History of Physics

NEWSLETTER

A FORUM OF THE AMERICAN PHYSICAL SOCIETY • VOLUME XI • NO. 6 • SPRING 2012

News of the Forum:
Roger Stuewer Awarded 2013 Pais Prize

By Lillian Hoddeson and Michael Riordan

Roger H. Stuewer has been chosen to receive the 2013 Pais Prize for the History of Physics in recognition of his intellectual contributions to the field, as well as for his untiring efforts in fostering its development. In its citation, the Pais Prize Selection Committee recognized him "for his pioneering historical studies of the photon concept and nuclear physics, and for his leadership in bringing physicists into writing the history of physics by helping to organize and develop supporting institutions and publications."

Stuewer's research on the history of the light quantum was published in the definitive scholarly volume, *The Compton Effect: Turning Point in Physics* (1975), as well as a series of widely read articles. This body of work explains why Einstein's 1905 proposal that light consists of individual quanta was rejected for almost two decades by virtually all physicists until it was confirmed by Arthur Compton's X-ray scattering experiments, published in 1923. Drawing upon Compton's research notebooks and many other archival resources, Stuewer's analysis was set in the context of attempts to understand the nature of X-rays and gamma rays.

During the 1980s, as one of the first historians to examine the discovery of the neutron and the rise of nuclear physics, Stuewer again combined his scientific knowledge with a deep understanding of the social, political, and institutional contexts of his subjects to write a series of pivotal articles. These influential publications include "The Nuclear Electron Hypothesis" (1983); "Rutherford's Satellite Model of the Nucleus" (1986); and "The Origin of the Liquid-Drop Model and the Interpretation of Nuclear Fission" (1994). His studies of early nuclear physics culminated in a brilliant demonstration of how the liquid-drop models as developed in Berlin and Copenhagen influenced the work of Lise Meitner and Otto Frisch and led to their famous formulation of the theory of uranium fission.

Stuewer's scholarship is only one of his important contributions to our discipline. Throughout his lengthy career, he has brought the history of physics to wider audiences and helped practicing physicists contribute to the history of physics in collaboration with historians. Stuewer edited several volumes in the history of science — for example, *Nuclear Physics in Retrospect* (1979), the proceedings of a historical symposium on nuclear physics in the 1930s, which he organized and sponsored at Minnesota in 1977. Among the participants and contributors were Hans Bethe, Otto Frisch, Maurice Goldhaber, Edwin McMillan, Rudolf Peierls, Emilio

Roger Stuewer giving a cross handshake to congratulators Gloria Lubkin and Greg Good

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Segrè, John Wheeler and Eugene Wigner. His model for this gathering became the basis for subsequent symposia and scholarly volumes on the history of particle physics organized by Laurie Brown and others. In 1997 Stuewer and John Rigden founded and began serving as the co-editors of the journal *Physics in Perspective*. Among the most prestigious journals in the history of physics today, it publishes articles by a mixture of physicists, philosophers and historians.

Stuewer has also been highly productive in building social institutions to help physicists and historians work together. For example, he established the Program in History of Science and Technology at the University of Minnesota, which in 2007 merged with its Program in History of Medicine to form the largest such program in the United States. Its success is due in part to Stuewer's insistence that both scientists and historians be included. He served as Director of the Program from 1975 to 1989. Stuewer was also a co-founder of the APS Division of the History of Physics — and its successor, the Forum on the History of Physics — having served on its Organizing Committee in 1979–1980. He has served on the DHP and FHP Executive Committee, and as the Forum Chair and Forum Councilor, representing it on the APS Council. The series of annual Seven Pines Symposia, which Stuewer founded in the mid-1990s, has had a significant impact on the history and philosophy of physics by bringing together prominent physicists and leading historians and philosophers of physics for discussion of key issues in the foundations of modern physics.

We heartily congratulate Roger Stuewer on his receipt of the Pais Prize, one of the highest honors in the history of physics. ■

History *of* Physics NEWSLETTER

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

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

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Fiftieth Anniversary of AIP's Center for History of Physics and Niels Bohr Library & Archives

By Greg Good

In September, 1962, J. Robert Oppenheimer spoke at the dedication of the new "Niels Bohr Library" at the American Institute of Physics in New York City. Oppenheimer, already diagnosed with cancer, was keenly aware of the passing of the generation of physicists that had transformed our understanding of the physical world in the early 20th century. He and the audience knew that it would require a dedicated effort to preserve the history of modern physics and they saw AIP as a viable sponsor of that effort.

AIP's History Programs now include the Niels Bohr Library & Archives and the Center for History of Physics. In September 2012, 150 physicists and supporters attended the fiftieth anniversary and listened to distinguished historians of physics Gerald Holton and Roger Stuewer's stories of how these programs grew from a dream to a reality. Professor Holton was not only present at the beginning of these programs, he helped to guide their development. Professor Stuewer provided advice starting in the 1970s.

Today, AIP's History Programs work closely with APS and AIP's nine other member societies "to preserve and make known the history of modern physics and allied sciences." Both Holton and Stuewer represent this guiding principle of mutual support, since both are now recipients of the Abraham Pais Prize for the history of physics. While Holton received the prize in 2008, it was a special honor to witness Roger Stuewer's surprise when he was announced as the Pais Prize recipient for 2013 at the beginning of the anniversary ceremonies.

