History of Physics
NEWSLETTER

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Report from the Chair:

Meeting Challenges to Science

By Bill Evenson, Forum Chair

While retaining high credibility with the public in general, science faces challenges to its authority and reliability on particular issues on many fronts today. These include attempts to subordinate science to political or economic interests, questions about the validity of cosmological and biological evolution, and conflicts over global warming and climate change. A common thread running through all these challenges is that the findings of science have produced a picture of reality that conflicts with preconceptions that support a sense of individual identity or with an existing worldview that is intertwined with economic or political or religious interests.

It is not the role of science—much less the history of physics—to challenge or undermine economic or political or religious commitments. Nevertheless, when these are inconsistent with the findings of science, the foundations on which we build both our identity and the lenses through which we see the world need to be adjusted. This is not to challenge beliefs, but to ask that the interpretations underpinning these beliefs be understood as "interpretations" and hence subject to adjustment in their applications to scientific issues.

So what can history of physics do in the face of these challenges? Non-scientists need exposure to good history of science so they can understand that while scientific conclusions are not final or absolute, nonetheless science is far from arbitrary; it is supported by careful experiments and argument. Real science always includes false starts, errors, and misleading experimental results, and scientific conclusions are uncertain to some degree. But we need to teach students and the general public that not all uncertain knowledge is unreliable or dismissible. There are widely varying degrees of uncertainty in different kinds of knowledge. The strength and breadth of evidence for a theory must be weighed. And many scientific theories, including Big Bang cosmology and neo-Darwinian evolution, are on very firm ground indeed. In short, science needs to be judged on the basis of the strength of the evidence and arguments for its claims. And careful histories of science as it is actually carried out can contribute importantly to the understanding of these issues by non-scientists and scientists alike.

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Forum Chair Bill Evenson, Utah Valley State College

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I therefore urge that we bring our interest in history of physics to strengthen the understanding of science among our students and the general public at every opportunity.

Meanwhile, most of the regular Forum activity takes place among physicists, much of it at physics meetings. You will read later in this newsletter about many splendid sessions organized by FHP at the 2007 March and April APS meetings. More excellent sessions are planned for 2008 (see preliminary plans outlined on p. 3). Please plan to attend the 2008 March meeting (New Orleans, March 10–14) or April meeting (St. Louis, April 12–15) and to contribute to our sessions.

I am especially pleased to report that many more contributed papers in history of physics than ever before were submitted in 2007 for the March and April meetings, enabling us to have two contributed sessions at each one. I urge you to continue this trend and report on your history projects through contributed talks again next year. Any APS member can give a history talk at the March or April meeting in addition to a technical paper. Most of the history talks had to be scheduled for the usual 12-minute APS meeting length in 2007, but we will continue to schedule 24-minute talks whenever possible, depending on the number of talks and the meeting rooms available.

In an innovation this year, we reached beyond the March and April meetings and cosponsored a session at the Frontiers in Optics 2007/Laser Science XXIII meeting on the "history of Bell's Theorem and its experimental verification." This was the APS Division of Laser Science's annual meeting, September 16–20, in San Jose.

Two activities initiated by Virginia Trimble have become very important for the Forum: donated support for students giving contributed talks in memory of a significant colleague who has passed on (John Bardeen and Rolfe Glover Studentships in 2007, thanks to the Bardeen family and to Richard Prange) and donated sponsorships for invited lectures at APS meetings (Franco Rasetti lecture in March 2007, thanks to Bob Resnick, and Samuel K. Allison lecture in April, thanks to Jim Cronin). The donors can choose who is to be honored (among deceased physicists), and the Forum program committee selects the speaker.

I was reminded by the recent death of Ralph Alpher that not all contributions to physics are adequately appreciated and rewarded during the physicist's life. A named lecture or studentship in honor of someone you respect can be appropriate for those who made important contributions, with or without adequate attention during their career. Please consider whether you can make a full ($1,200 to $1,500 for named lectures) or partial contribution and contact me or one of the Forum officers.

The Pais Prize for History of Physics has been even more successful than hoped in calling attention to outstanding career-long work in physics history. The 2007 Prize recipient was Max Jammer, well known "for his groundbreaking historical studies of fundamental concepts in physics, including his comprehensive account of the development of quantum mechanics." He is the author of *Concepts of Space* (Harvard University Press, 1954), *Concepts of Force* (Harvard University Press, 1957), *Concepts of Mass in Classical Mechanics and Modern Physics* (Harvard University Press, 1964), *The Conceptual Development of Quantum Mechanics* (McGraw-Hill, 1966), *The Philosophy of Quantum Mechanics* (Wiley, 1974), *Einstein and Religion* (Harvard University Press, 1999), *Concepts of Mass in Contemporary Physics and Philosophy* (Princeton University Press, 2001), and *Concepts of Simultaneity: From Antiquity to Einstein and Beyond* (The Johns Hopkins University Press, 2006). Truly an outstanding record of achievement.

A Few Reminders: Please remind students interested in the history of physics that the Forum can offer limited travel support to a few of them who wish to present talks at the March or April APS meetings. They should apply be email to me, Bill Evenson, at bill@evenson.ch after submitting their abstracts to APS. Also remember to send a short record of the work of retiring scientists (yourself or colleagues) to the Center for History of Physics, as explained by Virginia Trimble in the last issue of this newsletter. Likewise, continue to send department histories to the Center for History of Physics and JDJackson@lbl.gov. And please nominate your deserving colleagues for APS fellowship—see more on p. 3.

Special thanks are due to now Past Chair Virginia Trimble for her significant leadership as well as financial contributions to the Forum these last three years. And welcome to those who were elected to the Forum Executive Committee in 2007, who took office after the April APS meeting: the new Vice Chair Gloria Lubkin (American Institute of Physics); and Gordon L. Kane (University of Michigan) and George O. Zimmerman (Boston University), both elected to three-year terms as Committee 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.

Editor
Michael Riordan
Institute of Particle Physics
University of California
Santa Cruz, CA 95064
mriordan@ucsc.edu
(831) 459-5687

Associate Editor
Robert H. Romer
Physics Department
Amherst College
Amherst, MA 01002
rhromer@amherst.edu

Editors' Corner

When I began serving as program chair several years ago, our contributed sessions at the annual APS meetings were embarrassing. There were none at the March meeting, in fact, and only a few forgettable papers were delivered in a lightly attended session in April.

How things have changed. Now, thanks to the efforts of our program chairs, the Forum offers contributed sessions at both meetings—and they attract excellent papers and large, engaged audiences. I experienced this personally last April when I gave the first paper in a contributed session. To my surprise and pleasure, the room was packed with more than 50 listeners!

These sessions are a venue where—in keeping with APS traditions—any APS member who wishes can stand up and give a brief talk on a topic in the history of physics. Participatory democracy in action, it helps bring to light subjects that science historians may have overlooked. I experienced this process myself when the next speaker, Ramanth Cowsik of Washington University, lectured about Homi J. Bhabha (well known for calculations of electron-positron scattering) and his 1930s work anticipating the discovery of the muon. I had no prior awareness of it.

The contributed-paper sessions have also become a venue for students interested in the history of physics to air some of their nascent ideas before receptive audiences. Aiding them financially are named "studentships," funded by donations from our members to help cover travel costs. We look forward to continuing this worthy practice.

Brief accounts of these 25 contributed papers are included in this newsletter, written by the chairs of the sessions. In this manner we bring this new work to the attention of all Forum members and perhaps help connect the authors to others with similar historical interests. This is a valuable service the Forum can render its members, fostering further communications on the history of physics.

—Michael Riordan, Editor

Special Manhattan Project Session

By David C. Cassidy

The Forum on History of Physics and the Forum on Physics and Society are co-sponsoring an invited session on the Manhattan Project, to be held during the next APS April Meeting, in St. Louis, MO, on 12–15 April 2008. The speakers will be:

- Cynthia C. Kelly, President of the Atomic Heritage Foundation. Ms. Kelly has been an outstanding leader in recent years of an effort to preserve important Manhattan Project sites. She has organized a number of symposia on various aspects of the Project.
- Val Fitch, Princeton University, who served as an SED (or Special Engineering Detachment, US Army) soldier, one of many who actively participated in Los Alamos projects. He will speak on life at Los Alamos in 1942–1945.
- David C. Cassidy, Hofstra University, a historian of science, who will speak on historical aspects of the Manhattan Project and its impacts on society.

We also invite contributed papers on the Project, which will be included in the contributed papers session to be held during the meeting.

We therefore offer a special invitation to all Manhattan Project physicists who served during the war years to come to this session, as well as to an evening reception to be held in their honor during the meeting. We expect to invite Project veterans to participate in a round-table discussion, which will take place the evening of the reception. Some travel support may be available for such individuals. We ask any interested Manhattan Project veterans to contact the organizers:

- Ben Bederson, New York University, ben.bederson@nyu.edu, 212-995-7695
- David C. Cassidy, Hofstra University, chmdcc@optonline.net. 516-463-5537. ■

Call for Fellowship Nominations

By Gloria Lubkin

The FHP Committee on Fellows is seeking suitable candidates to be named APS Fellows through the Forum on the History of Physics. These nominations should be based on achievements in the history of physics and must be sent directly to the APS office in College Park, Maryland. The criteria for fellowship and complete instructions for submitting nominations are given at: *http://www.aps.org/fellowship/fellinfo.html.*

The FHP unit deadline for the receipt of all materials at APS is 15 May 2008. All nominations submitted to APS will be forwarded to the FHP Fellowship Committee for review. This committee will make its recommendations to the Forum Executive Committee; if approved, the nominations will then go to the APS Council for final approval.

