History of Physics

NEWSLETTER

A FORUM OF THE AMERICAN PHYSICAL SOCIETY • VOLUME X • NO. 5 • FALL 2008

Report from the Chair:

Using History of Physics in Education

By David C. Cassidy, Forum Chair

Twenty-five years ago, in April 1983, the U.S. Department of Education published a landmark report titled "A Nation at Risk." The report sent shock waves rippling through American society, painting a bleak picture of American education and warning of dire consequences for the nation's competitiveness. "Our concern goes well beyond matters such as industry and commerce," the report went on to say. "It also includes the intellectual, moral, and spiritual strengths of our people which knit together the very fabric of our society."

Many of the report's assessments and warnings remain valid and are not limited to the United States. They spawned a number of education reform efforts, especially in science, that continue to this day. Although American students and even well-educated adults still display a disturbing lack of scientific literacy, gradual improvement has resulted. One of the most important and influential factors in this reform has been not a particular initiative but an educational approach. This approach originated most directly from one of the leading members of the commission that produced "A Nation at Risk"—Gerald Holton. In a fitting coincidence, almost exactly on the 25th anniversary of this report, Holton received the 2008 Abraham Pais Prize for the History of Physics during the April 2008 APS meeting. As noted in the citation, "His writing, lecturing, and leadership of major educational projects introduced history of physics to a mass audience."

We are delighted to be able to publish in this newsletter Holton's elegant and insightful Pais Prize address, "Of What Use is the History of Science?" (see p. 5). Without giving too much away, I would like to underscore what has been a hallmark of his reform approach, as presented in this address. He argues that one of the most important uses of the history of physics is in physics education, by providing students with a unifying perspective on the discipline and a sense of the grand adventure—the human drama of

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General Leslie R. Groves and J. Robert Oppenheimer, leaders of the Manhattan Project (see pages 9, 14–15).

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struggle and success—that has brought us to our current understanding of nature. Without distorting or oversimplifying the past, history can lend an excitement and a purpose to the study of physics, while uniting into a meaningful whole the isolated technical details of problem-solving, individual principles and diverse topics.

Together with other ongoing physics education initiatives, the historical approach pioneered by Holton for half a century has proved enormously influential and successful in such efforts as the famous *Project Physics Course* as well as in nearly every recent standards initiative in science education, such as those by the American Association for the Advancement of Science, the National Science Foundation, and the National Academy of Sciences. I congratulate Gerald Holton warmly on this award.

I am also pleased to report a number of exciting developments. For the first time in Forum history, the Executive Committee, after careful consideration and a unanimous vote, will welcome an elected student among its members. This is the result of changes in our membership. According to the most recent count, the Forum has 3929 members, constituting 8.5 percent of APS; of these members, 26.4 percent are students. We are very pleased to welcome students of all levels to the Forum, and I would like to emphasize that we strongly support and encourage a greater voice for students and young people in Forum affairs. In view of my above remarks, we are of course delighted at this student interest in the history of physics. The Executive Committee has asked the Nominating Committee (chaired by Bill Evenson) to put forth student candidates for one of the two open three-year Member-at-Large positions in the spring 2009 election. The Nominating Committee welcomes recommendations of graduate students suited for this position. It also welcomes nominations for the other Member-at-Large position, as well as for Vice Chair and for Forum Councilor. Self-nominations are welcome, too.

While student numbers have grown, membership in two other categories is below desired levels: female members, who represent only 9.8 percent of Forum members for whom gender is known; and working historians and philosophers of physics. Suggestions of ways to encourage either or both are welcome.

The Forum website at *http://units.aps.org/units/fhp* is a valuable source of information on members and activities. George Zimmerman (*goz@bu.edu*) has volunteered to help us realize its full potential. As our first Webmaster, he is working closely with APS staff on giving the site a needed facelift, enhancing it with multimedia materials from our program sessions, and making it more useful and accessible.

Our sessions at the March and April APS meetings continue to draw excellent speakers (both contributed and invited) and large audiences, as you will see from the meeting reports on pp. 8-13 of this newsletter. Please plan to attend the 2009 March meeting (March 16–20 in Pittsburgh) or the "April" meeting (May 2–5 in Denver), and perhaps contribute a paper to our contributed sessions (see p. 8). Students are especially encouraged to offer contributed papers; limited travel support for them is available. In this regard, the Forum continues to solicit donations in memory of physicists to be used in support of students giving contributed papers and of invited lecturers. For more information, please contact Secretary-Treasurer Thomas Miller (*millertf@bc.edu*).

Zimmerman and Robert Arns (*Robert.Arns@uvm.edu*) are continuing the effort initiated by David Jackson to encourage physics departments to help foster the history of physics by recording their histories, preserving important documents, and asking retiring scientists to provide a short record of their work and future plans. Copies of these materials will be deposited at the AIP Center for History of Physics. For more information, see the article on p. 4 or contact either of these two physicists.

Please consider nominating a deserving colleague for the Abraham Pais Prize and for APS fellowship through the Forum. Female candidates are especially welcome. Currently only 29 of the 602 fellows in the Forum are women. More information on either distinction may be found on p. 3 of this issue and on the Forum website.

Special thanks are due Past Chair Bill Evenson for his excellent leadership and smooth running of Forum affairs. And a warm welcome to those who were elected to the Executive Committee (whose terms began after the April 2008 APS meeting): the new Vice Chair Daniel Kleppner (MIT); and Francis Everitt (Stanford University) and Robert Arns (University of Vermont), both elected to three-year terms as Members-at-Large. ■

History *of* Physics
NEWSLETTER

The Forum on History of Physics of the American Physical Society publishes this Newsletter semiannually. Nonmembers who wish to receive the Newsletter should make a donation to the Forum of $5 per year (+ $3 additional for airmail). 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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Editor's Corner

With this issue, we begin two important changes in the "History of Physics" newsletter. First, as approved by the Forum Executive Committee during its April meeting in St. Louis, the newsletter will be mailed to all members in a print version once per year, generally in the fall. The spring edition will be available only via the Internet and Web. The principal reason for this decision is cost. We save over $6,000 per year, which can be better spent for other Forum activities. And with the Internet and Web accessible to almost all our members, those who really need or want a print version (for whatever reason) can easily download and print their own copy from our web site (*http://www.aps.org/units/fhp*). Another option is to access the HTML version on the same site, which is being upgraded to improve its accessibility and ease of use.

We will still be printing a limited edition, however, mainly for distribution to libraries and history or physics departments that have requested such print copies. This will continue to help promote the newsletter's availability to potential readers outside the Forum who may be interested in its contents. If any members would like to continue receiving paper copies, please contact me (*mriordan@ucsc.edu*), and I will include your name on this list.

Finally, as approved by the Executive Committee at the same meeting, we are adding Dwight E. ("Ed") Neuenschwander as an Associate Editor of the newsletter, effective this issue. A faculty member at Southern Nazarene University in Bethany, OK, he brings many years of experience to the task, having edited the magazines *Radiations*, the official publication of the physics honor society Sigma Pi Sigma, and *The SPS Observer*, the magazine of the AIP Society of Physics Students. Ed will be learning the ropes on the job, in preparation to replace me as Editor a year hence, with the fall 2009 issue. I suspect I'll still be involved with the newsletter in one way or another, but he will then be in charge of its direction and publication.

— Michael Riordan

Call for Nominations: The Abraham Pais Prize

The Forum on the History of Physics calls for the nomination of candidates for the 2010 Abraham Pais Prize for the History of Physics, the purpose of which is to recognize outstanding scholarly achievements in the history of physics. This Prize is sponsored jointly by the American Physical Society and the American Institute of Physics. Awarded annually since 2005, it includes $10,000 and a certificate citing the contributions of each recipient (as many as three), plus an allowance for travel to an APS meeting to receive the prize and deliver an invited lecture on the history of physics. Past Prize winners are Martin J. Klein (2005), John L. Heilbron (2006), Max Jammer (2007), and Gerald Holton (2008).

A nomination for the Pais Prize should consist of:

- a letter of not more than 1,000 words evaluating the nominee's qualifications, in light of the Rules and Eligibility for the Prize (see *http://www.aps.org/programs/honors/prizes/pais.cfm*), and identifying the scholarly and professional achievements to be recognized; as there is no official nomination form, this letter will constitute the formal nomination.
- a list of the nominee's most important publications; reprints of up to five of the nominee's articles may be included.
- at least two but not more than four seconding letters.
- an (optional) biographical sketch.

