History of Physics
NEWSLETTER

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Report From The Chair

by Robert H. Romer, Amherst College, Forum Chair

2005, the World Year of Physics, has been a good one for the History Forum. I want to take advantage of this opportunity to describe some of FHP's activities during recent months and to look forward to the coming year.

The single most important forum event of 2005 was the presentation of the first Pais Prize in the History of Physics to Martin Klein of Yale University. It was only shortly before the award ceremony, at the Tampa meeting in April, that funding reached the level at which this honor could be promoted from "Award" to "Prize." We are all indebted to the many generous donors and to the members of the Pais Award Committee and the Pais Selection Committee for their hard work over the last several years that has led to the establishment of the first-ever History of Physics prize. Professor Klein delivered his Pais Prize lecture ("Physics, History, and the History of Physics") at the Tampa meeting, in a session jointly sponsored by the Forum on Physics and Society, a session at which the Szilard Lecture was also given. We thank FPS for generously sharing their time with us; the combination of Pais and Szilard lectures in the same session seems to be an event well worth continuing.

And now the second Pais Prize winner has been announced — John L. Heilbron of UC-Berkeley. Professor Heilbron will officially receive his prize in the spring of 2006. All those who wish to suggest names of possible future awardees are encouraged to send suggestions to Professor Michael Nauenberg of UC-Santa Cruz, who now becomes chair of the Pais Selection Committee.

The Forum sponsored several sessions of invited lectures at the March meeting (in Los Angeles) and the April meeting (in Tampa), which are more fully described elsewhere in this Newsletter. At Los Angeles we had two invited sessions under the general rubric of "Einstein and Friends." At Tampa, we had a third such Einstein session, as well as a good session on "Quantum Optics Through the Lens of History" and then a final series of talks on "The Rise of Megascience." A new feature of our invited sessions this year is the "named lecture." The purpose of naming a lecture is to pay tribute to a distinguished physicist while simultaneously encouraging donations to support the travel expenses of speakers. This new idea was suggested by Virginia Trimble, and as a result of action by the APS Council any forum is now authorized to have one such talk at any meeting at which it sponsors invited sessions. Naming the talk honors the physicist so designated, but the talk need not be on the particular topic on which that person did his or her work. Thanks to the generosity of the Goldhaber family, we were able to provide travel support to Lillian Hoddeson whose April talk was entitled "Megascience on the Prairie: The Powers and Paradoxes of Pushing Frontiers at Fermilab (The Gertrude Scharff-Goldhaber Lecture)." And I myself was pleased to be able to honor my own thesis adviser, Robert H. Dicke, by assisting with the travel expenses of A. J. Kox, whose talk at the March meeting was called "Einstein and Lorentz (The Robert H. Dicke Lecture)." We hope that named lectures will continue, with other physicists being honored in this way in future years; send

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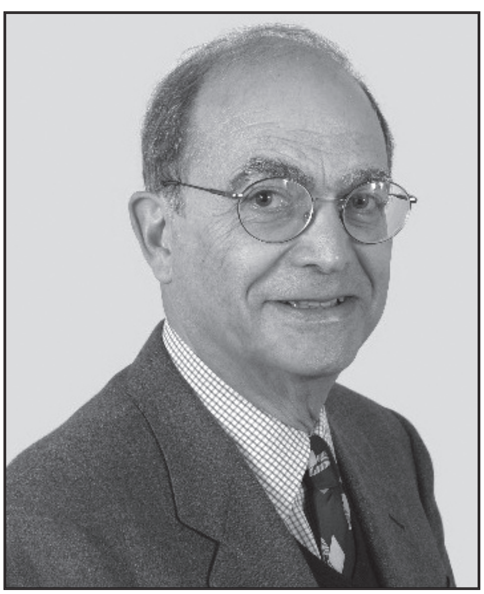

John L. Heilbron, winner of Pais Award in History of Physics

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your suggestions to Bill Evenson, who will become Program Chair next year, with responsibility for organizing talks for 2007.

Contributed papers are on the increase, with eight at the Los Angeles meeting and twelve at the Tampa meeting. In recent years, we have been able to assign a 24-minute block of time to each contributed paper, twice the usual time. APS's willingness to do this, however, is contingent upon our having only a relatively small number of submissions. Of course we hope to attract more and more contributed papers, even though the result may well be a reduction in time allowed for each paper.

Several of us have recruited quite a few new members for the forum by rather aggressively passing around sign-up sheets at various meetings. We invite those interested simply to put down their names, even if they think they may already be members of the forum; we then send the lists to College Park, where APS staff will check for duplication and then enroll the new ones. In my own department, I acquired half a dozen new members simply by walking the hall and asking my colleagues. In this small department, I was embarrassed to learn that I was the only forum member. I should have taken that trip before, and I urge forum members to follow my example. Remember that membership in the first two fora is free, and even beyond that, the annual cost of an additional one is only $7.

At the April business meeting, it was my pleasure to deliver an APS Fellowship certificate to Bill Evenson, who did valiant work for the forum as newsletter editor for many years and is now once again a member of our executive board, this time as Vice-Chair. At the April meeting, we also said farewell, for the second time in twelve months, to Ken Ford, who has done noble work as Secretary-Treasurer, retiring from that important position in the spring of 2004, only to be pressed into an emergency 9-month stint beginning in the summer of 2004. Ken, on behalf of all forum members, thank you again!

Forum finances are in rather good condition at the moment. We were able to offer more generous than usual travel support to some of our invited speakers this year, and we are also able – for now – to continue to print the Newsletter in paper as well as electronic form twice a year. However, the long term outlook is not so clear. The current happy situation is highly dependent on donations from a number of benefactors; I want to mention in particular the financial help provided by Virginia Trimble, without whom we probably could not afford to produce paper copies of this newsletter.

I will briefly mention several other new ventures. The Historic Sites Committee has now chosen an initial set of sites and is at work arranging for the installation of plaques. Thanks to a donation from the Bardeen family, we provided partial travel support to the March meeting for one student who was giving a contributed paper. And in cooperation with the APS Topical Group on Gravitation, we were able to help send speakers to a number of institutions, primarily four-year colleges. For reports on both of these programs, see Virginia Trimble's reports in this issue.

History of Physics NEWSLETTER

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John Heilbron Recipient of Pais Prize

by Allan Franklin, University of Colorado Boulder

John Lewis Heilbron, Professor Emeritus of History and History of Science at the University of California at Berkeley and a Member of the Modern History Faculty of the University of Oxford and Senior Research Fellow in the Oxford Museum for History of Science and Worcester College, Oxford, is the winner of the 2006 APS/AIP Prize for History of Physics "For his ground-breaking and broad historical studies, ranging from the use of renaissance churches for astronomy, through 17th and 18th century electrical science, to modern quantum mechanics."

Heilbron was educated at the University of California at Berkeley where he received A.B. and M.A. degrees in physics in 1955 and 1958 and a Ph.D. degree in history in 1964 under Thomas S. Kuhn. After a term as Assistant Director of the Sources for History of Quantum Physics Project, he began his academic career as an Assistant Professor of History at the University of Pennsylvania (1964 1967) and then returned to Berkeley, rising through the academic ranks to become Professor and Director of the Office for the History of Science and Technology in 1973, Class of 1936 Professor of History and History of Science in 1985, and Professor Emeritus in 1994. He also served as Vice Chancellor of the University of California at Berkeley from 1990 to 1994. He has held visiting appointments as Andrew Dickson White Professor at Large at Cornell University (1985-1991), the California Institute of Technology (1997), and Yale University (2002-2004). Since 1996 he has been a Member of the Modern History Faculty of the University of Oxford and Senior Research Fellow in the Oxford Museum for History of Science and Worcester Col-

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lege, Oxford.

Heilbron's publications on the history of physics have been groundbreaking and of astonishing breadth. As one writer said, "his major books deal with a stunning variety of subjects including electricity in the seventeenth and eighteenth centuries, Max Planck and his moral dilemmas, the use of churches in early modern Europe as solar observatories, the development of geometry, Henry Moseley, and Ernest Lawrence and his laboratory." These works, and the large number of papers he has published, are uniformly of outstanding quality and display an ability to deal with the technical aspects of science as well as the social, political, and institutional contexts in which science has been pursued in the past. His book, The Sun in the Church: Cathedrals as Solar Observatories (Harvard University Press, 1999), was awarded the Pfizer Prize of the History of Science Society, its highest book award, in 2001.

Simultaneously with producing this splendid body of work, Heilbron has enthusiastically and effectively taught many undergraduate and graduate courses and has directed a variety of doctoral dissertations. He also has edited, for the past twenty-five years, Historical Studies in the Physical Sciences (which he expanded in 1986 to include the biological sciences), one of the leading journals in the history of science. As an editor he has had an enormous and beneficial influence on work done in the history of physics, both because he has published only work that meets his own exacting standards, and because of his legendary critical and clarifying editorial comments and revisions of the papers he has published in this journal.

Heilbron's scholarly work has brought him widespread international recognition. He has received the Sarton Medal of the History of Science Society (1993), its highest award, the Pictet Prize of the Association for the History of Science and the Société de Physique et d'Histoire Naturelle (2004), and honorary doctorates from the University of Bologna (1988), the University of Pavia (2000), and the University of Uppsala (2000). He has been elected to the Royal Swedish Academy of Sciences as a Foreign Member (1987), to the American Academy of Arts and Sciences (1988), the American Philosophical Society (1990), and has served as President of the Académie Internationale d'Histoire des Sciences. ■

The Life Of Physics

by Associate Editor Michael Riordan, University of California, Santa Cruz

Lately we are awash in biographies of important physicists. A new one appears almost monthly. Just in the past year or so, we have seen biographies of Max Born, Robert Hooke, William Thomson and Robert Noyce — plus three of J. Robert Oppenheimer, no doubt timed with the 60th anniversary of Trinity. I often receive unsolicited copies of these biographies from authors and publishers hopeful I will anoint their latest efforts in the periodicals for which I write reviews. I wish I could accommodate them, but usually can't.

Why are we witnessing this flood? Is the reading public suddenly starved for the intimate details of the lives of great physicists? Are these men and women such paragons of scientific virtue — or examples of moral turpitude — that they deserve all this attention and scrutiny?

I believe this publishing phenomenon occurs, at least in part, because the biographical form solves one of the thorniest problems that historians and writers face: organizing large amounts of often confusing material into a logical, coherent whole. The rhythms of a major life in physics can provide a convenient structure on which to build a narrative. Find those rhythms and effectively convey them to your readers, and you are well on the way to composing an engaging literary symphony. The scientific biography also gives the writer a means to portray a particular moment in history, as viewed through the eyes of an important figure. If he or she is a major physicist, then of course there will be frequent interactions — some of them not very pleasant — with their now-towering contemporaries. All of this makes for good reading, but it is also an effective way to do the history of physics.

In The End of the Certain World, a recent biography of Max Born by Nancy Thorndike Greenspan, for example, we get a close-in account of how quantum mechanics emerged in Weimar Germany from the viewpoint of one of its major protagonists. Born discourses almost daily with Niels Bohr, Albert Einstein, James Franck, Werner Heisenberg, David Hilbert, and other physics luminaries of the 1920s. But looming in the background of all the intellectual ferment is the rise of National Socialism and the Third Reich, eventually driving this now-famous Jewish physicist into self-imposed exile — along with many of his colleagues. The dispersal of this innovative scientific community lends poignancy to the story of Born's life.

