. Neuenschwander

[1] Paul Horgan, *The Heroic Triad: A Portrait of Three Southwestern Cultures* (Holt, Reinhart, & Winston, 1970).

History of Superconductivity Sessions

By George Zimmerman

This year was the 100th anniversary of the discovery of superconductivity, and the APS March meeting in Dallas featured over a dozen sessions devoted to the phenomenon. The one sponsored by the Forum on the History of Physics took place at mid-morning on Monday, March 21, 2011. The five speakers at the session, chaired by Martin Blume (Fig. 1), dispelled some of the commonly held stories about the discovery of superconductivity, pointing to the intense interest of the physics community in that discovery. Some of the most famous names in physics at the time tried to find a theoretical explanation of the phenomenon, with little success. The speakers also highlighted some of the important discoveries about the nature of superconductivity that led to a theoretical understanding and reformulation, culminating in the Bardeen-Cooper-Schrieffer (BCS) theory [1]. The session concluded with a review of the present applications of superconductivity and their future promise.

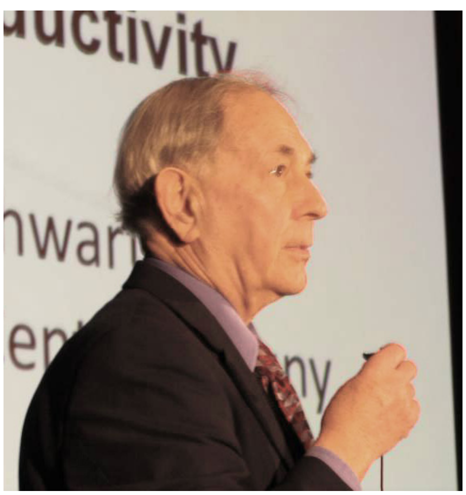

Fig. 1. Session Chair Martin Blume

The first speaker of the session was Dirk van Delft, Director of Museum Boerhaave at Leiden, who published an article in the September 2011 issue of *Physics Today* on the same subject and one in the March 2008 issue on the liquefaction of helium [2, 3]. The discovery of superconductivity can be pinpointed to 8 April 1911. Unlike the popular story that the observation of superconductivity was first thought to be a short circuit in the apparatus, the lab notes of Heike Kamerlingh Onnes reveal his awareness that he had discovered a new phenomenon. The results were announced in October of that year, both at Leiden and at the First Solvay Conference held in Brussels at the Metropole Hotel. The latter brought together such luminaries from the world of physics as A. Einstein, who was the second youngest member at the conference, M. Curie, M. Brillouin, H. Poincaré, and others. Another point highlighted by van Delft was that many of the experimental discoveries attributed to Kamerlingh Onnes, a theoretical physicist, would not have been possible without the expert technicians he had in his employ. While he acknowledged these technicians, they were not made co-authors on the publications.

The second speaker was Brian Schwartz (Fig. 2), currently at the Graduate Center at CUNY and previously associate Director of the Francis Bitter National Magnet Laboratory. Schwartz outlined the discoveries and phenomenological or other theories which lead to the eventual formulation of the BCS theory [1]. These included the Meissner effect, by Meissner and Ochsenfeld in 1933 [4], the isotope effect discovered independently by E. Maxwell and B Serin *et al.* in 1950 [5, 6], and the energy gap (See reference 30 in [1]) in 1953-4 which became evident from many experimental results. Schwartz also reviewed some of the phenomenological formulations, among which were the two fluid theory developed by Casimir and Gorter in 1934 [7] and also L. Tisza, the Heintz and Fritz London formulation of Maxwell's equations for superconductivity [8], the Pippard non-local theory [9], and the Ginzburg-Landau equations [10].

Because the third speaker, Leon Cooper—the "C" in the BCS theory [1]—was unable to attend the March meeting, a video was shown of him speaking before an audience at Brown University on December 10th, 2010 entitled "The Road to and from BCS."

His lecture briefly described the discovery of superconductivity by

Fig. 2. Brian Schwartz of CUNY.

Kamerlingh Onnes and the interest it evoked in the world of physics at that time. It then went on to an enumeration of some of the unsuccessful attempts at theories trying to explain the phenomenon. Those attempting explanations included Einstein, Feinman, Froelich, and even Bardeen [11], the "B" in BCS. Cooper then detailed the intense activities and calculations undertaken by him, Schrieffer and Bardeen, who wrote an article on superconductivity in the *Encyclopedia der Physik* only a few years before the BCS theory was published. The talk concluded with a review of the technological promises of superconductivity and a call in support of pure research, since the outcome of such investigations might not be utilized for years or decades. A link to this talk is available on the web at:

https://dropbox.brown.edu/download.php?hash=679d1d78.

John M. Rowell, the fourth speaker, who is now at the Arizona State University and was at The Bell Labs from 1961 to 1983, described developments after the BCS theory from 1957 to 1967, which enabled superconductivity to be applied to the production of high magnetic fields for particle accelerators and Magnetic Resonance Imaging. His talk entitled "Giaever, Nb3Sn, and Josephson" described the experiments, which led to the measurement of the energy gap and the discovery of the

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"Hard or Type II" superconductors, which enabled the manufacture of wires which were superconducting in high magnetic fields although partially penetrated by them. The understanding of the Abrikosov vortex structure contributed greatly to that advance. He then went on to the prediction of the Josephson effect and its applications in extremely sensitive measuring devices.

Fig. 3. Paul Chu of the University of Houston.

The final speaker was Paul Chu (Fig. 3), of the Texas Center for Superconductivity, University of Houston, who with scientists at Bell Labs is widely credited in the creation of materials raising the transition point of high temperature superconductors above the boiling point of liquid nitrogen, thus making them usable with much less maintenance than required by the previously used cryogen, helium. Chu begun by pointing out the excitement with which discoveries are made, especially in superconductivity, and reviewed the history of experiments aimed at the discovery of materials with ever higher superconducting transition temperatures. These included the efforts of B. Matthias, who managed to raise the transition temperature of alloys above 20K. Chu then discussed the discovery of high temperature superconductivity in 1986 by J.G. Bednorz and K. A. Miller, and the quest to synthesize materials with higher temperature transition points. That brought renewed attention to the subject, culminating in the "Woodstock of Physics" at the 1987 March meeting in New York City, and a demonstration before President Ronald Reagan – the pinnacle of the United States national interest in superconductivity. Although the number of publications dealing with high temperature superconductivity has declined, research is ongoing and applications are coming on line. The talk concluded with a vision of the great promise in the application of high temperature superconductivity to the storage and transmission of "green" electrical power in the near future.

All of these talks will be posted on the FHP website in the near future.