Here are some specific instructions about the nomination process, taken from the APS website. Before submitting your nomination, make sure that the nominee is a member of the Society in good standing. Obtain supporting letters from two sponsors, who do not have to be APS members. Submit a complete original nomination packet (nomination form and supporting letters) and one copy of the entire packet prior to the unit deadline, 15 May 2008. The nomination form may be downloaded from the above website. The nomination materials should be sent to:

Executive Officer
ATTN: Fellowship Program
The American Physical Society
One Physics Ellipse
College Park, MD 20740-3844.

Fellowship nominations may be submitted at any time, but must be received by the deadline for the next review. ■

The 2007 Abraham Pais Prize Lecture: The Historical Development of the Physical Concept of Time

By Max Jammer

The Irish physicist and mathematician John Lighton Synge proclaimed in 1959 that of all physical measurements that of time is the most fundamental and its theory "the most basic theory of all." Twenty years later the Belgian physicist and chemist Ilya Prigogine declared that "the concept of time is much more complex than we thought." Indeed, having studied the basic notions in physics such as space, mass, force, simultaneity and written on each of them a detailed monograph, I postponed a similar treatment of the concept of time because I realized that just by being the "most basic" it is also the most complex of all notions in physics and therefore a rather complicated subject of research. In fact, time, as perceived by us, is both "flowing" and "enduring," and its passing always lasts.

If I venture nevertheless to offer a survey of the conceptual development of the notion of time, I do so because I limit myself to the role of time only in physics and ignore as far as possible any general metaphysical, psychological or biological issues. The presentation thus ignores the history of the notion of time as conceived in the myths and religions of ancient civilizations and begins, after some brief remarks about the Pythagoreans, with the theories of time as proposed by the Pre-Socratics, Plato and Aristotle. After a critical discourse on the early

2007 Pais Prize recipient Max Jammer

proponents of an idealistic interpretation of the notion of time such as that of St. Augustine, medieval theories of time (such as those that proposed the atomicity of time) are discussed. After a presentation of sixteenth century discussions of time (e.g., by Bruno or Gassendi), Barrow's and Newton's theories of physical time are critically analyzed. This is followed by a brief study of the conceptions of time by Locke and Berkeley and subsequently by Leibniz, who is often regarded as the first proponent of a relational or causal theory of time. Following some brief remarks about Hume's conception of time, Kant's critical investigation of the notion of time is analyzed.

There follows a discussion of theories of an "arrow if time" as a result of the existence of irreversible thermodynamic processes. After a brief discussion of Poincaré's thesis of the conventional status of a temporal metric, Einstein's interpretation of distant simultaneity and consequently his definition of time via simultaneity (as presented in his famous 1905 paper on relativity) are discussed. This is followed by some remarks on the concept of time in the general theory of relativity. A brief outline of the role of the concept of time in modern cosmology—in particular, Hawking's notion of "imaginary time"—conclude this essay. ■

Editor's Note: This is the abstract of Max Jammer's Pais Prize Lecture, which was delivered on his behalf by Pais Prize Committee Chair Michael Nauenberg at the APS April Meeting in Jacksonville. The full paper is being published as "Concepts of Time in Physics: A Synopsis," *Physics in Perspective* **9** (2007), pp. 266–280.

Reports from the March APS Meeting
Denver, CO, 5-9 March 2007

The 20th Anniversary of High-T_c Superconductivity: "Woodstock of Physics" Revisited

By Bill Evenson

On Monday, March 5, the Forum sponsored a symposium commemorating the 20th anniversary of the "Woodstock of Physics" session at the March 1987 APS meeting, where early work was announced on high-T_c superconductivity. It was an exciting and wonderfully successful session, with over a *thousand* people in attendance. Organized by Bill Evenson and Paul Grant, the session was chaired by Brian Maple, one of the original meeting chairs.

This anniversary session had eleven 15-minute talks, more like the original "Woodstock" than a regular APS invited session. The March 1987 affair was a post-deadline session with about fifty speakers, most of whom were allowed only seven minutes. It was held in New York City, and about two thousand physicists packed the room and overflowed into the hallway until after 3 a.m., when the session finally ended. The speakers this year were either participants in the 1987 session or deeply involved in those early days, and many others present in 1987 were in the audience for this anniversary. Speakers reminisced about that exciting time 20 years ago and commented on progress since then. To set the stage, Chair Brian Maple (University of California, San Diego) gave an "Introduction and Overview of the 1987 'Woodstock' Session." He showed slides of press clippings and gave a sense of the broad interest and excitement at that meeting.

Georg Bednorz (IBM Research, Zurich Research Laboratory), one of the original discoverers of high-T_c superconductivity who shared the Nobel Prize for this discovery with Alex Müller, talked about "The Discovery of High-T_c Superconductivity and the Countdown to the Rally." He briefly addressed the guiding ideas on the road toward high-T_c superconductivity and the early work at the IBM Zurich Research Laboratory. He spoke about the environment and the decisive circumstances that in January 1986 led to the breakthrough with the discovery of superconductivity in cuprates. The pre-"Woodstock" period, which lasted less than a year, covers the time in which the Zurich team tested different La_2CuO_4-based compounds, confirmed the Meissner effect, and studied flux-trapping in these new materials. As news of the discovery started to spread during this period, the IBM/Zurich group experienced mixed reactions, ranging from silent skepticism to polite but cautious congratulations. This changed dramatically into excitement with the confirmation of the IBM/Zurich results by the Tokyo and Houston groups led by Tanaka and Chu, and culminated in the launching of the new field at the famous March 1987 meeting after the discovery of the 90 K superconductor. After that, it was "off to the races!"

C. W. "Paul" Chu (University of Houston, Hong Kong University of Science and Technology and Lawrence Berkeley National Laboratory) spoke about "High T_c: The Discovery of RBCO." (RBCO stands for R = rare earth, B = barium, C = copper and O = oxide.) In his abstract, Chu wrote: "It was said by Emerson that 'there is no history; there is only biography.' This is especially true when the events are recounted by a person who himself has been heavily involved, and the line between history and autobiography can become blurred. However, it is reasonable to say that discovery itself is not a series of accidents but an inevitable product of each development stage of scientific knowledge, as was also pointed out by Holton." The discovery of RBCO high-T_c superconductors was no exception. In his presentation, Chu briefly recounted several events that were crucial to the discovery: those occurring before 1986 that sowed the seeds for the group's later high-temperature superconductivity research; those in 1986 that were critical to its discovery of the 93 K RBCO soon after Bednorz and Müller's discovery of the 35 K high-T_c superconductor; and those that came in 1987 after the barrier of the 77 K boiling temperature of liquid nitrogen was finally overcome.

Douglas J. Scalapino (University of California, Santa Barbara) described "Some Prehistory to Woodstock" in relating the story of two talks that provided a preview of the excitement that was to spill over at the 1987 gathering. The first was an unscheduled talk on LaBaCuO by Prof. K. Kitazawa on 5 December 1986 at a Materials Research Society symposium on superconducting materials held in Boston. The second was a quasi-public disclosure by Chu at UC Santa Barbara in late February 1987 regarding his work on superconductivity above 77 K.

The next seven talks described high-T_c activities that immediately followed the announcement of the discovery by Bednorz and Müller: "The 1987 High-T_c 'Woodstock' Session and High T_c at IBM," by Paul M. Grant (IBM Research Staff Member, Emeritus); "Bell Labs and High T_c," by Robert J. Cava (Princeton); "High T_c and Condensed Matter Theory in 1987," by Marvin L. Cohen (University of California, Berkeley); "Early High-T_c Activity in Japan: The Franco Rasetti Lecture," by Shoji Tanaka (Superconductivity Research Laboratory/ISTEC, Tokyo, Japan); "High T_c at BellCore," by Laura H. Greene (University of Illinois at Urbana-Champaign); "High T_c at Stanford," by Aharon Kapitulnik; and "High-T_c Superconductivity—1987," by Douglas Finnemore (Iowa State University). Space does not permit full discussion of these talks, but the early

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work of the Tanaka group in Japan warrants additional comment. (Note that Tanaka gave the Franco Rasetti Lecture, funded in honor of Franco Rasetti through a much-appreciated donation from Robert Resnick).

Tanaka writes: "From 1960 to 1980, R&D on superconductivity in Japan was carried out mainly to improve A15 superconducting wires and magnets. Improvements of wires were made mainly in the National Institute for Metals, and improvements of superconducting magnets were made in the Japan Atomic Energy Research Institute for future nuclear-fusion reactors, the National Railway Laboratory for future maglev trains and also in the Electro-Technical Laboratory for magneto-hydrodynamical generators. I began the study of BPBO ($BaPb_{1-x}Bi_xO_3$) in 1975 and at that time the research on oxide superconductors was limited to my laboratory in the University of Tokyo. During the study of this new superconductor, we learned quite a lot on how to make ceramic samples as well as how to measure electrical conductivity and magnetic susceptibility at low temperatures. In 1982, Prof. S. Nakajima organized a rather small group for investigating 'New Superconducting Phenomena,' and I became a member of the group. In 1985, Nakajima expanded the research group to include more than five experimentalists and five theoreticians. The research was on new superconducting materials, with funding from the Ministry of Education of Japan. In late October 1986, we followed the first paper of Bednorz and Müller, and immediately obtained high-temperature superconductivity and reported it at a group meeting in early November. In early December, we confirmed that $La_{2-x}Ba_xCuO_4$ was a true high-temperature superconductor, with a critical temperature of 28 K. *Asahi Shimbun*, Japan's largest newspaper, announced this result in its science section, and many people knew that high-temperature superconductivity had been discovered. Then many physicists and chemists rushed to this field, and many new kinds of materials were soon synthesized. The Ministry of Education, the Ministry of International Trade and Industry, and the Agency for Science and Technology began to make new development plans of their own. Superconductivity fever had reached Japan."