We at AIP look forward to many more years of close cooperation with the APS Forum for History of Physics! Much history remains to be documented through oral histories and archival collections. And many stories remain to be researched and written. Our job is far from over. ■

Big, Bigger, Too Big?
From Los Alamos to Fermilab and the SSC

By Lillian Hoddeson, Pais Prize Invited Talk, Atlanta Meeting, April 2, 2012

I am honored to be awarded this year's Pais Prize, and in particular for my work on big labs, which I started more than 35 years ago as I was making a switch from physics to history of physics. I was then taking graduate courses at Princeton in history of physics while teaching physics at Rutgers. To use some solid-state physics I had studied in grad school, I decided to write one of my term papers on basic research at AT&T, a group that eventually turned into Bell Laboratories. This topic pleased my Princeton mentor, Tom Kuhn, because I was able to go on and study how the quantum theory of solids impacted Bell Labs. During this period Charles Weiner and later Spencer Weart, the first and second directors of the AIP Center for History of Physics, taught me how to conduct oral history interviews for use in studying recent physics.

My plan to write a book about Bell Labs never materialized because of two unexpected, wonderful opportunities that arose after I moved to Illinois in 1977, opportunities that led to histories of Los Alamos and Fermilab. It is largely those histories that I'll draw on in this talk, about the road that led from Los Alamos to the SSC via Fermilab. As we know, this road was eventually blocked, but is still worth studying.

Los Alamos. The story of how I came to study Los Alamos had some humorous moments even in my very first visit there, in March 1977. Soon after my arrival, I was contacted by Mike Simmons and Dave Sharp, coheads of the Theoretical Physics Division. They knew I was an historian and interested in laboratories because an article of mine on Bell Labs had just appeared in Physics Today. They had come upon a safe full of interesting wartime documents which they had been ordered to send to the National Archives. At that time Los Alamos had a records center but no archives. They were reluctant to send the safe to Washington because they had heard that this would be like sending the papers into a black hole.

I was happy to try to help, but I had no clearance. I was escorted to the safe, and had to turn my back while David Campbell, Gordon Baym, and Mike Simmons examined the documents. I listened with great interest to their comments: "Wow, here's a handwritten calculation by Bethe!" "Here's Feynman's notebook!" "This letter addressed to Henry Farmer [Fermi's code-name] starts out 'Dear Enrico!'." My favorite overheard comment was "My God, Gamov's gin bottle!" (Gamov was known to keep that cherished object safe in the safe.) All this led eventually to the Los Alamos archives and to its history project. The first outcome of that was the technical history, Critical Assembly, which I coauthored with archivist Roger Meade and two history graduate students, Paul Henrickson and Catherine Westfall. At first Los Alamos was not sure it wanted to fund the project. The director of the laboratory, Harold Agnew, is reputed to have told Simmons, "Los Alamos needs a history project about as much as it needs topless waitresses in the South Mesa Cafeteria."

This remark is difficult to understand. Project Y, the Los Alamos lab's original name, was a very well-funded, large military research facility run jointly by Robert Oppenheimer and General Leslie R. Groves. Its goal was to build uranium and plutonium bombs in time for possible use in World War II by the summer of 1945. Most people at Project Y had a security clearance and worked "behind the fence," but Oppenheimer nevertheless shaped the lab into a research facility, with theory and experimental divisions, study groups, and seminars, because he understood that fundamental research, and especially physics, was essential to solving the technical problems. That is why people like Bethe, Feynman, Rudolf Peierls, Edward Teller, John von Neumann, Luis Alvarez, Bob Bacher, Bob Wilson, Ed McMillan, Niels Bohr, Enrico Fermi, Normal Ramsey, George Kistiakowsky, and many other leading scientists were needed there.

The original program soon grew into a much larger effort in response to a physics discovery made in April 1944 by three of Emilio Segré's graduate students. Working in a secluded New Mexico canyon, they found that in reactor-made plutonium there is a small but significant amount of naturally occurring spontaneous fission, a process emitting neutrons. As the spontaneous fission rate was five times that of the cyclotron-produced samples used until then, attempting to assemble a plutonium weapon using gun assembly—which is a slow process in comparison with the speed of a nuclear explosion—was far too risky. The extra neutrons could set off the explosion too early, causing a ``fizzle." An alternative method was needed to assemble a plutonium weapon. Uranium could be, and was, assembled by the gun method; for plutonium, the only conceivable possibility was implosion, a far more rapid assembly. But it was a stretch to make implosion assembly work because the physics was poorly understood.

Groves didn't want to waste the huge investment already made in plutonium production, so he simply ordered Oppenheimer to make an implosion bomb by summer 1945. This required a total reorganization and expansion

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Manhattan Project Mystery

By Cameron Reed, Department of Physics, Alma College, Alma, Michigan 48801 USA

For several years I have been researching the history and physics of the Manhattan Project, the United States' World War II-era effort to develop nuclear weapons. Copies of thousands of original Project documents are now readily available through the National Archives and Records Administration (NARA); users can request either microfilmed copies of sets of documents or pdf images recorded on a DVD. It can be fascinating to troll through these sources; on many occasions I have found myself reflecting on the idea that a document was at one time in the hands of President Roosevelt, Robert Oppenheimer, General Leslie Groves or some other Project leader. Sometimes one comes across a document that raises a minor historical mystery. My purpose in this brief article is to describe such a case.

On page 388 of his history of the Manhattan Project *The Making of the Atomic Bomb*, author Richard Rhodes reproduces a partially-dated handwritten note from President Franklin Roosevelt to Vannevar Bush that reads: "V.B. OK – returned – I think you had best keep this in your own safe FDR" (Figure 1). Bush was the Director of the wartime Office of Scientific Research and Development (OSRD), within which lay responsibility for investigating possible military applications of nuclear fission. Rhodes interprets the date of the note as January 19, 1942 and describes its context as the President returning to Bush a copy of a November 6, 1941 report on the possibilities of explosive fission with uranium-235. The report had been prepared by a committee chaired by University of Chicago physicist Arthur Compton and laid out in detail the physics of a fission bomb as well as estimates of the destructive action of such a weapon and the feasibility of various isotope separation methods. Bush discussed the report personally with FDR on Thursday, November 27 of that year – just about the time a Japanese task force was setting sail on its mission to attack Pearl Harbor. In their official history of the United States Atomic Energy Commission, historians Richard Hewlett and Oscar Anderson attribute the note to the same date and circumstance (their p. 49); their work is probably the source of Rhodes's later attribution.

