

History of Physics

NEWSLETTER

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Teaching the History of the Manhattan Project

By B. Cameron Reed, Alma College, reed@alma.edu

The development of nuclear weapons during the Manhattan Project in World War II is one of the most significant events of the twentieth century. The strategic, scientific, national policy, and security issues associated with nuclear weapons are never far from the headlines and will remain with us for generations to come. If asked “What was the Manhattan Project and who was involved with it?”, many individuals would probably answer that it had something to do with the making of atomic bombs and perhaps offer a name such as Oppenheimer or Fermi or (erroneously) Einstein. Beyond that, knowledge would likely become fuzzier, with some aware that the *Trinity* bomb was tested in New Mexico before *Little Boy* and *Fat Man* were dropped on Hiroshima and Nagasaki. While some respondents would know that those devices used uranium and plutonium as their “active ingredients,” how many could explain how those materials were obtained and why their properties demanded radically different designs for the Hiroshima and Trinity/Nagasaki weapons? How many could give a cogent explanation of the term “critical mass?” Indeed, many of our physics majors probably graduate with little better understanding of the science underlying nuclear weapons than that with which they emerged from high school.

For several years I have taught a general-education course on the history of the Manhattan Project to students at Alma College in an effort to address, in a small way, the general lack of knowledge in this area. In this article I describe my experience in developing and teaching this class. I would be interested in knowing about the experiences of anyone else doing something similar.

Alma College is a strictly undergraduate liberal-arts school of about 1,300 students located in central Michigan. In addition to choosing a major, every student must complete a requisite number of credits in the humanities, social sciences, and natural sciences. These general-education



Students at the Trinity Test Site monument. Photo courtesy of Ed Neuenschwander and the Society of Physics Students.

requirements comprise about one-third of a student’s overall credit requirements for graduation. Within the natural sciences area we have a physical-science requirement, with courses such as astronomy, environmental studies, general physics, and chemistry being popular choices.

Alma operates on a “4-4-1” schedule: two traditional four-month terms (Fall and Winter), followed by a one-month Spring term. The latter begins in late April and runs for about 3.5 weeks until just before Memorial Day. During Spring term, students take one course intensively, often meeting every day for 3-4 hours (or even more if a lab is involved); they are required to complete two Spring terms to graduate. This provides a venue for courses that would not otherwise conveniently fit into a

regular term: many offerings involve field work or a travel component. But since many students prefer not to travel for their Spring terms, a demand for on-campus classes always exists, particularly ones that carry general-education credit and have minimal prerequisites.

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Editors' Corner

To all the authors who contributed articles for this issue of our newsletter, I express my appreciation. The article by Cameron Reed on teaching the history of the Manhattan Project hit me rather close to home. For over twenty years I have taught a general-education course called "Science, Technology, & Society." This has been a fulfilling and enjoyable task. It allows me to meet students of all majors across the campus community. It presents an opportunity to share a glimpse into evidence-based reasoning, and helps students discover how the methods of science cannot be reduced to a universal checklist. Together we think about the implications, for individuals and across societies and timescales, of science and its applications.

Each semester this adventure includes two or three weeks of

discussions about nuclear weapons and their consequences. As with so many topics, I find this one is best approached through its history. Although I cannot go into the depth that Professor Reed does in his dedicated course, I can affirm some of his experiences. I have seen the looks of surprise when students realize that the authors of the first-generation nuclear weapons were people of warm humanity who found themselves in unprecedented circumstances. I have read the student essays and heard their in-class vocalizations as they struggle with the "what would I have done at the time" questions. I have seen their astonishment when they realize how much the Cold War really cost. I have witnessed their reactions when they see that policies are not always consistent with a nation's mythos and rhetoric.

This past semester one student, a psychology major, wrote "This week I have been thinking a lot about the scientists who developed nuclear weapons. The video that we watched last week [*The Day After Trinity* by Jon Else, 1980] certainly shed a different perspective on the whole issue. I am finding more and more that you really do not understand a situation until you are able to see it from the inside out..."

The study of the history of physics powerfully helps us to see physics, and its interactions with the larger society, from the inside out. Thanks again to all the authors who contributed articles for this issue of our newsletter. With each article, I find that I learn something interesting.

—Dwight E. (Ed) Neuenschwander,
Editor

History of Physics NEWSLETTER

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APS Policy on Peer Review Materials

By Michael Riordan, Forum Councilor

At the November 2009 meeting of the APS Council, Roger Stuewer moved that the APS establish a committee to make recommendations regarding release of peer-review materials on articles submitted to its journals. He noted that the APS did not then have an official policy regarding such release to scholarly researchers, and that it was largely left to the discretion of the Editor-in-Chief.

In the following months, a committee chaired by Forum member Robert Crease of the State University of New York at Stony Brook, and including current Forum Chair Daniel Kleppner, formulated a formal policy statement, which was approved unanimously by the Council

at its 18 April 2010 meeting. This statement can be accessed on the APS web site at: <http://publish.aps.org/peer-review-materials-policy>.

Briefly, it states that peer-review materials will not normally be released until 50 years have lapsed. No materials that identify living individuals will be released without their written consent. Exceptions can be made only at the discretion of the APS Editor-in-Chief.

The Council agreed that this policy represents a fair compromise between the needs of historians for access to this information and the requirements of the APS publishing department to protect the confidentiality of its reviewers. ■

Archiving FHP

FHP Webmaster George Zimmerman has been working with speakers at FHP sessions to have their presentations posted on the FHP website, <http://www.aps.org/units/fhp>. These presentations are valuable historical resources—on John Wheeler, Lev Landau, Silicon

Valley, the history of telescopes, and so forth. Photographs from the sessions are also on the FHP website. In progress is a posting of letters and documents on the founding of the FHP itself (originally, the Division of the History of Physics) during 1978-1980.

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Last fall this newsletter reported that Russell K. McCormmach had been selected as the recipient of the 2010 Abraham Pais Prize for the History of Physics, “for the study of German science in the 19th and 20th centuries and a major biography of Henry Cavendish (with Christa Jungnickel, his late wife), and for founding the journal *Historical Studies in the Physical Sciences*.” We promised at that time to offer additional notes on Professor McCormmach’s work.

Professor McCormmach’s work has had a profound influence in transforming the study of the history of physics from an activity of enthusiastic but untrained aficionados into an academic discipline worthy of a scholarly profession. That transformation required not only exemplary models of scholarship, but also the development of the infrastructure so necessary to scholarly discourse. Dr. McCormmach has been a key figure on both sides of the transformation.

Russell McCormmach was born in 1933 in Pendleton, Oregon. In 1959 he earned Bachelor’s degrees from the Washington State University and Oxford University, and in 1967 the PhD from Case Western Reserve University. He has taught physics, mathematics, and the history of science at several institutions, including Washington State University, San Francisco State University, Princeton University, the University of Pennsylvania, Johns Hopkins University, and Whitman College. He served as an assistant editor of the *Encyclopedia Britannica* in the early 1960s.

Dr. McCormmach was the founding editor of the journal *Historical Studies in the Physical Sciences*, editing Volumes I-III, and co-editing of Volumes IV-X.

A monumental history of German physics was forthcoming from Professor McCormmach and his late wife, the writer Crista Jungnickel, in the two-volume *Intellectual Masters of Nature: Theoretical Physics from Ohm to Einstein* (University of Chicago Press, 1986). McCormmach and Jungnickel also brought Henry Cavendish out of relative obscurity and misunderstanding with *Cavendish* (American Philosophical Society, 1996) and *Cavendish: The*



Russell K. McCormmach.

Experimental Life (Bucknell University Press, 1999). More recently, this was followed by McCormmach’s *Speculative Truth: Henry Cavendish, Natural Philosophy, and the Rise of Modern Theoretical Science* (Oxford University Press, 2004).