Another session on superconductivity was sponsored by the FHP at the April Meeting in Anaheim California. That session took place on May 1 and featured three speakers, with Martin Blume presiding. The first speaker, Peter Pesic of St. John's College, provided a historical timeline that began in 1832 when Michael Faraday liquefied chlorine, and included Van der Waals' formulation of the gas equations in 1873, William Ramsey's discovery of helium on earth in 1895, William Dewar's 1898 liquefaction of hydrogen, and Heike Kamerlingh Onnes—who was one of Van der Waals' students—liquefying helium in 1908. Three years later superconductivity was discovered in mercury. This, Pesic pointed out, was only one of the achievement of the "Golden Age" of Dutch science. An adjunct to the Kamerlingh Onnes laboratory in Leyden was a school for skilled technicians, and the use of that facility and personnel was a significant factor in the discovery of superconductivity. Giles Holst, one of Kamerlingh Onnes' assistants, was actually the one who first observed the phenomenon. At that time, the Leyden team also observed the 2.2K superfluid transition of liquid helium but was not aware of what it was. In 1914 the team demonstrated a conducting ball floating above a superconducting ring. A sketch of this experiment by Gerrit Flim, an assistant to Kamerlingh Onnes, was made in preparation for the actual experimental performance indicating the understanding of the superconductivity phenomenon.

David Larbalestier, of the National High Magnetic Field Laboratory and Department of Physics, Florida State University, provided an overview of the practical applications of superconductivity. He pointed out that it took much preparation and careful planning to liquefy helium in order to carry out the discovery. After being hired by Leyden in 1882, Kamerlingh Onnes did not publish any articles for ten years. However, from the time of the discovery of superconductivity, he was pursuing practical applications. According to Larbalestier's abstract, "In fact Onnes came to Chicago in 1913, just two years after discovering superconductivity, with a detailed plan to make a 10 T superconducting magnet!" The unexpectedly low critical current of mercury made that project unfeasible; but 48 years later, the discovery of type II superconductors made high field magnets possible. Larbalestier then went into the details of superconducting wire and magnet design and their applications in MRI and particle accelerators. The discovery of high transition temperature superconductors (HTTS), which took place 25 years ago, is only now leading to improvements in the superconductor tape-wire current-carrying properties over those of the type II superconductor technology. He went on to describe the problems that had to be overcome to manufacture HTTS wires of which brittleness and grain boundaries of the materials were prominent. He then reviewed methods by means of which one could overcome those difficulties. He concluded with the statement that "Today Nb-Ti (T_c = 9 K, H_{c2}(4K) = 10 T) is the conductor of choice. YBCO (T_c = 92 K) is a potential challenger. The search is on for much higher T_c – but that is another story…"

The third speaker, A. Zee of the University of California, Santa Barbara concluded the session with observations on how superconductivity theory, especially the Ginsburg-Landau formulation [10], extends far beyond superconductors and influenced particle physics

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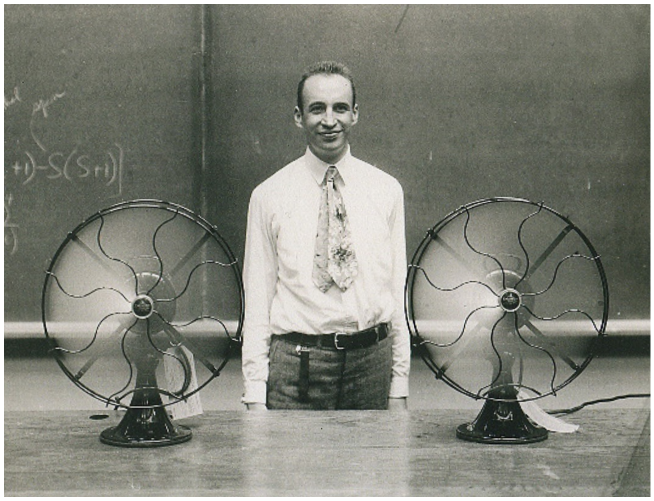
John Van Vleck: Quantum Theory and Magnetism

By David L. Huber

At the APS March Meeting on Tuesday, March 22, 2011, the FHP honored the contributions of John Hasbrouck Van Vleck to quantum theory and magnetism. The session was chaired by Chun Lin, from the University of Wisconsin-Madison and its invited speakers were Michel Janssen, University of Minnesota; David Huber, University of Wisconsin; Nicolaas Bloembergen, University of Arizona; Charles P. Slichter, University of Illinois, Urbana-Champaign; and Horst Meyer, Duke University. Among the attendees were Dr. and Mrs. John P. Comstock. Dr. Comstock, the son of Abigail Van Vleck's sister, inherited a collection of Van Vleck memorabilia from his aunt and has recently donated it to the University of Wisconsin.



Van Vleck lecturing at the University of Wisconsin in late 1920s or early 1930s. (Courtesy of David L. Huber)

The session began with Chun Lin presenting a brief outline of Van Vleck's life and career. He was born in Middletown, Connecticut in 1899 and grew up in Madison, Wisconsin where his father, Edward B. Van Vleck, was a professor of mathematics at the University of Wisconsin. John Van Vleck received his bachelor's degree in physics from the university in 1920. He then went to graduate school at Harvard University, where he received his PhD in 1922. After finishing his degree, he accepted a faculty position in physics at the University of Minnesota, where he remained until 1928 when he took a similar position at the University of Wisconsin. Van Vleck was at Wisconsin from 1928 until 1934. In 1934, he left to become a professor at Harvard University and was a member of the Harvard faculty from 1934 to 1969. He was President of the American Physical Society in 1952, received the National Medal of Science in 1966, and in 1977 shared the Nobel Prize in Physics with his former student Philip Anderson and Sir Nevill Mott.

The first invited talk was Michel Janssen's lecture on "Van Vleck from Spectroscopy to Susceptibilities: Kuhn Losses Regained." He pointed out that the old quantum theory, which Van Vleck had applied to the interpretation of optical spectra, did not give results for electric and magnetic susceptibilities that were compatible with confirmed results obtained with the classical theory – a situation referred to in the history and philosophy of science literature as "Kuhn Losses". While at the University of Minnesota, Van Vleck pointed out that this situation was remedied when the susceptibilities were calculated in the new quantum theory using matrix mechanics. At Minnesota, he also developed a general theory for the magnetic susceptibilities of atoms and ions that showed that in addition to the standard Curie term, there was a temperature-independent term arising from matrix elements of the Zeeman interaction between the ground manifold and excited manifolds.