Many, many laboratories around the world made important contributions to the science of high-T_c superconductivity. Although only a small sample of the many early researchers, the speakers in this session were able to recall some of the excitement of the 1987 "Woodstock of Physics" gathering. As Doug Finnemore, wrote, "The discovery of superconductivity in the cuprate class of conducting oxides brought a flash of sunlight onto one of the fields of condensed-matter physics that many of us had thought was rather mature and fairly well understood. In addition to opening a whole new class of materials to the study of correlated motion of charge carriers, it opened a new mind-set that materials with complex chemical bonding can lead to totally new phenomena. The tasks of materials preparation escalated, and with it came the development of

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Condensed Matter Physics at Synchrotron Facilities: History as Prologue to the Future

By Bill Evenson

On Wednesday, 7 March 2007, the Forum and the Division of Physics of Beams jointly sponsored a symposium on Condensed Matter Physics at Synchrotron Facilities. It was organized by Catherine Westfall, who enlisted the help of Denis McWhan and David Moncton—who chaired the session. About a hundred persons attended.

The session led off with Joachim Stöhr of the Stanford Synchrotron Radiation Laboratory speaking on "Soft X-Ray Science: From Photon Drought to X-Ray Lasers." He defined soft X-rays as 0.2–2.0 keV photons, with wavelengths of about 1–10 nm-which require the use of grazing-incidence optics. Stöhr described the major technical developments, beginning in about 1975, that made high-intensity soft X-rays available, today with meV spectral resolution, picosecond pulse lengths and nanoscale spot sizes. He pointed out why soft X-rays are so useful: large X-ray absorption cross sections; narrow lifetime widths; important absorption edges of elements in the spectral range; large resonance and polarization effects; and nanometer-scale wavelengths that allow nanoscale imaging. Their tunable energy and polarization permit control of electronic core-to-valence transitions that provide access to the fundamental charge and spin properties of valence electrons in matter. The large cross sections associated with absorption-edge resonances provide sensitivity to small numbers of atoms, as are encountered in nanostructures, ultra-thin films, interfacial layers and surfaces. Presently, the most advanced experiments use sophisticated spectro-microscopy and lens-less coherent imaging techniques with nanoscale spatial and picosecond temporal resolution. In summary, soft X-rays offer capabilities complementary to hard X-rays; spectroscopic studies reveal atom-projected charge and spin properties of valence electrons; microscopic studies reveal charge and spin distributions at the nanoscale; and time-dependent studies examine nanoscale dynamics at intervals down to tens of picoseconds. On the horizon are experiments with soft X-ray lasers which, among other things, will provide femtosecond snapshots of matter.

John Hill of Brookhaven National Laboratory spoke next on "Inelastic X-ray Scattering." The technique of

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inelastic X-ray scattering probes the dynamics of a system—its atomic and molecular excitations. Hill gave a broad overview of the field, its history, some recent experiments, and a brief look at the future. Inelastic X-ray scattering experiments may be divided into roughly two classes; those performed with meV energy resolution, which observe phonons and other collective ionic motions, and those performed with eV resolution, which look at electronic excitations. The first phonon experiments were done 20 years ago, and two recent examples were presented, one a study of liquid Al_2O_3 and the other observing the surface of liquid indium. The first electronic measurements were performed 40 years ago, and two recent examples were presented, one a study of excitons in organic semiconductors, and the other of mid-infrared excitations in cuprates. Inelastic X-ray scattering as a complement to neutron scattering has been suggested for many years, with first attempts dating back to the 1980s. The advent of hard X-ray third-generation synchrotron light sources has allowed its establishment as a routine, powerful technique for condensed-matter studies. It has enabled important breakthroughs in our understanding of phonon-like excitations in disordered materials and matter under extreme conditions. In looking to the future, Hill noted some recent developments at existing sources and pointed to proposed new X-ray sources, such as the NSLS-II project, which would lead to large gains for inelastic X-ray scattering.

The third talk was on "Surface Structure as a Foundation of Nanotechnology," by Ian Robinson of the London Centre for Nanotechnology and Diamond Light Source. The three generations of synchrotron sources achieved to date—parasitic, dedicated and undulator-based—have each revolutionized research in X-ray diffraction. Surface-structure measurements, demonstrated already with Coolidge-tube sources, benefited from the enormous flux gain of the first generation. Dedicated second-generation light sources such as NSLS allowed in-situ surface preparation and reliable, steady beams to be available when a surface was ready to measure. Third-generation sources, such as the Advanced Photon Source, had enormously improved brightness and coherence, and thus allowed access to the surfaces of nanoparticles. Robinson illustrated how these technological advances led to two significant scientific breakthroughs. The concept of crystal-truncation rods led to new views of how the surface is a modification yet still an extension of the bulk crystal structure. And the development of lens-less coherent X-ray diffraction imaging has allowed access to the structure of nanocrystalline materials by three-dimensional phase mapping of the particle interiors. The structural principles of these new nanomaterials are now being investigated using these new methods.

Next, Denis McWhan of MIT spoke about "Magnetic X-Ray Scattering." He compared magnetic scattering during the period 1965–1981, before synchrotron sources became widely available, and in 1985–1992. During the 1980s, three factors converged: the development of synchrotron sources; the development of techniques to grow new materials layer by layer; and the

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March Contributed-Paper Session I

By Bill Evenson

On Tuesday afternoon, March 6, the Forum held the first of two contributed-paper sessions at the APS Meeting in Denver. Chaired by Bill Evenson, it included six history talks witnessed by an audience of about 50.

Jean-François Van Huele of Brigham Young University led off with an interesting talk entitled, "The Missing Part in the Story of Spin: What is the Spin Content of Stern-Gerlach?" During the development of the idea of spin in quantum mechanics, after the Stern-Gerlach effect was known, this effect did not seem to influence the conception or acceptance of this idea, he explained. Although the experiment is widely interpreted today as a manifestation of spin, it was not seen that way initially and did not influence the development of the quantum-mechanical concept of spin. Van Huele examined the connection between spin and the Stern-Gerlach effect and reviewed the lack of mutual influence in the publication record, giving possible historical reasons for the absence then of what seems an obvious connection today.

Supported by a Bardeen Studentship, Cesar Rodriguez of the University of Texas, Austin, spoke on "The Entangled Histories of Physics and Computation." He focused on how the histories of physics and computation intertwine in a way relevant to quantum computation. Leibniz not only pioneered calculus but also left his footprint in physics and invented the concept of a universal computational language. This idea was further developed by Boole, Russell, Hilbert and Gödel. Boltzmann and Maxwell established the foundations of information theory, as later developed more fully by Shannon. Partly stimulated by World War II, von Neumann and Turing also played important roles in the field. Recently, new cryptographic developments have led to a reexamination of the fundamentals of quantum mechanics, and quantum computation is discovering a new perspective on the nature of information itself.

"Einstein's Jury: Trial by Telescope" was the topic of a talk by Jeffrey Crelinsten, author of the recent book, *Einstein's Jury: The Race to Test Relativity* (Princeton University Press, 2006). He addressed the process of acceptance of special and general relativity. Relativity was poorly understood between 1905 and 1930, and Einstein worked hard to make it more accessible to scientists and scientifically literate laypeople.

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March Contributed-Paper Session II

By Catherine Westfall

The contributed-paper session held by the Forum on Thursday morning, March 8, included four history papers, summarized below, and three others. About 25 people attended the session. The papers were interesting and the audience receptive, which made for a friendly and lively session.

The session began with a paper by Willem van de Merwe and Todd Ream (Indiana Wesleyan University) titled "Compartmentalization of Science, Power, and Social Responsibility as Exemplified in the Life of J. Robert Oppenheimer." Using recent biographies of Oppenheimer as a starting point, the presentation centered on his pre-Los Alamos years, noting his attendance as a child at the Ethical Cultural School in New York City, his studies as a physics graduate student in Europe, his sympathy with left-wing politics as a young professor, and his passion for poetry—including the *Bhagavad-Gita*—that endured throughout his life. Using Oppenheimer as an example, the authors concluded that having a high-quality liberal arts education *can* help physics students formulate a framework for a more meaningful career in science.

The next paper, "The English Revision of *The Blegdamsvej Faust*," by Karen Keck (Net Advance of Physics), added a different twist to the blending of science and the liberal arts. She addressed the updated version of Goethe's *Faust* presented at the 1932 meeting of quantum physicists at Niels Bohr's Copenhagen Institute. As Keck pointed out, the most widely read version of the play, which features Wolfgang Pauli tempting Paul Ehrenfest to accept a chargeless, massless particle, was an English translation of the German original provided by George Gamov's second wife for his book *Thirty Years that Shook Physics.* Although this portrayal of the play is well known, Keck provided a fascinating analysis of how Barbara Gamov rearranged and added to the parody to strength the similarities between it and Goethe's original and to reflect her husband's views, particularly on the international and cooperative aspects of physics.

The third paper in the session, "An 18th Century Thermometer Recipe," took us back in time and to another venue. Presenter Durruty Jesús de Alba Martinez (University of Guadalajara) told the story of a manuscript containing instructions on how to build a thermometer that was found in the Special Funds Collection of the Jalisco's State Public Library and attributed to Francisco Javier Clavigero (1731–1787). He was an important early educator at a the Colegio de Santo Tomàs, a Jesuit institution that provided college-level education before the opening of the University of Guadalajara. This manuscript is intriguing because it was inserted into a vellum-bound volume and is in Spanish, not Latin as in the rest of the volume. The paper explained how the instructions were actually used to construct the thermometer and speculated about how the manuscript could have been used as the experimental part of a physics course.