In a scientific biography, we of course get such glimpses of the community activity that is essential to doing almost all good science. This is certainly one way to portray these interactions. But here the biographical form suffers from putting too much focus on one person's contributions, often to the neglect or exclusion of other important work. Stray too far from your protagonist's role in the research, and you can easily lose your narrative way. In my own attempts at writing physics history, especially The Hunting of the Quark and Crystal Fire (coauthored with Lillian Hoddeson, who recently published True Genius, a biography of John Bardeen), I have sought effective ways to relate this essential community activity. There have to be major and minor characters in the drama, of course, but by using a diverse cast I have powerful discursive tools in hand. I can employ these actors, for example, to illustrate the varied and often contradictory interpretations that almost always accompany surprising new scientific data. But like many other non-fiction writers, I will probably feel incomplete until I've written the biography of a major scientist. It's an undeniable itch. One day I'll just have to give in and scratch it. ■

FORUM AFFAIRS

CALL FOR PAPERS

Yes, the time has come to contemplate what you might want to talk about at the March (13-17, Baltimore MD) and/or April (22-25, Dallas TX) 2006 meetings. Each member of APS is entitled to give one contributed talk at a Forum session at each meeting in addition to one you might give in a technical session without having either placed on the supplementary program Anyone, APS member or not, who attends ONLY forum sessions need not pay the registration fee, but we need to know well in advance if you plan to go this route.

The March meeting web site goes up in August with a 30 November abstract deadline, and sorting code for history = 18.3. www.aps.org/meet/MAR06.

The April meeting web site goes up in October with a 13 January abstract deadline and sorting code for history = P. www.aps.org/meet/APR06.

It would be a kindness to send a copy of your abstract to the program chair, vtrimble@astro.umd.edu when you submit it, to provide some advance guidance on numbers and range of topics.

Whether the contributed papers will be scheduled for 24 minutes (a recent FHP privilege) or 12 minutes (the norm for the rest of APS) will depend on how many there are. Thus we won't know which it is until after the abstract deadline. Everyone who submits a contributed abstract will be notified as soon as possible after the deadline whether the talks will be 12 or 24 minutes. In the long run, we would probably all benefit from having lots of contributed papers in the shorter format.

ANNOUNCEMENTS FOR MARCH & APRIL MEETINGS

The highlight of these will, of course, be your contributed talk (see previous item). But there will be some attractive invited sessions as well.

March will feature history of critical phenomena (session chair and co-organizer Joel Lebowitz) and topics in history of low temperature physics (session chair and organizer George Zimmerman).

April will include, first and foremost, the second Pais Prize Lecture by John Heilbron (see p. 2). In addition, there will be two sessions, shared with the Divisions of Nuclear and Particle Physics, marking the 50th anniversary of the discovery of parity-nonconservation (organizer for FHP, Noemie Benczer Koller); two sessions, shared with the Division of Astrophysics, on Cosmology: Past, Present, and Future (co-organizers J. Ryan for DAP and V. Trimble for FHP), and a session shared with Committee on Status of Women in Physics (program chair M. Sher) on pioneering women in astronomy.

If you have an idea for an invited session for 2007 and are willing to do some of the work, the person to talk with is the 2007 program chair, William Evenson, (bill.evenson@uvsc.edu).

WORLD YEAR OF PHYSICS SPEAKERS BUREAU

Throughout 2005, FHP has helped the APS Topical Group on Gravitation to operate a speakers bureau to provide talks on general relativity, history of physics, Einstein and his work, and the parts of astrophysics related to relativity (black holes, cosmology, and such). The primary audience is 4-year colleges, with the goal of helping to encourage graduates to remain in physics. About 150 requests have been received, from many kinds of organizations, and about half of these (including nearly all from 4-year colleges) have been filled.

The program will continue to operate for at least another year, still hosted by the University of Texas, Brownsville. They paid for the first year, and we are now hunting up the $15,000 that will be needed for the second year. There are three things we hope you will consider doing:

a. Volunteer to be a speaker, by sending a message to vtrimble@astro.umd.edu, providing your name, coordinates, and what topics you would be willing totalk about at the undergraduate level

b. Host a speaker at your institution, go to http://www.phys.utb/WYPspeakers/REQUESTS/howto.html

c. Make a donation in support of this, or indeed any other FHP activity. Just mail a check to American Physical Society, One Physics Ellipse, College Park MD 20740, with a cover note explaining that it is to support the FHP/TGG speakers bureau or FHP activities.We will probably need a name different from World Year of Physics for future operations. There will be a small prize for the best idea sent along with your offer to be a speaker.

BARDEEN STUDENTSHIP AND LECTURE

FHP expects to be able to provide partial travel support ($500) for one student giving a contributed talk at the MARCH APS meeting in an FHP session, thanks to a donation from the family of the late double Nobelist John Bardeen. Either the student or the advisor should be a member of APS and FHP.

If you have, or are, a student who plans to give a history of physics talk at the March Baltimore meeting, when the abstract is submitted, please send a copy to vtrimble@astro.umd.edu with a note indicating that it is a candidate for the Bardeen studentship. FHP will let you know as soon as possible after the 30 November abstract deadline whether you are getting the money. The March sorting code for history papers is 18.3.

It is probable, but not certain, that there will also be a Bardeen Lecture at one of the FHP March invited sessions.

FROM THE NOMINATING COMMITTEE

This fall the FHP Nominating Committee will assemble a slate of candidates for the election in 2006 of a vice-Chair (subsequently in following three years to become Chair-elect, Chair, and past-Chair), two members-at-large of the Executive Committee to serve two years, and one replacement member-at-large to serve one year. We welcome suggested names of possible candidates and in particular self-nominations. Please include the person's email address and a brief CV, if possible. Send info by email to nbyers@physics.ucla.edu, Nina Byers, Chair, C. Holbrow, cholbrow@center.colgate.edu, C. Gearhart, cgearhart@csbsju.edy, J. D. Jackson, jdjackson@lbl.gov, G. Trilling, ght@lbl.gov, H. Lustig, h_lustig@yahoo.com.

CHECK OUR WEBSITE!

Please go to our website for up-to-date information on Forum activities. There you will find complete listings, with addresses and affiliations, of all committee members, copies of all previous Newsletters, FHP By-Laws, and other material of interest.

REPORTS OF INVITED PAPERS AT MARCH AND APRIL 2005 MEETINGS

A slew of invited papers relating to Einstein were presented at both the March (condensed matter) and April (general physics) APS meeting, held in Los Angeles and Tampa this year. See the Spring 2005 FHP Newsletter for a listing of all the papers presented. We offer here summaries of some of these talks and of several others of interest to our readers, mostly prepared by their presenters.

Einstein and Condensed Matter Physics—Special Session At March Meeting

Marvin L. Cohen Department of Physics University of California, Berkeley and Materials Sciences Division Lawrence Berkeley National Laboratory

The international community of physicists chose 2005 as the "World Year of Physics" (WYP). The choice of 2005 was motivated by a desire to associate the WYP with the 100th anniversary of Einstein's marvelous year when in addition to his work on special relativity, Einstein explained the photoelectric effect and did seminal work on Brownian motion and molecular sizes. Einstein considered his paper on the photoelectric effect as his truly revolutionary work of 1905. It was this paper which earned him the Nobel Prize in 1921. This paper answered the question, "Is light a particle or a wave?" Einstein's answer essentially was "yes." Although there is a great deal of evidence that Einstein was "uncomfortable" with quantum theory throughout his career, his paper on the photoelectric effect laid the foundations for this fundamental field of physics.

In 1905, the concept that macroscopic matter was made of atoms and molecules was not universally believed by scientists. It is said that this nonacceptance of the atomic/molecular theory of matter was a contributing factor in Ludvig Boltzmann's decision to commit suicide. Much of his pioneering work in statistical physics relied on this concept, and in 1905 Einstein clearly believed and contributed both to the establishment of the atomic/molecular model of matter and to the statistical physics approach to studying properties of groups of atoms and molecules. Einstein's thesis focused on molecular sizes and his work on Brownian motion explored statistical physics approaches to explain Brown's 1828 experiments.

Although special relativity and its famous equation E = mc2 along with Einstein's classic work on general relativity around 1915 have attracted the lion's share of attention when Einstein's work is discussed, the other papers of 1905 and his work on Bose-Einstein condensation have had as big an impact and perhaps a bigger impact on science as a whole. It is this fact that motivated the special session at the 2005 March Meeting of the American Physical Society in Los Angeles. In contrast to the more usual presentations of Einstein's scientific work on relativity and gravity, this special session focused on Einstein's contributions to the foundations of condensed matter physics or solid state physics.

The first talk by Professor Alex Zettl from the University of California at Berkeley began with the doctoral thesis proposals Einstein made to his professors. As Zettl listed the proposals, seven in all, except for the final one on molecular sizes, he described how each was rejected. Zettl then went on to illustrate how influential Einstein's thesis work has been on science in general and condensed matter physics in particular. Because this work considered the size, geometry, and interactions of nanoscale objects, it is essential for modern day nanoscience. Zettl showed examples of applications to condensed matter physics and his own work on nanoscience and gave the impression that if Einstein were alive today, he would be working in these fields.

The next presentation was made by Professor Moses Chan from Pennsylvania State University. Professor Chan recently observed evidence that solid helium could be a "supersolid" that has undergone a Bose-Einstein transition. Bose and Einstein's suggestion that a condensation of many Bosons into a single momentum state arose as a consequence of the Bose-Einstein quantum distribution function which differed from Boltzmann's at lower temperatures. The condensed state exhibits quantum coherence over macroscopic distances. Chan used examples like liquid He-4 at temperatures below 2.176K and alkali atoms in the vapor phase as examples of systems that have undergone Bose-Einstein condensation. Chan also made the observation that He-4 is a superfluid and there is evidence for superfluidity of the alkali atoms system. He described his current experiments which appear to demonstrate superflow in solid helium and discussed models for interpreting this "counter to intuition" phenomena. Again, it was mentioned that Einstein would have greatly enjoyed experiencing all these developments associated with Bose-Einstein theory.

The final talk in the program was by Professor Zhixun Shen from Stanford University. Professor Shen has spent much of his career studying solids using photoemission spectroscopy. He emphasized how over the last 100 years we have learned an enormous amount about solids, molecules, and atoms by studying how "Einstein's photons" liberate electrons from solids. Because photoemission can be used as a direct method for measuring electronic structure, Shen emphasized that it is a special probe for exploring some of the "deepest questions of quantum physics." He also emphasized how there has been "enormously improved resolution" in photoelectron spectroscopy. This improvement allows investigation of superconducting gaps in addition to detailed electronic structure useful for materials physics and for studying correlation effects among electrons. Again, it was felt that if only Einstein could see what happened to the photoelectric effect. . .

When the session was over, the audience stood around and recounted Einstein stories feeling perhaps that the presentations had gone by too quickly. By identifying our roots and the paths back to Einstein's work, we felt we could claim him as our own. We knew we could only claim part of him, but that was enough. ■

Einstein and Millikan

Charlotte E. Erwin, California Institute of Technology. The author is indebted to Judith R. Goodstein for her prior work on this subject and for her helpful discussions during preparation of this paper.

Albert Einstein traveled to California to talk with scientists at the California Institute of Technology in Pasadena over the cosmological implications growing out of the theory of relativity. Einstein was one of a stream of distinguished visitors invited to campus by Caltech's head, physicist Robert A. Millikan. He came to Caltech for three consecutive winter terms, 1931, 1932 and 1933. He also came to confer with astronomers at the nearby Mount Wilson Observatory where by 1930 the researches of Edwin Hubble on the velocity-distance relationship of galaxies challenged Einstein's cosmological constant—his concept of a static universe. Ultimately Einstein did accept Hubble's expanding universe. In Pasadena he discussed his theory and its interpretation with Caltech's physicists Paul Epstein, Richard C. Tolman, and J. Robert Oppenheimer, and with astronomer Fritz Zwicky; with the Mount Wilson staff, including the Observatory's director, George Ellery Hale, Hubble, Charles St. John, Walter Adams; and with visitors including the Dutch astronomer Willem de Sitter and William Wallace Campbell from the University of California's Lick Observatory. In Pasadena Einstein also met for the first time A. A. Michelson, the first American to win the Nobel Prize in physics (1907), for measuring the speed of light.