McCormmach’s well-known novel *Night Thoughts of a Classical Physicist* (Harvard University Press, 1982) takes the reader into the mind of a fictional professor, Victor Jakob, an elderly physics instructor at a small German university in 1918. The sense of what “revolution in physics” meant to those who lived through it comes to life as we participate in Professor Jakob’s struggles to cope as the permanency of his cherished scientific principles come crashing down all around him.

In an interview with *Contemporary Authors* (1 January 2004), Professor McCormmach said “When I want to understand something, I invariably look to see how it came about in time, that is, I study it through its history...I study not only how science has changed in time but also how the rest of the world has changed with it...I use different approaches: I write history to depict the working relationships of large numbers of scientists, biography to deal with the experience of a particular scientist, and fiction to get at the meaning of science in individual lives.” More about Professor McCormmach can be found at <http://aps.org/units/fhp/awards/pais/index.cfm>. ■

For many years I have personally been interested in the history and physics of the Manhattan Project. By the year 2002 I felt that I had acquired enough understanding of the physics of the topic that I was ready to offer a Spring-term course with a prerequisite of basic algebra. I have now taught the course a total of five times since then, as well as a spin-off “First-Year Seminar” course that was offered for the first time in Fall 2009.

The text for the course is Richard Rhodes masterful book *The Making of the Atomic Bomb*, from which the course draws its title. Our Spring terms run about 19 instructional days, which corresponds to about one chapter per day. Students are expected to read each chapter the night before class. Class time then consists of me explaining the material with the aid of numerous PowerPoint slides, occasional videos, simple blackboard calculations, and demonstrations with equipment such as a Geiger counter and simple radioactive sources, an *e/m* tube to illustrate the idea of using magnetic fields to separate isotopes, and spectrum tubes. Students are often astonished to find that household smoke detectors and Fiesta ware dishes are nuclear.

In the first incarnation of the class I stuck closely to the one-chapter-per-day prescription, but this proved not fully satisfying. Rhodes devotes space to topics such as weapons development in World War I and the persecution of Jews, which, while relevant to his setting the historical stage, are not directly germane to the science of nuclear weapons. There is a lot to cover in 3.5 weeks; deleting these sections from the “required reading” has freed up time to go into more detail on the science.

Like the chapters in Rhodes’ book, the course material goes largely in chronological order. We begin with the discovery of X-rays and radioactivity as the opening acts of modern physics, then move on to the work of Rutherford and Bohr, the discovery of the neutron, the work of Enrico Fermi, the discovery of fission and

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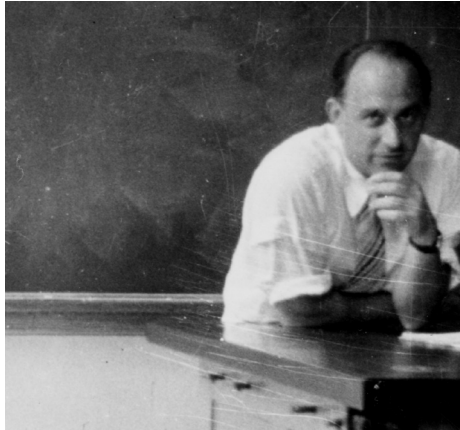
APS Meeting Reports on Forum-Sponsored Sessions: 2010 'April' Meeting

Remembering Enrico Fermi

By Gloria B. Lubkin, Forum Past Chair

An invited FHP session called "Remembering Enrico Fermi," organized and chaired by James Cronin (University of Chicago), was a highlight of the joint APS-AAPT meeting in Washington in mid-February (the "April" meeting). The audience of several hundred, in the largest room available at the meeting, appeared touched by the reminiscences of the three speakers, all of whom worked for Fermi at Chicago. Cronin was a graduate student at Chicago 1951-55. He edited a book that grew out of a 2001 Chicago meeting commemorating the hundredth anniversary of Fermi's birth, *Fermi Remembered* (University of Chicago Press, 2004). In his introductory remarks at the session, Cronin said that when Fermi moved to Chicago immediately after the end of World War II, his passion was to reach the highest energy possible. The mid-1950s began the golden age of particle physics.

The first talk, by T. D. Lee (Columbia University), was called "Fermi at Columbia and Reminiscence of Chicago Days." Lee said that when the first anti-Semitic law was passed in Fascist Italy in 1938, Fermi wrote to George Pegram, then chair of the Columbia physics department. Fermi reminded Pegram that when they had met a couple of years earlier, Pegram asked if Fermi would be interested in coming to Columbia. When Pegram promptly offered him a job, Fermi cabled his acceptance. Then Fermi wrote again to ask if Pegram knew of any job opportunities in the US for other outstanding young Italian physicists, and mentioned Emilio Segrè (who was in Berkeley on a fellowship); Bruno Rossi, Giulio Racah, Ugo Fano, and Franco Rasetti. Fermi described each person's expertise, marital status, and number of children. Fermi was "very humane," Lee said. "His foremost worries were about his colleagues and their survival if they remained in Italy."



Enrico Fermi in the classroom, date unknown. Photo by Samuel Goudsmit, courtesy of Emilio Segrè Visual Archive, Goudsmit Collection.

That December Fermi and his family left Italy for Stockholm, where he received the 1938 Nobel Prize for Physics. Instead of returning to Italy, the Fermis arrived in New York on 2 January 1939. When Niels Bohr arrived in New York two weeks later, Laura and Enrico Fermi met him at the dock. Although Bohr had been told by Otto Frisch about the experiments of Otto Hahn and Fritz Strassmann, the theory of fission by Frisch and Lise Meitner, and Frisch's experimental confirmation, Bohr did not mention the remarkable discovery to Fermi. On 20 January, Lee said, Willis Lamb returned from Princeton, where he had just learned of the fission discovery from Bohr. It was a Saturday morning and the Pupin Physics Lab was deserted, but Lamb was eager to spread the big news. Fermi arrived after lunch, and Lamb told him. Fermi quickly disappeared to his lab, realizing that there was a much easier way to observe fission. On 25 January, Fermi and several colleagues succeeded.

After World War II, in 1946, Fermi moved to Chicago. That same year Lee arrived from China on a fellowship. "One of the most precious events in Chicago was in 1948 when Maria Goeppert-Mayer gave an interesting but somewhat confused talk on levels

of different nuclei and why there were some mysterious stability numbers." At the end of her talk, Fermi said, "Is there any indication of spin-orbit coupling?" The bell rang and that was the end of the seminar." A few weeks later she gave a second seminar, "and that was the shell model." At this second seminar Lee initially thought, " 'No, not again,' because the first [seminar] was totally confusing. But the second seminar was totally different. Maria started with Fermi's question and the magic numbers worked out magically. This then led to the 1963 Nobel Prize in Physics."



Fermi with Maria Goeppert-Mayer. Photo courtesy of AIP Emilio Segrè Visual Archive, Goudsmit Collection.

Fermi once asked Lee the approximate temperature of the Sun at its center. Lee replied, "Ten million degrees." Fermi asked "How do you know?" Lee told him he had looked it up. Fermi asked if he'd verified the number and Lee replied, "It's really complicated. It's not so easy to integrate these equations." Fermi suggested that Lee build a huge specialized slide rule that would enable the solution of two radiative transfer equations, one that involved the 18th power of the temperature, and the other that involved the reciprocal of temperature to the 6.5th power. Over the next few weeks Lee built a slide rule that was 6.7 feet long, and carried out the necessary

Remembering Enrico Fermi

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integration. "It was great fun," he said.

The second talk was by Richard Garwin (IBM Research Labs), whose topic was "Working with Fermi at Chicago and Los Alamos." Garwin came to Chicago in 1947. After a few months of course work he got up the courage to ask Fermi if he could help in Fermi's lab. Fermi had a machine shop in his lab, including a lathe and a cutting saw. Although he had great respect for the technicians in the central machine shop, he felt they were too fastidious, and would take ten times as long to make something ten times more accurate than was needed. When the new Chicago cyclotron started working the beams needed to be extracted and targets positioned. In the cyclotron, rather than having the targets mounted on probes, Fermi devised a trolley cart that moved along the cyclotron rim. The cart carried a small copper or carbon target that could be flipped into and out of the beam.