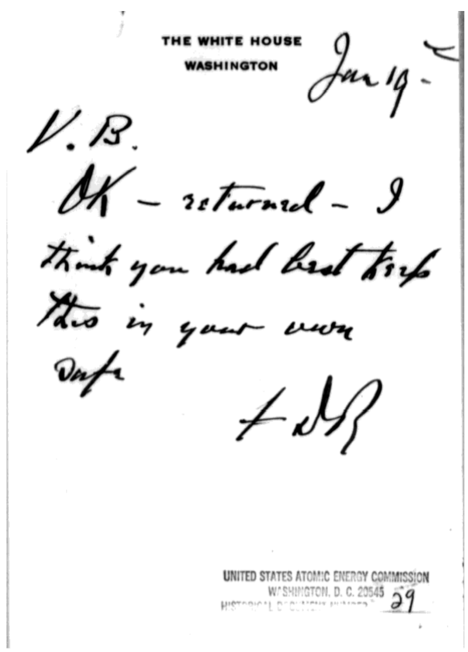
THE WHITE HOUSE
WASHINGTON
Jan 19 –
V.B.
OK – returned – I think you had best keep this in your own safe
FDR
UNITED STATES ATOMIC ENERGY COMMISSION
WASHINGTON, D. C. 20545
29

Figure 1. January 19 or June 19?

While Roosevelt's note was not an official "go-ahead" for an American atomic-bomb project, its brevity and the authority conveyed by the scrawled initials give it a compelling sense of drama. Indeed, Hewlett and Anderson argue that the fundamental decision to proceed had occurred on October 9, 1941 when Bush had met with Roosevelt and Vice-President Henry Wallace to discuss a British report on the possibility of fission bombs as well as the need for post-war control of nuclear energy. At the October 9 meeting Roosevelt directed that discussion of policy issues was to be restricted to a group comprising himself, Bush, Wallace, Secretary of War Henry Stimson, Army Chief of Staff General George C. Marshall and James B. Conant, Bush's deputy at the OSRD. This group came to be known as the "Top Policy Group."

I suggest here, however, a strong possibility for a different date for Roosevelt's note. The note appears as image 0945.jpg of Reel 1 of NARA microfilm set M1392 ("Bush-Conant File Relating to the Development of the Atomic Bomb, 1940-1945"); it is one of a number of documents concerning Bush's reports to and conferences with the President. Curiously, the immediately preceding image, 0944.jpg, is a copy of a letter of June 17, 1942 from Bush to FDR which was the cover letter for a June 13 report on the subject of "Atomic Fission Bombs" which had been endorsed by the Top Policy Group. This report lays out an ambitious $85 million plan for construction of an isotope-enrichment centrifuge plant, a pilot-scale gaseous diffusion plant, an electromagnetic plant (both for separating isotopes), a heavy water plant, an "atomic power installation" (reactor), and continued fundamental-physics research. Bush's cover letter is also marked with "V.B. OK FDR" (Figure 2).

It seems that the month written at the top of FDR's note could be read as either "Jan" or "Jun", and so it might actually refer to the June 13, 1942 report instead of to Compton's November 1941 report. Indeed, in their discussion of the June 13 report Hewlett and Anderson write (their p. 75): "On June 19, with the Presidential approval in hand, Bush had authorized … ." A Presidential response within two days may seem speedy, but was by no means unprecedented. For example, on March 9, 1942 Bush had sent FDR an extensive update on the status of the project; the record contains a typewritten note clearly dated March 11, 1942 which is signed by Roosevelt and which acknowledges return of the report to Bush.

In fairness, a number of counterarguments to this speculation can be posed. While the "a" (or "u") in the month is not closed (which might argue for "Jun"), neither is the "a" in the word "safe". If Roosevelt annotated the June 17 letter why would he have felt compelled to send a separate note two days later? A copy of Compton's November 1941 report also appears in the record

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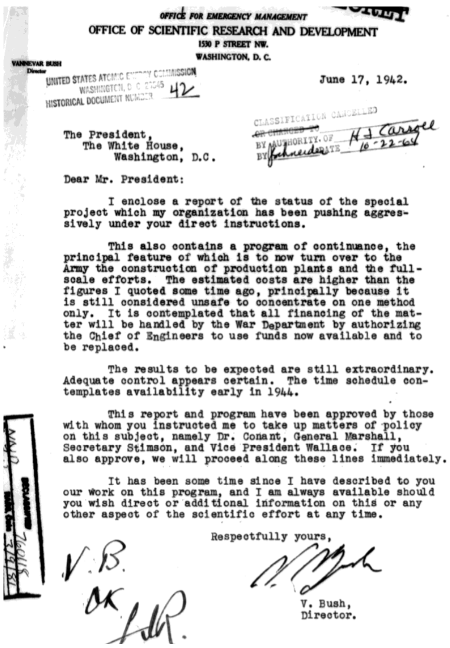

OFFICE FOR EMERGENCY MANAGEMENT
OFFICE OF SCIENTIFIC RESEARCH AND DEVELOPMENT
1530 P STREET NW.
WASHINGTON, D. C.

VANNEVAR BUSH
Director

HISTORICAL DOCUMENT NUMBER 42

June 17, 1942.

CLASSIFICATION CANCELLED
BY AUTHORITY OF H J Carroll 10-22-64

The President,
The White House,
Washington, D.C.

Dear Mr. President:

I enclose a report of the status of the special project which my organization has been pushing aggressively under your direct instructions.

This also contains a program of continuance, the principal feature of which is to now turn over to the Army the construction of production plants and the full-scale efforts. The estimated costs are higher than the figures I quoted some time ago, principally because it is still considered unsafe to concentrate on one method only. It is contemplated that all financing of the matter will be handled by the War Department by authorizing the Chief of Engineers to use funds now available and to be replaced.