Five copies of the complete nomination package should be mailed to the next Chair of the Selection Committee:

Prof. Laurie Brown
1500 Hinman Avenue #102
Evanston, IL 60201

Deadline for receipt of all materials is 1 May 2009. After three consecutive review cycles that do not result in a nominee being selected for the Prize, a new, updated nomination package must be submitted for the nominee to be considered again. ■

Call for Fellowship Nominations

The Fellowship Committee calls for the nomination of suitable candidates for APS Fellow through the Forum on the History of Physics. These nominations should be based at least in part upon achievements related to the history and philosophy of physics. The Forum deadline for the receipt of all materials at APS is 15 May 2009. Procedures for nomination have recently changed. The new procedures are now available at:

http://www.aps.org/programs/honors/fellowships/index.cfm
(click on Nomination Instructions).

According to these procedures, all nominations are to be submitted to the APS via the online nomination package provided at the indicated web site. The nominees must be APS members in good standing, which may be confirmed through the above website. A sponsor (nominator) and a co-sponsor, both of whom must be APS members, are required. Up to two supporting letters from other individuals, who do not have to be APS members, may be also submitted by uploading to the site. Please visit the above web site for further information and to obtain a list of the required documentation.

Nominations will be forwarded to the Forum Fellowship Committee for review. This committee will make its recommendation to the Forum Executive Committee, and after that all nominations will go to the APS Council for approval. Fellowship nominations may be submitted at any time, but must be received by 15 May 2009 for the next review.

For further information, please contact the chair of the Forum Fellowship Committee, Daniel Kleppner at *kleppner@mit.edu*, or the APS fellowship officer at *fellowship@aps.org* or by telephone at (301) 209-3268. ■

Editor's Note: As this issue went to press, Stephen Brush of the University of Maryland was named the 2009 Pais Prize winner "for his pioneering, in-depth studies of the history of 19th and 20th century physics."

Preserving Departmental Histories

By Robert Arns and George Zimmerman

The two of us, members of the Forum Executive Committee, have taken over from retiring member David Jackson as the advocates for writing and creating a register of institutional and departmental histories. The Forum renews its call (from *APS News*, January 2007, 'and "History of Physics" newsletter, Spring 2008, p. 5) to every physics department to help preserve its history and record of accomplishments by updating an existing history or preparing a new one. Such a coherent historical narrative may be supplemented by specialized records and documents—for example, annual faculty lists and course descriptions from university catalogs. These histories and records should be deposited with the AIP Niels Bohr Library & Archives and entered in the Forum's new Register of Departmental Histories and Records (see below).

If an up-to-date historical record is not already on file at the Bohr Library, the Forum urges a physics department to prepare or update a history of its department and any research laboratories, and to send a copy (in whatever form) to the Niels Bohr Library & Archives, c/o Joseph Anderson, AIP Center for History of Physics, One Physics Ellipse, College Park, MD 20740-3843 (email: *janderso@aip. org*). Placing these historical documents on the departmental web site would also give interested parties better access to them.

The Forum has established a separate Register of Departmental Histories and Records, to be published periodically in the "History of Physics" newsletter and on its web site. Entries should be standard bibliographic citations, indicating availability in institutional or departmental libraries, through web links, and (we hope) at the Bohr Library. This Register will provide another tool for obtaining information about past activities in physics research and education, to serve as a starting point for more focused searches. Please send Register entries for existing histories, other materials and new entries as they are produced to George Zimmerman, Physics Department, Boston University, Boston, MA 02215 (email: *goz@bu.edu*). The materials themselves should be sent to the Niels Bohr Library at the above address.

The Forum congratulates those departments with an up-to-date history on file in its library and at the Niels Bohr Library & Archives. If such a history does not exist, please prepare one! Forum members are encouraged to take the initiative in doing so.

Register of Departmental Histories and Records

Submitted January 2007 to July 2008

American Physical Society

"History of the APS Forum on Physics and Society"
http://units.aps.org/units/fps/history.cfm

Boston University

"History of the Boston University Physics Department, 1906 to 1970"
by George O. Zimmerman
http://physics.bu.edu/history

University of California, Berkeley

"History of the Physics Department, University of California, Berkeley, 1950–1968"
by August Carl Helmholz, edited by John David Jackson (Berkeley, CA: University of California, 2004), 75 pages.
availability: UCB Physics Department; UCB Library; Niels Bohr Library

Case Western Reserve University

"Physics at a Research University: Case Western Reserve 1830–1990"
by William Fickinger (Cleveland, OH: Case Western Reserve University, 2006), 360 pages.
availability: Library of Congress, Catalog LCCN 2005933988; also at the CWRU bookstore and on the web at *http://www.phys.cwru.edu/history*

University of Hawaii Physics Department

"History of the UH-Manoa Physics Department,"
compiled by Vincent Z. Peterson
"Dirac and Heisenberg in Hawaii,"
by San Fu Tuan
Both available at
http://www.phys.hawaii.edu

University of Hawaii Institute for Astrophysics

"Origins of Astronomy in Hawaii,"
by Walter Steiger
"Astronomy in Hawaii, 1964–1970,"
by John Jefferies
Both available at
http://www.ifa.hawaii.edu/ifa/history.htm

Michigan Technological University

"History of the Physics Department, Michigan Technological University"
http://www.phy.mtu.edu/alumni/history

University of Notre Dame

"Seventy Years of Nuclear Physics at Notre Dame: 1937–2007"
by Michael Wiescher
"History of the Department of Physics"
"The Early History of Nuclear Physics at Notre Dame"
"The First Accelerator at Notre Dame"
"Accelerator Memoirs,"
by Cornelius. P. Browne
"Early Days of Nuclear Physics at Notre Dame and the Manhattan Project"
"The Nuclear Structure Laboratory at Notre Dame,"
by Michael Wiescher
"Cornelius P. Browne, 1923–2005"
All available at *http://www.nd.edu/~nsl/html/about_history.html*

Purdue University

"History of the Purdue Physics Department"
Newsletter articles and other material, collected on a web site: *http://www.physics.purdue.edu/about_us/history*

Williams College

"Williams College's Hopkins Observatory: The Oldest Extant Observatory in the United States"
by Jay M. Pasachoff, published in *Journal of Astronomical History and Heritage* 1:1 (1998), 61-78. ■

Pais Prize Lecture:

Of What Use is the History of Science?

By Gerald Holton

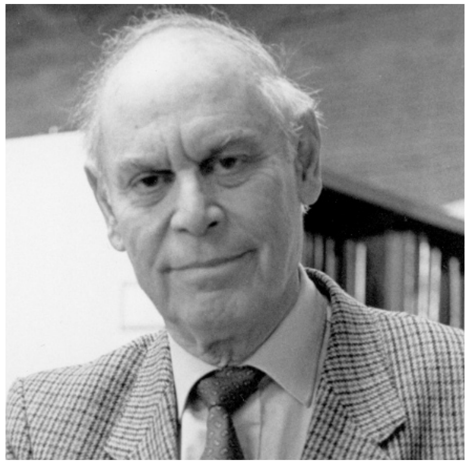
I am deeply grateful for the honor extended to me, and rather overwhelmed by it, especially as the Prize is offered in memory of Bram Pais, who was an extraordinary physicist, and who also has no peer as a historian of physics. His countless essays and thirteen books helped to define the field, and set the highest bar. I knew him quite well. We also served together on the committee that helped turn out the first volumes of the *Correspondence of Albert Einstein*. I am glad to say that in our extensive correspondence he chose to be kind about my own work. He was, in a way, the modern conscience of the history of science; and it is in this spirit that I wish to make two points, both concerning the fact that history matters, especially in this somber time, for so many in physics and beyond.

If you heard or read Leo Kadanoff's Presidential Address to the April APS Meeting (published in *APS News*, July 2008), you know that this society and its members have a big task now to stem the decline in research funding, in status, in education, and in the general scientific literacy of the public—not only for ourselves but also for our country.

What I have to relate here may, I do believe, give added conviction and authority to those who want to be effective in this difficult task. My first point concerns the *sense of self*, the intellectual identity, of each of us individually; and my second, related, point will concern our opportunity, perhaps even duty, to our students.