The session ended with a paper that took the audience in yet another direction, "Historical Perspectives on Respiratory Fluid Dynamics and Flow Phenomena Deep in the Lung." Authors Josue Sznitman (ETH Zurich) and Akira Tsuda (Harvard School of Public Health) gave a historical review of the 30-year-long study of respiratory fluid dynamics and flow phenomena deep in the lungs. The authors noted that after the first pioneering work on flow resistance in passageways, researchers conducted studies elucidating the nature of airflow in the upper (nose, larynx) and conducting passageways. At first, relatively little attention was given to the airflow in the deeper regions of the lung, characterized by 300 million pulmonary alveoli providing gas exchange with blood. For a long time the argument prevailed that airflow velocities in the alveolar region are negligible due to a large increase in the total cross-sectional area at that level; in fact, this view is still taught in medical schools. In the last 20 years, however, new theories have been developed to explain the experimental observation of convective mixing of inhaled particles deep in the lung. These theories suggest that convective airflow in the alveolar region is in fact relevant, and posit that alveolar flows are much more complex than previously thought, perhaps even exhibiting chaotic flow. The authors concluded that such discoveries constitute a small revolution in our understanding of respiratory flows deep in the lung. ■

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totally new spectral probes of the electron gas and the electronic structure in metals."

That same Monday, March 5, the Division of Condensed Matter Physics held a special evening session on "50 Years of BCS Theory," celebrating the 1957 publication of "Microscopic Theory of Superconductivity" (*Phys. Rev.* **106**, 162) by John Bardeen, Leon N. Cooper and J. Robert Schrieffer. Several hundred attended the session, which was chaired by Charles Slichter. It featured three speakers: Douglas J. Scalapino (University of California, Santa Barbara) on "The Impact of the BCS Theory on Condensed Matter Physics"; John M. Rowell (Arizona State University) on "The Impact of the BCS Theory on 50 years of Superconductivity and Condensed Matter Physics"; and Gordon Baym (University of Illinois at Urbana-Champaign) on "BCS—from Atoms and Nuclei to the Cosmos." They discussed the profound influence of BCS theory throughout our field, from condensed-matter physics to elementary-particle theory and cosmology. ■

Reports from the April APS Meeting
Jacksonville, FL 14-17 April 2007

Building the Elements: 50 Years of B^{2}FH Nucleosynthesis

By Virginia Trimble

In 1957 there appeared two papers that were, on the one hand, summaries of many years of work by the authors and others on the origins and abundances of the chemical elements, and, on the other, the stage-setter for the next 50 years of the study of nucleosynthesis. One of the papers, by Alastair G. W. Cameron, was a Chalk River Report (CRL-41, with a brief summary in *Publications of the Astronomical Society of the Pacific* **69** (1957), 201), not widely read at the time. The other paper, by E. Margaret Burbidge, Geoffrey R. Burbidge, William A. Fowler, and Fred Hoyle, was published in the open literature (*Rev. Mod. Phys.* **29** (1957), 547) and quickly became known throughout the community as "B^{2}FH."

It was to celebrate the 50th anniversary of those events and to gauge the progress made since and give perspectives for the future that the double session "Building the Elements: 50 Years of B^{2}FH Nucleosynthesis" was co-sponsored by the Forum on History of Physics and and the Division of Astrophysics. The good news is that Geoff Burbidge was there (as our Samuel K. Allison Lecturer, sponsored by Jim Cronin of Chicago—where, it turned out, the Burbidges had actually known Allison during their post-doctoral years). The bad news is that we never managed to persuade APS that the "2" in "B^{2}FH" should be a superscript, in the APS Bulletin or anyplace else. The sessions were superbly chaired by Robert V. Wagoner, himself a pioneer in nucleosynthesis calculations. Part of another story, perhaps, is the fact that Allison's advisor at Chicago was chemist William D. Harkins, apparently the first person to compile a table of abundances of the elements ordering them by nuclear rather than chemical properties. Allison's thesis work was an attempt to liberate alpha particles from mercury vapor using a strong electric discharge. They didn't make any, but that was a lot to expect for any experiment in those pre-Cockcroft–Walton days.

The single detail I carried away most carefully from the talks was Geoff's indication that they and Cameron had been aware of each other's work and in communication well before publication. The details of time scale of writing and publication of the B^{2}FH paper was driven not by the competition (consider Darwin and Wallace and all!) but by the departure from Cambridge of Fowler and Hoyle to attend the July 1957 Vatican Conference on Galaxies, so that the final draft was completed by the Burbidges. My own involvement with the subject began with a 1974 NATO summer school on nucleosynthesis in Cambridge (which was published as Trimble, *Rev. Mod. Phys.* **47** (1975), 877)

David Arnett (University of Arizona), who was a student of Cameron and has been making elements in stars since 1967 (well, perhaps in models of stars; computers in those days were too small to hold real stars) set the stage for the sessions. He mentioned the essential roles of Hans Suess and Harold Urey in compiling an accurate table of "cosmic" abundances, and of Edwin Salpeter in recognizing the need for helium-burning via a triple-alpha process to get to heavy elements. New in the 1957 papers were the processes we now call (following B^{2}FH more closely than Cameron) slow and rapid neutron capture, *p*- (for proton, though it might now be seen as spallation of neutrons or even neutrino-induced reactions), and alpha- (now divided into C, Ne, O, and Si burning) processes, and a mysterious *x*-process responsible for the light elements (which we now attribute to the early universe and to cosmic-ray spallation). Arnett noted that Cf-254 was then assigned the role in supernova light curves that we now attribute to Ni-56 and that there were uncertainties in the primary source of neutrons used in the *s*-process.

The great triumph of these 1957 syntheses was in providing a process and a plausible site for all the nuclides in existence to be made in at least approximately the right proportions. It is now essential, Arnett said, to incorporate a better description of convection than the faithful old mixing-length approximation and to do three-dimensional stellar evolution calculations, if only to be sure that putting in more physics doesn't reduce agreement between observations and predictions. He showed a number of historical photographs, including the classic photo of B^{2}FH grouped in correct order around the model steam train engine presented to Fowler on his 60th birthday, celebrated in Cambridge, where the four of them first met in the 1950s—and where, a dozen or so years later I first met them (as well as Wagoner and Arnett) collectively.

Brian Fields (University of Illinois) tackled the essential topic of Big Bang nucleosynthesis (BBN), not considered at all in the 1957 papers, which specifically addressed processes occurring only in stars. He reminded us that Fowler and Hoyle had made key contributions to modeling cosmological nucleosynthesis, including the first modern calculation aimed at a universal He/H = 0.1 ratio (Hoyle & Taylor, *Nature* **203** (1964), 1018) and the first to produce a bit of Li-7 as well as the H and He nuclides (Wagoner, Fowler, and Hoyle, *Ap. J.* **148** (1967), 8). Fields generously concluded that: (a) as currently understood, BBN and results from analyzing the cosmic microwave background (CMB) radiation are mostly in good accord; (b) considerations of nucleosynthesis by themselves measure the baryon density of the universe; and (c) there is perhaps a lithium problem,

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in that the so-called Spite plateau in old, non-convective stars falls at a smaller abundance than the BBN prediction, which is consistent with the other light nuclei. This discrepancy could be a hint of something exciting, for instance a dark-matter component consisting of SuperWIMPs, whose early annihilation could have destroyed some Li-7. The obvious alternative, some settling in the stars concerned, sounds just as exciting to me (but then I'm an old fashioned astronomer, much attached to stars). Fields reminded us that the baryon supply deduced from BBN (and the CMB) is considerably larger than the amount detected in stars, with the rest to be distributed somehow among hot X-ray gas, a warm-hot ionized medium, and cooler Lyman-α clouds. Other issues to be sorted out include the possibility of a Li-6 plateau in old stars, seeming to require a cosmological component, and the production of deuterium in stellar flares and such. This is small now, or we would see lots of 2.2 MeV photons, but could have been non-negligible in population-III stars.

Chris Fryer (Los Alamos National Laboratory), who makes supernova models, mostly the core collapse sort, began by remarking that B^2FH had left the details of supernova explosions as an exercise for the reader, and that one-dimensional models had done such a good job for decades that there was little incentive to explore more complex configurations. He then discussed some of the observational indicators of dynamically and chemically important asymmetries in real supernovae and their remnants, including the mixing of ejected layers in SN 1987A and in the Cas A remnant, and the extreme jet structure that arises from collapse to a black hole and from the merger of neutron stars in production of gamma-ray bursts. Some of his own calculations (and those of Maeda and others) have already shown that critical products, such as the ratio of Ti-44 to Ni-56 and the *r*-process yield, change significantly with asymmetry, because some stuff is ejected that would otherwise fall back, and conversely. Speaking from the audience, Stirling Colgate (also of Los Alamos), a pioneer of supernovae and their deeds, expressed some worry about the robustness of an *r*-process that seems to depend a good deal on environmental details, given that the product ratios are so constant from star to star, even among old metal-poor ones. Clearly work remains to be done, even on the classic *s*, *r*, and *p*-processes, including, as Fryer noted, a new *rp*-process.

Dieter Harmann (Clemson University) addressed gamma rays as an indicator of nucleosynthesis. This may strike you as an odd combination, but it is not if you want to know what is going on at present! An atom of Fe-56, Al-27, or Ti-48 has nothing to tell you about its age and little about where it was synthesized. But unstable atoms such as Ni-56, Al-26, and Ti-44 decay with half-lives short by astronomical standards, typically to excited product states that de-excite, emitting gamma-ray lines in the MeV range, so that we know "when" (now) and "where" (the direction of the source) this happened. A little less directly, we can see Co-56 and later Co-57 powering the light curves of core-collapse supernovae. Among the items he noted were: (a) the uniqueness of Cas A as a Ti-44 source (it is odd in other ways as well); (b) the joyous detection of Co-56 gamma rays just months after visible light and neutrinos arrived from SN 1987A (thanks to some hard-working balloons plus a lucky satellite launch soon after); and (c) the (I think!) remarkable result that Al-26 is largely made by massive stars (versus, e.g. novae) because the gammas coming from its decay co-rotate with the inner disk of the Milky Way—a discovery that required the high energy resolution of the satellite INTEGRAL. Hartmann also noted that a great deal more could be done with gamma rays from these and other unstable nuclei if detectors were flown with comparable energy resolution and much larger collecting areas.