The results to be expected are still extraordinary. Adequate control appears certain. The time schedule contemplates availability early in 1944.

This report and program have been approved by those with whom you instructed me to take up matters of policy on this subject, namely Dr. Conant, General Marshall, Secretary Stimson, and Vice President Wallace. If you also approve, we will proceed along these lines immediately.

It has been some time since I have described to you our work on this program, and I am always available should you wish direct or additional information on this or any other aspect of the scientific effort at any time.

Respectfully yours,

V. Bush,
Director.

V.B.
OK
FDR.

Figure 2. Letter from Vannevar Bush to President Roosevelt, June 17, 1942.

but it bears no Presidential annotation, which could suggest the need for a separate acknowledgement. While the proximity of FDR's note to Bush's June 17 letter on the DVD supplied to this author is suggestive it is by no means a conclusive piece of evidence. My experience is that documents in these records are often very chronologically scattered; I often have to resort to printing them out and rearranging them in order to get a coherent picture of some issue. If the note does refer to returning Compton's November report, January 19 would represent a lapse of some seven weeks between the Bush/FDR meeting and the return. But such a delay may not have been unreasonable in the hectic days and weeks following Pearl Harbor. Roosevelt may also have been further delayed because he was hosting Winston Churchill for the First Washington Conference, which ran from December 22, 1941 to January 14, 1942.

It seems that convincing arguments can be mounted for either a January 19 or June 19 interpretation. But establishing the exact date will admittedly make little difference to historical analyses of the Manhattan Project: work on the possibility of fission-powered weapons was underway well before the beginning of 1942. For this writer this historical footnote reminded him of advice he received many decades ago from an eighth-grade teacher: "When you write something, date it." To which I would add: "And do so clearly and completely."

References

Richard Rhodes, The Making of the Atomic Bomb (Simon and Schuster, New York, 1986). Touchstone paperback edition, 1988, p. 388.

Reed, B. C. "Arthur Compton's 1941 Report on explosive fission of U-235: A look at the physics," Amer. J. Phys. 75(12), 1065-1072 (2007).

Richard G. Hewlett and Oscar E. Anderson, Jr. the New World 1939/1946. Volume I of a History of the United States Atomic Energy Commission (University Park, PA: The Pennsylvania State University Press, 1962), p. 49. ■

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of the laboratory in the summer of 1944—just one year before the deadline. Oppenheimer added, among other things, a new explosives research division under Kistiakowsky, and a new implosion research division under Bacher. Thus what started as a small, back-burner implosion program grew into a model ``big science'' program. But the program was very different from existing physics labs, and not only because of the secrecy. First, its work required scientists to collaborate with military and engineering people. Second, it had access to effectively unlimited funding. Third, it had an extremely tight military deadline. Not science as usual! The project had to move faster, and its products, the weapons, had to work reliably. The result was a conservative and redundant research strategy aimed at avoiding risk. Both bombs—and especially the implosion bomb—turned out to be overdesigned clunkers. The strategy paid off, but the special conditions that nurtured the new approach could continue only under the unique wartime pressures.

Fermilab My work on Fermilab also started out in an unusual, somewhat humorous way. Bob Wilson had included an archives area in the blueprints for his distinctive new Hi-Rise. His archives committee contacted Joan Warnow, the archivist at the AIP Niels Bohr Library and Center for the History of Physics, who told the committee's chairman Dick Carrigan that I had just moved to Illinois and might be available. I hadn't expected to work on history of high-energy physics—and certainly not as an archivist, for which I had no training—but I was interested and agreed to help on a part-time basis.

Not long after, Bob Wilson came by and asked how I was doing. He was unimpressed when I told him what I'd learned about computerizing collections and protecting documents. When I pulled out the new fireproof boxes I had bought, he struck a match and set one on fire, causing huge flames in the middle of the history room, which fortunately died down quickly. Wilson said, "Well good. They work!" and walked out. It took me a while to realize that what Bob really wanted was a history project, with an archives and a reading room to offer culture to his staff. That suited me fine. I was delighted and relieved when in 1983, Leon Lederman, Fermilab's second director, hired Adrienne Kolb to handle the archives. She and I have worked together ever since. Later, Catherine Westfall joined us in studying Fermilab's early history, and we all eventually collaborated on *Fermilab: Physics, the Frontier, and Megascience.*

Many ties connect Fermilab to both Lawrence's Lab and Los Alamos. For instance, Wilson had been one of "Lawrence's boys" when he was in grad school, and in 1967 when the 200 BeV project came to Illinois, it came from

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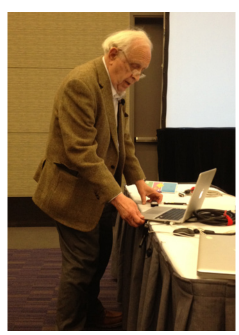
March 2012 FHP sessions at the APS meeting in Boston

Peter Pesic, Co-Chair, FHP Program Committee 2011-2012

2012 marked two anniversaries that the Forum on History of Physics wanted to observe through sessions at the APS March meeting in Boston. In the period 1861-63, James Clerk Maxwell assembled and published his version of what we still call his equations, so fundamental to physics even one hundred fifty years later that this seemed an apt occasion to look back to this seminal passage in physics. Paul Cadden-Zimansky, a member of our Executive Committee representing students and early-career physicists who was then a postdoc at Columbia University (and now has accepted a post at Bard College), gave the original impetus to this observance by reminding us that 1862/2012 marked the anniversary of Maxwell's essay "On Physical Lines of Force," which set out in clear, uncomplicated prose his understanding of how Michael Faraday's work on electrodynamics marked out a path to a new mathematical and physical understanding of those phenomena in their wholeness.