David Huber's talk focused on Van Vleck's investigation of magnetic susceptibilities while he was at Wisconsin. The talk began with a discussion of the work of Van Vleck and his graduate student Amelia Frank, who applied the general theory of magnetic susceptibilities developed at Minnesota to trivalent rare earth ions. He and Frank showed that the temperature-independent term was the dominant term at low temperatures for Eu^{3+} where the total angular momentum of the ground manifold was zero. They also showed that the temperature-independent term, often referred to as Van Vleck paramagnetism, made a significant contribution to the susceptibility of Sm^{3+}. The calculations of Van Vleck and Frank were done for isolated atoms and ions. In 1932 Van Vleck and his post-doctoral students Bill Penney and Robert Schlap published a series of three papers on the influence of the electrostatic fields coming from neighboring ions on the magnetic susceptibilities of rare earth and transition metal ions. Utilizing a theoretical approach developed by Hans Bethe (1929), they were able to account for hitherto unexplained differences between the susceptibilities of the free ions and the susceptibilities of the corresponding ions in crystalline environments.

The year 1932 also marked the publication by Oxford University Press of Van Vleck's classic monograph, *The Theory of Electric and Magnetic Susceptibilities*, which had a major impact on the investigation and understanding of the properties of magnetic materials. At the time of his death, Van Vleck left an incomplete set of notes for a second edition of his book which were given to Chun Lin by his widow Abigail Van Vleck. Although the notes are largely fragmentary, there is an entirely new chapter on the local field that would have replaced the chapter contrasting the susceptibilities in the old and new quantum mechanics. A PDF of the new chapter is available on the University of Wisconsin Physics Department web page,

http://www.physics.wisc.edu/vanvleck/.

Nicolaas Bloembergen, who for many years was Van Vleck's colleague at Harvard, spoke about Van Vleck's role in the development of the theory of magnetic resonance line widths in solids when there are significant exchange interactions between the resonating spins. Van Vleck showed that the presence of the exchange interactions led to a decrease in the dipolar line width

John Van Vleck

Continued from previous page

relative to the value it would have in the absence of such interactions – a phenomenon known as "exchange narrowing". This result was obtained by an analysis of the second and fourth moments of the line shape function. Van Vleck found that the exchange interaction did not contribute to the second moment, only to the fourth. Bloembergen also spoke of the important role that Van Vleck played in his own career at Harvard and his interactions with Dutch physicists such as C. J. Gorter and H. A. Kramers. John Van Vleck was awarded the Lorentz Medal by the Royal Netherlands Academy of Arts and Sciences in 1974.

The title of Charles Slichter's lecture was "Remembering Van: Three Madison Families and other Tales." In it, he spoke about the influential roles played at the University of Wisconsin by his grandfather, Charles S. Slichter, and the fathers of John Bardeen and John Van Vleck. The tale begins in 1903 when Charles Van Hise, a distinguished geologist, was named President of the University of Wisconsin. In 1904, Van Hise recruited Charles Bardeen, John's father, to found a medical school at the University. In 1906, Van Hise appointed Slichter to head the mathematics department. Slichter's first action as department head was to recruit Edward B. Van Vleck to bring strength in pure mathematics. John Bardeen and John Van Vleck did their undergraduate work at Wisconsin, finishing in 1920 and 1926, respectively. After Bardeen finished his Master's in engineering, Van Vleck provided guidance and help, recommending him to Trinity College, Cambridge University for a fellowship and later to Harvard University for appointment as a Junior Fellow. Charles Slichter, who did his undergraduate work at Harvard, had Van Vleck as an advisor. Van Vleck recommended he remain at the university for his Ph. D. and later suggested that he do his doctoral research on magnetic resonance with Edward Purcell.

Horst Meyer spoke about Van Vleck's work on the magnetic susceptibility of the clathrate compounds and how it was stimulated by the measurements carried out at the Clarendon Laboratories, Oxford University by A. H. Cooke, W. P. Wolf and himself. By trapping paramagnetic molecules such as O_2 in clathrate cages, they were able to extend measurements of the susceptibility temperatures below 1 K. The data from these measurements were the first test of earlier theoretical predictions of Van Vleck on the low temperature behavior of the susceptibilities of free molecules. Not surprisingly, it was found that there was a discrepancy between experiment and theory. This discrepancy stimulated collaboration between Van Vleck and Mary O'Brien from Oxford who was on sabbatical at Harvard at that time. O'Brien and Van Vleck showed that the discrepancy was due to the free molecule approximation and disappeared when a 'hindering potential' was taken into effect. Later measurements by Meyer and colleagues involving electron spin resonance and infrared absorption confirmed the magnitude of the hindering potential inferred by Van Vleck and O'Brien in O_2 and NO. In later years, Van Vleck became interested in the properties of rare-earth iron garnets and worked with Meyer on the interpretation of his calorimetric and NMR experiments. ■

Superconductivity Sessions

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Fig. 4. David Larbalestier

theory and quantum field theory. Those views paralleled those given at a talk at the March Meeting session J3, on March 22, 2011, given by Frank Wilczek, who presented a "modern perspective on some classic applications of the ideas of superconductivity theory to fundamental particle physics: spontaneous chiral symmetry breaking in vacuum QCD, the Higgs mechanism in electroweak theory, and color superconductivity in dense hadronic matter; and also the confinement problem." That session was a Kavli Foundation Special Symposium: Nobelist Perspectives on 100 Years of Superconductivity, sponsored by the DMP and DCMP. ■

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(All photographs in this article by George Zimmerman)

Continued from page 1

War II developments in quantum field theory: effective, relativistically invariant, computational methods; Feynman diagrams; and renormalization theory. The conference also witnessed the elucidation of the structure of the mesonic component of cosmic rays.

The Shelter Island, Pocono and Oldstone conferences were small, closed, and elitist in spirit. In a sense they mark the postponed end of an era, that of the 1930s, and its characteristic style of doing physics: small groups and small budgets. None cost more than $1,500. The ravages of inflation are illustrated by the fact that the average expenditure to accommodate all the 25 attendees for one night at the elegant inns where the conferences took place was $200.

Coming after World War II, these conferences reasserted the values of pure research and helped to purify and revitalize the theoretical physics community. They also made evident the new social reality implied by the newly acquired power of the theoreticians, and helped integrate the most outstanding of the younger theoreticians into the elite: Richard Feynman, Julian Schwinger, Robert Marshak, David Bohm, and Abraham Pais at Shelter Island, plus Freeman Dyson at Oldstone.

The Shelter Island, Pocono, and Oldstone conferences were the precursors of the Rochester Conferences on High Energy Physics. But they differed from these later conferences in important ways. While the Shelter Island, Pocono, and Oldstone conferences reflected the style of an earlier era, the Rochester Conferences were more professional and democratic in outlook and had the imprint of the new era: the large group efforts and the large budgets involved in machine physics. And whereas Shelter Island and Pocono looked upon quantum electrodynamics as a self-contained discipline, the Rochester Conferences saw "particle physics" come into its own, with QED as one of its subfields—albeit one with a privileged, paradigmatic position.