Tammy Smecker-Hane (University of California, Irvine) addressed the largest unsolved problem in our understanding of nucleosynthesis—putting all the processes and their sites together to account for the full range of stellar populations in galaxies as a function of redshift. Her own work focuses on the Milky Way and nearby (mostly dwarf) galaxies, where one can, to a certain extent, resolve individual stars and measure their ages as well as compositions. Even quite small, simple-looking galaxies have had multiple star-formation events, and the products can be shown to have differing amounts of input from the nuclear reactions characteristic of stars of different initial masses and compositions and, especially from core-collapse versus thermonuclear-explosion supernovae. An important idea with which she left us is that, at present, *ad hoc* parameter-fitting models of galaxy evolution (where you choose an initial mass function, rate, duration, and so forth for each episode of star formation) adequately describe the range of galaxies and star populations observed. But she looked forward to a time when nucleosynthesis and chemical evolution join smoothly onto models now being calculated for galaxy formation, which start with gravitating dark matter, follow the inflow of baryons into the resulting dark halos, and all the rest, so that star-formation histories are physically determined rather than just parametrically fit.

The final speaker, Geoff Burbidge himself, is an unregenerate dissenter from the majority view that a hot, dense early universe was responsible both for the light nuclei and for the isotropic microwave radiation. Instead, he attributes them to processes in active galactic nuclei, probably associated with whatever produces the large (non-cosmological) redshifts seen for these objects, in turn as part of a cyclic model of the universe called "quasi-steady state." He began with a retrospective of the situation around 1950, when George Gamow was trying to make all the elements by neutron captures in the early universe, but falling into the holes at A = 5 and 8—though Hoyle had already shown in 1946 that conditions inside highly evolved stars might, uniquely, reach the high densities and temperatures required to fuse atoms up to and beyond iron. Burbidge also gave a straightforward, if skeptical, summary of conventional

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History of Gravitational Waves and General Relativity

By Bill Evenson

On Sunday, April 15, the Forum on History of Physics and the Topical Group on Gravitation cosponsored a symposium on the "History of Gravitational Waves and General Relativity," chaired by Kameshwar Wali. About 150 people attended.

The session began with a talk titled "Traveling at the Speed of Thought: Proving the Existence of Gravitational Waves," by Daniel Kennefick (University of Arkansas). He pointed out that gravitational waves (GW) represent an almost unique instance of unfinished business in the history of modern physics. One of numerous novel concepts that arose in the revolutionary period of the early 20th century, gravitational waves retained their place in the new physics for nearly a century in the complete absence of experimental confirmation. Thus it was only natural that their theoretical development was marked by repeated debate over whether they really existed or played any kind of role in astrophysical systems. He explored how physicists came to accept the theoretical rationale for this phenomenon in the absence of experimental evidence—until recently, for example, in the case of binary stars. The name "gravitational wave" was coined by Henri Poincaré, in a positive use of the electromagnetic analogy. But Max Abraham noticed that dipole waves did not exist and suggested that the electromagnetic analogy therefore argued *against* GW. In 1916 Einstein claimed in a letter to Schwarzschild that GW did not exist, but by mid-1916 he had changed his mind due to the linearized approximation to general relativity, and in 1918 he produced the quadrupole formula. Skeptics tended to focus on points of *dis*-analogy with electromagnetism, while GW advocates hewed to the positive points of analogy. In 1936 Einstein and Rosen became convinced that GW did not exist; their paper submitted to *Physical Review* generated critical comments from the referee and a controversy when Einstein objected to having his work reviewed! Kennefick discussed the course of these controversies (including the quadrupole-formula controversy). For more on this history, consult his recent book, *Traveling at the Speed of Thought: Einstein and the Quest for Gravitational Waves* (Princeton University Press, 2007).

Richard Isaacson (formerly of the National Science Foundation) delivered the next talk, "Development of LIGO: A View From Washington," in which he painted an interesting picture of the life and challenges of a large project, the Laser Interferometer Gravitational-wave Observatory, as viewed from inside the federal funding system. LIGO is an audacious project attempting both to confirm the existence of gravitational waves and to harness them as a new probe of the cosmos. Achieving its demonstrated sensitivity required that many technologies be advanced orders of magnitude beyond the state of the art when the project began. The development of the LIGO project transformed gravitational physics from a small-scale, individual-investigator effort into a major new international big-science collaboration. For three decades, the participant community experienced all the struggle and pain that normally accompany such a transition. The small-science phase began in the early 1970s with a $53,000 NSF grant to Rainier Weiss. The transition to big-science funding occurred in 1993 with a multi-year $271,900,000 commitment. In 2007 the LIGO laboratory employed 180 people, and a total of 540 are participating in the LIGO Science Collaboration. LIGO has become NSF's most expensive project. It has been a high-risk, high-reward gamble, always full of high promise that has yet to pay off. Construction of this frontier facility required a 100-fold expansion of the annual NSF budget for research in this subfield. In the face of this challenge and opportunity, the U.S. government invested scarce research funds with vision and patience, according to Isaacson, and managed a new, long-term, risky, and expensive investment with wisdom. Hallmarks of the project's development have been bottom-up setting of priorities, long-term continuity at the NSF, a small core of expert NSF staff members, university-managed R&D and science, community access, NSF oversight that included getting good people involved but then staying out of the way (which Isaacson called "trust but verify"), and acceptance of the argument that solving cosmic mysteries is a good justification for allocating scarce resources to such a risky new endeavor. He concluded that such a high-risk, high-cost project could probably not be brought up this way in today's constricted environment.

The final talk in this session was titled "Anecdotes, Facts, Opinions and some History of the Theory of Relativity," by Ezra T. Newman (University of Pittsburgh). He gave a brief summary of the history of relativity, including well-known and some less-known facts about this history. Newman mentioned two footnotes on special relativity: that Jean Eisenstadt has claimed that in 1786 Robert Blair proposed what later became known as the Michelson-Morley experiment; and that Woldemar Voigt demonstrated the Lorentz transformation and stated a universal speed of light in an 1887 article. Newman referred to the Einstein-Cartan letters as sources for understanding Einstein's many attempts to generalize general relativity (GR) to unified field theories. An early success of GR was Schwarzschild's spherically symmetric solution in 1916. No one initially understood the nature of the Schwarzschild singularity, said Newman, but Kruskal, Finkelstein, and Wheeler cleared this up in a way that led to modern black-hole physics. Newman mentioned Einstein's argument with de Sitter that led to the cosmological constant and its relevance to modern cosmology. The period from 1930 to 1950 was one of stagnation in GR, he observed. Around 1935 Einstein doubted the possibility of observing gravitational lensing, except from the sun, but the following year Fritz Zwicky argued that it could and must be observed or the theory was wrong. In the early 1950s, there was a renaissance in GR, as Bondi's mathematical clarifications opened up the field again and brought great support for the idea of gravitational waves. ■

March Contributed-Paper Session I

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Its acceptance was largely due to the astronomy community, which undertook precise measurements to test Einstein's astronomical predictions. The well-known 1919 eclipse measurements that made Einstein famous still did not convince most scientists to accept relativity, said Crelinsten. The 1920s saw numerous attempts to measure the gravitational bending of light, as well as solar line displacements and even an aether drift. He discussed how astronomers approached the "Einstein problem" in these early years before and after World War I, and how the public reacted to what they reported, as well as how this work helped to shape attitudes we hold today about Einstein and his ideas.

"Forty Lost Years of Coherent States" was the topic addressed by Kavan Modi University of Texas, Austin, recipient of a Bardeen Studentship. He pointed out that Schrödinger introduced the minimum-uncertainty state in 1926 in his effort to satisfy the correspondence principle. But it was almost forty years later, in 1963, that Glauber put these states to use in what is now known as the quantum theory of optics, giving them the name used today, "coherent states." Soon thereafter, Sudarshan completed Glauber's unfinished work in achieving the full theory of quantum optics. Crucial mathematical work had been done in the intervening years so Glauber could make use of these states. Modi discussed what Schrödinger had been trying to do, why Glauber was attracted to these states, and why they were forgotten for almost forty years.

Norman Redington spoke on "The Reception of the Kaluza Theory in Britain, 1921–1958." Kaluza's five-dimensional unified theory was part of a wider program to geometrize physics that was largely abandoned in the wake of the mid-1920s successes of quantum mechanics. However, a small group of British physicists continued to work on the subject through the middle decades of the 20th century. Redington reviewed their efforts and the reception of these ideas by the physics community.

The final talk was "On the Origins of the Raman Effect" by Somaditya Banerjee of the University of Minnesota, recipient of a Bardeen Studentship. He reviewed the events that led to the discovery of the Raman effect by C. V. Raman and K. S. Krishnan at Calcutta in 1928. He presented evidence that although the effect was generally seen as providing strong evidence for the quantum nature of light, Raman himself was a staunch supporter of the classical wave theory of light. Banerjee placed this historical analysis in the context of a larger project seeking to understand the role of Raman scattering in the experimental verification of the quantum dispersion theory of Hendrik A. Kramers, which formed a conceptual bridge between Bohr and Sommerfeld's "old" quantum theory and Heisenberg's matrix mechanics. ■

Condensed Matter Physics at Synchrotron Facilities

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realization that X-rays could probe the magnetic properties of materials. In addition to magnetic X-ray scattering, most magneto-optical effects have been extended from the visible to the soft X-ray region. Because both beam energy and polarization are tunable, synchrotron sources are element- and site-specific probes—and there are large resonant enhancements in the scattering or absorption cross sections at atomic absorption edges. Synchrotron radiation is routinely used to study the magnetic polarization of different components of a material and to separate their spin and orbital angular momentum densities. It allows one to probe magnetic polarization level-by-level (*p*, *d*, or *f*) and component-by-component using resonant scattering. It has also been used to determine the magnetic polarization at interfaces and surfaces and magnetism in extreme environments using small samples. In addition, synchrotron radiation can be used to determine the interplay between the atomic, orbital and magnetic ordering in materials. Future possibilities include further development of the spectroscopic aspects of magnetic scattering and probing magnetism on smaller length scales and at shorter time intervals.