As to the first point: physicists and other scientists tend to be understandably oriented above all to the future of their field rather than to its past. Such are the characteristic identities of pioneers at a frontier, rather than of scholars focusing on the past.

Let me illustrate this view, together with a rare conversion experience. In 1972, the Enrico Fermi summer school of physics was held in Varenna, Italy, on Lake Como. The topic was "The History of 20th-Century Physics."



2008 Pais Prize winner Gerald Holton

The faculty for this school consisted of a small group of physicists and historians of science. To our delight, Paul Dirac agreed to participate. We, the faculty, all met before the school started, to synchronize our work. Dirac listened intently, and finally spoke up in his quiet way, saying "I don't understand why there *should* be a history of physics. Either a thing happened, or it did not." This remark produced panic among the rest of us.

Near the end of the summer school's term, Dirac gave a set of lectures to our students, saying at the start: "I have learned a great deal here at Varenna. . .I have learned to appreciate the point of view of the historian of science. . .[By contrast,] the research physicist wants rather to *forget* the way by which he attained this discovery. . .. He feels perhaps a bit ashamed, disgusted with himself, that he took so long."

"However, with the understanding of what the historians of science are concerned with," Dirac continued, "I have tried to think over the past. . . [and] how these things led me to the style of work which I followed later in life." And then he gave a splendid set of three historical lectures, "Recollections of an Exciting Era," which were published later.

To be sure, few scientists have experienced a conversion like Dirac's. Only rarely is a researcher interested in reading one of the publications on the history of science, or for that matter in reading a physics paper or volume published many decades in the past—as was done by I. I. Rabi. He wrote that one day he happened to be reading, for sheer pleasure, Maxwell's *Treatise* of 1873. That gave him a clue for quickly measuring the magnetic susceptibility of a crystal, a central question in his research project at the time. It was for him not the only time that history helped to transform a present puzzle into a future solution.

For Rabi, and for relatively few physical scientists today, such as Steven Weinberg and Freeman Dyson, a sense of the historical development leading up to their current physics preoccupation has been important for a more comprehensive sense of self. And, I maintain, it should be so for far more scientists. For in truth, for each of us, the science research project of today is the temporary culmination of a very long, hard-fought struggle by a largely invisible community of our ancestors. Each of us may be standing on the shoulders of giants; more often we stand on the unrecognized graves of our predecessors. To know nothing about them is, to me, as limiting in one's self-regard as not knowing one's actual parents.

I was lucky to realize this simple fact as a Ph.D. student under P. W. Bridgman. He was not only a hard-driving experimental physicist, who was awarded the Nobel Prize in 1946, but he also eloquently wrote on what was called the operationalist approach to the methodology of science. The first thing a new student of his would do was read his great text, *The Physics of High Pressure*. And there his first chapter is titled "Historical Introduction"—29 pages on the great sequence of prior high-pressure experimenters, some 75 of them, starting with Hans Christian Oersted in 1823. This is one example of acknowledging the serious

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debt any advance pays to its genetic forebears.

To illustrate further, let me refer to work I did with two research associates some time ago, published under the title "How a Scientific Discovery is Made."* As you know, in 1986 and 1987, there appeared out of the blue several papers on high-temperature superconductivity by the Swiss physicist Alex Müller, formerly a student of Wolfgang Pauli's, and by Müller's former student, Johannes Georg Bednorz. Starting in 1983, at the IBM lab in Switzerland, they worked rather secretively, in order that if they failed, they could, as Müller told me, give their work a "burial in very restricted family circumstances, so as not to jeopardize Bednorz's career." Yet they caused a great sensation when they announced their findings. They had broken through a long-standing barrier, reaching superconductivity at about 30 K by the completely unconventional use of a ceramic compound with a perovskite structure. Others quickly converged on this new field, and pushed the transition temperature to over 130 K.

I became interested in just how Müller and Bednorz made their discovery. Specifically, what had been the historic treasury of intellectual and material resources that were available to them and were used by them? Happily, both men cooperated with us in giving interviews and exchanging letters. I especially wanted to know how they fitted into the grand, age-old network of available knowledge on the way to the new knowledge. How did their work fit into the big jigsaw puzzle whose pieces were prepared by previous advances?

So we traced, in their own key publications, the explicit and implicit serious citations. Then we looked at the explicit and implicit citations in the publications of *those* immediate ancestors; and in fact we went further back in this way for a total of about four intellectual generations.

Analyzing the original five papers that comprised the announcement of their breakthrough revealed the number of silent resources that they had put to use: for example, tools for standard observation techniques that are no longer referred to explicitly, such as x-ray diffraction (Max von Laue), or the criteria for identifying superconductivity, namely zero electrical resistance (by Heike Kamerlingh-Onnes in 1911), and the Meissner effect, implicitly referring to a 1933 publication by Walther Meissner and Robert Ochsenfeld. Similarly, the platinum thermometers that Müller's team used imply references to an 1887 publication by one Hugh L. Callendar of the Cavendish Laboratory, which ushered in the platinum-resistance thermometer as a practical means of measuring temperature.

The origins of the apparatus Müller's team used to liquefy helium stems of course from the principles of cooling, laid out first by the British physicists William Thomson and James Joule in the 1850s, and by the French chemists Nicolas Clément and Charles-Bernard Desormes in 1819. And so forth. Unwittingly but documentably, the stage for Müller and Bednorz's discovery in the 1980s had been set by earlier scientists—many long dead, if not forgotten.

And there was one special ancestor of Müller's work: Johannes Kepler. Müller told me that he had an unusual fascination with perovskites, which have a very high degree of symmetry, and which he had used with great success in many other research projects. This fascination had originally stemmed from his having been a student in Pauli's class, when Pauli was sharing his ideas on an essay he was writing on Kepler and his archetypes, especially those five Platonic, highly symmetrical bodies. So it turns out that Kepler had helped Müller and Bednorz discover high-temperature superconductivity!

From these and many other examples we can generalize that any significant advance relies, not vaguely but documentably, on a large, international, identifiable set of earlier contributions, all serving the emergence of new science or technological achievement. This fact also supports the old assumption that there is some underlying unity in science and technology, not a unity found by one grand synthesis, but a different unity, an *operational* one, in which the interlinking parts of science and technology help one another.

The lesson here is that Dirac was correct in his advice in his Varenna lectures. Indeed, every advance reported in an APS meeting or publication is a new fruit on an old family tree, one with many branches, near and far. Moreover, these long-gestated fruits of science have nourished not only current physicists, but were, and continue to be, crucial aid for other sciences, for applications—and for the forces working on behalf of enlightenment, of reason and sanity, and potentially for upgrading the human condition.

In this recognition lies a large part of the moral authority of the scientific profession. And when not enough scientists assert it, others rush in, to define it in their destructive ways, as they have done again and again. I dare to confess frankly that a good part of the reasons for my doing some of the things the award citation asserted about my activities has been largely motivated by the view that our physical sciences, when seen through the twin lenses of the achieved present and the painful development over centuries, are at least as important a part of humanity's culture and long-term health as any other enterprise.

Of course, at this point I hear some skeptical voices. For many scientists, the adrenaline of the day-to-day excitement in the lab is quite enough to feel utterly secure within themselves. Others make do very well with a combination of good work at the bench or desk, plus important public service, like many of our role models, or those who battle the tone-deaf administrators and the scientific deniers of our time.

Assuming these roles is of course needed, too—and is fulfilling for those who do, and crucial for the rest of us. But there is at least one role that seems to me to require from the scientist a living sensitivity and witness to serving as a link in a grand chain of being. This role is that of educator.

And so I come to my second point: how best to attend to the opportunity,

**American Scientist*, Vol. 84, No. 4 (July–August 1997), pp. 364–375.

perhaps *duty*, that we may have to our students.

If you accept the suggestion that many working scientists deserve a larger, more secure sense of identity, being confident beneficiaries of the past and contributors to the present culture and civilization, it follows that they have also an opportunity to help their young colleagues and students at least to glimpse their own role in this great venture. This can be done easily when one is teaching physics, where we convey to students many of the great breakthroughs, from Galileo to Richard Feynman, and today's favorite topics. What I am about to suggest applies to any of these, but let me concentrate for a moment on the opportunity to teach relativity theory in this mode, as one example.

Students usually look forward to being introduced to this topic, and there are by now hundreds of ways to present the main concepts and equations, and their uses. That must be done. But many instructors have found that there is in addition even more excitement and result, by making a little room to give students a glimpse of why and how this theory came about, and thus became a key part of physical science.