Garwin recalled, "Among my regrets is one already cited by T. D. When I joined the faculty in 1949 after I got my PhD, I was given my own laboratory at the Institute of Nuclear Studies." Garwin was busy doing experiments on the betatron and planning experiments for the cyclotron. "One day Fermi came in (I'd missed the seminar) and in typical fashion asked if I'd thought about including spin-orbit interactions in the calculation of wave functions and energy levels. So I thought about that." Two weeks later Fermi came in and asked Garwin if he had made any progress on the idea and Garwin said, "None. So he said he'd talk to Maria Mayer about it. I'd simply lacked the courage to put down what I was doing, pick up a new challenge, where, in fact, I would not have done as well as Maria."

Because Chicago paid faculty salaries only nine months in a year, Fermi suggested that Garwin consult for Los Alamos in the summer. Fermi had gone to Los Alamos in Fall 1944 and worked there through 1945. "He wasn't in charge of any development group, although there was an F group named for Fermi. He was a treasured

consultant known as 'the Pope.'" If anyone needed to know the answer to a calculation he'd show them how to do it, "or *in extremis*, provide an answer." During the summer of 1950 Garwin shared an office with Fermi, where people would come to talk to him.

One visitor was Frederick Reines, who suggested that with all the explosions taking place at the Nevada Test Site, he could put a detector underground and detect antineutrinos from the beta decay of fission products. A 17-kiloton nuclear weapon would be made with a few kilograms of uranium and yield neutrinos only from fission products. Fermi responded that it would be preferable to use one of the modern reactors that burned several kilograms of uranium each day. "So Fermi's suggestion led Reines to do the more feasible experiment—at a reactor—for which Fred received the Nobel Prize in 1995."

At Los Alamos Fermi was concerned with many things, including the Taylor instability. "It's a very important phenomenon in nuclear weaponry and other fields," Garwin said. Fermi tried a two-dimensional model, in which a broad tongue of fluid moves into dense material and a narrow tongue moves in the opposite direction and falls. He solved the problem numerically but wasn't satisfied with the solution. One afternoon, John von Neumann came by, saw Fermi and asked what he was doing. Von Neumann returned fifteen minutes later and wrote on the blackboard how to approach the problem analytically. Fermi leaned against his doorpost and said, "That man makes me feel I know no mathematics at all."

"Fermi used to say he could solve only six problems, but he was very good at transforming any other problem into one of those six." His crude approach found the narrow tongue proceeding into vacuum with a large amplitude and uniform acceleration at $8/7 g$. In the more rigorous analytical calculation that he did with von Neumann, the limiting acceleration was still $8/7 g$. "Fermi had it very right."



Enrico Fermi. Photo by Samuel Goudsmit, courtesy of Emilio Segrè Visual Archive.

Back at Chicago, Garwin recalled, Edward Teller (a Chicago faculty member) dropped by with his latest enthusiasm. Fermi remarked, "That's the only monomaniac I know with more than one mania."

The Fermi memorial session's last speaker was Fermi's last graduate student—Jerome Friedman (MIT), whose title was "A Student's View of Fermi." Friedman recalled that when he entered the University of Chicago in 1950, "I had no credentials in physics." During his short time in Chicago, Friedman said, Fermi led the group that discovered the so-called (3,3) resonance, which has isotopic spin $3/2$ and spin $3/2$. Fermi did not jump to conclusions, and "it took him some time to accept its validity. It was the first indication of substructure in the nucleon." His work also served to confirm charge symmetry and isotopic spin conservation in the strong interaction.

When Friedman passed the PhD exam, he summoned all his courage and asked Fermi if he could do his PhD research under Fermi's supervision. "He said, 'Yes.' I was overjoyed, and he didn't even ask about my qualifications." On a few occasions Friedman was invited to parties at the Fermi home. "His students were his extended family. Often there were parlor games; he was very competitive and clearly liked to win."

"Fermi was a cheerful man with a good sense of humor," Friedman said. Fermi enjoyed the annual Christmas party where the students poked fun

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Secrecy and Physics

By Dan Kleppner, Forum Chair

Peter Galison's talk "Secrecy and Physics" described how antecedents of modern secrecy can be observed in the censorship practices that accompanied the growth of Czarist and European newspapers during the 19th century. At that time the censorship focused on political and diplomatic issues rather than technical ones. Galison showed examples of Russian newspapers that had been censored with black paint, known as "caviar." Censorship was so common that Freud saw manifestations of it in psychological disorders.



Peter Galison (left) and Dan Kleppner (right) at the "Secrecy in Physics" session. Photo courtesy of George Zimmerman.

At the time of the Great War, when letters were the chief medium for sharing ideas, censorship was widespread. Letters were often marked "open" so that the censors would not damage them. Such pervasive censorship forced intellectuals to live under a cloud of oppression. When the United States entered the war it passed a Sedition Act to suppress opposition to the war, and an Espionage Act to prevent activities counter to U.S. interests. Neither Act, however, was aimed at technical information. Secrecy that focused on technical information, rather than political issues or diplomatic embarrassments, was a product of World War II with its nuclear and radar projects.

The Atomic Energy act of 1946, the McMahon Act, established secrecy policy for nuclear issues. Section 9 of the Act, as submitted to Congress in 1945, held that information not specifically classified could not be withheld. But the law, as signed by President Truman the following year, had been modified by Section 10, declaring that information was to be barred unless it was specifically declassified, introducing the concept that information is "born secret." There has been an ongoing struggle to find the appropriate balance between the level of secrecy needed to ensure security, and the information exchanges necessary for technical and scientific progress. This tension can cause mistrust between parties. Galison sees such mistrust as the underlying reason for the establishment of Livermore Labs as a separate national laboratory to carry forward the H-bomb project.

Despite emphasis on technical matters, after WWII secrecy has still been invoked to avoid embarrassment. Galison illustrated with the Reynolds case of the 1948 crash of a B-29 that carried several civilian scientists. The victims' families sued the government on grounds of incompetence. The case ended in a 1953 Supreme Court ruling, which favored the Air Force claim that matters of secrecy prevented them from sharing the accident report. The suit was dropped. By 2000, with the release of certain documents related to the crash, it was found out that the government's claim had no merit, and the case was pursued in 2003, when family members of the original plaintiffs petitioned the Supreme Court to re-open the case. That petition was denied, resulting in new complaint filed in federal district court.

Finally, Galison spoke briefly about making the film *Secrecy*, which was shown at the meeting. Making a film about topics one cannot talk about presents obvious challenges! The film addresses the problem of balancing

national security and democracy, and questions of how information can flow, be sequestered, or be shared.

Steven Aftergood of the Federation of American Scientists, Project on Government Secrecy, in a talk entitled "Secrecy and Physicists: Intersections of Science and National Security," described a 1994 *Science* article by Glenn Seaborg called "Secrecy Runs Amuck." Seaborg's alleged that the Department of Energy censored his personal diary, removing material totally unrelated to national security. His larger point asserted that the national security system had "run amuck." Since Seaborg's article the secrecy system has considerably expanded. In 1994, 3.5 million objects were classified. In a recent year, 23 million objects were classified. Such classification costs nearly ten billion dollars per year. Nevertheless, Aftergood does not believe that secrecy has "run amuck," at least not in the world of physics. There has been a notable declassification of material in the past two decades, particularly for inertially confined fusion.



Glenn Seaborg (right). Photo courtesy of Emilio Segrè Visual Archive.

The landscape of secrecy includes broad categories of restricted but not classified information, including SUTI (sensitive unclassified technical information), UCNI (unclassified controlled nuclear information) and more than 100 other categories. In three years these will be replaced by a single category, Controlled Unclassified Information, or CUI. President Obama, in Executive Order 3526, includes two references to classified information policy on scientific and technical matters. One broad category requires that information may be classified only if its disclosure could cause damage. The second states that basic research not clearly related to national security cannot be classified. A presidential directive issued in the Reagan administration requires that the products of fundamental research shall remain unrestricted if possible, and if they must be controlled the process can only be done by national security classification.