In California Einstein received continuous attention from the press and public. He posed for pictures, attended banquets, gave speeches, toured movie studios, dined with stars. He attended local concerts, played chamber music on a borrowed Guarneri violin, and enjoyed trips to the California desert. His observations on the local scenery, the people, culture, politics—and science—are recorded in three diaries kept during the California visits.

Einstein's visits to Pasadena were part of Millikan's campaign to make Caltech into a world center of physics. But Millikan's plans to secure Einstein permanently met with increasing difficulties. Einstein's liberal social and political views, such as his declared pacifism and support for disarmament—which he openly expressed in America—became a source of worry to Millikan. He felt that Einstein was being manipulated by extremists. Meanwhile, the political situation in Germany and in the world deteriorated. In 1932 Einstein had met and spoken with Abraham Flexner, who was in the process of founding the new Institute for Advanced Study in Princeton, New Jersey. During Einstein's third Pasadena visit, Hitler became chancellor of Germany. By the time Einstein openly declared that he would not return to Berlin—in March of 1933—he had already accepted a permanent appointment at Flexner's new institute. The position imposed no duties on him, and it included support for his mathematical assistant, Walther Mayer. The careful Millikan had let Einstein slip through his fingers. Einstein never returned to Caltech. Millikan succeeded nonetheless in building a solid edifice for physics in Southern California. ■

Einstein, Mach, and the Fortunes of Gravity

David Kaiser, Massachusetts Institute of Technology

Early in his life Albert Einstein considered himself a devoted student of the physicist and philosopher Ernst Mach. Mach's famous critiques of Newton's absolute space and time -- most notably Mach's explanation of Newton's bucket experiment -- held a strong sway over Einstein as he struggled to formulate general relativity. Einstein was convinced that his emerging theory of gravity should be consistent with Mach's principle; in fact, Einstein was the first to coin the term, "Mach's principle." Yet Mach's principle, then as now, was a bit of a moving target. Sometimes, Einstein used it to mean that there could be no such thing as absolute acceleration: a body's acceleration must always be described as caused by and relative to other bodies. Other times, Einstein invoked "Mach's principle" to mean that a given body's mass should arise via "gravitational induction" from all other masses in the universe. If several large masses were moved near a test object, Einstein maintained, its own mass should increase. Finally, at other times Einstein said that "Mach's principle" held that the sum total of the universe's masses should completely determine a metric field, which in turn would determine local inertial effects. Late in 1916, Einstein told his friend Willem de Sitter that Mach'sprinciple (in any of these variants) had played an important "psychological" role, spurring Einstein on his quest for a relativistic theory of gravity even when he despaired of finding satisfactory equations. Yet it is not clear that any of Einstein's variants of "Mach's principle" would have pleased Mach himself. The famous positivist might well have wondered if we can ever have definite, positive experience of all the masses in the universe.

Once completed, Einstein's general relativity enjoyed two decades of worldwide attention, only to fall out of physicists' interest during the 1930s and 1940s, when topics like nuclear physics claimed center stage. Gravity began to return to the limelight during the 1950s and especially the 1960s, and once again Mach proved to be a major spur: Princeton physicists Carl Brans and Robert Dicke introduced a rival theory of gravity in 1961 which, they argued, satisfied Mach's principle better than Einstein's general relativity did. They introduced a new scalar field in addition to Einstein's metric field. In the Brans-Dicke scheme, Newton's gravitational constant -- which fixed the strength of gravity -- varied over time and space, as the inverse of their new scalar field. All matter interacted with the new scalar field, which in turn led to matter's observed inertial behavior. The Brans-Dicke theory, and the new generation of experiments designed to test its predictions against those of general relativity, played a major role in bringing Einstein's beloved topic back to the center of physics. Throughout the twentieth century and into the twenty-first, the quest for the relativity of inertia has thus proven remarkably productive. ■

Einstein, Noether, and Energy Conservation

Nina Byers, Physics Department, UCLA

While working on the general theory of relativity, Albert Einstein wrote to David Hilbert "Yesterday I received from Miss Noether a very interesting paper on invariant forms. I am impressed that one can comprehend these matters from so general a viewpoint. It would not have done the old guard at Göttingen any harm had they picked up a thing or two from her. ..." Noether was in Göttingen at that time and following Hilbert's discovery of the Hilbert-Einstein lagrangian proved two theorems, along with their converses, which resolved the conundrum of the absence of local energy conservation in the general theory. These theorems are of great generality in establishing the fundamental connection of symmetries and conservation laws. They have profoundly influenced modern physics. This talk told the tale of how Noether came to do this work. It was a detour from her main line of research which was the development of modern algebra. The talk presented a description of those two theorems and a brief account of Noether's scientific life. She is universally acknowledged as one of the leading mathematicians of the twentieth century, It is worth noting that scientific societies generally did not admit women at that time and her paper was read to a meeting of the Gesellschaft der Wissenschaften zu Göttingen by Felix Klein. (See E. Noether, "Invariante Variationsprobleme," Nachr. v. d. Ges. d. Wiss. zu Göttingen 1918, pp235-257; the paper in German or in English translation is available in CWP at http://cwp.library.ucla.edu.) The absence of local energy conservation in the general theory had been of concern to Klein along with Einstein, Hilbert, and others. Noether's paper enabled them to solve the problem regarding energy in the general theory on which they had been working.

(For further details, see Byers' paper at http://cwp.library.ucla.edu/articles/noether.asg/noether.html.) ■

Max Born and Albert Einstein: A Friendship

Nancy Thorndike Greenspan, author, The End of the Certain World: The Life and Science of Max Born (Basic Books, 2005)

Berlin, 1918. Two friends, one meticulous in the uniform of the Prussian army, the other slightly disheveled in the recognizable black suit of an academic, both lean from the severe deprivations of the war. They had spent the last three years becoming close, bonded together in part by their assimilated Jewish backgrounds, their abhorrence of Prussian militarism, their dedication to the pursuit of truth through science, and their love of music. Lunching at one of their homes or at a neighborhood café, friends Max Born and Albert Einstein often found respite from the politics and gloom of war through long discussion about the general theory of relativity. Born later described this theory with a sense of awe – "the greatest feat of human thinking about nature."

Born treasured his relationship with Einstein: He considered this "dark, depressing time … with much hunger and anxiety… as "one of the happiest periods of our life because we were near to Einstein." He spoke for both himself and his wife Hedi, who was equally close to the Einstein family, Einstein's step-daughter being one of her best friends. The two physicists had first met years earlier when Born was trying to derive a theory of the electron based on Einstein's insights in special relativity. Einstein had not been particularly impressed. At that time the neophyte physicist Born was too mathematically oriented for the intuitive Einstein. Working out the problems of the general theory, mostly alone as was his want, the latter had discovered the virtues of this discipline.

Born's work environment sharply contrasted with Einstein's. During the war years, when he directed a branch that applied physics to weapons technology, he discovered an ability to organize a research facility. This talent, together with a knack for finding brilliant young assistants, led Born to collaborate. At the University of Göttingen, where he was head of the Institute for Theoretical Physics from 1921 to 1933, eight of his assistants and students later received Nobel Prizes, a group that excluded other of his well-known assistants such as Robert Oppenheimer and Edward Teller.

Einstein's solitary work atmosphere echoed his general style of relationships. Born later wrote, "For all [Einstein's] kindness, sociability and love of humanity, he was nevertheless totally detached from his environment and the human beings included in it." Both men looked to science as an escape from the social world, but the reserved and cautious Born was acutely sensitive to his surroundings. His emotional involvement frequently blocked this escape route. Einstein never seemed similarly hampered.

In letters to Born, Einstein often painted an overly optimistic picture of world events that allowed him to evade dealing with quandaries. Where Born worried deeply about the effect of increasingly desperate economic conditions in 1921 Germany, partly resulting from the harsh terms of the Treaty of Versailles, Einstein viewed the slovenliness of the French and the growing disunity among the Allies as undermining the intent and impact. He told Born, "You need not be so depressed by the political situation. The huge reparation payments and the threats are only a kind of moral nutrition for the dear public in France, to make the situation appear rosier to them. The more impossible the conditions, the more certain it is that they are not going to be put into practice." Born disagreed. "We are not going to pay as much as is asked for," he wrote. "But I can see the effect of this power politics on the minds of the people; it is a wholly irreversible accumulation of ugly feelings of anger, revenge, and hatred. … It seems to me that new catastrophes will inevitably result from all this. The world is not ruled by reason; even less by love." Certainly uncannily prescient remarks.

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By the mid-1920s, the two came to see the physical world differently as well. In the early summer of 1925, Werner Heisenberg, working with Born in Göttingen, provided the final conceptual breakthrough for a quantum theory, from which Born, with the help of his assistant Pascual Jordan and then Heisenberg, expanded to the basic formulation of quantum mechanics. Einstein wrote, “The Heisenberg-Born concepts leave us all breathless, and have made a deep impression on all theoretically oriented people.” It is Einstein’s only comment on the initial stages of quantum mechanics in their letters.

A year later, Born added another foundation to quantum theory – the statistical interpretation of Schrödinger’s wave function – that the waves were not continuous clouds of electrons, as Erwin Schrödinger proposed, but rather represented the probability of finding an electron in a certain place after a collision. Born’s theory was the death knell of determinism. Here, Einstein was more judgmental, “Quantum mechanics is certainly imposing. But an inner voice tells me that it is not yet the real thing. The theory says a lot, but does not really bring us closer to the secret of the ‘old one.’ I, at any rate, am convinced that He is not playing at dice.” Over the years, as the friends argued the nature of the universe, Einstein did not deem Born’s theory wrong, just incomplete.

They never agreed and had a final go at it as Born retired from the University of Edinburgh in 1953, his permanent home after exile from Germany in 1933. But when Born won the Nobel Prize a year later, Einstein graciously wrote, “it was your … statistical interpretation of the description (of quantum theory) which has decisively clarified our thinking. It seems to me that there is no doubt about this at all, in spite of our inconclusive correspondence on the subject.”

The last round in their scientific argument may have covered feelings of a more personal nature. The previous fall, Born had told Einstein he was retiring to Germany. Einstein was one of the last, if not the last, friend that Born informed. Given Einstein’s attitude – that all Germans were responsible “for the monstrous crimes of the Nazis” – Born obviously feared disapproval. In fact, a few years earlier, when Born was keeping secret that he would return, he had tried to soften up Einstein by writing, “I did share your opinion, but now come to another conclusion. I think that in a higher sense of responsibility, en masse does not exist, but only that of individuals. I have met a sufficient number of decent Germans, only a few perhaps, but nevertheless genuinely decent. I assume that you, too, may have modified your wartime views to some extent.” In response to this tepid defense, Einstein replied that he had not. Consistent with this, he reacted to Born’s proposed move by referring to Germany as “the land of the mass-murderers of our kinsmen.” The phrase shook Born, perhaps because down deep, he agreed. He did not share with Einstein that he was returning only to placate his wife.

Against a global backdrop of two world wars, the Holocaust and the atom bomb, Max Born and Albert Einstein exchanged views and debated politics, religion, and science, early on replacing ‘Sie’, the formal German for you, with ‘Du’, the form used among family members and close friends. Over the years, their lean figures filled out; Born reunited with his homeland while Einstein scorned it; they clashed over the nature of science. Yet, even though they never saw each other after 1932, their friendship endured for forty years until Einstein’s death in 1955. ■

Leonard Mandel and Experimental Tests of Quantum Mechanics

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Everybody knows the “mind-boggling” experiment that Leonard Mandel published, with Xingyu Zou and Lijun Wang, in 1991, in which they showed that it was not the making of an observation that determined which of two complementary properties would be manifest, but rather it was the setting up of an experimental situation that would permit such an observation to be made.In this talk, I argue that this experiment had roots in Leonard Mandel’s work that reach back to the 1960s and his study of the interference of light beams from two independent sources. I argue that one version of the experiment was first proposed in the 1970s, in the context of a debate that was then in progress -- a debate over the range of validity of quantum electrodynamics for optical phenomena vis-a-vis the range of validity of semiclassical theories.