Our Forum uniquely bridges the concerns and perspectives of active physicists, of students, of historians of physics, and all interested in the development of science. To do justice to this rich variety of audiences and interests, our session on February 27, 2012 entitled "One Hundred Fifty Years of Maxwell's Equations," chaired by Edward Gerjuoy (University of Pittsburgh), enlisted five distinguished speakers to approach Maxwell's achievement in different ways, to show how Maxwell discovered his equations, how they affected the physics of his time and our own, including current perspectives on this seminal discovery.

The first three speakers addressed him primarily in the context of his own time and its immediate aftermath. C. W. F. Everitt (Stanford University) began by presenting his account of "The discovery of Maxwell's equations." Everitt, author of a wonderful short biography of Maxwell, discussed the blend of physical modeling and mathematical innovation that enabled Maxwell to write his equations. Presenting many unfamiliar and interesting images from Maxwell's time, Everitt also surprised us by noting that Leonhard Euler had earlier put forward a concept of lines of force, usually attributed to Faraday and Maxwell. Bruce Hunt (Univ. of Texas) addressed "The Maxwellians and the Remaking of Maxwell's Equations," bringing forward a rich variety of material concerning the rethinking of Maxwell's work in the generations immediately following him, drawing on Hunt's outstanding book on this period and new work he has in progress, especially concerning the role of the transatlantic telegraph cables in the development of electrodynamics. Jed Buchwald (Caltech) brought the story still further forward in his account of "Using Maxwell's equations in the late 1800s," the period in which the equations more and more closely approached the form now most familiar to physicists. Buchwald's amazing command of the intricate technicalities revealed to the audience the full sophistication and achievement of late nineteenth century electrodynamics, in the hands of physicists like Oliver Heaviside. His exposition, in concert with those by Everitt and Hunt, helped us see the strata of inference and changing perspectives that lie under the textbook treatments of Maxwell's equations, so often presented as if they came from nowhere.



The final two speakers looked back at Maxwell's equations from present-day physics and its concerns. Roy Glauber (Harvard) gave an overview of "Maxwell's equations and quantum optics," a daunting task, given the vast scope and manifold ramifications of quantum electrodynamics, its practical and theoretical consequences, throughout the twentieth century, up to the present day. Glauber's deep familiarity and long involvement in these developments, especially in connection with the optical model for which he was awarded a Nobel Prize, gave his presentation special interest. Finally, Frank Wilczek (MIT) talked about "Taking off from Maxwell's equations," especially the development of gauge field theories that ultimately stem from Maxwell's discoveries. Wilczek, a Nobel laureate for his work on quark confinement, gave a dynamic overview of the development of gauge theories into the Standard Model as it stands today, harking back at many points to Hermann Weyl and ultimately to Maxwell as its founders and pioneers. Wilczek's talk was filled with nice explanatory touches and moments of new insight; he illustrated that vacuum polarization provides a natural scale of a new sort, using amazing animations produced by computer calculations of quantum chromodynamics. The large audience present seemed deeply interested by this series of talks.

2012 also marks the centenary of the birth of Edward Purcell, whose seminal work brought nuclear magnetic resonance (NMR) into the world and transformed radio astronomy, among other signal accomplishments as teacher and researcher. Our session on "The Scientific Legacy of Edward Purcell (1912-2012)," held on leap year day (February 29, 2012), was in itself a rare and special event. Chaired by Gerald Holton (Harvard), on this unique occasion several of Purcell's closest collaborators remembered his work and assessed its enduring significance.

Nicolaas Bloembergen (Univ. of Arizona), one of Purcell's first graduate students and himself a Nobel laureate for his work stemming from NMR, gave a unique and touching account of "Purcell and NMR." Bloembergen arrived at Harvard just after Purcell's experiments that first demonstrated NMR; he was able to give many insights into that discovery and its immediate consequences in his own work. We learned how Purcell was able to make great discoveries

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on a shoestring, using borrowed equipment on weekends. This was especially apparent in the contribution by Harold I. Ewen (EK Associates) on "Purcell and the development of radioastronomy." "Doc Ewen" (as he became known), as Purcell's graduate student, received the challenge to detect the hyperfine transition of hydrogen in interstellar space. Unfortunately, personal reasons prevented Doc from attending and speaking, but his talk as read by Prof. Holton (who had been a much-admired teacher of his at Harvard) still brought forward the flavor of those times, when an "impossible" task (as some thought) came to pass. Doc Ewen showed the full effect of Purcell's constant insight and generous advice (and his finding a $500 grant at a crucial moment, when sums like that could make or break a project). As several people observed, modern radio astronomy really began with this discovery, which almost immediately enabled the first mapping of the Milky Way galaxy (and which some thought was worthy of a Nobel prize of its own).

Howard Berg (Harvard) gave a wonderful talk "On small things in water moving around: Purcell's contributions to biology," with which Berg had been long involved. Berg's engagingly informal and perceptive account described his own transition from medical school to graduate study in physics and thence to work in biological physics, to which Purcell signally contributed. Berg brought forward many examples, including Purcell's lovely work on "life at low Reynolds number," the problem of bacterial locomotion and swimming. From his long and extraordinary perspective in this field, Richard Garwin (IBM Watson Research Center) gave a detailed account of "Purcell's work advising the government," opening a striking perspective on the changing role of scientists advising the government and Purcell's own crucial role therein. Garwin was able to use a number of recently declassified documents to describe for the first time a number of the projects on which Purcell had worked, especially concerning high-altitude reconnaissance.