Aftergood described several recent examples showing how the impact of the government secrecy system ebbs and flows. The Invention Secrecy Act of 1951 asserts the right of the government to control disclosure of privately generated information. The Act permits any one of several Presidentially-selected governmental agencies to classify patents whenever they believe disclosure would be detrimental to national security. At the end of last year, over 5000 inventions had been controlled, including 103 new ones in 2009. The potential conflict between this law and the First Amendment has not been tested.

Another problem occurs when secrecy interferes with public deliberation of scientific issues. Aftergood has a pending Freedom of Information Act request for information on the list of inventions, but there has so far been no response. Last year the Jasons carried out an unclassified study on science

and technology for national security. Although the study was unclassified, the Pentagon has refused to release the report on the grounds that it is “deliberative.” Another case of official secrecy interfering with public debate concerns a series of studies by the Department of Energy on the costs and risks of nuclear disarmament. The Federation of Atomic Scientists is actively concerned with disarmament, but has been denied permission to see the studies. The Defense Technical Information Center announced last fall that it would offer a portal for providing information on publicly supported fundamental research, but only to classified users. This seems to conflict with the presidential directive that stipulates only through national security classification can information be restricted.

A final episode that Aftergood related concerned a Brazilian physicist who, last April, published a 400-page volume on the physics of nuclear explosives. Most of the book is a digest of the large quantity of public literature already available on the subject, but the author used unclassified computer models to do numerical simulations of a U.S. thermonuclear weapon. The International Atomic Energy Agency urged the Brazilian government to restrict the book. Instead, the government defended his right to work, the press took up the case as a tribute to Brazilian science, and the author became a public hero.

William Happer, in his talk “How Much Secrecy?” asserted that, although secrecy can be vital, extraordinary care is needed to get it right: not too much, not too little. From living systems to large technical projects, all systems require feedback to function. However, secrecy interferes with feedback, resulting in projects that can “run amuck.” Happer witnessed examples of this throughout his experience as a Jason, and while at the Department of Energy. Program managers are protective of

their programs and are often unwilling to let others have a look. Feedback is best provided by review committees. However, members of review committees are often part of a small community—or part of the program itself. Such a practice contradicts James Madison’s admonition that “No man should be a judge in his own cause.” Happer contrasted the successful operation of the nation’s two weapons laboratories that benefit from independent and “vigorous” review by the competing laboratory, with failures in Department of Defense and Central Intelligence Agency programs that lack independent reviews. He recommended the film *Secrecy* by Peter Galison for elaboration of these issues.

Happer then turned to a matter of secrecy by non-governmental agenda-driven organizations, citing the *New York Times*’ failure to report the mass starvation in the Ukraine in the early 1930s and Stalin’s Moscow Trials of the late 1930s. Happer’s chief concern today is the withholding of scientific data with respect to climate change. Tapes from the Climate Research Unit of the University of East Anglia that were recently made public provided evidence that data was intentionally withheld from “politically incorrect” groups. This was consistent with Happer’s own experience that skeptical scientists who attempted to get climate change data could be “stiff-armed.” Access to the data would permit the APS to carry out an independent study on climate change. Happer urged the APS to set aside the “mainstream media” position that it recently endorsed and establish an independent position based on its own analysis of the data. ■

Origins of Research and Teaching at Selected Physics Departments

By Ronald E. Mickens

On Monday 15 February, the Forum sponsored an invited session on “The Origins of Research and Teaching at Selected Physics Departments.” This event was co-sponsored with the Forum on Education and the American Association of Physics Teachers. The session was chaired by James Stith, and the three speakers were Hans von Baeyer (College of William and Mary), Warren Collins (Fisk University), and Jerry Gollub (Haverford College). A goal of these presentations was to allow the speakers to highlight the significance of teaching, education, and research accomplishments of their respective physics departments. The material to follow summarizes the three presentations as provided by the individual speakers.

Hans C. von Baeyer spoke on “250 Years of Physics at the College of William and Mary: 1760 - 2010.” He began by informing us that “the College of William and Mary was founded in 1693 as the second institution of higher learning after Harvard. Physics instruction began in earnest with the arrival of professor of mathematics William Small of Scotland who also taught astronomy and Newtonian physics. In 1760, two hundred and fifty years ago, Thomas Jefferson became Small’s student -- an association which he later credited with ‘fixing the destinies’ of his life. From his College in Aberdeen Small brought to the colonies the lecture system as well as the use of lecture demonstration apparatus. When he left Virginia after six years, he took along a commission to purchase an elaborate collection of instruments in London, which was duly sent back to America. The Revolution disrupted instruction, but in 1776 Phi Beta Kappa was founded at William and Mary, and by 1779 Thomas Jefferson, as Governor of Virginia, instituted a curriculum reform that included the creation of a formal chair of ‘mathematics and natural philosophy,’ as physics was then called.

“Though most of Small’s equipment was lost to war and fire, by the middle of the nineteenth century a more modest teaching collection had been assembled. The Civil War shut down the College, and it was slow to rebound. At the beginning of the twentieth century the College became a state university and went coed. Physics continued to be taught at the undergraduate level. In the 1960s, soon after Sputnik, an MS physics program was instituted, and the faculty grew correspondingly. When nearby NASA/Langley Research Center built the Space Radiation Effects Laboratory (SREL), with a synchrocyclotron to mimic the solar wind, it was realized that the machine could also benefit physics. William and Mary, as the closest state institution, would take the lead in this effort. In short order a PhD program was created, the William Small Physical Laboratory was built to house it, and the physics faculty grew to about 30. To avoid becoming overly specialized, the department was carefully structured around three major research topics (plasma, solid state, atomic & molecular), each with theoretical as well as experimental expertise, in addition to accelerator-based physics. The latter group established an international reputation in ‘intermediate energy physics’ between nuclear and particle physics.

“The Physics Department helped to lead the development of the College into a small research university by spawning related PhD programs and encouraging the creation of others. At the same time the benefits of graduate research for undergraduate teaching continued to be emphasized. At William and Mary, for example, all physics majors are required to complete a senior research project, usually in collaboration with graduate students and faculty. Other examples of the department’s involvement in physics teaching are the annual REU program, which has been offered since its inception by the NSF in 1987, and

the hosting of the International Physics Olympiad, which came to the US for the first time in 1993.

“In the 1980’s SREL became obsolete, and the modern era began with its replacement by the US Department of Energy’s Jefferson Lab. As did SREL, the Jefferson Lab benefits W&M in myriad ways. After the initial competition for the site of this electron accelerator for nuclear physics, W&M has continued to play a major role in its life. Faculty, graduate students, and undergraduates are attracted by it. Following its tradition, though, the department’s research program continues to be multifaceted. Solid-state physics has acquired an important NMR facility, intermediate energy physics has given way to neutrino research, and powerful lasers are used in atomic research. Today, 250 years later, both the William Small Laboratory and the Jefferson Lab are undergoing significant upgrades. The legacies of Jefferson and his mentor William Small at the College of William and Mary are secure.”

The second speaker was Warren Collins who presented a detailed history on “80 Years of Physics at Fisk University.” The University was established in Nashville, TN, by General Clinton B. Fisk within six months after the Civil War ended. The first students ranged in age from 7 to 70, but shared common experiences of slavery and poverty – and an extraordinary thirst for learning. With the sponsorship of the American Missionary Association, Fisk University was incorporated on 22 August 1867.

Elmer Samuel Imes, the second African-American to achieve the PhD in Physics, received a BA degree in 1903 from Fisk University. About 1910 he returned to Fisk where he served as an instructor of mathematics and science and completed his master’s degree in 1915. That same year, Imes went to the University of Michigan and began work in the laboratory of Harrison M. Randall, designing and

building high-resolution infrared spectrometers. In 1918, he published his dissertation results in *The Astrophysical Journal*. In the two decades after publication, this work was extensively cited in research papers, books, and reviews on the spectra of diatomic molecules.