Mandel proposed the same idea again in the early 1980s, but this time in the context of a different discussion. This was the dialogue between the followers of Louis de Broglie’s double solution theory and conventional quantum mechanics. At this time, Mandel thought that the experiment might be carried out using resonance fluorescence.At just about this time, however, Mandel began a series of researches into the phenomenon of spontaneous parametric down conversion. In his hands, and the hands of others, down conversion would lead to a cornucopia of results. And in particular, when he and his students came to execute the experiment, they made use of down conversion. This led to their well-know result for the interference of photons from two independent sources: If the experimental layout allows knowledge of the source of any photon by auxiliary measurements that do not disturb the layout, then interference is abolished.

By laying out this story, there are two points that I want to emphasize.The first is that there is another path to the 1980s and 1990s of experiments on foundations of quantum mechanics than the goes from Bohm to Bell to Clauser-Horne-Shimony-Holt.This is the path that goes through quantum vs. semiclassical electrodynamics that was such a prominent feature of quantum optics in the 1960s and 1970s.

My second point is that this story tells us one of the ways in which new physics is created. Ideas that have been formulated in one connection are brought into contact with new issues and new instrumentation. It is like a dance with a continual change of partners. But it is a surreal dance, because in the course of each transient encounter, the partners themselves change form. ■

Bose and Einstein

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On June 4, 1924, a relatively unknown Satyendranath Bose from Dacca University in East Bengal, India sent a short article to Einstein with an accompanying letter, which said in part,

Respected Sir,

I have ventured to send you the accompanying article to your perusal and opinion. I am anxious to know what you think of it. You will see that I have tried to deduce the coefficient 8pv2/c3 in Planck's law independent of the classical electrodynamics only assuming that the ultimate elementary regions in the Phase space has the content h3. I do not know sufficient German to translate the paper. If you think the paper worth publication, I shall be grateful if you arrange its publication in Zeitscrift fur Physik.

Satyendranath Bose as a young man
Photo supplied by Prof. Wali

He goes on to add:

Though a complete stranger to you, I do not feel any hesitation in making such a request. Because we are all your pupils through profiting only by your teachings through your writings.

Yours faithfully,

S.N.Bose

Einstein's reply came in the form of a postcard on 2nd July, 1924:

Dear Colleague,

I have translated your paper and given it for publication to Zeitscrift fur Physik *for publication. It signifies an important step forward and pleases me very much. However, I do not find your objection to my paper correct. For Wien's law does not presuppose the wave theory and Bohr's correspondence is not used. But this is unimportant. You have derived the first factor quantum-theoretically even if not quite rigorously on account of the polarization factor 2. It is a beautiful step.*

With friendly greetings,

Your A. Einstein

In a note appended to his translation and submitted for publication, Einstein said:

Bose's derivation of Planck's law appears to me an important step forward. The method used here also yields the quantum theory of ideal gas, as I shall show elsewhere.

Indeed, it appears, within a week or so after he received Bose's paper, Einstein presented his paper on 10th July, 1924 to the Prussian Academy of Sciences . It was an extension of Bose's work, titled, " On the Quantum theory of the Monoatomic Gas." He followed it up with two more papers in 1925, the second of which is well known for the prediction of a possible new state of matter that took 50 more years to demonstrate its existence (Bose-Einstein Condensation).

On the other side of the world, Einstein's postcard, saying that his derivation was an important step forward, was influential enough for Bose to be awarded a two year study leave in Europe. He had applied for such a fellowship in January of that year with no response. But as soon as the Vice-Chancellor saw the postcard, all the problems were solved. It gave Bose a sort of passport, a study leave for two years with a good stipend, a separation allowance for the family, sumptuous travel allowance with round trip fare. He also got a visa from the German consulate just by showing them Einstein's postcard. No fee required! He left for Europe in early September aboard a steamer of the Lloyd Triestino Line and arrived in Paris in mid-October. Abraham Pais, the author of 'Subtle is the Lord,' The Science of Life of Albert Einstein, says in his book, Bose's derivation of Planck's law was the fourth and the last of the revolutionary papers of the old quantum theory (the other three being by, respectively Planck, Einstein and Bohr)

High praise indeed! Pais continues:

For Einstein this period was only an interlude. He was already engrossed in his search for a unified theory. Such is the scope of his oeuvre that his discoveries in those six months do not even rank among his five main contributions, yet they alone would have sufficed for Einstein to be remembered forever.

To appreciate Bose's accomplishment and Einstein's own realization of its importance and its extension to ordinary matter, one needs to take a look at the struggle over several decades to unravel the true nature of blackbody radiation, that ultimately would lead to one of the two fundamental (Bose-Einstein) statistics based on the New Quantum Mechanics, the other being the Fermi-Dirac Statistics.

In 1859, Kirchhoff, based on pure classical thermodynamics, proved the theorem that the emissive power or the related spectral density (energy per unit volume) was a function, independent of the nature of the black body, of only the frequency and temperature. He had challenged theorists to find the precise formula for the spectral density (r(n, T)). Various theoretical attempts had

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failed to find such a formula until 1900, when Max Planck derived the well-known Planck distribution law that fitted perfectly the high precision experimental results that had taken nearly fifty years to obtain. But Planck's derivation would raise questions the next twenty years and occupy major figures of the time including Einstein. Recapitulating briefly, Planck had accepted the classical theory of electromagnetism to obtain the number of stationary vibrations, but had introduces a radical hypothesis of discreteness in calculating the average energy per degree of vibration. The thermal equilibrium of radiation at a temperature T was due to the exchange of energy between radiation and hypothetical material resonators that absorbed and emitted energy in discrete energy quanta of magnitude hυ. Einstein, in 1905, invoked Planck's quantum hypothesis to explain the photoelectric effect, but was critical of Planck's derivation. He noted that if the energy of a resonator could alter only in jumps, then for the evaluation of the average energy of a resonator in a radiation cavity, the electromagnetic theory [in calculating the number of independent vibrations, the first factor] cannot be used, for the latter does not admit any distinctive energy values for a resonator.

Between 1905 and 1923, several attempts including those of Debye and Planck(1910), Einstein (1916), and Pauli (1923) followed, but none of those was completely satisfactory. As Bose would say at the beginning of his paper, "in all cases it appears to me that the derivations are not sufficiently justified from a logical point of view. On the other hand the light quantum hypothesis combined with statistical mechanics (as adapted by Planck to conform to the requirements of quantum theory) appears to be sufficient for the deduction of the law independent of classical theory."

Indeed Bose's derivation of Planck's law was simple and straight forward. But it implied novel radical features: 1). Black body radiation consisted of zero mass particle-like light quanta of momentum and energy, hυ/c and hn, respectively 2). No reference to classical theory. Independent stationary vibrations replaced by the number of cells in one particle phase space 3). The probability law Bose used in distributing the number of quanta in the frequency range n and υ +dυ among the cells corresponding to the same frequency range, implied a new kind of statistics. It introduced a new type of statistical dependence or interaction between light quanta and also between material particles in Einstein's extension. This feature is often characterized as indistinguishability.

It is also remarkable that Einstein embraced Bose's distribution law and extended its application, almost immediately, to material particles with suitable modification to take into account the conservation of the number of particles. It was a giant leap towards a unified description of matter and radiation. Subsequently, in his second paper, Einstein linked it with de Broglie's matter waves that indirectly led to Schrödinger's wave mechanics and ultimately, in the hands of Dirac, led to types of quantum statistics based on the symmetry properties of the wave functions.

At around the same time Bose sent his first paper to Einstein, he sent also a second paper for Einstein's opinion and request to get it published in Zeit.fur. Physik. It dealt with Thermal Equilibrium in Radiation Field in the presence of Matter. Einstein translated it and sent it for publication as well, but with an added critical remark concerning Bose's derivation of the probability coefficients of interaction between matter and radiation. It contradicted Einstein's own derivation of the famous emission and absorption coefficients. Whether it is because of Einstein's criticism or because of the advent of new quantum mechanics, Bose's second paper received no further attention. A third paper of Bose, replying to Einstein's criticism was never published and there is no trace of it in Einstein archives.

After spending a year in Paris, learning mostly experimental work in the X-rays laboratory of Maurice de Broglie, Bose went to Berlin in October 1927. He met Einstein, who introduced him to several prominent physicists including Otto Hahn and Lise Meitner. It was an exciting time in Berlin. It was the beginning of the New Quantum Mechanics with colloquia on Heisenberg's Matrix Mechanics and Schrödinger's Wave Mechanics and their phenomenal success. Einstein proposed two problems to Bose to work on: first, the question whether the new statistics implied a novel type of interaction between the light quanta; the second was to see how the statistics of the light quanta and the transition probabilities shaped in the new quantum mechanics. Apparently Bose made no progress on either of the two problems. Bose had to return to his university after the two years of study leave. He devoted himself almost exclusively to teaching and guiding research. Then in 1953-54, within a span of less than a year, he wrote some five papers on the Unified Theory of Einstein, mostly mathematical in nature. In reply to Bose's letter sent presumably along with one of his papers, Einstein wrote,

Dear Professor Bose,

Thank you for your letter of September 20th. I am glad to see that you are interested in this theory and that you have devoted so much work and penetration to the solution of the equation.

I believe, to be sure, that the solution of these equations is not of great help toward the answer of the question: Do the singularity free solutions of the equation system have physical meaning? Are there at all singularity-free solutions which correspond to the atomistic character of matter and radiation? It seems to me that the mathematical methods available at present are not power enough to answer this question.

With kind regards,

Albert Einstein

4 October 1952

Bose was ever grateful and appreciative of Einstein's help in his early career, although he felt a certain amount of chagrin for Einstein's interpretation of the factor of 2 as due to "polarization," and not as, Bose had apparently attributed it the intrinsic spin of the light quantum. This has been historically a controversial point since the original paper that Bose sent to Einstein is not to be found any where. If it was true, Bose deserves the credit for postulating intrinsic spin to photon. ■

From Bohm to Aspect: Philosophy Enters the Optics Laboratory

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Quantum non-locality, or entanglement, is the key physical effect in the burgeoning and highly funded search for quantum computation. This effect emerged through investigation of the possibility of completing quantum theory with supplementary variables, an issue once considered very marginal in physics research. When a marginal theme enters the mainstream in science, its practitioners tend to forget forerunners and past contexts and practices that cannot be accommodated in the present story of success. Nowadays, physicists dispute priorities, present Bell's theorem and its experimental predictions as a case of sudden appearance, and emphasize the role of experiments and new techniques that have permitted them to bring into the laboratories some previous gedanken experiments. This talk deals with the ways that the issue of completing quantum mechanics, especially completing it according to the criterion of locality, was brought into laboratories and, later on, became a topic in mainstream quantum optics. I show that factors others than new ideas, experiments, and techniques were necessary for the flourishing of this field. Indeed, I argue that what was considered good physics after Aspect's 1982 experiments was once considered by many a philosophical matter instead of a scientific one, and that the path from philosophy to physics required a change in the physics community's attitude about the status of the foundations of quantum mechanics. I have argued elsewhere that a new attitude toward the foundations of quantum mechanics matured around 1970 related to subjects like the measurement problem and alternative interpretations of quantum mechanics, which were related neither to the Bell theorem nor to experimental tests.[1] In this talk, I argue that even concerning the Bell theorem and its tests a similar new attitude was required.Horne, Shimony, and Zeilinger[2] have produced a historical account of the concept of entanglement. They showed how this concept as a consequence of the mathematical structure of quantum mechanics was realized as early as 1926 by Erwin Schrödinger and how in the same year Werner Heisenberg explained the helium atom using states that are entangled. However, they also showed that in none of the first quantum mechanical treatments of many-body systems "was entanglement exhibited for a pair of particles which are spatially well separated over macroscopic distances" and that only with the Einstein-Podolsky-Rosen gedanken experiment, proposed in 1935, was this feature explicitly discussed. Chronologically, this talk is dedicated to the next chapter of this story, which runs until Aspect's 1982 experiments. ■

1 *Freire Jr., O. "The Historical Roots of 'Foundations of Quantum Mechanics' as a Field of Research (1950-1970)" Foundations of Physics 34(11): 1741-1760, 2004.*

2 *Horne, M., Shimony, A., and Zeilinger, A. "Down-conversion Photon Pairs: A New Chapter in the History of Quantum-Mechanical Entanglement," in J. S, Anandan (ed.). Quantum Coherence, 356-372, Singapore: World Scientific, 1990.*

Leo Szilard and the APS Boost-Phase Intercept Study

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The American Physical Society recently carried out a study of the possibilities for intercepting intercontinental-range ballistic missiles in their boost phase-the first few minutes of flight while their rocket motors are still operating (D. K. Barton, et al., Rev. Mod. Physics, 76, S1 (2004). This study was generated in the APS tradition of providing analyses of scientific and technical issues of concern to the public and policy-makers. The tradition of capable scientists devoting themselves to such studies is now an established element of the culture of physics and the broader scientific community. It originated in the waning days of World War II when Leo Szilard took the lead in organizing physicists to take responsibility for helping society to cope with the threats of nuclear weapons and the promise of nuclear power.