The final talk was on "The Use of Coherent X-Ray Beams to Study the Dynamics of Soft Condensed-Matter Systems" by Sunil Sinha of the University of California, San Diego. One of the most powerful techniques for studying dynamics in soft condensed-matter systems has been dynamical light scattering. Over twenty years ago, it was recognized that a similar application of X-rays (in order to achieve shorter length scales and avoid problems of multiple and stray particle scattering) could open up whole new areas of research. But the potential of coherent X-rays was not anticipated when high-brightness third-generation synchrotron radiation sources were planned! Nevertheless, their usefulness in this field has been enormous, making it possible to deliver intense beams of highly coherent X-rays and enable many new applications of X-ray scattering. In particular, the technique of X-ray photon correlation spectroscopy (XPCS), the X-ray analog of dynamical light scattering, has now become an exciting new research area with primary applications in soft condensed matter. The first observation of speckle by diffraction of coherent X-rays was reported in 1991, and a rapid expansion of this field followed. Current applications include studies of dynamical fluctuations in colloids and polymers and of surface fluctuations in liquid films and membranes. XPCS has yielded interesting new results on these systems, which are difficult if not impossible to obtain by other techniques. In the future, physicists anticipate XPCS with completely coherent beams and time resolutions down to nanoseconds. ■

Sputnik, 1957: Its Effect on Science in America

By Bill Evenson

On Monday, April 16, the Forum presented a symposium on "*Sputnik*, 1957: Its Effect on Science in America," in recognition of the upcoming 50^{th} anniversary of this pivotal event in the history of the Cold War. It was chaired by Bill Evenson, with about 250 people in attendance.

John Rigden (Washington University, St. Louis) led off the session with a talk on "Eisenhower, Scientists, and *Sputnik*." On October 4, 1957, the Soviet Union launched a 184-pound satellite, *Sputnik*, into earth orbit. This surprising event had a tremendous impact on Americans, for it called into question the capability of U.S. science vis-à-vis that of the U.S.S.R. President Dwight D. Eisenhower called "his scientists" to the Oval Office on October 15, and a meeting took place that Hans Bethe called an "unforgettable hour." I. I. Rabi, Chairman of the Office of Defense Mobilization Science Advisory Committee, made several proposals that the President accepted immediately. Rigden reviewed those initiatives, which were intended to raise the standards of U.S. science and to reassure the American public that the country was not falling behind the Soviets. These measures included appointment of a full-time, cabinet-level Science Advisor to the President, formation of the President's Science Advisory Committee (PSAC), and undertaking an intensive study of ways to strengthen U.S. science. Today, we can still feel the legacy of *Sputnik* and of the wise actions of President Eisenhower and "his scientists" in response to it.

"*Sputnik's* Impact on Science Education in America" was the subject addressed by Charles Holbrow (MIT and Colgate University). Before the launch of *Sputnik* in October 1957, pressure had been rising to mobilize America's intellectual resources to be more effective and useful in dealing with the Cold War. *Sputnik* suddenly released that pressure by stirring up a mixture of American hysteria, wounded self-esteem, fears of missile attacks, and a deep questioning of the intellectual capabilities of a popular, democratic society and its educational system. After *Sputnik*, the federal government took several remarkable actions. Besides Eisenhower's establishment of PSAC and the Presidential Science Advisor, the House and the Senate reorganized their committee structures to focus better on science policy. Congress created the National Aeronautics and Space Administration (NASA) and charged it to develop a civilian space program. It also tripled the funding for the National Science Foundation to support basic research and to improve science education and draw more young Americans into science and engineering. And Congress passed the National Defense Education Act, which involved the federal government to an unprecedented degree with all levels of U.S. education. Holbrow closed by reviewing the important effects of these post-*Sputnik* changes.

Roger Launius (Smithsonian Institution, National Air and Space Museum) wrapped up the session by addressing "An Unintended Consequence of the IGY: Eisenhower, *Sputnik*, and the Founding of NASA." The circumstances that resulted from the Soviet launch of *Sputnik* (an International Geophysical Year, or IGY, scientific satellite) led to numerous actions in the United States aimed at "remediating" the resulting Cold War crisis. This included the establishment of NASA, a civilian space agency charged with conducting an official program of scientific and technological space exploration, consolidation of Department of Defense space activities, the passage of the National Defense Education Act, plus a host of other actions. Launius discussed the fascinating politics of these changes, which have been interpreted as an appropriate political response to a unique crisis situation. Interest groups, for differing reasons, prodded national leaders to undertake large-scale efforts, something the President thought unnecessarily expensive and once set in place would be almost impossible to dismantle. But was the *Sputnik* "crisis" truly a crisis in a real sense? Or was it blown up into one by interest groups who used it for their own ends? Launius briefly traced some of the major themes associated with the IGY and *Sputnik*, and described the political construction of the crisis as it emerged in late 1957 and early 1958. He also discussed the transformation of federal science and technology that took place in the aftermath of *Sputnik* and how it set in motion a series of processes and policies that did not fully unravel until the end of the Cold War. ■

Building the Elements

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cosmology (including the discovery of the 2.3 K temperature of interstellar space by McKeller and Adams, though not Herzberg's spectacular misjudgment of it). But he reiterated his opinion that the best places to make helium are in *known* objects, such as stars and active galaxies, and he reminded the audience that, if you flash one-quarter of the hydrogen in the universe to helium, the resulting energy density comes quite close to that of the 2.7 K CMB radiation, about 4.5 x 10^{-13} erg/cm^3—a fact he regards as a key to understanding the universe today.

Science is a self-correcting process, though the time scale is occasionally very long. Sometimes the last supporters of an idea die, leaving no heirs (a suggestion Geoff attributed to me, though it goes back at least to Planck and perhaps Maxwell); sometimes people change their minds (well, Dennis Sciama did, though he remains nearly unique); and sometimes a new synthesis appears that reveals truth in each of two or more opposing views—think of mantle convection and plate tectonics

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April Contributed-Paper Session I

By Paul Halpern

On Saturday afternoon, April 14, the Forum sponsored the first of two contributed-paper sessions on the history of physics. It featured a wide range of topics, focused on 20th century elementary particle physics, quantum physics and astrophysics. In all, seven contributed papers were delivered, including two by promising young undergraduates who received Rolf Glover studentships to support their travel to the APS meeting.

The session began with a talk by Michael Riordan (University of California, Santa Cruz), "Toward the Standard Model: The Transformation of Particle Physics Experiments, 1964-1979," detailing how collider experiments in the 1970s unraveled the substructure of hadrons. He described the gradual steps by which experimenters used colliding-beam machines to create high-energy collisions, examining the by-products using electronic detectors that increasingly surrounded most of the solid angle around the collision site. From the hard-scattering events that were observed, they concluded that protons, neutrons and other hadrons possessed internal components, then called partons. Physicists came to identify these constituents with the quark model, proposed in 1964 by Murray Gell-Mann. Hence collider experiments of the 1970s offered the major evidence for the emerging Standard Model of particle physics.

The second presentation, "H. J. Bhabha and the Birth of the Second Family of elementary particles," given by Ramanath Cowsik (Washington University), concerned physicist Homi Jehangir Bhabha's explorations from December 1936 until October 1937 of the properties of the particle now called the muon. He demonstrated how Bhabha cleverly anticipated the muon's existence as a charged particle similar in many ways to the electron but much more massive. In the 1940s, Bhabha's hypothesis would be verified by the muon's experimental discovery.

The third paper, by Rolf Glover Studentship recipient Dan Maronde (who delivered it) and Costas Efthimiou (both from the University of Central Florida), addressed the "History of the Heisenberg Uncertainty Relation." Maronde detailed the story of how Werner Heisenberg came to propose the well-known inequality involving pairs of observables in quantum mechanics.

In the fourth presentation, "Heisenberg: Paralleling Scientific and Historical Methods," Calla Cofield (University of Massachusetts, Amherst), another recipient of a Rolf Glover studentship, examined questions related to the gathering of information about the history of science and speculated about whether these historical methods could be rendered as rigorous as those used to validate scientific hypotheses. She framed her arguments in the context of the controversies surrounding the life of Werner Heisenberg, described how very few historians have interviewed members of Heisenberg's family, and spoke about her own interview with Jochen Heisenberg, the late physicist's son and a professor of physics at the University of New Hampshire.

Next, Virginia Trimble (University of California, Irvine) presented "Early Photons from the Early Universe," a history of various twentieth century efforts to detect and interpret the isotropic background electromagnetic radiation of the universe—stemming from both cosmological and non-cosmological sources. She focused on the confusion caused by background starlight having an equivalent temperature near 3 K (close to the 2.7 K cosmic microwave background radiation), the lack of appreciation of the discovery by Canadian astrophysicist Andrew McKellar that interstellar molecules are at about the same temperature, and two (of about seven) likely radio pre-discoveries of the cosmic background radiation from France and Bell Laboratories itself, the latter by Edward Ohm.