Imes returned to Fisk in 1929 to inaugurate Fisk's Physics Department and initiate a research program in infrared (IR) spectroscopy. Imes continued some experimental work, but his IR spectrometer was not of sufficient quality to do world class research, so he did not publish any more scientific papers.

After Imes' death from cancer in 1941, research in IR continued (mainly after WWII) through the efforts of James R. Lawson and Nelson Fuson. Lawson had been Imes' student at Fisk, and he, along with Fuson, all did their doctoral research at Michigan under Randall. As a result of this close relationship, Lawson was able to obtain a research IR spectrometer built especially for Fisk by the Michigan Physics Department's machine shop.

IR research rapidly caught on at Fisk with contributions from Fuson, Lawson, Marie-Louise Josien (a French chemist who studied under Jean LeComte), and graduate students enrolled in the Physics Department's MS Degree Program. Soon the whole IR Research group was publishing their research findings in the American Chemical Society's and American Physical Society's scientific journals. Thus, when these Fisk graduate students started reporting their scientific results in the Southeastern Section meetings of the ACS and APS, it became clear to the scientific community in the South that they could no longer hold meetings in southern hotels that would not serve blacks.

One consequence of this situation was the holding of the 1956 SESAPS Annual Meeting at Fisk University. Fisk's faculty and students presented about 30% of the scientific papers, and the meeting's banquet speaker was Nobel Prize winner Arthur Compton.

Beginning in 1953, the Fisk IR Spectroscopy Lab decided to offer a week's program of lectures and laboratory experiments to help industrial and governmental scientists learn infrared techniques and the interpretation of IR spectra. Fisk held this institute for more than 50 years and was aided in this effort by faculty from American universities and instrument companies, as well as scientists from government laboratories. In all, over 3,000 scientists were trained during the Fisk Infrared Institute's lifetime.

Since the 1990's, the Physics Department has acquired state-of-the-art equipment for research in surface physics, crystal growth, spectroscopy, and nanomaterials and sensors. In addition to 6 teaching faculty and 10 research faculty/staff, there are more than 20 graduate students and 20 undergraduate students involved in the various research programs.

Early in the first decade of the 21st century, the Fisk-Vanderbilt Master's-to-PhD bridge program was created. This allowed students to earn the MS degree at Fisk and also obtain fast-track admission to a participating Vanderbilt PhD program, all with full funding. Since 2004, the program has attracted 34 students, 30 of them underrepresented minorities (56% female), with a retention rate of 94%.

The last speaker for the session was Jerry Gollub and his topic was "Research and Education in Physics and Astronomy at Haverford College." He began by noting that "the most distinctive feature of Physics and Astronomy at Haverford College is the centrality of undergraduate research." According to the 1933 history of Haverford by Rufus Jones, a required year-long course devoted to student research in physics was founded in 1920. "Each student picks a field and a problem capable of solution with the apparatus available... The student covers the literature and carries out experimental work to the extent of about 100 hours each semester. A detailed report...in the form of a

scientific article is written each semester. Students take turns giving weekly presentations about their chosen field." Reading about "Physics 10" was an eye opener to me, as I had assumed that undergraduate research originated much later.

"I found and looked at some of these early student papers, dating from 1926-30. They were substantial, about 20 pages each semester. Many included significant experimental work, sometime inconclusive, but appropriate for the state of physics at the time. All showed a substantial mastery of the corresponding literature. The projects from 1926 had titles such as 'X-Ray Spectra and Atomic Structure,' 'Isotopes and Positive Rays,' and 'Exact Determination of Longitude by the Short-Wave Radio Time Signal Method.' Sometimes these early research students made mistakes, with occasional statements like this one: 'The positive nucleus is built up of protons bound together by electrons.' This predates the discovery of the neutron, of course.

"It is clear from these reports that the students from this era had learned a great deal, and had shown remarkable ingenuity. These investigations do not seem to have been based on the pre-existing research programs of faculty members, which is surprising in view of current practices. Today we would also provide much better training and preparation before presuming that students might do something original.

"Currently, most Haverford Physics students do research or independent work of some kind, and all present senior theses and give departmental talks about their work. Their writing is elaborately critiqued, and must be rewritten until a professional standard is achieved. Sample titles of senior theses maybe found at www.Haverford.edu/physics-astro.

"Many students publish papers with faculty mentors, and/or give talks at national meetings. In a recent two-year period, 16 students were

Forum on the History of Physics: Contributed Papers

By David C. Cassidy

It is not often that Leopold von Ranke, the 19th century historian of the Prussian state, appears prominently in a paper at an APS meeting. But so it was in Harry Lustig's personal account at his announced end of a 15-year excursion into the history of physics, titled "What I Have Learned in Reading and Writing History of Physics." What he has learned began with von Ranke's pioneering insistence on the use of primary documents as the foundation for recreating history "as it actually was." Although it is sometimes admissible to speculate about the past on the basis of the evidence, it is not admissible to tailor an interpretation to a preconceived thesis. Lustig's case in point, which he has closely studied during the past 15 years, is the intense controversy over Heisenberg and German fission research during World War II. While deploring the angry denunciations or heroic depictions of Heisenberg in some accounts, Lustig called for the release of additional documents pertaining to this case that are still withheld by American and British authorities. The audience celebrated Prof. Lustig's historical excursion with a standing ovation.

Following the document trail on



Harry Lustig. Photo courtesy of George Zimmerman

the other side of the war, Cameron Reed spoke on "Bullion to B-fields: The Silver Program of the Manhattan Project." Huge electromagnets were required for the operation of the "calutron" electromagnetic

isotope-separators at Oak Ridge. Since copper for the wiring was in short supply, it was more expensive than silver. In 1942, the Army requisitioned 14,000 tons of silver bullion bars from the U.S. Treasury, which were industrially processed into wire and wound into huge magnet coils. In his unique account, Reed reported on his painstaking study of the many documents pertaining to all aspects of the "silver program," from requisitioning and processing to restoring the precious metal to the Treasury. In an interesting take on conservation he found that more metal was actually recovered than was requisitioned, apparently because of the intense cleaning of the metal-working facilities. He also calculated that more energy was expended in producing the uranium bomb than was released in the explosion over Hiroshima.

Continuing the uranium theme, in 1939 Niels Bohr stunned an audience at the Washington Conference on Theoretical Physics with the news that German researchers had discovered nuclear fission. It was the highlight of the series of annual conferences held at George Washington University during the years 1935 to 1947 (except for three years during the war). Paul Halpern reported on the history of these conferences in his paper "The Washington Conferences on Theoretical Physics: Bringing the Spirit of Copenhagen to Foggy Bottom." Inspired by the annual theoretical physics conferences in Copenhagen and other European locations, after he arrived at GWU in 1934 George Gamow established the annual conferences, with the support of the Carnegie Institution. As in Europe, problems in nuclear and quantum physics were high on the agenda, and Bohr's several appearances always galvanized the meetings. The meetings ceased as the need for such thematic events declined and Gamow's attention turned to cosmology.

Llewellyn Hilleth Thomas is best known for his discovery of Thomas precession and for his co-development of the Thomas-Fermi statistical



J. D. Jackson (left), photo courtesy of George Zimmerman. Llewellyn Thomas (right), photo courtesy of Emilio Segrè Visual Archive, Segrè Collection.