Finally, but by no means last in significance, John Rigden (Washington Univ.) gave an eloquent portrayal of "Purcell the Teacher: In and Out of the Classroom." As editor of the *American Journal of Physics*, Rigden had worked closely with Purcell on his long-standing series of columns, "The Back of the Envelope," which posed and (in later issues) solved intriguing physics problems using simple methods of estimation. Speaking with passion and armed with many examples, Rigden reminded us that, above all, Purcell considered himself a *teacher*. Coming from someone so celebrated a researcher, this avowal should remind all of us of the centrality of teaching in physics, considered in its broadest sense of thoughtful questioning, learning, and discussing. If Purcell was such a learner, we should try our best to do likewise.

The large audience present seemed appropriately moved to have participated at a unique gathering of very special speakers, an occasion which probably will never happen again on this earth. Such opportunities to hear about the history of physics from some of its most important protagonists are history itself. ■

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Berkeley. At Los Alamos, Wilson headed the cyclotron group and later the experimental physics division. The network became most clear to me when I interviewed Priscilla Duffield, Lawrence's secretary at LBL. She later worked in a similar capacity for Oppenheimer at Los Alamos, and then for Wilson at the National Accelerator Lab, NAL, later named Fermilab. Before Duffield, Wilson's first assistant was Rose Bethe, who had helped Oppenheimer set up the Los Alamos housing office. In one interview, Wilson told me that in creating NAL he tried to recreate a science city reminiscent of Los Alamos.

Certain approaches in building Fermilab's accelerators and other apparatus resemble those used at Los Alamos, in that they often mixed engineering and scientific approaches—but unlike Los Alamos, it had a lot to do with the fact that funds, especially in the 1970s, were limited at Fermilab. But for Wilson, frugality was not just a response to limited funding but a matter of aesthetics. He liked to design minimally, taking measured risks and generally working with the least possible amount of money. He famously wrote: "Something that works right away is over-designed and consequently will have taken too long to build and will have cost too much." Subsequently, many experimentalists at Fermilab suffered because of Wilson's underdesigned Main Ring and inadequate experimental areas. But at least they had an accelerator to work with, with which they eventually discovered the bottom quark.

One of the best examples of the mixing of experimental and scientific approaches at Fermilab was in developing the pioneering superconducting accelerator magnets for Wilson's Energy Doubler, designed to double the energy of the Main Ring. It was a good idea, but the early magnets did not work well. Alvin Tollestrup succeeded in making working Doubler magnets using a brute force approach remarkably similar to that used at Los Alamos in its implosion development, building over a hundred prototypes and changing just one attribute from magnet to magnet. The Doubler was completed under Lederman, and became the basis of the Tevatron. With it Fermilab moved on to much bigger experiments, entering the regime we called "megascience," characterized by long-lasting "strings" of experiments and their follow-ups. In a limited funding context, these strings led to a general reduction in the number of problems being studied, a trend that continued for some time. The research yielded the 1995 co-discovery of the top quark, at the CDF and DZero detectors.

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The SSC. I want to preface my story about the SSC with a quote from George Eliot's *Middlemarch:* "In all failures, the beginning is certainly half the whole." In 1983, with encouragement from George Keyworth, President Reagan's Science Advisor, American high-energy physicists were inspired to "think big" and build the ambitious 40 TeV collider as an American project rather than as an international project as originally conceived. They were led to believe there would be "new money" for it beyond the base program. Such thinking resulted in the 1983 endorsement of the SSC by Stan Wojcicki's Wood's Hole HEPAP subpanel.

The two initial phases of the SSC, between 1983 and 1988—a feasibility workshop called the Reference Designs Study followed by a design workshop called the Central Design Group, or CDG—were directed by Maury Tigner, then widely considered to be the strongest in the new generation of accelerator builders. As these phases took place in Berkeley and as the SSC was then expected, at least by physicists at Fermilab, to end up at Fermilab, it appeared to some that history was repeating itself, especially as the URA, the consortium of research universities managing Fermilab, was also managing CDG. Frank Cole, head of Fermilab's library committee, had been involved in Berkeley's 200 BeV design study in the early 1960s and remembered the traumatic moment when Berkeley physicists learned that the machine would be in the Midwest, instead of California. Sensitive to the formal analogy between the SSC and Fermilab stories, Cole suggested in 1985 that Adrienne start collecting documents and prepare an evolving chronology about the evolving SSC for an eventual history. I subsequently joined the history part of her effort. It led to a proposal to DOE, also coauthored with Peter Galison, to fund an SSC history project. But while Alvin Trivelpiece, the Director of the Office of Energy Research at DOE had informally endorsed our idea of a history project, our proposal received no response. We didn't know yet that Trivelpiece, about whom I'll say more in a moment, was just then in the process of leaving DOE. Adrienne and I continued working, and in 1994, not long after the cancellation of the SSC, Michael Riordan asked to join with us. After succeeding in getting a substantial NSF grant in 1995 for a four-year (eventually five-year) project to write a history of the SSC, we interviewed many of the people involved in the history, collected huge numbers of documents (saving the entire archival collection of the discontinued SSC), and began writing draft chapters. But it was a much bigger story than we had realized, and finishing the research required a later NSF grant in 2008.

To get back to the CDG: like his Cornell mentor Robert Wilson, believed in building imaginative and frugally-designed accelerators. A crucial feature of CDG's design was the boldly small 4 cm aperture of its magnets, designed to achieve 6.5 Tesla. With an accelerator ring 52 miles in circumference, the CDG-designed machine was to achieve 20 TeV in each proton beam at the cost of roughly $3 billion. CDG's frugal Conceptual Design passed its intensive Temple review on April 1, 1986, even though some worried that a 4 cm aperture might not offer adequate field quality.