The sixth talk, "From Spectrum Analysis to Spectrochemical Analysis: Redefining the Boundaries of Spectroscopy," was presented by Mina Park (Seoul National University). It detailed spectroscopists' efforts in the 1930s and 1940s to expand the boundaries of their field beyond conventional applications in atomic physics and physical astronomy. As Park pointed out, the enormous success of quantum mechanics in theoretically modeling atomic spectra reduced the need for spectroscopy in that domain. Spectroscopists then turned to spectrochemical analysis as a new avenue for research, a change also motivated by spectroscope makers' need to find novel applications for their products.

Finally, Michael Nauenberg (University of California, Santa Cruz) talked about "Who First Discovered the Mass Limit of a Degenerate Star?" He explored the work of University of Leeds physicist Edmund C. Stoner, who became interested in Ralph Fowler's application of the exclusion principle to highly dense white-dwarf stars, for which classical physics was inadequate. In the early 1930s, Stoner developed several critical papers on white-dwarf mass limits. As Nauenberg pointed out, Stoner's publications preceded Subrahmanyan Chandrasekhar's better-known work on this subject, and Chandrasekhar used Stoner's relativistic equation of state in his work. Hence, the "Chandrasekhar limit" was probably anticipated by Stoner's results.

Overall, it was a very successful session with excellent attendance and many intriguing questions raised by audience members, who packed the room. The talks offered a splendid overview of some of the highlights of 20th century discoveries in physics, from the postulation of uncertainty in quantum mechanics and the development of the standard model of particle physics to formulation of theories of stellar collapse and the analysis of the cosmic background radiation permeating the universe. We commend, in particular, the student presenters who demonstrated exceptional promise in their research, and wish them well in further explorations of the history of physics. ■

April Contributed-Paper Session II

By David C. Cassidy

A lively round of seven 12-minute contributed papers and one 24-minute paper kept the audience on its toes on Monday, April 16, during the APS Jacksonville meeting.

Clayton A. Gearhart (St. John's University, Minnesota) opened the session with the longer paper on "Walther Nernst, Albert Einstein, Otto Stern, and the Specific Heat of Hydrogen." In 1911, following Einstein's 1907 quantum theory of specific heats of solids, Nernst suggested a related theory for the specific heat of hydrogen gas in which, surprisingly, the rotational energies were not quantized. Two years later Einstein and Stern published an improved treatment of the problem, in which they suggested, even more surprisingly, that Planck's newly introduced zero-point energy might eliminate the need to quantize physical systems. Gearhart's paper brought together a number of previously explored facets of quantum history, showing in the end how unsettled quantum theory was at that time.

Danian Hu (City College of New York) described in his paper "Einstein in the City" Einstein's first visit to the United States in April 1921, when he appeared at an American scientific gathering at City College. He had chosen that venue for its role in the education and Americanization of many of the impoverished immigrants from Eastern Europe. Einstein began his long-time association with philosopher Morris Cohen and physicist Reinhart Wetzel at City College. More broadly, the reception he received in the United States indicated the lagging acceptance and comprehension of his relativity theories there, as compared with Europe, at that time.

Paul Halpern (University of the Sciences in Philadelphia), in "Einstein's Viscous Advice Flowed Freely Nonetheless," examined a letter from Einstein to a mysterious J. Lens, dated 13 November 1930. Halpern's detective work revealed the recipient as Jan Lens, a medicinal chemistry student in Utrecht who had asked Einstein for advice for his doctoral thesis on the properties of lyophilic colloids. The problem concerned the "Einstein relation," his 1906 formula (revised in 1911) for the viscosity coefficient of fluids containing small colloidal particles. In the letter, he offered suggestions about a possible extension of his formula to larger particles with more complex interactions. Halpern observes from this episode not only Einstein's "free-flowing generosity" toward students, but also his continuing interest in this practical topic while working intently on the theoretical problem of unifying gravitation and electromagnetism.

James Beichler (West Virginia University at Parkersburg), "The Unfinished Revolution: Einstein's Revenge," offered a thought-provoking extension of Thomas Kuhn's theory of scientific revolutions. According to Kuhn, revolutions follow from crises, yet, Biechler notes, crises are often recognized only after the revolution has occurred. Instead, Beichler argues, revolutions arise from the success of the previous science, which carries in it the seeds of revolution in the form of unsolved problems. He pointed to the example of Newtonian science, which—although hugely successful—never resolved the dichotomy of mind and matter. While the 20th century revolutions in physics and psychology have weakened the dichotomy, he said, they have not removed it, thereby sowing the seeds of the next revolution.

In a joint paper with Catherine Westfall (Michigan State University) entitled "Fermi's Conundrum: Proliferation and Closed Societies," Wendy Teller examined a letter of 4 January 1946 sent by Congresswoman Emily Taft Douglas to Enrico Fermi. As the result of lobbying efforts, Douglas was attuned to the emerging problems of science and international relations. In the wake of Hiroshima and Nagasaki, she was among the first to realize the danger of proliferation through the clandestine diversion of nuclear materials. She was also among the earliest to recognize the need for inspections to maintain international control, and wrote to Fermi for advice. His pessimistic response revealed the position of one of the key figures in American nuclear policy at that time. Only the free exchange of information and protection of people reporting violations would provide adequate controls, Fermi believed, but some governments would almost certainly not abide by these conditions.

William Mendoza (Defense Trade Controls Policy, U.S. Department of State) delivered "Physics in the International Arms Control Effort: A History and Introduction," co-authored with Ann Ganzer and Amy Westling. Mendoza reviewed the history of US arms control, going back as far as 1775, up to the Arms Export Control Act of 1976, the U.S. Munitions List, and the Missile Technology Control Regime Annex, all of which involve strong connections to physics in both theory and measurement. These laws defined various categories of weapons under control as technologically advanced, from nuclear weapons and missiles to the hardening of electronics and the development of night-vision devices.

Ruprecht Machleidt (University of Idaho) presented "Evolution versus Creation in the Public School Curriculum: History of the Legal Battles." These battles have been centered on the Establishment Clause in the First Amendment to the Constitution, he noted, dividing the history into three phases. The first extended from the Scopes Trial in 1925 to the 1968 negation of the Butler Act banning evolution; the second, 1970–1987, entailed the mandate of equal time for evolution and creationism. The third phase, from 1990 to 2005, saw rise of the intelligent design movement, which attained particular success in the Dover, PA, school district. After a six-week trial, the movement was so soundly defeated that it did not appeal to the Supreme Court. Although creationism in schools has been defeated in each instance, he said, a fourth phase is likely to appear.

In his well-researched paper, "Founding *The Physical Review:* American Physics in the 1890s," Guy Emery (Bowdoin College) offered an insightful account of the early years of the journal, founded at Cornell by Edward

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Building the Elements

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as an umbrella under which Neptunists and Plutonists, uniformitarians and catastrophists could all have lived (and if you don't know what they thought, you should, as we are after all a history forum!). So perhaps some day, something coming out of the multiverse concept, or branes, or string theory, or eternal inflation, or. . .will be able to reconcile seemingly diametrically opposed concepts of cosmology. That multiple ideas continue to be heard is a vital part of that process, so it should not surprise those who were not there to hear it that Burbidge received the longest and loudest of the rounds of applause in the sessions, though most of us firmly disagree with him! ■

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L. Nichols in 1893. Emery noted that *PR* originally carried advertising and included obituaries, book reviews, and letters of correspondence. In addition, 6 percent of the articles were written by women, reflecting the large number of women physicists then at Cornell. In his analysis of their content, Emery identified three themes of contrast, circa 1900: elitist physicists versus the non-elite (the *PR* then had a relatively non-elitist attitude); the United States versus Europe; and classical versus modern physics. He is presently exploring all these themes in greater detail. ■

Student Travel Grants

The Forum on History of Physics announces the availability of a small number of travel grants for students presenting contributed or invited papers in Forum-sponsored sessions during the March or April 2008 APS meetings. For more information, contact David C. Cassidy, chmdcc@optonline.net. ■

The Seven Pines Symposium

By Roger Stuewer

The eleventh Seven Pines Symposium was held May 2–6 on the subject of "Emergence: From Physics to Biology." This annual gathering is dedicated to bringing prominent historians, philosophers, and scientists together in a collaborative effort to probe and clarify significant foundational issues in science, as they have arisen in the past and continue to challenge our understanding today. The meetings occurred at the Outing Lodge at Pine Point near Stillwater, Minnesota, a beautiful facility surrounded by spacious grounds with many trails for hiking and birding. This idyllic setting and the superb cuisine available make the Lodge an ideal location for small meetings. Lee Gohlike, its owner and the founder of the Seven Pines Symposium, outlined its goals in his opening remarks.

Unlike in typical conferences, the talks are limited to 30 minutes, with twice as much time devoted to discussions following the talks and long midday breaks to permit small groups to assemble at will. As preparation for the talks and discussions, the speakers prepare summarizing statements and background reading materials that are distributed in advance to all participants. This year 22 leading historians, philosophers, and scientists were invited to participate in the symposium.

Each day the speakers set the stage for the discussions by addressing major historical, philosophical, and scientific issues pertaining to the central subject of the symposium. Thus, the morning of Thursday, May 3, was devoted to the general topic of "Conceptual Framework: Concepts of Emergence," with Jeremy Butterfield (Cambridge University) speaking on "Illustrations from Physics" and Kenneth Schaffner (University of Pittsburgh) talking about "Illustrations from Biology." That afternoon the general topic was "History of Concepts of Emergence," with Michael Silberstein (Elizabethtown College) concentrating on physics and Manfred Laubichler (Arizona State University) on biology. On the morning of Friday, May 4, speakers addressed the question "Can Chemistry be Fully Reduced to Physics?" with Jeffrey Ramsey (Smith College) and Eric Scerri (UCLA) offering two different perspectives. That afternoon Kenneth Waters (University of Minnesota) examined the question "Can Biology be Reduced to Physics and Chemistry?" while Lee Gohlike gave a fascinating talk on "The Evolution of the Mercedes Racing Car, 1901–1914." The morning of Saturday, May 5, was devoted to the topic of "Emergence and Reductionism," with Leo Kadanoff (University of Chicago) concentrating on physics and Ricardo Azevedo (University of Houston) on biology. That afternoon the topic was "Complex Networks," with Stuart Kauffman (University of Calgary) focusing on physics and Michael Travisano (University of Minnesota) on biology. Roger H. Stuewer (University of Minnesota) chaired the closing discussion on Sunday morning, May 6.