The small, elitist character of the Shelter Island, Pocono and Oldstone conferences embodied the ideal of MacInnes, a NAS member and a past president of the New York Academy of Sciences. For several years before becoming its president in 1944, he had helped arrange a series of small conferences sponsored by that organization, "designed to promote active discussion of different scientific topics." For the conferences to achieve their aim, MacInnes felt it was essential that the topics "be in areas in which actual work was in progress and that participation be restricted to currently active investigators in the designated fields." Although an early conference sponsored by the New York Academy had achieved this objective, he believed that the effectiveness of the later ones had been impaired "by their success in attracting too large a crowd." MacInnes felt very strongly that attendance should be limited. In October of 1944 he declined to run again as New York Academy president when a decision on this matter by its council went against him, and in January 1945 he resigned from the Academy over this issue.

In the fall of 1945, MacInnes sent Jewett a proposal for the NAS to sponsor occasional meetings "of the relatively few men doing active research work in each field." These conferences would address a topic on which active research was occurring; have a small number of papers presented that would be distributed before the conference; have much more time for discussion than for presentation of papers; and be limited to at most 25 or 30 participants. The papers, revised in the light of the discussion that took place, would be published as a NAS monograph.

Jewett liked the idea and felt confident that the NAS would be willing to sponsor the undertaking. He suggested that MacInnes pick out one or two problems that seemed promising and use these as "pilot plants." Given this encouragement, MacInnes, after discussion with colleagues, suggested two conferences, one on "The Nature of Biopotentials" and the other on "The Postulates of Quantum Mechanics." Biopotentials were the focus of the research of his colleague and friend W. J. V. Osterhout and were of considerable importance in his own work. The topic of the second conference was an area that intrigued MacInnes, who at the time was studying wave mechanics.

K. K. Darrow—the urbane, and by then perennial, secretary of the American Physical Society, a theoretical physicist who had been at Bell Telephone Laboratories since 1925 and who was a popularizer of science of some stature—offered MacInnes his help in organizing the conference on quantum mechanics. The two of them held extensive discussions with Leon Brillouin and Wolfgang Pauli. However, MacInnes did not like Pauli's suggestions because "Pauli was planning for too many of the older men, and [MacInnes thought] that the best results would be to get out the coming generation." Thus, at Darrow's suggestion, the topics to be discussed and the responsibility for drawing up a list of participants was given over to John Wheeler, who had also attended the meetings with Pauli. Wheeler was indeed representative of the "younger men."[1] During the war he had distinguished himself at the Chicago Metallurgical Laboratory and borne major responsibility for the design and construction of the Hanford reactors. Wheeler's stature in the theoretical physics community was clearly recognized. In the fall of 1945, he was chosen to present a paper on the "Problems and Prospects in Elementary Particle Research" at the Symposium on Atomic Energy and Its Implications, the high point of the first postwar meeting of the NAS, which it sponsored jointly with the American Philosophical Society. At that symposium Wheeler shared the limelight with Enrico Fermi, J. Robert Oppenheimer, Eugene Wigner, Arthur H. Compton, Harold Urey, and Irving Langmuir. His impressive address gave proof that indeed a new generation of American physicists was taking over the intellectual leadership in the emerging field of "elementary particle" physics.

Under the guidance of Jewett, MacInnes, and Darrow, the Shelter Island Conference would reinforce that message. Darrow in his letter inviting Wheeler to oversee the agenda of the conference made explicit what had been implicit with MacInnes and Jewett: the conference was to be an

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American one, designed to "bring out" the young American theoretical physicists who had played such a large role in the successful war effort. The three organizers wanted the conference to demonstrate that theoretical physics in the United States had come into its own with the younger men who had been born and trained here. The conference was to prove the strength of American theoretical physics not only in wartime activities but also in "pure" physics. These conferences gave further proof of the remarkable intellectual powers of the leading theoreticians. The final list of participants corroborates this intent.

The conference was eventually set to occur on Monday, Tuesday, and Wednesday, June 2–4, 1947, with Darrow serving as its convener and chairman. Its timing was determined to accommodate the presence of Oppenheimer, who was also to write a 500-word paper outlining subjects for discussion. The other two physicists asked to prepare papers for the conference were Victor Weisskopf and Hendrik Kramers—a long-time friend of Wheeler—who at the time was visiting the Institute for Advanced Study after having chaired the UN scientific and technological committee on nuclear energy and taught at Columbia.

Weisskopf's paper outlined the problems faced in elementary particle physics in broad and general terms, and urged that the conference include a discussion of fundamental experiments to be done with the almost-completed "very powerful accelerators in the energy region of 200–300 MeV." Oppenheimer's outline was more narrowly focused and concentrated on cosmic-ray phenomena.

Kramers, for his part, chose to review the difficulties encountered in QED since its inception in 1927 and to indicate one way out of these problems. He pointed to his own work of 1938–1940, and that of his students, Serpe and Opechowski, which had been carried out in 1940, presenting a theory in which all structure effects had been eliminated and describing "how an electron with *experimental mass* behaves in its interaction with the electromagnetic field."[2]

By the end of May, the preliminary results of Lamb and Robert Retherford—that contrary to what the Dirac equation said about the energy levels of an electron in a Coulomb field, the $2s_{1/2}$ level of hydrogen lies 1,000 megacycles above the $2p_{1/2}$ level—had been widely circulated. Schwinger and Weisskopf discussed the theoretical implication of the experiment on their train ride from Boston to New York to attend the conference and agreed that the effect was very likely quantum electrodynamic in origin. Schwinger later recalled their also coming to the conclusion that since the electron self-energy was logarithmically divergent in hole theory, the energy difference between these levels would be finite when calculated with that theory.

In fact, the matter had probably been discussed even earlier over the lunches Schwinger and Weisskopf periodically shared, since this hydrogen level shift was one of Weisskopf's current research interests. In the fall of 1946 he gave the problem of a hole-theoretic computation of the $2s$–$2p$ level shift in hydrogen to Bruce French, who had been working on it since then. Weisskopf would very likely have told Schwinger of this research, elicited his reaction, and sought his advice on effective computational approaches.

The conferees gathered in New York on Sunday June 1, 1947, at the AIP headquarters on East 55th Street. From there they were taken "on an old and shaky" bus to Greenport at the western end of Long Island. On the final phase of the trip, they were accompanied by a police motorcycle escort, and their bus didn't stop at any traffic lights. As they passed each county line a new police escort would meet them. In Greenport the conferees were wined and dined as guests of the Chamber of Commerce, paid for by John C. White, its president. In an after-dinner speech, he said that he had been a Marine in the Pacific during the war and that he—and many like him—would not be alive were it not for the atomic bomb.