Szilard felt a personal responsibility for the problems posed to humanity by nuclear weapons because he played a crucial role in their creation. He was the first person to suggest that a nuclear chain reaction could lead to a major release of energy. That was in 1933, a few months after the neutron was discovered. When Hahn and Meitner discovered fission, he immediately recognized the threat to the western world if Germany were to build a nuclear weapon. Szilard was a confidant of Einstein and convinced Einstein to write two letters to President Roosevelt about these developments. These resulted in the establishment of the Manhattan project and the creation of nuclear weapons.

Well before the first nuclear bomb was tested, Szilard pondered how society could cope with its awesome power. In 1946 he founded, with Einstein, Hans Bethe, Victor F. Weisskopff, and others, the Emergency Committee of Atomic Scientists. This evolved into the Federation of American Scientists, which continues today to be an essential resource on scientific issues relating to national welfare. The founders of the Emergency Committee of Atomic Scientists inspired many other scientists to become seriously involved in public policy issues. From this origin sprang organizations such as the Pugwash Conference, the Union of Concerned Scientists, the Council for a Livable World, and the American Physical Society's Panel on Public Affairs and Forum on Physics.

Szilard was remarkably prescient. Years before intercontinental-range ballistic missiles were created, he described the ease with which they could deliver nuclear weapons, foreseeing the nuclear stalemate of the cold war. He also noted that a much simpler way to deliver nuclear weapons would be to smuggle them into the country. Many observers believe that this poses the chief threat to our security today.

This is the history that underlay the decision by the Council of the American Physical Society to launch its study on boost-phase intercept for missile defense. On a personal level, the legacy of eminent scientists being involved in such tasks undoubtedly motivated many members of the Study Group to devote the considerable effort required. Thus, one can see a direct link between the creation of the APS study and the life of Leo Szilard. ■

Quantum Optics and Quantum Tests—E.T. Jaynes

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In the first 15 years after the development of the laser Edwin Thompson Jaynes was the central figure in a challenge to quantum theory that provided a dramatic highlight as well as a topical focus in the emerging field of Quantum Optics. The founding document of this challenge was an internal report by Jaynes published first by the Stanford Microwave Laboratory in 1958. In this note I will argue that through his report and its consequences Jaynes was the first to suggest a laser-optical based test of a fundamental theory of physics, and helped to make such tests one of the most productive themes in Quantum Optics.

Edwin Thompson Jaynes
Photo supplied by Prof. Eberly

The historical stage is easily set because the construction and operation of the first laser in 1960 is so widely known and well documented. A vivid impression of the earliest activities in Quantum Optics, and of the variety of motivating forces behind the work, is easily available today by inspection of the Programs and Proceedings of early conferences of quantum optics and quantum electronics. Quantum Mechanics itself was not at issue, officially, at these meetings but for many scientists the development of the laser forced attention onto questions about the quantum nature of light that had been deferred for a quarter century. Open lack of agreement about what was and wasn't truly quantum mechanical about coherent light, comes through time after time in reading the old Proceedings. So the question that gradually became key was this: Is it really necessary that light be quantized? Is it possible that photons only offer a handy way of speaking and calculating, and could be discarded altogether?

Jaynes was interested in exactly these questions even before the first laser was built. His 1958 Stanford Microwave Laboratory Report 502 treated theoretically what he called a ``maser model" for which he found two exact solutions. The solutions differed because in his treatment the maser field was quantized in one case and not in the other. This made any differences in the two solutions interesting, and over the following decade Jaynes gradually began to realize ways that the differences could be tested experimentally.

Jaynes was able to demonstrate that simply by including radiation reaction in his non-quantized theory, which he called ``Neoclassical Theory" (NCT for short), it became immediately superior to other non-quantized (and therefore presumably non-fundamental) theories of radiation. He did this by deriving from NCT the correct formula for the natural lifetime of spontaneous emission, the reciprocal of the Einstein A coefficient. This quantity was unable to be calculated before Dirac's introduction of field quantization in 1927, and since then had been accepted as intrinsically quantum electrodynamic. One question was then natural: is NCT equivalent to or perhaps even superior to standard quantized-field quantum electrodynamics (QED)? Thus Jaynes' next step was sensible, to shift his attention from the maser-laser model to the neoclassical theory of radiative corrections generally, and specifically to atomic line shifts and natural line shapes. He realized that NCT should be capable of calculating a second "sacred" quantity of standard QED, the hydrogen Lamb shift. Again, with NCT it could be done without field quantization, so a correct Lamb shift result from an NCT calculation would subject QED, which Jaynes regarded as philosophically untenable, to a challenge from an entirely new direction.

A Lamb shift calculation was presented by Jaynes as a proposal for NCT in an invited talk at the Second Rochester Conference on Coherence and Quantum Optics in 1966, under the title ``Is QED Necessary?" Jaynes' three-page abstract suggests the nature of his talk. He wrote, for example, ``The recent literature of optical coherence theory and quantum electronics all appears to be based on one underlying assumption, (which is that) present Quantum Electrodynamics has an Absolute and Final validity that makes it a kind of Oracle competent to make pronouncements about all other (e.g. semiclassical) theories." (All capitals are his.)

Skeptics made a variety of calculations to prove that Neoclassical theory was at least wrong if not totally absurd. An unexpected finding was soon encountered: so-called quantum processes may have fully adequate semiclassical explanations. That is, they can be quantitatively reproduced by theories that don't reject quantum mechanics as a whole, but that associate quantum mechanics only with the properties of matter (atoms, molecules, electrons, etc.) and not with any interacting radiation. Even the photoelectric effect, the very process that in 1905 established the reality of light quanta, can be correctly treated in this way, as can Compton scattering, by which physicists had begun to persuade themselves 40 years earlier that photons were ``really" particles, having momentum as well as energy.

Jaynes' focus on the hydrogenic Lamb shift, the thesis topic for Michael Crisp and Carlos Stroud in 1969-70, attracted wide and generally scornful attention to NCT but soon, as curiosity about NCT correctly understood, that is, if his Neoclassical proposal was considered only as a proposal, as Jaynes insisted, then QED should logically also be considered only a proposal, both proposals being eligible for serious test in whatever new way one could imagine. Many detractors of NCT could agree that its very existence did suggest new tests of radiation theories, tests that were becoming feasible with the rapidly increasing sophistication of optical experiments in laser laboratories. A key contribution is this regard was success in the contemporaneous effort to build continuously frequency-tunable lasers, most prominently by use of optical dyes as active media.

I believe that Jaynes' initiative, starting in 1958, can be seen historically as the first proposal that prompted new experiments for the direct testing of a foundational theory, in this case quantum theory, by use of modern quantum optical methods. Ironically, it was the second initiative in this same direction, the well-known experimental tests of quantum mechanics focused on Bell Ineqalities, that in Jaynes' own view struck the most telling blow against NCT. Analyses of a variety of complex experimental tests were gradually growing by 1972 into what Jaynes would ultimately concede was a convincing refutation of NCT, but the starkly negative implications of the Bell tests were immediately clear to Jaynes when presented with the first results of the now-famous Clauser-Freedman experiment at the 1972 Rochester meeting. In Jaynes' own words about that experiment "if the experimental work is confirmed by others, then my work will lie in ruins."

Such dramatic terms are I believe misapplied to NCT, which I argue should be judged historically a success. As background, one can first of all recognize a sense in which quantum mechanics was too successful too easily. This permitted a period of enthusiastic use without deep understanding. Not much sympathy was available for those inclined to examine critically its mysterious elements, in case the theory might be found a bit unstable, and the bright new home for physical philosophy be found only a house of cards. Following this remarkably long period, Jaynes was perhaps the very first challenger to succeed passing an important screening. He expressed a point of view incompatible with elements of standard QED, as even such eminences as Dirac did, but Jaynes was able to take two further important steps. He also successfully explained the openings for new tests being provided by experimental methods engaging the power and coherence of laser optics, and he was uniquely able to proceed not negatively but constructively because he was in command of the fully developed alternative NCT proposal for radiation theory.

Jaynes used this unique position to constantly advance his insistence that no theory should be allowed to be above reproach, not even QED. This was gradually conceded by NCT's most active opponents, and eventually widely appreciated. We can now see that his work gave us the first instance of a quantum optical test of a fundamental theory of physics. Such tests now have a well-respected role as a highly active research theme within Quantum Optics worldwide. ■

Megascience and the Powers and Paradoxes of Pushing Frontiers

Lillian Hoddeson

I would like to honor the memory of Gertrude Sharff-Goldhaber by offering a frank and provocative portrayal of ironies, conflicts, and paradoxes intrinsic to modern big physics, based on a case study of Fermilab. The first irony is that the version of "megascience" (which is bigger than "big science") that emerged at Fermilab grew out of a vision of small science. Robert R. Wilson used the image of the lone explorer of frontiers as an overarching theme of his laboratory. But unlike the pioneers of the American West, Wilson's frontiersmen were of noble spirit; these adventurers were not only bold and willing to take risks, but frugal and committed to democratic principles. Wilson (and many others) in time recognized the tensions inherent in this romantic picture, among them that it is, as Wilson wrote, "almost as hard to reach the nucleus by oneself as it is to get to the moon by oneself," or that "nook and cranny" experiments can not typically compete with the larger and better supported studies at other labs. Although Wilson completed his Main Ring, he could not navigate the tighter budgetary context he faced at the next frontier, his planned superconducting accelerator designed to double the energy. He stepped down in 1978.

When Leon Lederman took the reins, he realized that in the more limited funding environment of the late 1970s, any increase in the scale of the experiments demanded some sort of decrease in the program. Lederman's decision to complete Wilson's superconducting accelerator allowed Fermilab to maintain its position at the high energy frontier, but the price was to forgo the discovery of the W and Z particles, later found at CERN. Moreover the competition for limited resources (such as particle beam; detectors, experiment halls; and even journal space) caused the experiments in Fermilab's fixed target program to take the form of "experiment strings," where each follow-up experiment differed from the previous one in along a string only a small number of aspects, typically some change in its detector. These strings were invisible mini-institutions devoted to particular experimental traditions, e.g., the study of "charmed" particles. One conflict arose because convincing the laboratory to invest in more costly detectors required arguing that they would be long-lived facilities that numerous groups could employ, yet once a group had invested the time and effort to develop a large detector, it could not afford to give it up for many years. Certain kinds of research became entrenched at the laboratory, while the subsystems of large detectors were shaping the collaboration social structures. Further paradoxes and unintended consequences derived from the mismatch between the 15 to 20 year time scales of higher energy colliding beam experiments and the much shorter academic time spans, such as the time to become tenured. Collaborations were increasingly driven by bureaucratic arrangements.