Even in Einstein's own writings, it is easy to find what he regarded as the immediate antecedents of his theory. I would recommend turning to one of Einstein's early love letters to his future wife, Mileva Maric. Writing in August 1899, he says he has been reading Heinrich Hertz on Maxwell's theory, and he presents to Mileva his conclusion: "The introduction of the word 'Ether' in the electric theory has led to the conception of a medium of whose motion one can talk, without, I believe, connecting with that assertion a physical sense."

So, in 1899, six years before his 1905 paper, he already had the audacity to dismiss the ether. Later, Einstein added that the Fizeau experiment of 1851, stellar aberration, and Michael Faraday's induction experiment were the critical antecedents to his own work. And in his autobiography, written in 1946, he added that his early self-education included reading Gustav Kirchhoff and Hermann Helmholtz, especially on Maxwell's theory. Indeed, he referred to his own approach to physics as the Maxwellian Program.

Now that we have begun to make the student aware of some of the steps, so to speak, of a ladder up through which relativity came into being in Einstein's mind, we can stop at this important point to explain what in Einstein's view is that Maxwellian program. It is of course an exemplification and realization of the oldest motivating force in physics, namely, the attempt at a grand synthesis, at a unification of disparate elements—a tradition I have liked to call the Ionian Enchantment, going far back in time.

In a way, some of the most recent works being presented at this APS meeting are children of that great family dynasty: the movement toward unification within a branch of science, going back to (among others) the Vienna Circle for the Unity of Science, then further back to the syntheses worked on by Maxwell, by Faraday, by Oersted, by Kant and the nature philosophers. This takes us all the way back to Newton, who in his preface to the *Principia Mathematica* said he hoped that by mechanical principles one could "derive the rest of the phenomena of nature," and ultimately back to Thales of Miletus in ancient Greece. And then we can go forward to what Einstein initially called the generalized relativity theory, and on to today's ideas of a theory of the synthesis of all forces. Giving some idea of this grand arc is showing science as a living being, with huge energy, struggles, despair, visions, vexations, and victories.

In short, when students are dealing with the work of any of those who helped our current science to be born, they should see that physics, through the centuries-long application of rationality, intuition, and skill, has achieved a high degree of organic coherence, rather than being just one detail after another, like those separate chapters in so many textbooks. So, should not at least some of us, when teaching, for example, about Einstein's work

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Physical Review Letters Turns Golden

All this year our beloved journal of letters *PRL* has been celebrating its 50th anniversary. In July 1958 Physical Review Editor Samuel Goudschmidt began this new journal as a tentative experiment, taking what would have been brief letters to the Editor and publishing them separately in their own volumes in order to make important results available quickly. It became an enormously successful journal in its own right, and is now emulated by scores of similar letters journals worldwide. Thus it is indeed fitting to look back and review the course of its publishing history.

Since January 2008 *PRL* has been republishing important editorials and letters on a special golden-anniversary web site,

http://prl.aps.org/50years.

Compiled by former APS Editor-in-Chief Martin Blume (who recently ran for Forum Vice Chair), this site features a few milestone letters from each year, including short essays on their contents and significance. There is also a web timeline highlighting important events in the history of the *Physical Review* journals.

In recognition of this anniversary, the Forum sponsored special invited sessions at the March and April APS meetings devoted to *PRL* and its history. The March meeting featured talks by Marvin Cohen, Saad Hebboul, Jack Sandweiss, Charles Slichter, and Eugene Stanley (see the article by George Zimmerman, p. 9). At the April meeting, Associate Editor Robert Garisto spoke about "Half a Century of *PRL*," while Michael Peskin and Michael Turner addressed the impact the journal has had on particle physics and cosmology.

One brief but touching event occurred at the April meeting, where a large group of referees were recognized for their outstanding service to the journal. Without their dedicated, self-effacing efforts, *PRL* would not be the preeminent journal of physics it has indeed become. ■

Reports from the Annual APS Meetings

Student Presentations at the 2008 Forum Sessions

By Virginia Trimble and George Zimmerman

In recent years, the Forum has begun attracting some of our younger colleagues interested in the history of physics to present papers on their work at APS meetings. Since 2005 we have been offering awards of $600 for partial travel support to graduate or undergraduate students who have submitted abstracts accepted for our contributed sessions at the March or April meetings. These awards honor distinguished physicists no longer alive, as selected by the donors, beginning with the Bardeen student awards in 2005. Two graduate students and one undergraduate student received such awards at the 2008 APS meetings.

The two students at the March meeting were Cesar Rodriguez-Rosario and Kavan Modi, who were both about to defend their Ph.D. dissertations at the University of Texas, Austin. They presented their work in both the Forum and general theory sessions. The award citations, checks, and brief biographies of the physicists being honored were presented at the outset of our contributed session.

Rodriguez-Rosario was born and did his undergraduate work in Puerto Rico. His Ph.D. thesis is on theoretical quantum mechanics, focusing on quantum information, open quantum systems, and decoherence. His presentation, entitled "The Increasingly Disordered History of Entropy," traced the long debate about entropy from the mechanics of Lucretius to modern information theory, with illustrious contributors such as Boltzmann, Carnot, Clausius, Laplace, Maxwell, von Neumann, Prigogine, Shannon, and Szilard. The award was given to honor Gregor Wentzel (1898–1978), who earned his doctorate in 1921 as a student of Arnold Sommerfeld at the University of Munich. Wentzel, Hendrik Kramers, and Léon Brillouin independently developed what was known as the Wentzel-Kramers-Brillouin approximation, also known as the WKB method, classical approach, and the phase-integral method. In 1948 Wentzel began a professorship at the University of Chicago, retiring in 1970 after a distinguished career.

Modi was born in India and at age 13 came to the United States, where most of his education occurred. He attended Embry-Riddle College and took part in programs of the Aeronautical University in Daytona Beach, Florida, where he studied space systems engineering; he also was a summer intern at the Kennedy Space Center and Fermilab. His thesis concerns unstable quantum systems, open quantum systems, and quantum optics. Modi's presentation, "The Stolen Brain of Einstein" describes the long journey this brain has taken in the last 52 years. It was removed by pathologist Thomas S. Harvey, who did not really have permission to do so. Only later did he convince Einstein's son Hans that this was done was for good purposes. Modi's award honored Rolfe Eldridge Glover III (1924–2004), a professor at the University of Maryland, one of the researchers to find evidence for the energy gap in superconductivity, which was a fundamental idea used in formulating the Bardeen-Cooper-Schrieffer theory of superconductivity.

At the April meeting, Princeton undergraduate Samuel Fletcher was the student awardee, sponsored by Donat G. Wentzel in honor of his late mother-in-law Maria Goeppert-Mayer (1906–1972). Fletcher spoke about "The Manhattan Project and its Effects on American Women Scientists," displaying extraordinary grace under pressure. His prepared talk had somehow vanished in the Chicago airport, but he reconstituted it the night before the Manhattan Project session (see pp. 12–13) as a PowerPoint presentation. And then the projector failed completely! (It was later discovered that a critical component had not been plugged in.) But the speaker carried on, with fewer exact numbers and lists of names than he had planned, and with some extraordinarily interesting details.

Fletcher cited the 1999 book *Their Day in the Sun* by Ruth Howes and Caroline Herzenberg, about the women of the Manhattan Project, and its review by Margaret Rossiter. Of about 130,000 people involved, only about 300 were women at Los Alamos, and another 100 or so at Hanford. There were also at least a few women working under the bleachers in Chicago. To a considerable extent, all were employed because of the wartime labor shortage, though some of the women (Leona Marshall Libby is the one you will most probably have heard of) were every bit as qualified as many of the men. They were, of course, usually paid less.

Many of the technically trained, though non-Ph.D., women worked as "computers," processing numerical data according to algorithms laid down by others. The extent to which various women were provided with household help, including child-care, displayed an ambiguous (though not necessarily unfair) attitude toward their multitasking, highest priority going to those who worked full time and had children. (The Los Alamos birthrate was remarkably high, presumably for other reasons.) In a gross generalization of what Fletcher said, participation in the Manhattan Project was not the career-booster for women that it became for many of the men, who returned to civilian life with job offers at multiple prestigious institutions.

We are particularly pleased to note that Fletcher plans to pursue graduate work in philosophy of science at UC Irvine next year.