Darrow chaired the conference, but Oppenheimer was the dominant personality and "in absolute charge." MacInnes recorded in his diary that "it was immediately evident that Oppenheimer was the moving spirit of the affair." Darrow in his diary gave a revealing account:

> As the conference went on the ascendency of Oppenheimer became more evident—the analysis (often caustic) of nearly every argument, that magnificent English never marred by hesitation or groping for words (I never heard "catharsis" used in a discourse on [physics], or the clever word "mesoniferous" which is probably O's invention), the dry humor, the perpetually-recurring comment that one idea or another (incl. some of his own) was certainly wrong, and the respect with which he was heard.
>
> Next most impressive was Bethe, who on two or three occasions bore out his reputation for hard & thorough work, as in analyzing data on cosmic rays variously obtained.

On the first day of the conference, Lamb presented the most recent data from his experiment with Retherford. In the following discussion, Oppenheimer pointed out that if one calculated the *difference* in the $2p$ and $2s$ energy levels using hole theory, a finite answer might be obtained, given that the divergences encountered in both were logarithmic (see Fig. 2).

Lamb was followed by Rabi, who presented the data that J. E. Nafe, E. B. Nelson and he had obtained on the hyperfine structure of H and D. That afternoon Rossi reported on the experiment that had been carried out in Rome by M. Conversi, E. Pancini and O. Piccioni on the absorption of mesons in the atmosphere. The next morning Kramers presented his version of the *Lorentz theory of an extended charge* in which structural effects had been encapsulated in the experimental mass of the particle. He concentrated on the *classical* version of his non-relativistic theory, although Bethe's notes make clear that in the last part of his talk he indicated what quantizing the theory would do (see Fig. 3).

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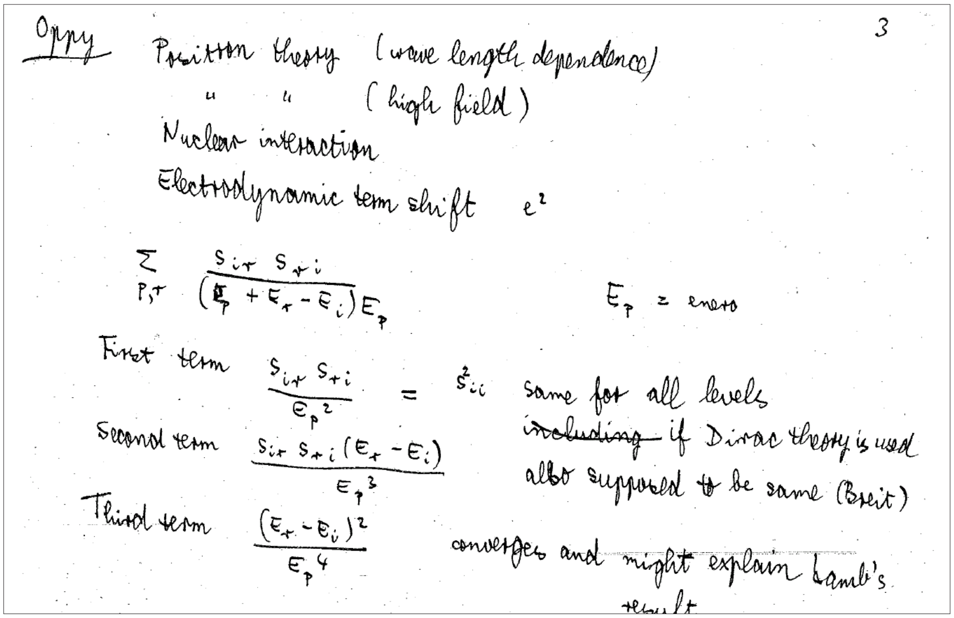


3

Oppy Positron theory (wave length dependence)

" " (high field)

Nuclear interaction

Electrodynamic term shift e^2

$\sum_{p,\tau} \frac{S_{i\tau} S_{\tau i}}{(E_p + E_\tau - E_i) E_p}$ E_p = energy

First term $\frac{S_{i\tau} S_{\tau i}}{E_p^2} = S^2_{ii}$ same for all levels ~~including~~ if Dirac theory is used

Second term $\frac{S_{i\tau} S_{\tau i} (E_\tau - E_i)}{E_p^3}$ also supposed to be same (Breit)

Third term $\frac{(E_\tau - E_i)^2}{E_p^4}$ converges and might explain Lamb's result

Fig. 2. Bethe's handwritten notes of Oppenheimer's talk. (Courtesy of Rose Bethe)

Kramers' talk was clearly influential. His use of the Hertz potential and his derivation of the potential that the dressed electron with experimental mass experiences became Schwinger's point of departure in his quantum electrodynamic calculation of the Lamb shift and of the magnetic moment of the electron.

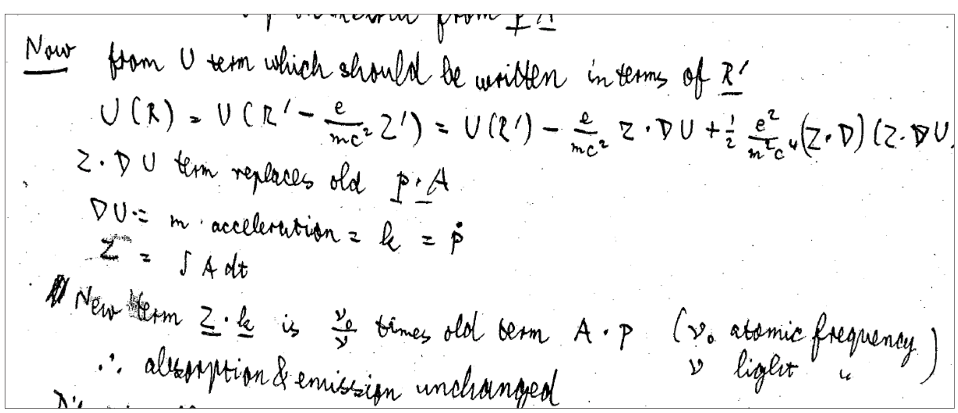
In the afternoon of the second day, Weisskopf reviewed the divergence difficulties in the hole-theory calculations of the self-energy of an electron. In a paper that he had written in 1939, he had indicated that in hole theory the divergence would be logarithmic to all orders of perturbation theory. According to Breit's notes of the conference, Weisskopf agreed with Oppenheimer's hunch that a hole-theoretic calculation of the difference in the 2*p* and 2*s* energy levels would not diverge. During the ensuing discussion, Weisskopf and Schwinger indicated how a hole-theoretic calculation of the level shift might be attempted and suggested further reasons why a finite result might result from applications of Kramers' ideas.