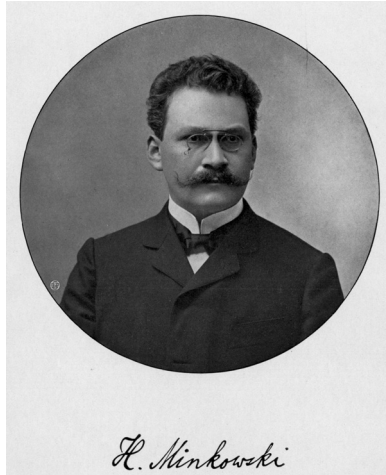
atom, both done before he was 24 and before he received his PhD. As John David Jackson revealed in his paper "Llewellyn Hilleth Thomas: An Appraisal of an Unappreciated Polymath," during his subsequent career Thomas, who lived from 1903 to 1992, made many significant contributions across a wide range of other disciplines, including astrophysics, molecular physics, accelerator physics, computer design, software and hardware development, and defense research. Trained at Cambridge and Copenhagen, Thomas arrived at Ohio State University in 1929. He spent the war years at the Ballistic Research Laboratory, Aberdeen Proving Grounds, then joined IBM's Watson Scientific Computing Laboratory and Columbia University until his retirement years, which he spent at North Carolina State University. Jackson outlined many of Thomas's other significant but lesser known accomplishments, all of which lead to an appreciation of Thomas as a consummate physicist and applied mathematician.



Charles W. Clark. Photo courtesy of George Zimmerman.

Among three other biographically oriented papers, Charles W. Clark spoke on “Ettore Majorana and the Birth of Autoionization.” Aside from the mystery of Majorana’s disappearance in 1938, Clark, speaking for the three authors of the paper, pointed out several puzzles regarding the treatment of Majorana’s work on autoionization. He was one of the first to apply quantum mechanics to the spectroscopy of many-electron atoms. In so doing he was the first to solve several outstanding problems by developing the theory of autoionization in 1931. Yet references to his work appeared only sporadically in the subsequent literature, and the development of the modern theory of autoionization occurred essentially without the benefit of his prior work.

Felix T. Smith asked the question “Did Minkowski Change His Mind about Non-Euclidean Symmetry in Special Relativity?” We have only three published documents on relativity written by Hermann Minkowski, all within 14 months before his sudden death on 12 January 1909. The first (A) is the text of a lecture he gave to mathematicians at Göttingen on 5 November 1907. He never published this lecture, although it announced that the world is “in a certain sense a four-dimensional, non-Euclidean manifold” and supported that notion by showing the geometry of relativistic velocity space to be non-Euclidean. Only six weeks later he submitted for publication a long paper (B), containing most of the results in A and much more, but leaving out any statement about its non-Euclidean geometric implications. The third paper (C) is his most famous, the lecture he presented at a scientific congress on 21 September 1908. It was devoted to the concept of four-dimensional space-time. It does not mention the non-Euclidean aspect at all. Historians have long wondered why Minkowski never published some of the most prominent conclusions of his first lecture, those dealing with non-Euclidean geometry.



Herman Minkowski, 1915. Photo courtesy of Emilio Segrè Visual Archive, Born Collection.

Ten years ago Scott Walter discovered in A an error in sign in an equation for the velocity four-vector, but that error does not contaminate the lecture’s conclusion about the non-Euclidean geometry of velocity space. Smith’s analysis showed the velocity equations were incomplete in other ways as well. Thus the velocity paragraph in A seems to have been a very late and hasty insert, written at the moment Minkowski was discovering the true covariant form of the velocity vector, before he had all the details correct. But the form of the equations brought to him the sudden insight that all this was a new case of a mathematically old story, the non-Euclidean geometry of Lobachevsky and Bolyai. It appears this new insight came to Minkowski very close to the date of the lecture, and he thought it so important that he added it to his text even though it was not complete. With this understanding, the more mature earlier text of the lecture can now be separated from the less ripened inserted material, and the latter examined on its own. Doing so sheds much light on Minkowski’s creative process—the rough guess at what he thinks the form of the result should be, testing it against his other expectations, new adjustments, while learning from the process. In his mathematical work, Minkowski was well known for

his powerful geometric insight and imagination; it is now possible to see how he brought this to bear on the velocity problem, and how it helped bring him to his non-Euclidean insight. Minkowski probably wanted to explore the topic further before publishing it. Arnold Sommerfeld published paper A six years after Minkowski’s untimely death. The answer to Smith’s original question is “No:” Minkowski did not change his mind.

In “Twist ‘Til We Tear the House Down: How Clifford Solved the Universe in 1870,” James Beichler argues that British mathematician W. K. Clifford had introduced a curved hyperspace theory into physics in 1870, long before similar theories of the early 20th century. Clifford and his followers developed their curved-space theory for electromagnetism, which is one reason it has been overlooked by most historians. Yet because Clifford argued for the reality of space curvature in electromagnetism, he facilitated the acceptance of Einstein’s curved space-time for gravitation, and he may be regarded as a precursor of modern unification theories. His theory of “matter as curved space” influenced Cartan’s spinor theory and Penrose’s twistor theory.

In one of two concluding papers, Clarence A. Gall presented a historical view of the development of Gustav Kirchhoff’s black-body radiation distribution functions. Kirchhoff introduced a new function based on emission as a decay process that fits the Stefan-Boltzmann and Wien displacement laws exactly. It contradicts the traditional assumption that a hotter body always emits more intensely than a colder body for all wavelengths, which is supported by observational infrared astronomy. Finally, Jeffrey Boyd described the ancient emission and intromission theories of sight, and presented a speculative hybrid theory developed in 1996. ■

Manhattan Project

Continued from page 3

its interpretation by Meitner, Frisch, Bohr and Wheeler, the evolution of the understanding of the role of different uranium isotopes in the fission process, the Szilard/Einstein letter, and the opening of World War II. After discussing the establishment of the Manhattan District, we look at what was done at Oak Ridge, Hanford, and Los Alamos, devote a class to the *Trinity* test, and then discuss the Hiroshima and Nagasaki missions. In the last couple days of class we look at some of the effects of nuclear weapons, the numbers of postwar tests and weapons in the world today (these always surprise students), and what relevant treaties are in effect. I also take some time to give students some direction on where to look for credible sources of material on the Project. (A Google search on "Manhattan Project" yields over 3 million hits.)

My goal for the class is that students emerge with a correct understanding of the history and science of nuclear weapons. They should appreciate that the development of nuclear weapons was in no sense "pre-ordained," and that even many of the leading figures of the nuclear research community at the time were skeptical that such weapons could ever be realized. Students should be able to explain how these weapons were developed, how they work, what problems were overcome in making them, how a reactor differs from a weapon, why implosion is necessary for an efficient weapon, and why a lump of natural uranium or even a subcritical mass of U-235 sitting on a lab bench would be perfectly safe. Equally important, I always make sure to tell them something of the lives of the people involved: of Lise Meitner fleeing Germany just months before the discovery of fission, of Enrico Fermi and Hans Bethe and others making their way to America, of Oppenheimer's brilliant, eclectic, and ultimately tragic life. For many students this may be the only physical science class that they take during their college career, and I want them

to know not only of the importance of the law of conservation of energy but also that science and engineering are done by real people who possess all the typical human strengths, fears, motivations, and fallibilities.

Grades are established in the usual ways: I always have tests which are multiple choice, short-answer, and fill-in-the-blank questions as well as brief homework assignments where students might have to balance reactions or compute an energy release or the like. On various occasions I have had them write a 1000-word end-of-course reflective summary paper or participate in a last-day session where they get a random question: "What were you most surprised to learn?" "What aspect of the Project would you want to learn more about, and why?" "Who associated with the Project would you like to have lunch with and what would you ask him or her?" When I did the course for a group of First-Year students in Fall 2009, the more extended time of a regular term allowed me to add an extra reading/writing component: each student was randomly assigned a different book to read and prepare a report on, with one cycle of submit-revise-resubmit. In most cases these were biographical works; one obviously cannot get into technical depth with such a class. The regular-term incarnation of the class also permitted more time at the end to examine weapons effects, postwar developments, and topics such as the CTBT and START talks.