The twelth annual symposium founder Gohlike has had a lifelong interest in the history and philosophy of science. To plan the annual symposia, he established an advisory board consisting of Stuewer, Chair; Michel Janssen (University of Minnesota), Vice Chair; John Earman (University of Pittsburgh); Geoffrey Hellman (University of Minnesota); Don Howard (University of Notre Dame); and Robert M. Wald (University of Chicago). Also participating in the eleventh annual Seven Pines Symposium were Mark Borrello, Alan Love, Antigone Nounou, and Serge Rudaz from the University of Minnesota, and Philip Stamp and William Unruh from the University of British Columbia. The twelfth annual Seven Pines Symposium will occur May 7–11, 2008, on the subject, "The Known and Unknown Universe." ■

Out of the Shadows:
Contributions of Twentieth-Century Women to Physics

Edited by Nina Byers and Gary Williams
Cambridge University Press, 2006, 471 pages, illustrated, $35.00.

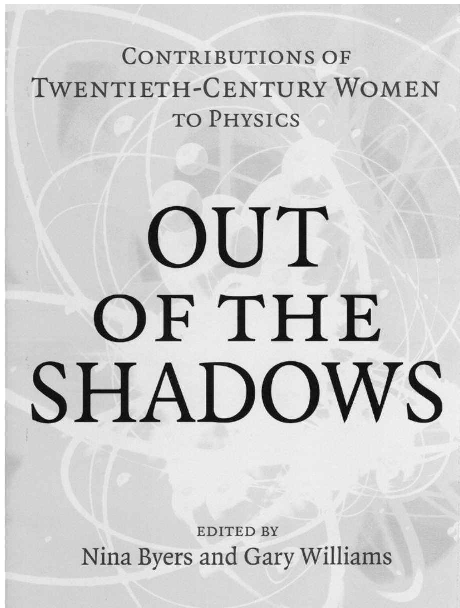
Reviewed by Eugenie V. Mielczarek

Out of the Shadows is a compilation of the scientific contributions made by forty women physicists and of the suffocating discrimination they experienced. Compilations are an important condensed archival source tracing history and providing references. The strength of this book is as a historical record of this discrimination.

It's all here: nepotism rules or skittering around them, which left women employed but unpaid for as long as thirty years; denial of tenure until elected to the National Academy of Sciences, awarded the National Medal of Science, or recognized with prestigious honors from foreign scientific societies; and exclusion from the Nobel Prize. Even the benign announcement of an impending marriage was enough to bring the Dean of Barnard to request Harriet Brooks, who had worked with Ernest Rutherford and J. J. Thomson, to resign.

More than a record of historical facts, biography is a celebration of human endeavor. For women who dreamed about studying physics, the exemplars of physical scientists were all men. There were few mentors but lots of anti-mentors, men who actively discouraged women. In the 1960s a dean of students confided that "I always try to talk them—prospective women science majors—out of it." Women reading these stories will experience painful *déjà vu.*

As a child I was inspired by a biography—of Louis Pasteur. I imagined how wonderful it must be to spend a life deciding what mysteries of science were the most important and solving them. Later as a teenage in the 1940s, I was warned that thinking about physics or even worse attempting to make a living at it, was socially unsuitable for a woman. But my father Theodore Vorburger, who was comptroller of the American Institute of Physics,



hired several girlfriends and me as summer employees to straighten out the membership and journal subscription lists. Often a supervisor would announce that someone like Fermi or Oppenheimer was in the building, and I could glimpse these famous men on the grand wooden staircase of the AIP's New York offices. One morning I met Melba Phillips on these stairs; she was tall and standing very straight. Although we exchanged few words, this chance meeting strengthened my resolve. Here was a *woman* physicist, and I was thrilled to meet her. Although her life was an inspiration to many physicists like me, she was not included in this volume.

The barriers chronicled in this book seem unbelievable in 2007 but how far are we removed from them? While writing this review, I told a friend how Lise Meitner was required to hide under a seat in the physics lecture hall. She responded with some of her own experiences as a science major at a large public northeastern university during the mid-1950s. Students in a required chemistry course were seated segregated by gender, but her first name was genderless and she found herself among the men. After discovering this, the professor quickly reorganized the class so she was seated among the women. And upon meeting her embarrassed male advisor from the physics department, he at first explained that he did not accept female advisees, but he reluctantly made an exception in her case.

Among the successes of *Out of the Shadows* are the selection of contributing authors and a seamless editorial transition from chapter to chapter. These forty sketches, each about 3000 words long, are written with obvious endearment by a cross section of colleagues and family members. Each woman's life and scientific contribution are described, with excellent references to her publications and other biographical material. Compilations can be dry and hurried. Starting with excellent informative introductions, Byers and Williams have produced a lively, important historical record. All scientists will find it useful and fun to read.

However, there are a few surprises, including a curious comment by Freeman Dyson in foreword. He divides women into those who dedicate their lives to science, giving Emmy Noether and Marie Curie as examples, and those "who worry about making a living and raising a family under modern conditions." Would anyone preface a compilation of biographies of male scientists with these constraints? Dyson partly redeems this gaffe by describing with heart-warming prose several women physicists, including his mentors Mary Cartwright, Vanna Cocconi, and Cecile DeWitt-Morette. But in describing Cocconi he explains that for "any young woman who chooses Vanna as a role model, the first priority should be to find a husband like Giuseppe."

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Out of the Shadows

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And for whom is this book intended? Before reading it, I expected it had been written to inspire women age 16 and older—the formative years of learning and career decision-making, an age when such a compilation would have been very important to me, for one. Pioneering can be a lonely and daunting endeavor. I would have loved to study such a record when I was in high school. But for the most part, the level of scientific description in each chapter presupposes a fair knowledge of physics. The scientific contribution of each woman is written for other scientists. A younger audience will come away mystified.

I reluctantly concluded the book was written for working women scientists, as a celebration of women's contributions to physical science—and not primarily to inspire scientifically unsophisticated youth. Thus I strongly encourage Byers and Williams to consider selecting perhaps twenty of these women and to publish an edition written for high-school level readers. It is important to open this record to them, too. For as Chen-Shing Wu once said, "Never have so few contributed so much under such trying circumstances." (quoted by Sharon Bertsch McGrayne in *Nobel Prize Women in Science*, 2nd edition, Citadel Press, 1998, p. 8). ■

Eugenie Vorburger Mielczarek is emeritus professor of physics at George Mason University. With Sharon Bertsch McGrayne, she has written a popular science book, *Iron: Nature's Universal Element.* She has also reviewed four biographies of women scientists for *Physics Today* and the *American Journal of Physics.*

We Hear That…

Spencer Weart will be stepping down from his position as Director of the AIP Center for History of Physics when someone can be found to replace him, according to reliable sources at AIP. That may take awhile, however. It's difficult to imagine the Center without him—especially as he's headed it for more than three decades. As a teacher of the history of physics at Stanford and UC Santa Cruz, I especially appreciate the award-winning web site on the subject that Spencer was instrumental in creating and developing. The best source on the history of physics available on the web, I tell my students. He's also played a big role as a member of many Forum committees, and we hope he will continue participating after he leaves the Center.

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The long-awaited history of Fermilab, *The Ring of the Frontier: The Rise of Megascience at Fermilab*, by Lillian Hoddeson, Adrienne Kolb and Catherine Westfall, has been accepted for publication by University of Chicago Press. But given the pace of university-press publishing, it will probably be another year before the book is in print. Watch for a review in these pages.

. . .

We also suspect that Robert Crease of SUNY Stony Brook is getting close to publishing the second volume in his series on the history of Brookhaven National Laboratory, to accompany his excellent 1999 book, *Making Physics*, but have not heard anything definite along these lines. Crease has been nominated by the Forum as an APS Fellow.

Forum member Luisa Bonolis writes to say that her history of the Italian electron-positron collider AdA (Aniello di Accumulazione), "Bruno Touschek versus machine builders: AdA, the first matter-antimatter collider," was recently published in *Rivista del Nuovo Cimento*, Vol. 28, No. 11 (2005), pp. 1-60. Conceived by Touschek in the 1950s, AdA was the immediate precursor of the much larger machine ADONE ("big AdA") at Italy's Frascati laboratory. Electron-positron colliders have long since become one of the principal workhorses of high-energy physics.

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On the solid-state physics front, watch for my two articles on the central role of Fairchild Semiconductor Corporation in the development of the silicon microchip and the establishment of Silicon Valley. The first, titled "From Bell Labs to Silicon Valley," appears in the Fall 2007 issue of *INTERFACE* magazine, while another will be published in the December 2007 issue of *IEEE Spectrum* on the planar processing technique that made it all possible. Physicists Robert Noyce, Jean Hoerni and Jay Last made crucial contributions to silicon microchip technology, which has had such a tremendous impact on modern society.

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Finally, we sadly note the passing on 12 August 2007 of Ralph Alpher, who with George Gamow and Robert Herman wrote the famous 1948 *Physical Review* paper, "The Origin of Chemical Elements," which adumbrated the modern Hot Big Bang theory that dominates cosmology today. ■

—Michael Riordan, Editor

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History of Physics
NEWSLETTER

Forum on History of Physics
American Physical Society
One Physics Ellipse
College Park, MD 20740