Thus within a decade and a half, Wilson's vision of the lone pioneer who conducted frugal experiments at the energy frontier was replaced by a megascience whose consequences were threatening to undermine the frontier vision that gave rise to this research. The larger investments necessary for more complex experiments produced commitments, which were reducing the explorable frontier, at least in a certain sense, to but a few of the most fundamental problems. ■

Some Observations on DOE's Role in "Megascience"

Alvin W. Trivelpiece Director, Oak Ridge National Laboratory (Retired)

Introduction There are some fields of science and technology that require specialized facilities to make effective progress at their respective frontiers. For historical reasons some of those facilities have been conceived, built, and operated by the Atomic Energy Commission and its successor agencies, the Energy Research and Development Administration and the Department of Energy. These facilities have included particle accelerators, nuclear reactors, synchrotron light sources, high performance super computers, etc. Each of these facilities and programs were key to the advancement of goals of these agencies and at the same time advanced the state of the art in many fields of science and technology.

As director of the Office of Energy Research (OER) from 1981 to 1987, I was an ex officio member of the Department of Energy's (DOE) Budget Review Committee (BRC). Undersecretary Joe Salgado chaired this committee during President Reagan's second term. At one BRC meeting Salgado expressed concern that the unplanned cost growth for securing nuclear materials and for environmental considerations was going to make it difficult to fund some of the regular departmental mission programs and projects. These costs had grown from around $100 million to around $1 billion without being in the out-year budget target. That meant these costs had to be absorbed out of existing programs and projects. Some of these would come out of budgets for the Office of Energy Research. I was very distressed and said that if we kept on the present trajectory we would end up with a collection of laboratories and facilities at which everything was secure, everything was cleaned up, and no body was doing anything useful. Following my intemperate outburst, there was a long pause – a really uncomfortable moment of silence. Then Salgado asked what I had in mind? I said that in addition to all of security and clean-up work, we needed a scientific facilities revitalization plan for the energy research labs. OK, he said, "Just what did I propose?" I blurted out that we should put new synchrotron light sources at Berkeley and at Argonne, a relativistic heavy ion collider at Brookhaven, and an advanced neutron source at Oak Ridge National Laboratory. I am not sure that I accurately recall what happened next, but we did go back to complete the budget decisions before us. Salgado asked me to meet with him in a few days to explain why I shouldn't be fired.

Four projects are born At that meeting, Salgado asked me to meet with someone in the Office of Management and Budget (OMB) and some members of the Congress to learn what they thought about such a program. Much to my pleasant surprise, almost everyone I talked with had the same message. Namely that it was about time the Department did some sensible long-range planning of this sort. One of the features would be that the annual appropriated funds for these projects would be constant during the time that were being constructed by staggering their start dates appropriately.With those two bridges crossed, the next step was to involve the directors of the four laboratories —Berkeley, Argonne, Oak Ridge and Brookhaven. I invited the respective directors to Washington to explain what I had done and what I expected them to do. I told them that we were prepared to proceed with the four facilities in an ordered and orderly fashion. Berkeley would be first and Oak Ridge was to be last. I told them that the key to making this work was that they agree to the plan and that they support each other's proposal as if the facility in question were to be built at their laboratory.What is amazing is that this born-of-frustration plan almost worked as conceived. In fact three out of the four projects went ahead as originally planned. They had their challenges and problems, but to the best of my knowledge they are all producing results consistent with their intended purposes. The one that didn't go as planned was the Advance Neutron Source at Oak Ridge, which was to be a substantial upgrade to the High Flux Isotope Reactor — from 100 Megawatts to about 300 Megawatts. And even then, the ANS be morphed into the Spallation Neutron Source (SNS) that is nearing completion.

Superconducting Super Collider Of course not everything works out as planned or hoped. The SSC was another major focus of attention for me. To get it started, it was necessary to close down Isabelle at Brookhaven, which was one of my most painful experiences as director. The funds recovered from the close down were used to get the SSC started. Eventually about $100 million went into a design that resulted in an estimated cost of $4.4 billion. This was the amount that used in my presentation to President Reagan at a Cabinet meeting on January 26, 1986. After listening to the pros and cons of the proposed plan to build the SSC, the President took out a card and read the following that he said was read to Kenny Stabler during the time that he was quarterback for the Oakland Raiders football team.

I would rather be ashes than dust; I would rather that my spark should burn out in a brilliant blaze than it should be stifled in dry rot. I would rather be a superb meteor, every atom of me in magnificent glo, than a sleepy and permanent planet.

The President said that Kenny was asked the significance of this thought, and that Kenny replied that it means, "Throw Deep." We all laughed, the President left the cabinet room, but by this story he telegraphed his intention to approve the project at about $4.5 billion. The next morning he approved the decision memorandum. Not a bad day at all.Unfortunately the SSC was not built. This shattered the lives and dreams of many friends and colleagues. There are many technical reasons given for its demise, but there may be a more relevant political reason for its termination.

A couple of years ago, I was told about a conversation between the Governor of Texas, the Honorable Ann Richards, and President Clinton early in his administration. He asked her if she wanted to fight for the SSC. She said no. That meant it would no longer be an administration imperative, meaning that DOE would not put it at the top of its priority list and thus, that the SSC was most likely doomed. I devoted several years of my life helping the SSC get funded. I remain disappointed and regard its termination as a major scientific tragedy. Furthermore, I also suffered more directly in that the termination of the SSC resulted in the collateral damage of losing the ANS at Oak Ridge.

What is the take away lesson from the SSC experience? George Santayana wrote, "Those who cannot remember the past are condemned to repeat it." ■

Submicroscopic Nature Needs Megascience

Leon Lederman, Illinois Mathematics and Science Academy

The history of "submicroscopic nature", that is, the history of particle physics, begins in the early 1950s and builds on the construction of a post WWII series of particle accelerators developed to study nuclear physics and which had been applied to the collisions, in the earth's atmosphere, of cosmic rays. These were high-energy particles generated in cosmological events and colliding with oxygen and nitrogen in our atmosphere to create new particles. These studies discovered muons, pions, kaons, and lambdas – the beginnings of a vast particle "zoo". Clearly, studies of the inhabitants of the zoo required energetic collisions; the higher the energy of the accelerator, the more extensive was the range of masses that could be produced and studied. Our paper reviews the developments over the past 50 years. As accelerators grew, so did the particle detectors and the sizes of the experimental groups. This will bring us to Fermilab in 2005. Finally, we describe the ~900 physicist groups that are cheerfully collaborating, building particle detectors designed to peer deeply into the structure of matter, based upon the Large Hadron Collider (LHC), an accelerator of unprecedented size, cost, and complexity. The story then takes us from the 100 MeV (108 eV) "atom smashers" of 1950, to the ~10 TeV (1013 eV) behemoth now under construction in Europe. The LHC will be the first accelerator to be constructed collaboratively by all the regions of the world. Thus, we move from dozens of machines often on University campuses around the world, to one single megascience device shared by physicists around the world. The motivation for this evolution is physics, which is the driving force. Today the Fermilab Tevatron is observing collisions at 1.9 TeV with unprecedented luminosity hoping to make the discoveries which theoretical physicists confidently expect: the Higgs particle, evidence for supersymmetry and, even more speculative, evidence for higher spatial dimensions. LHC will come on in a few years to certify the expectation of theory or to create a new revolution in particle physics. The final point in this megascience survey is the speculative possibility of creating a muon collider where positive and negative muons at several TeV each will make head-on collisions. The R&D underway is difficult but interesting! ■

REPORTS ON CONTRIBUTED PAPERS

FHP Holds Successful Meeting in Los Angeles; First March Contributed Paper Session

Report by Frieda A. Stahl, California State, Los Angeles

Eight authors responded to FHP's call for contributed papers to be given at the March APS meeting, in addition to the Forum's traditional April activity. The result was session L19, on Tuesday afternoon, March 22. It drew an audience averaging about 100. Nina Byers presided, and the talks covered a broad range of historical events connected with physics through people as well as ideas.

The highlight of the session was the third paper in the program. Attendees filled the room to hear the account by Steven Moszkowski (UCLA) of his family's long friendship with Einstein, titled "Personal Recollections of Albert Einstein." The friendship began in Europe during WWI and continued in the U. S., until Einstein's death in 1955. Moszkowski displayed images of many letters, first to his grandparents, then to his parents, and eventually to him as a youth and later as a physicist beginning his career. He also showed photographs, some with Einstein's inscription, and German poems composed by Einstein. A few of the documents were hand-written. To Moszkowski's mother, who had asked Einstein's advice on bringing up her precocious son, he answered. "Leave him alone." Questions following this talk were numerous and enthusiastic.

Ronald Mickens (Clark Atlanta U.) opened the session. In his paper, "Physics at Fisk University," he recounted the development of the physics program at Fisk, a historically black college founded in 1866 for the advanced education of newly freed slaves. Philosophically it was aligned with W.E.B. DuBois, who advocated traditionally academic education rather than vocational, which Booker T. Washington promoted. Beginning in 1931under the leadership of Dr. Elmer Imes, the research Imes had begun in infrared spectroscopy grew into the Fisk Infrared Spectroscopy Institute (FIRI), now internationally known.

Two papers were devoted to basic concepts and the pioneering physicists who pursued their underlying theories. In the first, titled "Attempts to Link Quanta and Atoms Before the Bohr Atom Model A," Ananth Venkatesan (Northeastern U.) spoke on the work of Arthur Haas (1844-1941), whose work preceded Bohr's. Haas tried to derive Planck's h from the electronic charge and mass and the atomic radius, although the values of these fundamental constants were not yet well-established. Haas's work does not appear in texts, although Bohr acknowledged him after deriving his own model of the atom, as did Sommerfeld. The other of these papers, by Charles W. Clark (NIST), was "Ugo Fano, Enrico Fermi, and Spectral Line Shapes." Clark discussed the equation derived by Fano (1912-2000) for the asymmetric profile characteristic of spectral lines. Fano's definitive paper, "cited thousands of times," appeared in Phys. Rev. 124, 1866-1878 (1961). That paper was hugely successful because it was broadly applicable in both atomic and condensed matter physics.

More on the subject of citations was detailed in the paper by Sidney Redner (Los Alamos and Boston U.), who spoke on "Citations From More Than a Century of Physical Review." Working with the

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P. R. editorial staff, he analyzed general trends of growth in papers and citations over 110 years, 1893-2003. Their data showed a sharp dip during the WWII years (but not WWI). The average was calculated to be 8.8 citations per article, within a very broad range. The top papers were rated by "impact," defined as the number of citations divided by that average. Walter Kohn was found to be the co-author of both the two most cited papers. The "longest-lived" paper, for which the citations go back 60 years, is the one by Einstein, Podolsky, and Rosen (EPR). The paper that first exceeded 1,000 citations is the one by Bardeen, Cooper, and Schrieffer (BCS).

A different category of pioneering was recounted by Frieda Stahl (Cal State L.A.) in her paper, "Sarah Frances Whiting: Foremother of American Women Physicists." Whiting (1846-1927), a secondary-level math teacher, became self-taught in physics and astronomy, initially through public science lectures available in New York City around 1860-70. Recruited in 1876 by the founders of Wellesley College to teach physics, she prepared to do so by attending MIT as an unenrolled visitor in the then-new labs being developed by Edward Pickering, the first such program in the U.S. In 1878 she set up the second instructional lab program in the U.S. at Wellesley, the first for women students, and expanded it to include astronomy, emphasizing photometry and spectroscopy. She worked to update her knowledge by visiting research labs at numerous eastern campuses, and spent her two sabbaticals in Europe visiting labs there. She was instrumental in persuading a wealthy trustee to endow an observatory at Wellesley, inaugurated in 1900, which she directed, in addition to chairing both the physics and the astronomy department. In 1905 Tufts University awarded her an honorary D.Sc. degree.