Alma is a small school; I can reach only a tiny number of students with this material. Since 2002 just over 100 students have taken the course. Many tell me that they find it fascinating and informative; history students in particular seem to enjoy it, but students come from all disciplines. Beginning in 2007 I began taking an end-of-course survey, asking students to imagine themselves as President Truman in the summer of 1945 but with the benefit of some understanding of the functioning and effects of nuclear weapons, and asking them to

choose (anonymously, if they desire) one of six statements that most closely matches their own thoughts. These range from (paraphrased) "The use of nuclear weapons against Japan without prior warning was entirely justified ..." to "Nuclear weapons are a moral abomination..." Responses from 45 students have been collected so far. About half of these chose the first option (use entirely justified, would have acted in same way as President Truman), which is followed by about 10 students each for options along the lines of the first bomb being justified but allowing more time and warnings before subsequent ones; and a sort of middle-ground default option stating that the ferocity of the war, the looming post-war geopolitical situation with Russia, and the tremendous resources that had been devoted to the project made the use of the bombs essentially a foregone conclusion, beyond any real control of any one person. One student opted for the "moral abomination in any circumstance" option.

There are plenty of books, papers, websites and videos out there on this topic. I humbly suggest that a good place to start is a Resource Letter of over 100 sources that I published in the September 2005 issue of *American Journal of Physics*. Later this year or early next I will be publishing a text under the Springer imprint titled *The Physics of the Manhattan Project*, which will treat the physics of the project at about a junior level. A tentative table of contents can be found at www.manhattanphysics.com.

The Manhattan Project is a virtually open-ended vehicle for teaching our students some physics, history, political science and sociology, and I encourage readers to think about offering something along the lines I have described. If you are already doing so I would love to hear from you: What has been your experience? In what sort of venue to what student population? What has worked and what hasn't? ■

Call for Nominations for APS Fellowships

The FHP Nominating Committee, chaired by Gloria Lubkin, is charged with selecting candidates to stand for election to FHP Executive Committee positions. The next election will be held around the beginning of 2011. The Nominating Committee will meet over the next several months and welcomes suggestions from FHP members, including self-nominations. The suggested candidates must be members of FHP at the time of nomination. The positions that must be filled are:

(a) Vice-Chair, whose primary responsibility is to help the Chair-Elect develop and arrange FHP sessions at meetings, as well as chairing the FHP Fellowship Committee. The Vice-Chair is expected to become Chair-Elect, Chair, and Past Chair in subsequent years.

(b) Two At-Large Executive Committee positions. The At-Large members serve three-year terms and hold positions on various standing and *ad hoc* committees of the forum.

Please send suggestions to the FHP Secretary-Treasurer, Tom Miller, at millerf@bc.edu.

The FHP Bylaws state that if more than 5% of the members (approximately 195 persons) suggest the same person, and if that person expresses willingness to serve, their name will appear on the ballot in addition to persons selected by the Nominating Committee. ■

Call for Physics Department Histories

Has your institution written the history of its physics department and submitted a copy to the Niels Bohr Library and Archives? As history appreciators, it would be sadly ironic if any physics department's history were lost.

We recently received *A History of the Department of Physics of the University of Colorado at Boulder 1876-2001*, by Albert A. Bartlett and Jack J. Kraushaar. This impressive book, the size of a telephone book of a small city, was

Call for Department Histories

Continued from previous column

beautifully written, illustrated, and referenced. Few will have the time to pull together so thorough a history. However, the concern of importance is to *get it written down*, even if the product looks more like a student term paper than a commercially-published tome (one can always make it look nicer in the next edition!). As Bartlett and Kraushaar noted in their Introduction, "It was evident that the faculty who joined the Department during the first post-war wave of expansion, starting in the 1950s, were retiring and were cleaning out their file cabinets and were otherwise disposing of their records."

In a letter of 28 September 2009 that was written to department chairs, the FHP Executive Committee said encouragingly, "We are not asking department chairs to undertake the writing of departmental histories. We *are* asking you to preserve and make available historical accounts of what your department has done and accomplished. We are also asking that you encourage your retiring faculty to leave a vita and comments on their accomplishments, from their perspectives, on their careers. Also their future plans. These accounts need not represent a complete history. The best departmental histories that we have seen represent the gathering of material collected and summarized by a succession of different authors at different times...The most beneficial locations for these...histories are likely to be departmental websites. But we hope that you will also deposit a copy in the Niels Bohr Library & Archives...and please let us know when you do so." The NBLA contact person is

Joe Anderson
Director, Niels Bohr
Library & Archive
American Institute of Physics
One Physics Ellipse
College Park, MD 20740 - 3843
janderso@aip.org
(301) 209-3183

If you have any questions, please contact Bob Arns, University of Vermont (Robert.arns@umv.edu), or George Zimmerman, Boston University (goz@buphy.bu.edu). ■

Remembering Enrico Fermi

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at senior faculty members. Valentine Telegdi, then a junior faculty member, "had an uncanny ability to imitate some of the stellar figures in physics. At one Christmas party Val was enclosed in a huge box with flashing lights that represented a computer. It was named the Enriac or Fermiac; I don't remember which. Fermi's voice emanated from the box in a very slow, authoritative manner. This computer was supposed to answer any order of magnitude question and did so. Fermi was clearly amused."

At Chicago many speakers visited. Fermi would inevitably have many comments and questions. "His questions were gentle but sometimes devastating to the speaker. They usually started off with, 'There is something I do not understand.' And then it was a problem for the speaker."

In the summer of 1954 Fermi went to Italy, where he became ill. When he returned to Chicago in September, he and Friedman waved to each other in the corridor. "I was struck by how gaunt he looked." The next day Fermi had exploratory surgery and was found to have inoperable cancer. Subrahmanyan Chandrasekhar told Friedman that when he and Herbert Anderson visited Fermi at the hospital, they were initially at a loss for words. Fermi sensed their difficulty and put them at ease by asking, "Tell me, Chandra. When I die, will I come back as an elephant?" After that the conversation went smoothly.

After Fermi's death on 28 November 1954, Friedman was asked to gather together the things in his mentor's office. "I found only about four books, and one was by his boyhood friend, Enrico Persico. It was clear Fermi didn't need books. He worked out everything for himself. Over half a century later I still look back in awe at this great physicist and remarkable human being." ■

Heisenberg and the Interpretation of Quantum Mechanics: The Physicist as Philosopher

By Kristian Camilleri, Cambridge University Press, 2009, 199 pp., \$81.00

Quantum Theory at the Crossroads: Reconsidering the 1927 Solvay Conference

By Guido Bacciagaluppi and Antony Valentini, Cambridge University Press, 2009, 532 pp., \$126.00

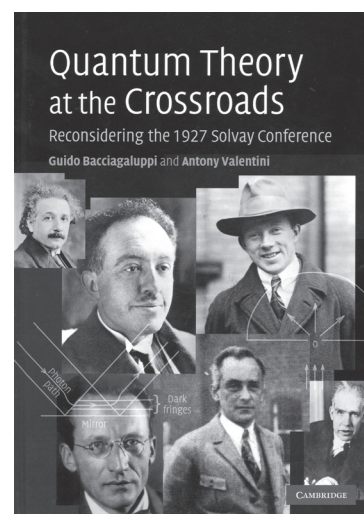
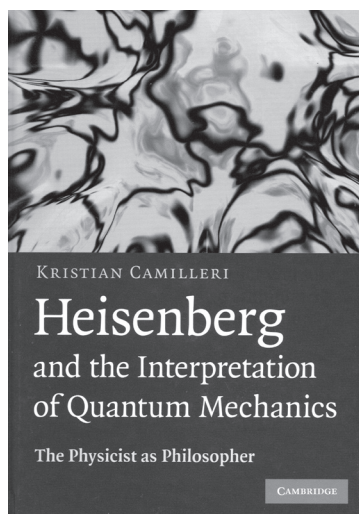
As depts of twentieth-century physics might be forgiven for thinking that the history of quantum mechanics is all wrapped up. After all, the origin of the quantum theory is one of the most celebrated stories in physics. The postulations of Planck and Einstein, the invention of Bohr's atomic model, the steady accumulation of spectral data, the breakthrough into quantum mechanics: this narrative has been with us since the new theory's authors put it into circulation. And yet there still seems to be infinite richness waiting to be discovered. A first wave of historical studies came in the 1960s through the 1980s, when the treasure troves of the archives were exploited and a round of in-depth interviews was analyzed and put to work. Now a new wave of studies is opening up new questions. This wave includes two significant books: one on Heisenberg's interpretative commitments by Kristian Camilleri; and one on the famous Solvay Conference on quantum theory by Guido Bacciagaluppi and Antony Valentini. Each takes us into the interpretative thickets of the new theory, with a spotlight on 1925-27, and each rewards its readers handsomely.