After the conference was over, Bethe performed his famous non-relativistic calculation on the train ride from New York to Schenectady. The paper in which he proved that the level shift would be accounted for quantum electrodynamically was completed by June 9 and circulated to conference participants.[3] Bethe did not acknowledge Kramers' talk in his paper even though this presentation had been crucial in stressing the importance of expressing observables in terms of the *experimental* mass of the electron.

This omission probably happened because Bethe thought that he had a much simpler way than Kramers to incorporate this insight. Bethe had noted that the quantum electrodynamically calculated self-energy of a free non-relativistic electron could be ascribed to an electromagnetic mass of the electron and —though divergent—had to be added to the mechanical mass of the electron. The only meaningful statements of the theory involve the sum of the electromagnetic and mechanical masses, which in combination is the experimental mass of a free electron. In contrast to Kramers' approach, Bethe's was a *model-independent* formulation of mass renormalization that did not assume an extended charge distribution of the electron. And in contrast to Schwinger and Weisskopf's initial insight, that a hole-theoretic calculation of the *difference* between the energies of two levels would be finite, Bethe's approach allowed computing the energy of each level and gave an unambiguous formulation of mass renormalization in the non-relativistic case. Moreover, he knew what was required to formulate an analogous relativistic prescription. Weisskopf and Schwinger, although emphasizing Kramers' insight, could not do so at Shelter Island.

In his 1983 talk commemorating Shelter Island I, Lamb made the remarks [4]:

> Kramers was there that year. When I heard Kramers talk at Shelter Island I it seemed to me that he mainly said that we should be applying the methods used by Lorentz for the classical [electron]. I could not see that he indicated how such a program could be carried out, and hence did not derive great inspiration from his talk…* Unlike Weisskopf, who was working on these problems actively before this wonderful time, Kroll and I were only inspired when we found out how Bethe had subtracted the self energy of a free electron.



Now from U term which should be written in terms of R'

$U(R) = U(R' - \frac{e}{mc^2} Z') = U(R') - \frac{e}{mc^2} Z \cdot \nabla U + \frac{1}{2} \frac{e^2}{m^2 c^4} (Z \cdot \nabla)(Z \cdot \nabla U)$

$Z \cdot \nabla U$ term replaces old $p \cdot A$

∇U = m · acceleration = $k = \dot{p}$

$Z = \int A\, dt$

New term $Z \cdot k$ is $\frac{\nu_0}{\nu}$ times old term $A \cdot p$ (ν_0 atomic frequency, ν light ")

∴ absorption & emission unchanged

Fig. 3. Bethe's notes of Kramers' Shelter Island talk. (Courtesy of Rose Bethe)

Continued from previous page

In his 1947 paper Bethe did acknowledge the comments by Weisskopf and Schwinger that a hole-theoretic calculation of energy level differences would be finite and in a footnote stated: "It was first suggested by Schwinger and Weisskopf that hole theory must be used to obtain convergence in this problem." Their insight justified the high-energy cut-off he introduced in his calculation.

Dresden, in his 1988 biography of Kramers, suggested that he did not receive adequate credit for his Shelter Island contributions. Bethe's notes indicate that Dresden was correct. To Bethe, the pragmatist for whom numbers were always the criterion of good physics, and who had just been so deeply and successfully involved in the war effort calculating numbers that translated into physical effects and measurable empirical data, the challenge was to get the numbers out and account for the magnitude of the $2s$–$2p$ shift in hydrogen and for the new values of the hyperfine splitting in H and D that Nafe, Nelson and Rabi had just measured. Accounting for empirical data would be explaining the data. Kramers' approach was too model-dependent, too theoretical, and too far removed from calculating numbers. For Bethe, the value of a novel idea was gauged by whether it could help you calculate numbers that could be compared with empirical data.

The importance of Bethe's calculation is apparent from Weisskopf's reaction to it. He received Bethe's manuscript on June 11 and after studying it wrote Bethe that he was…

> …quite enthusiastic about the result. It is a very nice way to estimate the effect and it is most encouraging that it comes out just right. I am very pleased to see that Schwinger's and my approach seems to be the right one after all. Your way of calculating is just an unrelativistic estimate of our effect, as far as I can see.
>
> I am all the more pleased about the result since I tried myself unsuccessfully to estimate the order of magnitude of our expression. I was unable to do this but I got more and more convinced that the method was sound…I would like to talk it over with you especially the `*korrespondenz Deutung*' of the effect.[6]

Bethe's calculation was a "crucial calculation"—a notion I owe to my Brandeis colleague and friend Howard Schnitzer. By introducing in a simple manner the concept of mass renormalization and its associated meaning and consequences, Bethe's calculation offered a new perspective on how to address quantum electrodynamical calculations and to obtain numbers that could be compared with experimental data. It was crucial because he could convincingly justify the cut-offs he had to introduce in his non-relativistic calculation and obtain a logarithmic expression that agreed with the observed level shift (see Fig. 4).

Inserting (10) and (9) into (6) and using relations between atomic constants, we get for an S state

$$W_{ns}'=\frac{8}{3\pi}\left(\frac{e^2}{\hbar c}\right)^3 \mathrm{Ry}\frac{Z^4}{n^3}\ln\frac{K}{\langle E_n-E_m\rangle_{\mathrm{Av}}}, \qquad (11)$$

where Ry is the ionization energy of the ground state of hydrogen. The shift for the $2p$ state is negligible; the logarithm in (11) is replaced by a value of about -0.04. The average excitation energy $\langle E_n-E_m\rangle_{\mathrm{Av}}$ for the $2s$ state of hydrogen has been calculated numerically[7] and found to be 17.8 Ry, an amazingly high value. Using this figure and $K=mc^2$, the logarithm has the value 7.63, and we find

$$W_{ns}'=136\ln[K/(E_n-E_m)] = 1040 \text{ megacycles.} \qquad (12)$$

Fig. 4. Excerpt from Bethe, "The Electromagnetic Shift of Energy Levels" [5].

Only Bethe could have evaluated the logarithmic contribution as quickly as he did. He had encountered similar logarithmic expressions when calculating quantum mechanically the energy loss of fast charged particles traversing matter in his *Habilitationsschrift* in 1929!

Schwinger at the time made another crucial—and perhaps more important—calculation, of the quantum electrodynamic contribution to the magnetic moment of the Dirac electron. His calculation was crucial because it was it was the first *field-theoretic* calculation of quantum electrodynamic radiative corrections. Up to this point, all such calculations had been based on hole theory. Schwinger's calculation asserted that a quantum field-theoretic description of both electrons and radiation is the generative and effective way to describe atomic and subatomic processes.