Marjorie Lundquist (Bioelectromagnetic Hygiene Institute) detailed a formidable medical history in her paper on the thesis that "A half-century ago physicists missed a major public service opportunity, costing the human race widespread chronic illness and many deaths!" She discussed the failure to recognize physiological trauma caused by exposure to high-energy pulsed microwave radiation (radar) during WWII, disregarded because it was non-ionizing. Yet simple classical analysis, beginning with Poynting's principle, accounts for the transfer of energy, momentum, and angular momentum, and resulting thermal effects on body tissue can be calculated. Medical observations disclosed cases of internal bleeding, leukemia, and brain tumors in exposed individuals. Not until 1953 was a limit of 10 mW/cm2 set for human exposure.

The eighth paper in the program abstracts was not given, because the author was absent, and the end-time of the session was approaching. The session was distinguished by double modules, 20 minutes for presentation and four for discussion. This allowance of time is important and advantageous for FHP papers, for which the subject matter represents developments over time. Papers on current physics research can be limited to 10 minutes for a summary of current results, with the background of antecedent research left to references. History does not proceed in that "now" fashion, and FHP papers benefit greatly from the innovative format of 20 + 4. ■

Summary of Three Contributed Paper Sessions at April Meeting

History of Physics I Report

by Kenneth Ford

Three of the four papers in this session were concerned with quantum mechanics (although from very different perspectives) and all four dealt mainly with events in the first third of the twentieth century.

1. Clayton Gearhart of St. John's University (Minnesota) covered the rocky history of the efforts to understand the specific heat of molecular hydrogen in the old quantum theory (1911-1925), focusing on the work of Fritz Reiche and Edwin C. Kemble around 1920. The old quantum theory, he reported, did a better job of explaining the spectrum than the specific heat of molecular hydrogen. But both sets of data seemed to imply that diatomic molecules could not exist in rotation-free states—an early example of zero-point energy! Not until David Dennison's work in 1927, using the new quantum mechanics, did theory finally match the temperature dependence of the measured specific heats.

2. Paul Halpern of the University of the Sciences (Philadelphia) presented numerous examples of quantum humor, much of it written and drawn by leading scientists of the late 1920s and early 1930s who had ties to Copenhagen. Halpern cited not only the well-known Faust parody of 1932 but many other examples as well, showing that Oskar Klein was among the most prolific and funniest, with George Gamow a runner-up. And, as Halpern revealed, even those not naturally attuned to humor, such as Wolfgang Pauli and Paul Ehrenfest, made game efforts to join in the fun. Finally, Halpern discussed the Journal of Jocular Physics, whose three issues appeared regularly every ten years: in 1935, 1945, and 1955.

3. William C. McHarris of Michigan State University argued that objections to the Copenhagen interpretation of quantum mechanics might have followed a different path and found more adherents if modern chaos theory had been available to researchers in the 1930s. He pointed out that the uncertain outcomes in classical chaotic systems bear a striking resemblance to the probabilistic outcomes in quantum systems, a resemblance that, in his view, may be more than superficial. In a presentation that was perhaps more advocacy than history, McHarris argued the case that quantum mechanics, at its core, may be a deterministic theory.

4. Anthony Nero of Lawrence Berkeley Laboratory presented his study of bachelor's degrees in physics awarded to men and women at Stanford University, 1900-1929. He found that in the first decade of the twentieth century, women (at

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Stanford) were as likely as men to earn degrees in physics, and in the second decade even more likely. These data were consistent with trends of the previous century. The apparent reason, he said, was men's preference for the classics in those times. Nero reported a sharp reversal in the 1920s, since which time men have been more likely than women to earn degrees in physics (at all universities, of course, not just Stanford). During the period studied by Nero, admission policies at Stanford led to a decrease in the percentage of women students, and thus perhaps to higher admission standards for women.

History of Physics II Report

by Roger Stuewer

The second session of contributed papers on the history of physics, which was well-attended, was chaired by Roger H. Stuewer (University of Minnesota). Ruprecht Machleidt (University of Idaho) argued that the Greek atomistic concepts of Leucippus and Democritus are not the true precursors of modern elementary particle physics; instead, he concluded, the underlying symmetries first envisioned by Plato in his Timaeus is close to the modern idea that geometric symmetries generate the elementary particles of matter, as Werner Heisenberg pointed out repeatedly in his writings. Durruty Jesús de Alba Martínez (University of Guadalajara) discussed the first experiments on radium that were carried out and published in 1904 in Guadalajara by two lay priests, Severo Días Galindo and José María Arrecola Mendoza, who as a result became the founders of modern scientific research there. Danian Hu (City College of New York) described how the Chinese physicist Li Fangbai (1890-1959), although educated in Japan, learned special relativity by reading the work of the American scientists Gilbert N. Lewis and Richard C. Tolman and thereby introduced its study into China, which was the first instance of American influence on the rise of theoretical physics in China, one that extended to general relativity in the 1930s and 1940s after one leading Chinese theoretical physicist was educated at Caltech and another at MIT. In closing the session, Joseph Kapusta (University of Minnesota) discussed two disaster scenarios for destroying the Earth that attracted widespread public attention. First, an article in Physics Today in 1993 mentioned an internal committee review that considered and dismissed the remote possibility that Lee-Wick density isomers might be formed in nuclear reactions produced by the Bevalac accelerator at the LBL, the authors of which then found themselves on the Unabomber's list of targets. Second, an article in the London Sunday Times in 1999 noted the remote possibility that strange quark matter or mini-black holes might be formed in nuclear reactions produced by the RHIC accelerator at the BNL, which again was dismissed by a committee whose report was published in the Reviews of Modern Physics.

History of Physics III Report

by Robert Romer

The first paper in the session, "My Half-Hour with Einstein", was given by Robert H. Romer of Amherst College. As his own small contribution to the "Einstein Year", Romer told how an acquaintance with Dr. Tilly Edinger, a Jewish refugee paleontologist at Harvard, had led to an introduction to Einstein and a one-on-one conversation in 1954, a year before Einstein's death, when Romer was a second-year graduate student at Princeton. Among other things, Romer remembered Einstein's curiosity about evolution and paleontology, and, of course, he wanted to know Romer's opinion about the foundations quantum mechanics. "Do you really believe that if someone here were to measure the spin of an atom, it would affect the simultaneous measurement of the spin of another atom way over there?" Romer clearly remembered his own inadequacy in trying to respond, while observing that now, half a century later, he still found quantum theory perplexing. But he also distinctly remembered what a kind man Einstein was and how adept he had been at putting an embarrassed graduate student at ease. A more complete account of this conversation and how it came about has recently appeared in the March, 2005 issue of The Physics Teacher. The second paper was given by Harry Lustig (formerly of the City College of the CUNY), on "Did Heisenberg Spit at Max Born?" Such an unpleasant event has been described in several places (notably in a 1985 book by Arnold Kramish). Lustig has searched Born's obituaries as well as his autobiography and has consulted a number of American and German historians of science. Lustig's conclusion: "Although it is difficult to prove a negative, it is highly unlikely that Heisenberg spit at Born." The next paper was by Thomas Miller (Hanscom AFB) and Benjamin Bederson (NYU), on "The Rayleigh Papers", and was presented by Bederson. Surprisingly, most of Lord Rayleigh's papers now reside at the Air Force Research Laboratory at Hanscom Air Force Base in Lincoln, Massachusetts. Bederson described how this came about. Bederson and Miller have had the opportunity to study this important archive, which includes Rayleigh's handwritten scientific notes that span his entire career. Bederson showed a number of interesting examples from these notes, including, for instance, the notes that describe his original identification of argon. ■

BOOK REVIEWS

JOHN WALLER

Leaps in the Dark, The Making of Scientific Reputations

Oxford University Press, 2004 xii + 291 pages $24.95

Reviewed by Ben Bederson

John Waller is carving out a niche for himself concerning the problem of assigning proper credit to the discoverers of major scientific advances, as well as for what you might call the messier side of scientific discovery. After a well-received first "popular" book, "The Discovery of the Germ," he has written two books that focus on scientific accomplishments that were over-hyped, unfairly attributed, or more generally, had been presented to the public in such oversimplified fashion as to leave strongly incorrect images of their development in the minds of the public. The first of these books "Einstein's Luck" (titled "Fabulous Science" in Great Britain) was reviewed by Allen Franklin in the Spring 2005 issue of this Newsletter. It was not a very favorable review; based primarily on Franklin's criticism of Waller's retelling of the Milliken oil drop experiments. His second book along similar lines is "Leaps in the Dark", subtitled "the making of scientific reputations", in which he examines a number of important case studies. To be more accurate, the subtitle could have been "the unmaking of some scientific reputations", because he tends to pull down by a peg or two the exalted status of such medical and scientific icons as Semmelweiss (germ theory), Lind (scurvy), and, from the physics world, Sir Robert Watson-Watt (radar). He also discusses cases where great discoveries were greeted with skepticism or disbelief by knowledgeable contemporaries by placing these within the context of the times in which they were made. The prime example he cites is Newton's work on prisms and the makeup of visible radiation, since he had considerable trouble convincing contemporaries of the validity of his prism experiments. (He could of course also have cited Galileo's difficulties in convincing contemporaries of the existence of the moons of Jupiter and the phases of Venus!)

While in some cases (as with Watson-Watt) he severely criticizes the subject for vastly exaggerating his own achievements at the expense of others, his main point is a valid one: in almost all the cases he considers, the great advances made were really due to the steady accumulation of information and understanding, over years, by many people, and that singling out one person as the star actor is unfair to all the others who played major roles, and perhaps more seriously, giving the public the wrong idea about how science really works. It is the public's need for heroes that tends to narrow the field to one or two of the most visible achievers. On the whole it is difficult to quarrel with this claim. There are really very few great advances in science (if any) which would not have occurred absent one crucial individual. Without the giants of physics, such as Newton, Maxwell and Einstein, would we have mechanics, electromagnetic theory, and relativity? In all probability we would, and we also would have quantum mechanics without Bohr. But stellar accomplishments really do cause tectonic shifts in scientific advance, however rarely. Even so, these do not appear out of the blue; it is important to know what has happened in the past, even for giants. In this sense the present Waller book is useful and interesting . It is perhaps best taken in small doses, but it is an enjoyable read, bearing in mind that he is perhaps trying a bit too hard to make his points.

Almost all of his sources are secondary. His documentation consists primarily of listings of earlier books, from which he himself presumably obtained his information. Although I do not customarily enjoy pointing out errors in book reviews, I really have to note one pretty strange one (actually, three), where the author refers three times to the Nobel Prizes being awarded in Oslo, Norway rather than Stockholm, Sweden. Obviously he must have been thinking of the Nobel Peace Prize. ■

MARIO BERTOLOTTI

The History of the Laser

Translated from Italian by M.B.B. Boringhieri; IOP Philadelphia,2005, 300 pp, $55

Reviewed by Charles H. Townes

This is an excellent discussion of the scientific background of the laser and how ideas for it developed. It covers pertinent interactions between scientists and their many different contributions, the development of various types of lasers, their performance, and the wide variety of technical and scientific applications. The author has remarkable knowledge of the field, and gives not only an excellent scientific and historical description, but also interesting details, including many personal and pertinent events.This account of laser history begins with the origin and development of basic scientific discoveries which provided the necessary background for masers and lasers. It proceeds to the earliest "in principle" ideas about amplification by stimulated emission of radiation, next the ideas which envisioned practical systems, and then the achievement of real masers and lasers.

Origins of the many different types of lasers are discussed, along with detailed and personal events which are interesting and a vital part of the story. This is followed by laser characteristics, early searches for applications of this breakthrough development, and finally a rather complete coverage of the now many applications of lasers in both science and technology. Overall, the book is impressive in its rather complete coverage and the interesting details connected with all aspects of laser science and history. The author has obviously worked studiously to put together a fascinating presentation of much information.

As one of the participants in laser history, I can say the book is remarkable in its completeness and insight, as well as details of interesting incidents. I believe overall it gives a generally accurate picture and has much correct information which is not generally available. But as one might guess, some of these details are unfortunately not completely correct. Many little errors will probably be noticed by people involved in the story who are still around, such as myself. I illustrate the problem of errors in detail by noting some with which I am personally quite familiar.