Please contact Forum Secretary-Treasurer Thomas Miller (*millertf@bc.edu*) if you would like to honor a physicist in this way at the 2009 meetings, and urge interested students to apply for support next year as well. ■

Forum Sessions at the March Meeting

By George Zimmerman

The APS March 2008 meeting in New Orleans featured two invited sessions sponsored by the Forum. The first, celebrating the Fiftieth Anniversary of *Physical Review Letters*, chaired by Reinhardt Schuhmann (Managing Editor, *PRL*), and organized by the *PRL* editors, was held on Tuesday, March 11. The second, held on Thursday, March 13, focused on Industrial Physics History. This session was chaired by Gloria Lubkin. Both of these well-attended sessions included five speakers, whose topics ranged from a historical overview to a personal reminiscence.

At the *PRL* session, Saad Hebboul of *PRL*, whose talk was entitled "*PRL* at 50: A History of Moving Physics Forward," recounted its early history from its beginnings as a section in *Physical Review* to a full-fledged journal in its own right fifty years ago, and since then, to its current significance. The next three talks concerned major topics typically discussed at March meetings: Eugene Stanley (Boston University) discussed "Phase Transitions and Critical Phenomena." Marvin Cohen (University of California, Berkeley) spoke about "Condensed Matter Theory: From Models to First Principles." Charles Slichter (University of Illinois, Urbana-Champaign) then delivered a talk entitled "NMR and the BCS Theory," in which he recounted the experimental as well as theoretical implications of work in the 1950s. He emphasized the measurement of the spin-lattice relaxation time and the isotope effect in superconductors, which formed the experimental foundations for the BCS theory. In the final talk, titled "The Future of Scientific Publishing," Jack Sandweiss (Yale University and *PRL*) discussed various possible scenarios for future developments in both print and electronic publications.

The speakers at the Industrial Physics History session all have held or still hold leadership positions in industrial-research labs. Paul Horn (recently retired as Director of Research at IBM and now at New York University) gave a talk titled "Industrial Research at IBM" in which he described the lab's many research achievements, ranging from microchip development to the self-assembly of microcircuits to modeling of geological water cycles. He was followed by James Hollenhorst (Vice President and Director of Molecular Technology at Agilent Labs), with a talk titled "Reflections on Three Corporate Research Labs: Bell Labs, HP Labs, Agilent." He recounted the cultures of—and the relationships and clashes between—the research and industrial departments of these three corporations.

The John Bardeen Lecture was delivered by Robert Frosch (currently at the Kennedy School of Government at Harvard), whose positions have included NASA Administrator (1977-1981) during the Carter Administration, Assistant Secretary of the Navy for Research and Development, and Vice President for Research at General Motors Research Laboratories. His talk, which was titled "Application Oriented R&D: Aphorisms & Anecdotes," centered on his long experience in trying to develop and manage systems that have relevant and useful applications for solving the problems and reaching goals of the pertinent (client) organizations.

David Bishop (former head of the Micromechanics Research Department, Bell Labs), in a talk titled "The History of Science and Technology at Bell Labs," described the many accomplishments of Bell Labs since its establishment in 1925. The final speaker was Robert Doering (now Senior Fellow in Silicon Technology Development at Texas Instruments). In a talk titled "50 Years of 'Scaling' Jack Kilby's Invention," Doering discussed the development of integrated circuits and the prospects for future developments once miniaturization encounters the limits of fundamental physics.

The well-attended contributed paper session on March 12 featured several speakers and the presentation of Forum Student Travel Awards. All the Forum sessions were audiotaped; these tapes will be combined with the visual presentations on a DVD given to the AIP Center for History of Physics.■

Los Alamos and the Manhattan Project

By Don Howard

Living history in the form of personal memoirs was a delightful feature of one of the engaging Forum sessions at the April APS meeting in St. Louis. It focused on the Manhattan Project, on the occasion of the 65th anniversary of the Project's launch in early 1943.

Cynthia Kelly began the session with a talk, "A History Worth Preserving," reporting on the exemplary work of the Atomic Heritage Foundation, which she founded and directs, in identifying and preserving physical sites important in the Manhattan Project and the history of atomic physics more generally. A principal focus of this talk was the success in salvaging the V-site at Los Alamos, the facility where the Trinity device was assembled and one of the few remaining original structures at Los Alamos. Kelly also discussed examples of at-risk sites at Oak Ridge and elsewhere.

Next came Anthony P. French sharing his recollections of work as "A Very Junior Physicist at Los Alamos, 1944–1946." He worked there as part of the British contingent, serving under the direction of Egon Bretscher in Fermi's "F Division," where the "Super" or hydrogen bomb was the main focus of research. In his talk French focused on his personal odyssey from Britain to New Mexico, saving comments about his Los Alamos experiences for the ensuing panel.

The session concluded with a panel discussion organized by Ben Bederson and Clayton Gearhart, and hosted by David Cassidy, featuring seven Manhattan Project veterans. Besides French, they included Albert Bartlett, E. Leonard Jossem, Howard Kratz, Nathan Melamed, Murray Peshkin, and Julius Tabin. Former Forum Chair and "History of Physics" Editor Bederson closed out the panel and session with his recollections of service at Los Alamos in the Army Special Engineer Detachment. ■

Triumphs of 20th-Century Astrophysics I: Telescopes and Observations

By Virginia Trimble

At the April APS meeting in St. Louis, the Forum and the Division of Astrophysics co-sponsored two invited sessions on "Triumphs of 20th-Century Astrophysics." Organized by Virginia Trimble, they were each meant to feature three speakers focusing on the past, present and future of stellar astrophysics. Both sessions were expertly chaired by Ramanath Cowsik (Washington University, St. Louis), who set a standard of dress matched by few audience members or speakers.

For the first session, three knowledgeable speakers had agreed to discuss one important observatory from the recent past (Lick Observatory), one currently at peak productivity (Hubble Space Telescope), and a suite of future plans for 30-meter class telescopes and associated surveys. Unfortunately, the first speaker, Joseph Miller, a former Lick director, was forced to cancel on about 12 hours notice, due to a death in his family. His abstract indicates that he intended to address three significant aspects involved in the development of Lick and other observatories (Mt. Wilson, Palomar Mountain) that shifted the focus of world-wide observational astronomy to California between about 1900 and 1965. The first is location. Relatively dry mountain tops are much better telescope sites than the low-lying, near-urban environments that had been common in the past. Second is the improved technology of the telescopes themselves and of their focal-plane instrumentation (cameras and spectrographs). Third is a particular sort of community of users, relatively small (permitting both extended surveys and speculative projects) and closely coupled to development of instrumentation (permitting more effective use of it). In his stead, Trimble spoke for a slightly shorter time on these and other aspects of telescope history, focusing on the growth of what was the biggest in each generation, changes in basic designs, and some of the science enabled thereby.

Mario Livio (Space Telescope Science Institute) showed some of the glorious HST images that we have all gradually come to expect and described what he regards as Hubble's top seven scientific achievements, many done in partnership with ground-based and non-optical facilities, and—he was quick to say—not necessarily in order of importance. These were: (1) evidence for the acceleration of cosmic expansion and existence of dark energy; (2) an accurate measurement of the cosmic distance scale and the Hubble constant (currently with 10 percent error bars, like Edwin Hubble's, but 72 ± 8 rather than 536 ± 50 (km/sec)/Mpc); (3) the evolution of galaxies and the history of star-formation rates as revealed by sources in the Hubble Deep Field, whose shapes are more like train wrecks than like the classic sequence of ellipticals and spirals; (4) the existence and characterization of extra-Solar-System planets, including the presence of sodium, carbon, oxygen, and hydrogen in one planetary atmosphere and methane in another (from absorption features as the planet passes in front of its host star); (5) the existence and non-dissipative nature of dark matter in the so-called "bullet cluster," where the gas has been left behind while two clusters of galaxies and dark matter (revealed by gravitational lensing) passed through each other; (6) the stellar populations in M31 and other galaxies, and how they differ from Milky Way populations; and (7) the presence of supermassive black holes in the nuclei of virtually every large galaxy.