By this time, however, a number of clouds hung over the SSC's horizon. First, during 1985-6 the SSC's Washington base of supporters dwindled. Keyworth, Donald Hodel, and Jim Leiss left their posts and were replaced by people far less friendly to the SSC. Wilmot Hess, in particular, who succeeded Leiss, did not work well with Tigner. Second, the funding climate worsened and hope for new money faded. Many worried that the "Gramm-Rudman-Hollings ax" might fall on their work; furthermore, Presidential approval, which was required for the SSC given its multibillion-dollar construction budget, was not yet assured. President Reagan approved the SSC only in January 1987, citing the maxim of Kenny Stabler, the Oakland Raiders' quarterback: 'Throw deep!' Trivelpiece, who received this idiosyncratic endorsement, was by then the SSC's only remaining high-level advocate in Washington.

A third big cloud was technical, involving shorts and quenches plaguing the development of the SSC's superconducting magnets. After the retirement of Victor Karpenko, a former Livermore engineer who was one of the directors of the CDG magnet program, John Peoples of Fermilab was called in to help coordinate magnet development, then carried out by an awkward collaboration of groups at Brookhaven, LBL, and Fermilab. Peoples recalled, "I was a little bit like the plumber who'd been called in to fix the leaks and the toilets that are overflowing during a dinner party, but I wasn't exactly invited to dinner." Peoples also noticed what he called a "philosophical problem," a misalignment between the research practices of physicists and the military engineering types who responded to orders, unlike physicists who had to be seduced to work in collaborations. At Los Alamos, social misalignments of this kind were easily overcome because everyone shared a common urgent national goal, but at the SSC that was not the case. At the same time, Tigner was constrained in his leadership because he lacked control of the purse strings in paying the different laboratories for their work, which made it hard to efficiently "harness their abilities and enthusiasms," as he once told me.

The clearest sign that the SSC would not follow the road from Los Alamos via Fermilab came early in August 1988, when the DOE issued its official Request for Proposals (RFP) to manage and operate the SSC. More than a year earlier, CDG and URA had sensed, but not understood, a strangeness in the DOE's attitude toward the SSC. CDG had submitted an unsolicited management proposal that received no response. Written by Ned Goldwasser, this earlier proposal argued for sole-sourcing the SSC's management to URA. Meanwhile, in April 1987, Trivelpiece left the DOE. In retrospect, we can view this response to the unsolicited proposal as an indication that the SSC would not be treated in the same way as prior high-energy physics projects had been treated. But at the time this sign was not correctly interpreted by the physicists. And there was no Leslie Groves to explain it to an Oppenheimer-like leader. As Ed Knapp, the new head of URA, saw it, DOE did not seem to trust physicists to manage a project as big as the SSC. Jack Marburger later commented that any proposal with a multibillion-dollar budget was crossing an invisible line at the DOE beyond which a more elaborate funding and management process was required. If so, the physicists' hope to manage the SSC was doomed from the start.

Knapp read the DOE's RFP as a message that responses were expected to take the form of a DoD-style proposal. To write it, he hired Douglas Pewitt, then working for the Washington-based firm of SAIC. In the proposal, Pewitt included a detailed project management plan calling for teaming between physicists and industrial firms. URA then selected EG&G, Inc. and Sverdrup Corp. CDG was excluded from the proposal writing, to avoid what SLAC director Pief Panofsky saw as a potentially disqualifying conflict in which URA might appear to be taking advantage of its close relationship. This exclusion caused mistrust and resentment, especially as many others in the high-energy community were consulted. Among the off-limit topics was selection of the SSC Director. The CDG physicists considered Tigner the perfect leader, but others worried that he might project the wrong image in Washington. When on August 28, 1988, a selection committee chose Roy Schwitters as the director to be named in the URA proposal, with Tigner listed as Schwitters' Deputy Director, it caused enormous resentment at CDG. And when Waxahachie, Texas was announced as the SSC's home, on November 10, 1988, few members of the CDG were asked to go to Texas, and even fewer elected to go there. Tigner ultimately withdrew from the project when it became clear over the next few months that he and Schwitters had too many differences to be able to work together. In losing Tigner and the CDG, the SSC, and physics more generally, suffered a traumatic loss of continuity and institutional memory.

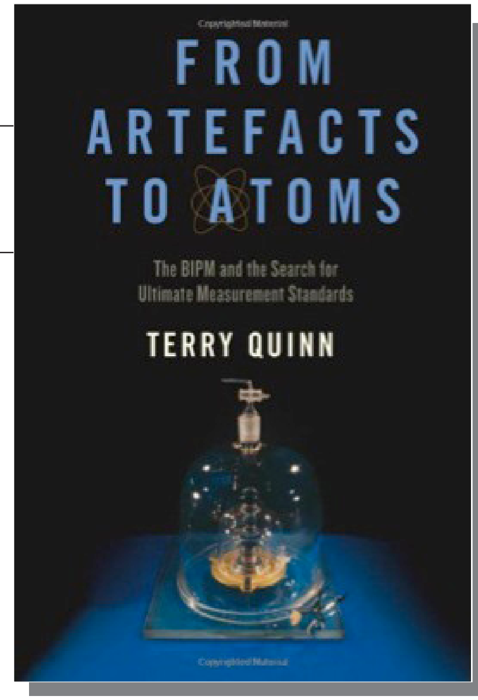
Meanwhile, the URA's response to the RFP had become a six-inch-thick, three volume document full of technical and practical details. The character of this proposal was fundamentally different from URA's elegant unsolicited proposal of 1987. Knapp later said of URA's response to the RFP, submitted on November 4, 1988, that "bureaucratically it was gorgeous." As DOE received no other proposals, a contract with URA was drafted. With its multibillion budget the SSC takes our story of big labs back to a pattern resembling Los Alamos, but without unlimited funding and without a compelling military mission.

By this time, shrewd observers whose careers were not tied to the SSC could read the future of the SSC in the tea leaves. In late October 1988, Fermilab magnet guru Dick Lundy offered three predictions to his friend Drasko Jovanovic, who was keeping an event logbook: first, that the site for the SSC would be in Texas; second, that URA would be the SSC's M&O contractor; and third, that "the project would fold in equal to or less than five years." Lundy recognized that the SSC, because of its high cost, had moved into a funding category that demanded a project management framework more like Los Alamos but without a wartime context to justify such a framework.