Camilleri, a historian and philosopher of science at Melbourne, starts from a thesis that has recently become attractive to historians and philosophers of science: There is no single coherent Copenhagen Interpretation—or at least there was none in the 1920s. Explicating Bohr's thought has occupied an army of interpreters, with less than satisfactory results. Camilleri's book *Heisenberg and the Interpretation of Quantum Mechanics* takes up another main architect of the Copenhagen Interpretation. Heisenberg's interpretative opinions have, strangely enough, garnered less critical attention.

Camilleri carefully traces how his ideas unfolded, disentangling them from Bohr's. The result is a masterful and enriching account that does much to re-complicate the story of quantum mechanics and the uncertainty relations.

Camilleri works through Heisenberg's *oeuvre* largely chronologically, analyzing his published papers and supplementing them with his letters. This is useful for storytelling, and it also helps with analytical clarity. He makes the point stick that Heisenberg's views changed significantly. This is true in the larger sense, as Heisenberg moved from an early strategic operationalism (or positivism), via a rethinking of Immanuel Kant's critical philosophy, to something Camilleri describes as a late-Wittgensteinian "linguistic turn." The same point about shifting views can be made at a finer level of detail, as Heisenberg's interpretative positions changed from 1925 to 1926, 1926 to 1927, 1927 to 1929, and so on.

While this emphasis on Heisenberg's changeable opinions could seem like punctiliousness, it is central to how Camilleri makes his case. His Heisenberg is not the late Heisenberg, committed to defending an interpretative common ground with Bohr. Rather, he is the working physicist of the 1920s engaged with the details of technical problems. Camilleri neatly demonstrates how



not just Heisenberg's views, but his approach, differed from Bohr's, giving substance to the familiar idea that Heisenberg started his thinking from the theory's mathematical formalism. Along the way, Camilleri shows how to understand important steps in Heisenberg's reasoning: his strategic use of observability, his understanding of the wave-particle duality and early matter waves, his "uncertainty" paper (which really deals with the possibility of quantum concepts) and the thought experiment on the gamma-ray microscope, and his take on Bohr's notion of complementarity.

Some of these arguments have already been advanced by other historians and philosophers. Camilleri adds important new insights and makes a case for the whole. He goes on to explore themes and topics that have received less attention, including Heisenberg's views on language, which may or may not be taken from Bohr. As an important source he points to Heisenberg's unpublished philosophical manuscript of 1942 (now translated into English, available at <http://werner-heisenberg.unh.edu>).

New Books of Note

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The result is a manageable-sized book that covers significant terrain. Camilleri does not address recent controversies over Heisenberg's role in the Third Reich, nor does he take up the whole course of Heisenberg's physical theorizing. Rather, we get a focused, in-depth account of important issues in his interpretative engagements. While the overall themes are of general interest, the details of the chapters are written for specialists, and the references to other historians and philosophers bear this out.

Bacciagaluppi and Valentini's *Quantum Theory at the Crossroads* also pushes us to rethink familiar tales. The focus is the monumental Fifth Solvay Conference in 1927, where the most important interpretative disputes over quantum mechanics were supposedly hashed out. The book is half a translation of the conference proceedings (originally issued in French, perhaps one reason it has not received adequate attention) and half a commentary on the scientific issues raised there. Both authors work on the foundations of physics, Bacciagaluppi as a philosopher, Valentini as a physicist, and their commentaries are scientifically well-informed and historically trenchant. They single-handedly retrieve the 1927 Solvay Conference from its undeserved fate, of always being gestured at but rarely understood.

Bacciagaluppi and Valentini make good on the claim that there was much more to the conference than what it is usually remembered for—the famous Bohr-Einstein debate. The mythology of that exchange, which took place outside the formal conference discussions, has crowded out nearly everything else. In fact, the conference's reports and discussions are a remarkable condensation of so much of what was stirring in quantum physics in these exceptionally rich years. The disputes of the day were physical, not just philosophical, as Bacciagaluppi and Valentini's commentary makes plain. And they were not resolved in the simple triumph of a Copenhagen Interpretation. Here the book's larger ambitions come into play.

Along with much else, the book is a

brief for the intelligibility of interpretative positions that were marginalized after 1927. Louis de Broglie's pilot-wave theory is presented on equal footing with Born and Heisenberg's quantum mechanics and Schrödinger's wave mechanics—as it was, quite plainly, at the conference itself. Bacciagaluppi and Valentini put particular weight on the observation that de Broglie proposed a multiparticle theory, not just a one-electron one (as David Bohm later suggested) and that his picture was extensively discussed at the meeting (contrary to what de Broglie later recalled). They also argue that de Broglie had answers to at least some of the criticisms he encountered, so the rejection of the pilot-wave theory was not foreordained. The authors start from the position that there is no longer an established interpretation of quantum mechanics; the pilot wave theory is very much in their sights. All the same, their account is historically sensitive, thoroughly grounded, and conspicuously fair. If it is mainly written for people working on the foundations of quantum theory, its introductory material and some of its commentary will be useful for all interested general readers.

It is a real service that Camilleri has explicated Heisenberg's thinking with such attention and that Bacciagaluppi and Valentini have brought the 1927 Solvay Conference back to light. These books go a long way toward something that has only recently seemed plausible—disassembling the appearance of early consensus around the interpretation of quantum mechanics. Rooting interpretative debates in the physics and providing critical historical context, the two books ought to open up far more interest in the consolidation of quantum mechanics.

Reviewed by *Cathryn Carson*

Cathryn Carson is Associate Professor of History and Director of the Office for History of Science and Technology at the University of California, Berkeley. She is author of *Heisenberg in the Atomic Age: Science and the Public Sphere* (Cambridge, 2010). ■

Research and Teaching at Selected Physics Departments

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co-authors of published papers, more than half of our undergraduates. Many students have won national awards for their work, including Goldwater and Churchill scholarships, Fulbright and Apker Awards, and (later) NSF Career Awards.

"Research training is seen by the faculty as important regardless of whether the student aspires to a career in scientific research or not. Actual career choices are broad, including almost all the professions and many neighboring scientific fields. There are quite a few prominent astrophysicists and physicists among them.

"The Physics and Astronomy curriculum supports this emphasis and culture of student research. For example, the Advanced Physics Laboratory teaches low noise measurement techniques, planning and design of experiments, sophisticated data analysis, effective use of library resources, and scientific writing. Experiments include work on microfluidics, quantum nanocontacts, quantum dots, and AFM imaging of synthesized nanotubes.

"Even first-year students can experience the spirit of investigation, for example through a seminar on astrophysics run as a problem-solving workshop, in which students investigate challenging questions such as: 'Why is there a maximum mass of a neutron star, what is this mass, and what happens when it is exceeded?' They learn about galactic structure, collapsed stars, and black holes, going far beyond what first year students normally do."

"The practice of 'education through research' that began in physics in 1920 spread throughout the Science Division by the 1950's, and eventually to all fields of study at Haverford. Undergraduate research mentoring is built into the normal teaching responsibilities of faculty members. As a result, the Faculty's commitment to research is as intense as occurs in prominent graduate institutions, and the focus on independent scholarship attracts many talented students to the College." ■

History of Physics

NEWSLETTER

Forum on History of Physics
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One Physics Ellipse
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