Elizabeth (Betsy) Barton (University of California, Irvine) summarized plans for some of the large telescopes of the future, especially the Thirty-Meter Telescope (a collaboration of Caltech, the University of California, and Canadian universities), the Giant Magellan Telescope (involving about ten institutions and to be sited at Las Campanas Observatory in Chile), and the Large Synoptic Survey Telescope (a still larger collaboration including UC Irvine). The first two have roughly equivalent collecting areas (seven 8-meter circular mirrors for GMT versus a Keck-style filled aperture of many hexagons for TMT, 492 at last count) and relatively small fields of view. By contrast, the LSST has a single 8-meter mirror with a much larger field of view that will survey the entire visible sky every three nights. TMT and GMT will both make use of adaptive optics, initially in the near infrared, and their science goals are somewhat similar: to see stellar and planetary systems in formation; to detect the "first lights" (probably stars but perhaps black hole accretion) that began to reionize the Universe between redshifts of about 20 and 6; to characterize the stellar populations in galaxies from the earliest times to the present; and, just possibly, to learn more about more-nearly-earthlike planets. The amount of information available from a mirror of diameter D scales at least as D^2, as D^4 when sky noise is comparable with signal from target sources, and even as D^6 with adaptive optics in crowded fields. There is also a European Extremely Large Telescope, intended to be 42 meters in diameter and having similar science goals as these two American projects. The LSST will map out dark matter via gravitational lensing and identify a wide range of transient sources ranging from near-earth asteroids to supernovae, which can be used to study dark energy. The TMT site is still to be determined, with candidates in Hawaii and Chile. ■

Triumphs of 20th-Century Astrophysics II: We Master the Stars

By Virginia Trimble

As in session I on observatories and telescopes, three experts were asked to address the past, present, and future of stellar astrophysics. All were present at this second session.

Matthew Stanley (Michigan State University, but soon to be at New York University) presented the intriguing tale of how we learned that stars run on nuclear energy, particularly the *p-p* and CNO (carbon-nitrogen-oxygen) cycles. He divided it into three pieces. First came establishing that there was a problem, after Joule demonstrated conservation of energy as a general principle, with ideas from Kelvin and Helmholtz about the in-fall of meteoric material or gravitational contraction as a solution, in good accord with the Kant-Laplace model of Solar System formation. Then came the discrepancy between the time scale of a few times 10^7 years permitted by gravitational processes according to Kelvin (who found a similar time for the cooling of the Earth) and the 10^8 to 10^9 years required by Darwin and others for geological processes and biological evolution to occur. Giant stars, especially Cepheids, also seemed to require a longer-lasting energy source than just contraction, as did some stellar dynamical processes. Resolution of this discrepancy required the concept of mass-energy equivalence; consideration of either transmutation of elements or proton-electron annihilation (by Jeans and Eddington); accurate measurements of nuclear masses (F. W. Aston and others); analysis of how an energy source could be distributed through a star and remain stable (by T. G. Cowling, for instance); demonstration that stars had a large hydrogen content (C. Payne); the concept of barrier penetration (R. Atkinson and F. Houtermans, G. Gamow); and detailed nuclear-reaction sequences devised by von Weizsäcker and Bethe. Stanley ended by noting that Rutherford had spoken in St. Louis at the 1904 World's Fair.

Stirling Colgate (Los Alamos National Laboratory) had been asked to address current issues in stellar astrophysics, especially high-energy ones like supernovae and gamma ray bursters. In the session he gave a typical Colgate talk, which is to say that I do not feel I understood it very well but nevertheless came away suspecting that he was probably right about many issues. He began by focusing on the formation of supermassive black holes in the early Universe (temporally and logically prior to galaxies and stars, in his view, and not assembled hierarchically). The resulting free energy is in excess of 10^{60} ergs, at least as much as will come from supernova kinetic energy over the rest of the life of a typical galaxy, and he drew an analogy with the enormous shock wave of the 11 February 1954 BRAVO hydrogen bomb test. Forming these enormous black holes from an initial perturbation spectrum is awesomely complicated—transcended by the even greater complexity of transforming this energy into the magnetic fields, jets, radio lobes, and the extragalactic cosmic rays we observe. Angular-momentum transport and large-scale coherent dynamos are essential processes that probably get missed by computer simulations of structure formation. He concluded that emission of 100 MeV photons and 10^{30} eV or more cosmic rays should be possible.

Mark McCaughrean (University of Exeter) was the Kenneth Greisen Lecturer. Greisen is the G of the GZK cut-off (the others being Georgiy Zatsepin and Vadim Kuzmin), and the lecture was sponsored by his astrophysicist son Eric and by former students Irwin Shapiro, Alan Bunner, and Donald Gilman. McCaughrean addressed what is surely the single most important unsolved problem in stellar physics—how they form—and he entitled the talk "Standing on the Shoulders of Giants," meaning the giant telescopes that, in the next decade or two, may enable us to answer some of the major outstanding questions about the birth and early evolution of stars, brown dwarfs, circumstellar disks, outflows, and planetary systems. The contributors will include X-ray and millimeter telescopes, and the James Webb Space Telescope (mostly infrared)—as well as the large ground-based optical facilities discussed by Betsy Barton in session I. Three of the major questions are the importance of feedback (for instance, once a massive star has formed in a region, do you get more or fewer stars there?); the processes responsible for the initial-mass function ($N(M)$ of stars when they form) and whether it is universal; and the formation, evolution and significance of disks around young stellar objects. In common with the objects and processes advertised by Colgate, angular-momentum transport and magnetic fields must also be important here. Resolved imaging of disks in the infrared will be particularly important to understanding how they change as young stellar objects age and if, how, and when they can form planets. ■

Eighty Years of Quantum Mechanics

By Don Howard

A high point among the Forum events at the April APS meeting was the session titled "80 Years of Quantum Mechanics: A New International Project," reporting on the work of a major new collaborative project on the history of quantum mechanics. With a distinguished advisory board, it includes a wide array of participating institutions such as the Einstein Papers Project, the Niels Bohr Archive, the Perimeter Institute, and the Chinese Academy of Sciences. This project is led by six primary institutions: the Max Planck Institute for History of Science and the Fritz Haber Institute (both in Berlin), The Johns Hopkins University, University of Notre Dame, University of Minnesota, and University of Pittsburgh. The overall project goal is to develop a deeper understanding of the genesis and development of quantum physics; it will be achieved by sponsoring individual and collaborative scholarship; through organizing conferences, workshops, and symposia; and by the design and establishment of new, mainly online, research tools and information sources. Two major international conferences have occurred to date, one in Berlin in summer of 2007 and one in Utrecht this past July. The third such conference will be hosted by the University of Minnesota in 2010.

The April APS session featured presentations by two of the project directors, Michel Janssen of the University of Minnesota, and Christoph Lehner from the Max Planck Institute for History of Science, each reporting on major research initiatives it has sponsored, and one by Alexei Kojevnikov of the University of British Columbia.

Janssen spoke on "Van Vleck and Slater: Two Americans on the Road to Matrix Mechanics." These theorists played a prominent role in the development of American physics starting in the mid-1920s. Not well appreciated is the role they played early that decade in recasting classical dispersion theory into a form adapted to the new quantum theory. Van Vleck and Max Born provided the first explicit derivation of Kramers' new dispersion formula employing the (Born) correspondence rule, while Slater introduced the idea of a virtual oscillator between each two electron orbits. Also underappreciated is how this work stimulated the genesis of matrix mechanics, for Slater's oscillators survived the demise of the Bohr-Kramers-Slater theory and became associated with the transition amplitudes that form the matrix elements in Heisenberg's matrix mechanics.

Under the title "Creative Confusion: Quantum Theory on the Way to Wave Mechanics," Lehner spoke about complementary work on the genesis of Schrödinger's wave mechanics being carried out in Berlin. Interesting and novel in this work is the emphasis on the role of continuing worries after 1910 about the proper physical interpretation of particle indistinguishability in quantum statistics. This problem engaged the attention of Debye, Ehrenfest and Einstein, among others. But it was Schrödinger who achieved the crucial insight in the summer of 1925 during the immediate run-up to the development of wave mechanics—an insight reflected in the fact that N-particle Schrödinger wave functions live not in physical space but in 3N-dimensional configuration space.