The Shelter Island Conference marked a major transition in the history of theoretical physics. It placed experiments on the spectrum of hydrogen that indicated deviations from the predictions of the Dirac equation at the center of theoretical interest. By obtaining reliable and precise values for these discrepancies, Lamb and Rabi posed a challenge to the theoretical physics community. Renormalization theory, the principal outgrowth of the discussions at Shelter Island, allowed the difficulties that had plagued quantum electrodynamics to be circumvented. The subsequent computational techniques devised by Schwinger, Feynman and Dyson allowed the calculation of the electromagnetic properties of simple atomic systems to previously unheard-of accuracy. These developments dispelled whatever doubts remained about the adequacy of QED and renewed hope that field-theoretic explanations of the nuclear forces would eventually be found. ■

Footnotes and References

[1] Wheeler, a brilliant young theoretical physicist, had been educated at Johns Hopkins University. As a National Research Council Fellow, he worked in 1933–34 with Gregory Breit at NYU on problems in quantum electrodynamics and spent the 1934–35 academic year at Niels Bohr's Institute in Copenhagen. He made important contributions to nuclear physics, especially in a paper on the scattering matrix and his researches with Bohr on the

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New Books *of* Note

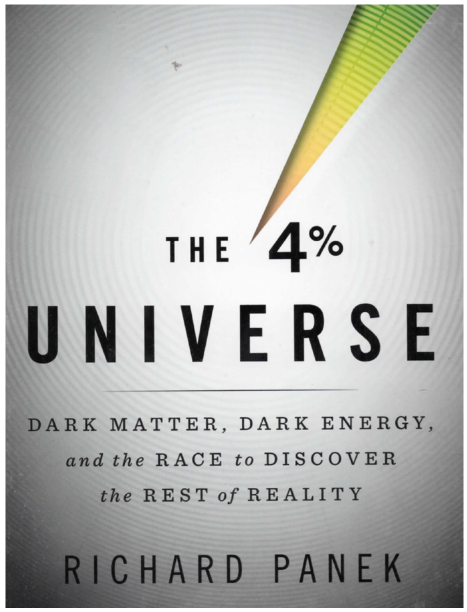
The 4% Universe
Dark Matter, Dark Energy, and the Race to Discover the Rest of Reality

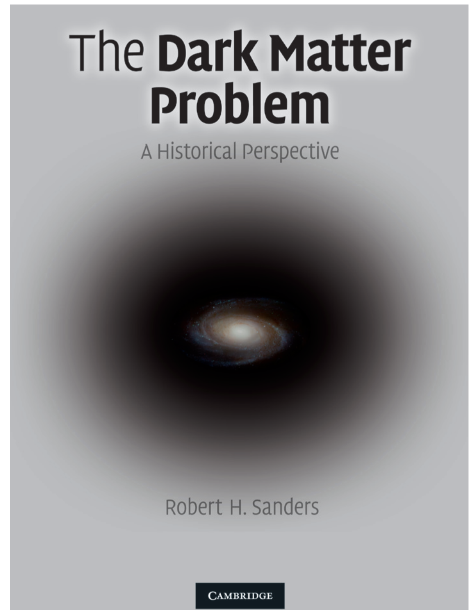
By ***Richard Panek,*** *Houghton Mifflin Harcourt, 2011, illustrated, 297 pp., $26.00*

The Dark Matter Problem
A Historical Perspective

By ***Robert H. Sanders****, Cambridge University Press, 2010, illustrated, 206 pp., $60.00*

Reviewed by Robert P. Crease





Certain scientific discoveries develop in a way similar to cities. Scattered research programs —settlements, in this loose analogy— spring up without much interaction at first. They begin to discover linkages with related programs, and interactions as well with clusters of others nearby. These interactions grow more extensive and diverse. Eventually a large research enterprise results, in which the work of any one part is related, directly or indirectly, with those of others. Then, in a visionary and crystallizing moment, a coherent structure emerges unexpectedly from the whole, rejuvenating it in a way that explains many puzzles, reorganizes and unites apparently unrelated research, and points the way to further efforts.

The twin discoveries of dark matter and dark energy—sure to rank as among the most revolutionary developments ever in the history of astrophysics, are classic examples. They became firmly established in the past decade thanks to a complex synthesis of evidence amassed by interdisciplinary teams of researchers studying such diverse phenomena as supernovae, galaxy rotations and spatial distributions, light-element abundances, variations in the cosmic microwave background radiation, gravitational lensing, and models of cosmological behavior.

Equally engrossing as the discoveries themselves is the problem of how to *write about* such wide-ranging, interdisciplinary events. It is indeed much like writing about a city: how you go about it depends on your audience and goals. You can write for city dwellers who want to improve some municipal feature, or for new inhabitants who need basic information about such things as sanitation, police, and navigating City Hall. You can write for tourists, who have no interest in these functional questions, but want to see the famous buildings, noteworthy museums, and urban movers and shakers. Finally, you can write for historians, who are primarily interested in what light this city's story sheds on those of other cities and on municipal life more generally.

A spate of new books about dark matter and dark energy—by the time this review appears there will no doubt be more—indeed reveals this range of approaches. Several scientific review articles have already been published on dark energy and dark matter; these are like handbooks for active practitioners. Two textbooks have recently been published: *Dark Energy: Theory and Observations*, by Luca Amendola and Shinji Tsujikawa (Cambridge University Press, 2010); and *Dark Energy*, by Yun Wang (Wiley-VCH, 2010); they were reviewed together in the June 2011 issue of *Physics Today*. These publications discuss the ins and outs of models, methods, and measurements—on the information, instruments, and techniques that future practitioners will need to know.

Two other books—*The 4% Universe*, by Richard Panek, and *The Dark Matter Problem*, by Robert H. Sanders—are aimed less at practitioners and more at outsiders interested in what's been happening in these research areas. They differ dramatically in style and substance, and what they capture (or fail to capture) helps reveal the special achievement a truly historical perspective might be.

Panek's is a fabulous popular science book written in the form of an evolving drama whose driver is the actions of a small number of individuals. He characterizes their personalities carefully and finely. One protagonist is Princeton theorist Jim Peebles, who is restless and even a daredevil, both physically and intellectually. "He loved identifying the next big problem,

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solving it, seeing where it led, identifying *that* big problem, solving it, seeing where it led: a bend-in-the-knees, wind-in-the-face rush into the future" (p. 23). Another protagonist is Carnegie Institution astronomer Vera Rubin, through whose eyes we learn what it's like to have alcoholic mentors and cope with colleagues who think that having a child disables one professionally. Panek builds his narrative around the feverish competition between the two teams hunting distant "Type 1a" supernovae, the Supernova Cosmology Project (SCP) centered at Berkeley, and the High-Z team centered at Harvard. Covering such intense scientific competition is a sure-fire way to grip your readers and hold their interest. The twists and turns of the story, indeed, provide material that is the envy of any novelist.