In any case, the SSC experienced continuing culture clashes, not only between physicists and military engineers, but also with the increasingly bureaucratic DOE. The cost of the SSC continued to grow, due in part to more bureaucracy and to design changes that added more conservative and expensive features, like a 5 cm. magnet aperture. But unlike Los Alamos, where cost increases aimed at reducing risk were no issue, Congress saw the SSC's cost increases as the result of poor management. The project was subjected to increasingly uncomfortable public and Congressional scrutiny, while the DOE's management procedures led to the alienation and withdrawal of many of the SSC's most creative scientists. Exacerbated by criticism from scientists in other fields, who feared that the SSC would cut into their fields' funding, and who pressed for its cancellation, and by being in a post-Cold War climate in which physicists had lost much of their earlier cultural prestige, the project failed to gain international support. All of these factors made the SSC crucially different from its predecessors and sealed its doom, closing the road from Los Alamos to the SSC via Fermilab. For American particle physics the death of the SSC was a tragedy that meant years of lost time, money, effort, and emotion. There were few gains for high-energy physics. One possible one is that after the SSC died, high-energy physics research seems to have become a bit more diversified, not nearly as focused as it had been on the single problem of the Higgs particle, which Lederman had originally called the "goddamn particle," because of its elusiveness, before his publishers changed it to the more charismatic "God particle." It may be that Lederman was right the first time. ■

New Books *Of* Note

From Artefacts to Atoms: The BIPM and the Search for Ultimate Measurement Standards



***By Terry Quinn** | Oxford: Oxford University Press, 2012, 440 pp., $110 (hardback)*

Reviewed by Joseph D. Martin

Terry Quinn wants to redefine the kilogram. This motive drives *From Artefacts to Atoms: The BIPM and the Search for Ultimate Measurement Standards,* his history of the International Bureau of Weights and Measures (BIPM).[1] The title hints at the key question Quinn—BIPM director between 1988 and 2003—raises: what is the significance of transitioning from measurement conventions based on object prototypes to standards tied to fundamental physical constants? The kilogram is the last remaining metrological standard still defined by a physical artifact: the International Prototype Kilogram (IPK), a platinum-iridium cylinder envaulted at the BIPM since 1889. Plans currently underway would replace the IPK with a definition in terms of Planck's constant. Through the history of the BIPM, Quinn, an enthusiastic supporter of these plans, describes an inexorable progression from object standards to absolute standards.

The book's eighteen chapters cleave roughly into thirds. The first six chronicle, in extraordinary detail, the quarter century culminating in the 20 May 1875 signing of the Metre Convention. Quinn describes competing standards in nineteenth century Europe, indicating how the commercial and political forces of an increasingly interconnected world shaped them. The Convention cemented the metric system as the standard for scientific measurement. It also provided for the creation of an institute—the BIPM—to maintain the standard, and a governing committee to oversee the Convention's implementation while navigating the geopolitical squalls that accompanied attempts to forge international consensus around a single system.

The next third recounts, in similar detail, the first half-century of the BIPM's operation. Quinn gives special attention to the creation of the prototype meters and kilograms, the linchpins of the metric system. The task of manufacturing prototypes reliable enough to sustain international confidence tested the mettle of Bureau scientists. The meter, for example, required identifying an alloy with an appropriately low coefficient of thermal expansion, casting that alloy with adequate consistency, and developing exquisitely accurate procedures for testing new prototypes against preexisting standards, among other challenges. Quinn also describes how the BIPM's role expanded in the early twentieth century. The Bureau's mission, originally restricted to maintaining the metric standards, was broadened to encompass all of metrology in 1921. The BIPM had become the world's premier site for high-precision instrument calibration, a status it attained on the power of experimental acumen honed making ever-finer measurements of length and weight standards. The Bureau's position as the source of international confidence in the accuracy of scientific measurement made it the natural institution to do for electrical, thermal, and other quantities what it had already accomplished for length and weight.

The BIPM's expanded purview paved the way for the International System of Units (SI). The push for a standard system of physical quantities begins the final third of the book, in which Quinn describes the transition from artifactual to physical quantities as measurement standards. In 1960 the SI was formally adopted and the first artifact standard became obsolete when the meter was redefined as a multiple of the wavelength of light emitted by krypton 86 during the transition between its $2p_{10}$ and $5d_5$ orbitals. This landmark, for Quinn, was the first step in a process that will likely culminate in a few years with the adoption of the "new" SI and the reclassification of the IPK as a historical object. Quinn closes with a clear synopsis of what is at stake in the debate over whether—or, more realistically, when—to redefine the kilogram. Does the aesthetic allure of a crisp theoretical system outweigh the practical difficulty of measuring new standards to the same accuracy as otherwise antiquated artifacts?

As it weaves its way through the 137 years since the Metre Convention, *From Artefacts to Atoms* is alternately a disciplinary history of metrology, an institutional history of the BIPM, and a socio-political history of measurement conventions. Readers might find the abrupt transitions between these threads disorienting, compounding the problem that an overabundance of minutia often obscures the larger narrative arc. The net result is twofold. First, the book's primary argument for redefining the kilogram does not emerge as cleanly from the historical exposition as the author might like. Second, these parallel stories only hint at the range of fascinating questions metrology poses, each one of which might sustain a book-length narrative. How does metrology interface with other scientific disciplines? How do the details of laboratory practice ground confidence in measurements? How does the social contract on which measurement conventions are built change when supported by fundamental physical constants rather than objects? This book, ultimately, does not address these questions head on. By suggesting them, however, it indicates the notable role metrology and its flagship institution played in the course of modern science. ■

[1]BIPM is the French initialism of Bureau International des Poids et Mesures.

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