Finally Kojevnikov spoke about "'Knabenphysik': The Birth of Quantum Mechanics from a Postdoctoral Viewpoint." While the concept of a "postdoc" hadn't really crystallized in a formal sense by the mid-1920s, the comparative professional youth of the founders of the new quantum mechanics is a striking sociological fact. Of the 80 authors of the 200 most important papers published from mid-1925 through early 1927, the majority were under 30 years of age, and that subgroup authored 65 percent of the papers. Moreover, 60 percent of these authors had received their PhDs after 1920. Heisenberg is typical of these young physicists. Their marginal professional status raises important questions about intellectual independence, and restrictions on where they could use their funding helps explain why Bohr's institute in Copenhagen became such an important center for research in the field. ■

April Contributed-Paper Session I

By David C. Cassidy

At St. Louis, seven speakers enlightened a standing-room only audience on a wide range of topics surrounding "The Manhattan Project and Beyond."

Project veteran E. Leonard Jossem opened the session with "Remembering Los Alamos." He arrived there in July 1945 and began work in the Experimental Physics (P) Division, designing and constructing specialized equipment. He later switched to F-Division where he worked on the "Super" or H-bomb. Jossem was a founding member of the Association of Los Alamos Scientists, which sought to prevent future wars through the international control of nuclear weapons. Among the ALAS initiatives was a packet sent to the mayors of major US cities containing a letter and a piece of fused sand from Alamogordo as a warning of what could happen to their cities in a nuclear war. An important success of the scientists' movement was the achievement of civilian control of nuclear energy under the Atomic Energy Commission.

Harry Lustig (City University of NY, emeritus), addressed the question "Did the Allies Know in 1942 about Nazi Germany's Poor Prospects for an Atomic Bomb?" The probable answer is that British intelligence knew about these prospects "well before 1945" but did not inform their US counterparts until very late. According to Arnold Kramish, in *The Griffin* (1986), German physicist Paul Rosbaud informed the British that the German effort had "ground to a halt" in 1942, but it is not firmly known when he delivered his report. British Scientific Intelligence officer R. V. Jones allowed that his agency was convinced of this fact by 1943. Because the official information about Rosbaud's work is still buried in secret British and American files, Kramish had to base his account on largely undocumented sources. General Leslie Groves was not briefed on the German status until May 1944 but refused to believe it. By then the project could not be stopped, nor probably could it have been in 1943. The full truth of what happened will not be known unless and until the Rosbaud files are declassified. (*Editor's Note:* See Lustig's letter on this subject on p. 15.)

Christine Hampton spoke on "Revisiting the 100-Year-Old Radioactivity Lectures of Frederick Soddy." In 1908 nuclear researcher Soddy (Nobel Prize in Chemistry, 1921) gave six experimental lectures at the University of Glasgow for his wealthy patrons and the general public. They were published a year later as *The Interpretation of Radium*. Since many academics still doubted the existence of atoms, Soddy in these lectures showed that "radium emanation" (radon), which is produced by a series of decays, not only exists but is a true, compressible gas, even though it could not be weighed by standard balances of the day. Soddy also foresaw the potential dangers of the energy released in radioactivity. In 1908 he wrote that it would become a question "of life and death to the inheritors of our civilization." Among the original group of radioactivity researchers, Soddy was the only one to witness the effects of the Manhattan Project and its aftermath.

Cameron Reed (Alma College) examined "Arthur Compton's 1941 Analysis of Explosive Fission in U-235: The Physics." Compton's analysis did not mention the British MAUD Report, which covered the same ground; it may be seen as a precursor to Robert Serber's later *Primer*. Compton's calculation of the critical radius was quite accurate, but that for the critical mass was roughly half the actual value. Neglecting the dependence on initial conditions, his results for the time of core expansion to non-criticality for a bomb of two critical masses of fissile material was very close to the modern value, but the calculated efficiency of 1.4 percent was far short of the modern value of 5.4 percent. Since most of the calculations were not especially difficult and are at the level of undergraduate physics today, one wonders why Heisenberg fumbled them at Farm Hall in his calculation there, and perhaps earlier. Reed's full paper on Compton's analysis was published in the December 2007 issue of *American Journal of Physics.*

Samuel Fletcher (Princeton University), recipient of the Maria Goeppert-Mayer Studentship, spoke on "The Manhattan Project and Its Effects on American Women Scientists." Like most histories, Manhattan Project histories have been concerned with only the leading scientists, who were in most cases men. Not until Ruth Howes and Caroline Herzenberg wrote *Their Day in the Sun* (1999) have the contributions of many of the women scientists to the Project been described. Women made up about 30 percent of the scientific and technical staff. Yet a comparison of salaries and other aspects of their lives and work indicated that cultural values about women in science did not change, although they were accorded sufficient support in their work. As in other areas of the economy, they were regarded as substitutes for men, and many lost their jobs as soon as the wartime labor shortage ended. This study suggests that significant social change is not possible without change in the culture at large (*Editor's note:* See also the related article on p. 8 by Virginia Trimble and George Zimmerman.)

Matthew Geramita (University of Michigan) then spoke on "X-Ray Spectroscopy, the Ellen Richards Prize, and Nuclear Proliferation: The Inspiring Life of Katherine Chamberlain." Chamberlain earned her doctorate at the University of Michigan and continued work there with George Lindsay on X-ray spectroscopy during the 1920s. When she explained a secondary absorption line observed by Dirk Coster, she was awarded the Richards Prize for outstanding young women scientists, which enabled her to work with J. J. Thomson at the Cavendish Laboratory. Returning to the States, she taught photography at Wayne State University in Detroit and wrote a widely read textbook on the subject. The atomic bomb inspired her support of efforts to control nuclear weapons through the popular movement for a world government. Despite the failure of that movement as the Cold War deepened, she remained undeterred in the effort to control these weapons.

Michael Friedlander (Washington University, St. Louis) took the audience "From Alamogordo to the Nuclear Test-Ban Treaty." As nuclear weapons testing continued during the years following Alamogordo, and with attention being drawn to the spread of radioactive fallout by Adlai Stevenson's 1956 presidential campaign, the Greater St. Louis Citizens Committee for Nuclear Information was formed in March 1958. It pursued an active campaign to educate the public on nuclear radiation and on the dangers of fallout. Linus Pauling's petition plus Committee lectures, press information, and Congressional testimony helped to bring about the Limited Test-Ban Treaty of 1963, which may be regarded as the direct result of public concern over fallout. Likewise, criticism by the Committee and Alaskan scientists of the plan to excavate an Alaskan harbor using nuclear explosions inspired public opposition and led to cancellation of the plan. These examples demonstrate that scientists' warnings can have public impact when the scientists stick to the facts and to their expertise, especially when they translate their technical knowledge of a problem into terms the general public can readily understand. ■

Pais Prize Lecture

Continued from page 7

as reflected in his equations, let it be known also that Einstein himself noted (in "*Motive des Forschens*," 1918) that "the supreme task of a physicist," as of any intellectual, is to form "a coherent and lucid world picture"?

And, for that matter, should it not be known also that Einstein urged a fierce defense of science, as well as upgrading the conditions of mankind? Would that not add greatly to the sense of self of future scientists, a sense that may be diminished if they see their main purpose only to do yet another narrow set of assigned tasks? And, just possibly, given this larger self-confidence as sons and daughters of an extraordinary family, would that not allow them, in this era of unreason and neglect, to act when necessary, on behalf of our profession—and beyond?

Dear friends and colleagues, having shared a call of conscience in Bram Pais' spirit, I thank you again for this honor, and for your attention.

—St. Louis, Missouri, 15 April 2008. ■

The Manhattan Project:
The Birth of the Atomic Bomb in the Words of Its Creators, Eyewitnesses, and Historians

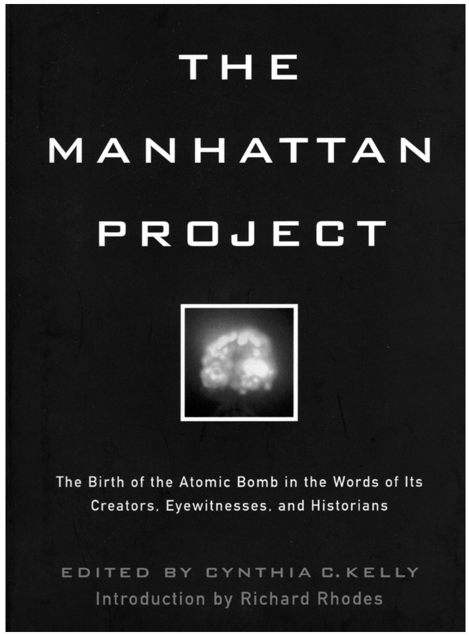
Edited by Cynthia C. Kelly, with an Introduction by Richard C. Rhodes
New York: Black Dog and Leventhal, 2007, photographs, illustrations, xiv+495 pp. , $24.95

Reviewed by David C. Cassidy

The Manhattan Project to build the atomic bomb during World War II has become the paradigm of Big Science. Everything about it was big: the record funding; the 125,000 people from all walks of life devoted to the effort; the unparalleled mobilization of industry, government, and science in the midst of a world war; the stupendous power and destruction unleashed by the weapons produced; and the legacy of profound human, historical, security, and political questions that remain to this day.