Panek keeps the number of players small and selects the most interesting ones. He also ruthlessly pares away all information from this extraordinarily complex and messy tale except for what readers *must* know to appreciate the denouement: the dark energy discovery itself. He says little, for instance, about the 1984 discovery of cold dark matter, about the 1990s studies of the spatial distribution of galaxies, and indeed about the activities of research programs not directly involved in this discovery. On the last page he quotes SCP leader Saul Perlmutter as follows:

> I have the impression that most people don't realize that what got physicists into physics usually is not the desire to understand what we already know but the desire to catch the universe in the act of doing really bizarre things. We *love* the fact that our ordinary intuitions about the world can be fooled, and that the world can just act strangely, and you can just go out and make it good over and over again (p. 243).

Panek cannot fully relate just how bizarre physicists find dark energy, but his carefully told tale succeeds in conveying something of their astonishment. His omissions are however the collateral damage necessary for his success in getting a messy story into popular form.

Sanders' book is quite different. The subtitle contains the word "historical," the introduction refers to the book as "narrative and personal," but perhaps the best term he uses is "overview," as the book is described in the front matter. While written from the perspective of a participant in this research, this book outlines the emerging threads that coalesced into the idea of dark matter. If Panek's narrative camera rests on the shoulders of a few well-chosen protagonists, Sanders' is pulled back to such a distance that human beings are all but invisible. The first time Peebles appears, for instance, it is as Jeremiah Ostriker's collaborator in determining how a rigid spheroidal halo insures the stability of galactic discs. The first time Rubin is mentioned is as a collaborator in the study of spectroscopic observations of rotation curves of spiral galaxies using emission lines of hydrogen and nitrogen molecules. Whenever Rubin is mentioned thereafter, it is usually in the form of "Rubin and collaborators" or "Rubin *et al*." You never learn that she's also a mother, or even a woman.

Sanders does, however, provide a comprehensive map of the background pieces to the discovery of dark matter and how they emerged, the sort of thing almost entirely missing from Panek's book. Six full chapters describe how the reigning theory of cold dark matter arose, another few detail its difficulties, and one chapter is devoted to an alternative: modified Newtonian dynamics. An appendix takes interested readers through the details of such issues as establishing distance in astronomy, weighing galaxies and clusters, and the growth of cosmological structures.

Thus Panek's book covers its subject from the close-in perspectives of a few individual observers, while Sanders describes events from a perspective so distant that, while the full extent of the enterprise becomes visible, the individual participants are all but invisible. Neither book provides what purists would deem a true history of its subject.

The audience for such a thorough historical account is neither tourists nor participants; its aim would be to explore what this particular story of science says about other stories of science and about science itself. It would take more of a mid-range perspective, examining the interaction between scientists and the expanding research frontier. Historical research might explore, for instance, how the principal discoveries happened, what constitutes an announcement and a priority claim, and the course of competition and cooperation among the groups involved. It would examine how scientists inherit certain assumptions and behavior patterns, apply them to problems, and in so doing transform their discipline.

At the end of 2009, for example, the Cryogenic Dark Matter Search collaboration circulated a paper announcing the first direct evidence for dark matter. On the day the paper was submitted, the collaboration held two seminars, after signaling the press in a way that generated much publicity and excitement. But the evidence turned out to be substantially weaker than adumbrated —in fact, it was hardly evidence at all. Panek took the initial announcement at face value, and used it to build excitement for his story, but made nothing of the subsequent disappointment. Sanders did not mention these events at all; they were irrelevant to his account. It is understandable why Panek and Sanders made little or nothing of this episode. Yet it is loaded with meanings for the history of astrophysics and cosmology, revealing much about the value of sufficient statistics, announcements and discovery claims, publicity, and the effect of competition. A full-scale history of dark matter and dark energy, yet to be written, would explore such meanings in far more detail. ■

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fission processes. In 1938, at the age of 26, he joined the Princeton Physics Department as an assistant professor.

[2] After receiving Oppenheimer's, Weisskopf's, and Kramers' papers, Bethe wrote Weisskopf in mid-May:

> I read your outline and got the impression that we should be careful not to try and do too much. I should like especially to hear from Kramers in great detail about his new theory. Generally, I think we should try to hear from people who have actually got some results and avoid as much as possible discussions in the vacuum. For this reason I am not much in favor of extending discussions of experiments to be done in future accelerating equipment. If our discussion of concrete theoretical problems leads to desirable experiments, this is very good; but I believe we should not continue the war spirit of planned research too much.
>
> Would it be possible to set one day aside for Kramers and have a really good discussion on that, then perhaps have half a day or one day for meson production starting with Oppy's theory, and then have another day devoted to the Piccioni experiment and its interpretation?

[3] Bethe's accompanying letter to Oppenheimer was brief and to the point:

> Enclosed I am sending you a preliminary draft of a paper on the line shift. You see it does work out. Also, the second term already gives a finite result and is not zero as we thought during the conference. In fact, its logarithmic divergence makes the order of magnitude correct. It also seems that Vicki and Schwinger are correct that the hole theory is probably [handwritten insertion] important in order to obtain convergence. Finally, I think it shows that Kramers cannot get the right result by his method.

[4] *Shelter Island II: Proceedings of the 1983 Shelter Island Conference on Quantum Field Theory and the Fundamental Problems of Physics*. Edited by Roman Jackiw, Nicola Khuri, Steven Weinberg, and Edward Witten (Cambridge, MA: MIT Press, 1985). At *, the editors note, Bethe interjected, "He did inspire me!"

[5] Bethe, H.A., "The Electromagnetic Shift of Energy Levels," *Physical Review* **72** (1947) 339–41, footnote 6.

[6] Weisskopf added:

> I do not quite agree with your treatment of the history of the problem in your note. That the $2S_{1/2}$–$P_{1/2}$ split has something to do with radiation theory and hole theory was proposed by Schwinger and myself for quite some time. We did not do too much about it until shortly before the conference. We then proposed to split an infinite mass term from other terms and get a finite term shift, just as I demonstrated it at the conference. Isn't that exactly what you are doing? Your great and everlasting deed is your bright idea to treat this at first unrelativistically. "Es mochte doch schon sein" if this were indicated in some footnote or otherwise.

Bethe had put such a footnote into his paper and answered Weisskopf a few days after receiving his letter. Weisskopf soon acknowledged that Bethe's "abstract" was "harmloser" (much more harmless) than he initially thought and agreed, "Let's forget about patent claims."

Supplementary Reading

Silvan S. Schweber, *QED and the Men Who Made It: Dyson, Feynman, Schwinger, and Tomonaga* (Princeton University Press, 1994). Chapter 4 discusses in some detail the Shelter Island, Pocono, and Oldstone conferences.

Freeman J. Dyson, *Disturbing the Universe* (Harper & Row, 1979)

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