The discovery of nuclear fission, the prospect of a bomb, the outbreak of war in Europe, and the start of a German military research effort had all occurred by 1939. But not until 1942 were the Manhattan Project, Enrico Fermi's first nuclear reactor, and the administrative apparatus for the project in place. And not until the following year, with the arrival of General Leslie R. Groves and J. Robert Oppenheimer (see photo on p. 1) in Los Alamos, did the Manhattan Project begin in earnest. By mid-1945 it had produced the uranium and plutonium bombs. Two years later the project was disbanded upon the founding of the Atomic Energy Commission.

Cynthia C. Kelly, president of the Atomic Heritage Foundation, and her advisory staff have assembled in one large volume an amazingly broad and inclusive array of eyewitness accounts, documents, reports, and interviews covering nearly every aspect of the Manhattan Project, from the early inklings of nuclear energy, to the work of building the bombs, to contemporary efforts in both art and history to comprehend the legacy of this transformative undertaking. *The Manhattan Project* is indeed, in the words of Richard Rhodes, a fitting "memorial anthology" to the 65th anniversary of the project's birth.



The 129 items included in this anthology are divided into nine sections, beginning with early thoughts about a fission bomb. This section is followed by others on the administrative maneuvering of 1942; the selection and collaboration of the "odd couple," Groves and Oppenheimer; the experiences of workers at Los Alamos, Oak Ridge and Hanford; the problems of secrecy, spies, and counterintelligence; the Trinity test of July 1945; the dropping of bombs on Japan; reflections of scientists, politicians, and later historians on the use of the bomb; and efforts since the 1950s to counter the threat of nuclear war. The only topics I regret not finding here are the science and the technology of bomb construction. A broadly accessible excerpt from an available source, or an appendix on the physics and its application, would have given readers fuller insight into what the project was all about and why such a massive undertaking was required to achieve its goal.

Essentially all the documents are presented in excerpts from a paragraph to several pages long, encompassing some of the key writings on each of the above topics. This approach works well, as each section provides readers with a broadly rounded introduction to the subject that they could not obtain from a single source. (Those wishing to consult the complete works will find them listed by section in the credits.) For example, the section on life in the secret cities provides a good sense of the "excitement, devotion, patriotism" (Oppenheimer) of the people involved. The section on the various people represented extends far beyond the senior scientists to stories of the many women workers, the male workers' wives, the children, a pipe fitter, the owner of a tea house, the southern blacks recruited to work at Oak Ridge, and a wonderful account of the "Tennessee Girls" hired as machine operators. (I wish there were more about the Native Americans of the Los Alamos region, many of whom were hired as maids and nannies.) Even more, this section provides a sense of the engagement and the nostalgia many later felt for that unique moment when people of all types and from many backgrounds joined together to achieve success in an exciting cause—a generation-defining event reflected perhaps in a similar way in later events such as Woodstock. It was, as Rhodes titles his introduction, "a great work of human collaboration."

But the project also took place within the darker context of war and the likelihood that participants were in a race with the Germans for the most powerful weapon ever devised. Only rarely in the early years does the broader goal of the project, the creation of a new weapon of war, intrude into

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the thoughts and sensibilities of those who knew its ultimate aim. "It was a very good idea," writes Richard Feynman, "although my conscience bothered me a little bit." In effect, the joy and excitement and challenge of the work transcended its goal. For many, the Manhattan Project as a great cause was decoupled from its horrific goal, becoming not only the icon of Big Science but, in the words of George A. Cowan, the symbol for success "in achieving seemingly impossible national objectives."

Only as the project approached its goal and the threat of a German bomb dissolved, did scruples begin to arise about the work. The second half of the book provides an excellent introduction to the dilemmas some of the scientists faced in the midst of the continuing war. Included here are Joseph Rotblat's striking four-page explanation of his decision to resign from the project; an account of Robert Wilson's Los Alamos seminars questioning the aims of the project; the letter transmitting the Franck Report in which, as historian William Lanouette points out, the physicists attempted to regain responsibility for their work; Leo Szilard's petition to President Truman against use of the bomb, signed by 155 physicists; and the government report by Oppenheimer, Fermi, Ernest O. Lawrence, and Arthur H. Compton finding no viable alternative to immediate use of the bomb without warning.

The last two sections of the book contain physicists' reflections on the dropping of the bombs, with excerpts from Oppenheimer's farewell to Los Alamos and efforts by Niels Bohr, Albert Einstein, and Bertrand Russell to achieve international control over the accelerating arms race. Included in these sections are excerpts from recent controversial research that raises questions about the traditional understanding of such topics as Truman's decision to drop the bombs, the necessity of using them to end the war before the planned ground invasion, and the early-1990s controversy over the Smithsonian's *Enola Gay* exhibit. These controversies, like those surrounding the German effort, will likely remain with us for a long time. The threat of nuclear war between nations—or unleashed by terrorists—will likewise remain. Perhaps, as Santa Fe Institute founder Cowan proposes in his concluding contribution, a new Manhattan Project is needed "to explore other forms of power and paths to peace."

The Manhattan Project is an excellent book about a defining event of our times, as well as of contemporary physics. It deserves wide attention.

David C. Cassidy teaches and writes at Hofstra University. He is author of *Uncertainty: The Life and Science of Werner Heisenberg* and *J. Robert Oppenheimer and the American Century*. ■

To the Editor:

At the April 2008 meeting of the American Physical Society I delivered a paper about information received by British intelligence, MI6, during World War II that Germany had given up on building an atomic bomb. (See summary on pp. 12-13.)

The principal informant, according to the book *The Griffin* by Arnold Kramish, was the German science editor Paul Rosbaud. His role was confirmed and praised by Samuel Goudsmit, the scientific head of the American ALSOS mission in Germany during the last months of the war, and by the British wartime intelligence officer, physicist R. V. Jones. While there is good evidence that Rosbaud's revelation came in 1942, official confirmation and details are still being kept secret by MI6, the British intelligence-gathering agency—as is any disclosure of what was done with the information.

I reported that Rosbaud's nephew, Dr. Vincent Frank-Steiner, had filed a complaint in a British court requesting MI6 to confirm (or deny) that a file exists and to put the content in the public domain. Frank-Steiner's attorneys argued that the family had a right to this knowledge and that—more than 60 years after the war's end—the material cannot be properly withheld. The lawyer for MI6 responded that the agency never releases any information unless it furthers British national interests.

At the time of my presentation, I was not aware that a decision had actually been rendered a week earlier. Following a closed hearing on 4 April 2008, at which only MI6 and its lawyers were present, the Court rejected the complaint in a one-line decision that gave no reason. The dismissal had been foreshadowed in a finding on February 26 that held it was proper for MI6 neither to confirm nor deny that such a file exists, whether or not it does. The ruling cannot be appealed in court.

Even if the timely knowledge that Germany was not in fact building an atomic bomb would not have deterred the Anglo-American Manhattan Project, official confirmation of who knew what and when—and the disclosure of how British intelligence handled the information—is of substantial historical and public interest. Members of the Forum and historians in general may therefore wish to consider what they might do to help obtain release of this information. As David Cassidy reported in his introduction to *Hitler's Uranium Club: The Secret Recordings at Farm Hall*, by Jeremy Bernstein, it took the concerted and energetic action of British scientists and historians to obtain the 1992 release of the until-then secret records of the surreptitiously recorded conversations of interned German nuclear scientists.

A similar initiative, joined by American (and other) historians, could again lead to a positive result. Anyone who wishes to comment on this letter or who has a suggestion about how to proceed to try to obtain release of the Rosbaud file can contact me by email at *h_lustig@yahoo.com*, or by writing me at 304 Chula Vista Street, Santa Fe, NM 87501.

—Harry Lustig, Santa Fe, NM

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