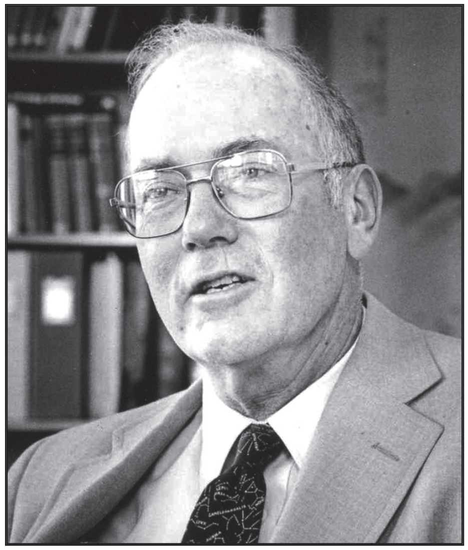
It is said that four institutions initiated microwave spectroscopy of gases shortly after World War II, which were Bell Labs, RCA, Westinghouse, and Columbia University. Actually, I believe there was no work of this type at Columbia University until after I arrived from Bell Labs in 1948 and that General Electric was another company which initiated early work. It is said that at Bell Labs I was assigned work on radar although what I wanted to do was theoretical physics. Actually, I've always been an experimental physicist and wanted to do experimental physics.

It is said that two friends called me at the laboratory to insist that I stop work towards the maser and thus wasting government money. Actually, the head of Columbia's physics department, Polycarp Kusch, and the previous head, I. I. Rabi (yes, they could be called friends) came personally to my office to insist that I stop such work.

It is said that Prokhorov met me for the first time in Great Britain at a 1955 conference and that Basov, "as soon as he read "Townes' letter announcing the construction of the maser, assembled a few months later the first Soviet maser." Actually, both Basov and Prokhorov were at the 1955 conference and we talked together at some length. Our maser was by then operating although nothing had been published on it. I explained its operation, and it was after this that Basov and Prokhorov went home and made their first working maser.

It is said that Gordon Gould received a telephone call at home from me asking for information on his bright thallium lamps. Actually, on October 25, 1957, I talked with him directly in my office, explaining my idea for an optical maser or laser, and asking how much power his thallium lamps achieved to see if they could excite enough atoms to make an optical maser work.

As a check on whether such errors in detail occurred in other parts of this history, I asked Arno Penzias to look at the account of his work, involving discovery of the "big bang". He also found several misleading details.



Charles H. Townes
Photo supplied by Prof. Townes

History is difficult to get exactly right and details are probably a little erroneous in many accounts, even though the overall pictures presented are more likely right. So one cannot trust all the details. However, it is most important that this account gives an objective, balanced view of the laser development, and there are in fact many accurate and interesting details. The book also provides a valuable and insightful view of the complex development of science and technology, and the many interactions within the community of scientists and engineers by which a field typically grows. And the whole story, with the many discoveries and applications associated with masers and lasers, is generally well covered. ■

NAOMI PASACHOFF

Ernest Rutherford: Father of Nuclear Science

Enslow Publishers, Inc., 2005

Reviewed by Forrest Westfall, Okemos High School, Okemos, Michigan

[Editor's note: this volume is part of series of scientific biographies intended for children. Accordingly, this review was written by the son of Ms. Pasachoff, 14 years old, who is interested in science.]

Ernest Rutherford was clearly an important scientist. In 1897, while investigating the newly discovered phenomenon of x-rays, he discovered alpha and beta radiation. In the first decade of the 20th century, he worked on the theory of radioactive decay and used this theory to deduce the earth's age. In 1908, he won the Nobel Prize in Chemistry "for his investigations into the disintegration of the elements." (p. 72) This was before he did the work that he is most famous for. Based on experiments with alpha particles, in 1910 he developed a new model of the atom, in the process discovering and naming the nucleus.

Rutherford was also important in other ways. For one thing, he mentored other eminent physicists. One was Niels Bohr, who used Rutherford's work to develop a theory of the structure of the atom, winning the Nobel Prize in 1922. James Chadwick, who worked with Rutherford as a graduate student, won the Nobel Prize in 1935 for discovering the neutron. Rutherford also helped in the effort in World War I to detect submarines. In 1925, he became president of the Royal Society. In 1931, just six years before he died, he became Lord Rutherford of Nelson. In the early 1930s, he also helped find jobs for refugee scholars fleeing from Nazi Germany.

This book really shows that a person has to work hard and be determined to be a successful scientist. To finance his studies, he had to work for scholarships. He won the Exhibition of 1851 Scholarship, for example. Rutherford also moved a lot in his career. He was born in New Zealand and went to University of New Zealand. Then he went to graduate school at the University of Cambridge working at the Cavendish Laboratory. He next became a professor at McGill University in Montreal, then he became a professor at the University of Manchester. In 1919 he became the director of Cavendish Laboratory.

This book does a good job in explaining why Rutherford was important to science. As a 14 year old, I found it very easy to read (I read it in about 2 hours). I would say it is appropriate for those in the last years of elementary school and in early middle school. ■

Remembering the Manhattan Project—Perspectives on the Making of the Atomic Bomb and its Legacy

Cynthia C. Kelly, Editor World Scientific,2004 xi + 188 pp, $45

Reviewed by Ben Bederson

Cynthia Kelly is the President of the Atomic Heritage Foundation, an organization which she founded with the purpose of protecting and perpetuating historic Manhattan Project sites. To this end she organized a symposium, held in Washington in 2002, assembling a number of Manhattan Project participants, historians, and others who presented talks, reproduced in this volume. (In the interest of full discloser, this reviewer was one of the participants). In addition she presents a plan, submitted to Congress, for a strategy on how to create permanent memorials to this historic enterprise. She offers detailed descriptions of the major Manhattan Project sites, and, finally, she reproduces the seminal "Memorandum on the Properties of a Radioactive Super-Bomb", by Otto R. Frisch and Rudolf Peierls (March 1940). Contributions include those of Richard Rhodes, on the use of the atomic bombs in World War II, James Schlesinger, on the consequences and repercussions of their development and use, Stephen Younger, on the tectonic changes ushered in to government research by the Manhattan Project, Richard Garwin, on the subsequent role of the science advisor in government, William Lanouette on Leo Szilard, Robert Norris on General Leslie Groves, Gregg Herkin on J. Robert Oppenheimer, and quite a few other, interesting talks. For anyone wishing a crash course on the Manhattan Project and its influence on the half-century that followed, this is an efficient and trustworthy way of getting it. Ms. Kelly is to be congratulated, and admired, for her unceasing efforts to keep the Manhattan Project alive in our national memory. (further information in available at www.atomicheritage.org.) ■

SIMON MITTON

Conflict in the Cosmos; Fred Hoyle's Life in Science

Joseph Henry Press, Washington, 2005

Reviewed by John Faulkner, Dept. of Astronomy & Astrophysics, UC Santa Cruz, CA

In the early 1970s Simon Mitton, a newly minted Cambridge Ph.D., resigned after a principled disagreement with the noted radio astronomer Martin Ryle. Fred Hoyle generously threw him a lifeline --- a temporary position at his own Institute of Theoretical Astronomy. A few months after Hoyle's own celebrated but murky resignation from his Cambridge posts, Mitton was appointed to a management position in the successor Institute of Astronomy. He was thus privy to the circumstances surrounding one of Britain's most notorious examples of academic squabbling and intrigue, as well as having many contacts with Hoyle's colleagues, students and rivals. Much later, as scientific editor with Cambridge University Press, he helped publish Hoyle's last co-authored book. These experiences made Mitton a natural candidate to write the first full-length biography following Hoyle's death at 86 in 2001. The result is a well-researched, lively, elegant and gripping account of one of the towering figures of 20th century astronomy and cosmology.

Setting aside Hoyle's spectacular cosmological controversies and tendentious forays into pre-biology and the origin of life, unbiased astrophysicists will acknowledge that he was responsible for much of the dynamism that reinvigorated British and indeed international astronomy after the Second World War. In the 1940s and 50s he substantially advanced our understanding of stellar structure and evolution, particularly of low mass stars. He was the first to establish that the age of the Galaxy exceeded 1010 years. In an astonishingly far-sighted 1946 paper he sketched element synthesis all the way up the periodic table to the iron peak, showing that those most tightly bound elements gave evidence of nuclear processing at then unheard of temperatures of 4 x 109 K or more. In 1953 came a brilliant piece of nuclear detective work that has taken on mythic proportions. Hoyle predicted the existence and properties of an overlooked energy level in the 12C nucleus, needed if reactions inside stars were to produce comparable amounts of 12C and 16O, as much terrestrial life requires. By such insights he created the postwar field of nuclear astrophysics and the accepted world view that all but the merest traces of certain low mass nuclei are forged inside the stars and then distributed, often via spectacular explosions, throughout interstellar space.

As an exponent and popularizer of the new astronomy Hoyle was without peer for several decades. In 1950 he coined the term "big bang" (not capitalized in the scripts) in a popular and controversial BBC radio lecture series that made his name a household word. While his professional work continued, popular books and science fiction followed in great profusion, accompanied by many television specials and serialized science fiction. In 1961 it was estimated that a full 80 percent of the British TV viewing audience watched the final episode of his BBC serial A for Andromeda. Casting this show, Hoyle discovered an unknown star of first magnitude: Julie Christie.

From the outset Fred Hoyle seemed destined to be a maverick. Substantial periods of truancy, coupled with dangerous chemical experiments (he made gunpowder and phosphine in the kitchen) made his parents despair. Getting into Bingley Grammar School by a fluke, Fred won support from a remarkably prescient headmaster. "The boy Hoyle has insight, energy, and originality," he told his old college in Cambridge, "I believe he will turn out to be a swan, no matter what sort of duckling or gosling he now appears." It still took three attempts before Hoyle was admitted to Cambridge to read natural sciences. Switching to mathematics despite concerned advice to the contrary, he hauled himself up to take the Mayhew prize as a top bracketed theoretical applied math/physics student in the notoriously difficult Part III examinations. With World War II approaching, his research supervisors kept disappearing. Paul Dirac briefly supervised Fred; he confessed himself intrigued by the idea of a supervisor who didn't really want a student guiding a student who didn't really want a supervisor!

Through his posting to the British Admiralty's Signals School, Hoyle did work of great significance to the war effort. Exploiting interference between direct and reflected radar beams, he solved the thorny problem of determining the elevation of approaching aircraft. Graphs for his practical method were widely distributed. That and significant work on anomalous over-the-horizon bending of radio beams made him known in central government. He was sent to Washington DC to share his knowledge with the U.S. Navy in a trip that would have many scientific consequences for him. This important work and the key role he later played in the ultimate success of the Anglo-Australian Telescope may surprise those who only know him as a controversial astrophysical theorist.

Mitton generally does a commendable job of presenting and summarizing Hoyle's science. Unfortunately, however, the account of his path breaking work with Martin Schwarzschild (Princeton) on the evolution of low mass stars to the red giant stage is quite garbled. Mitton explicitly asserts, "convection propels the outer layers of the star outward, greatly increasing its size"; he also implies that helium is ignited in the cores of such stars before they become red giants. In practice, convection grossly limits the sizes of such stars; failure to allow for it earlier in part created the conundrum --- of models apparently rushing to become far too large far too early --- that Hoyle and Schwarzschild (HS) resolved. And the ignition of helium ends the first red giant stage. The structure Mitton describes for a star that is finally a red giant --- including the notion that it has two nuclear energy sources of more or less equal importance --- is in fact that of the significantly smaller, post-red-giant, horizontal branch stars. (HS's difficulty in deriving a consistent description of this next phase inspired my own resolution of the problem --- with Fred Hoyle's blessing --- in 1966.) But this is a small flaw in what is otherwise a quite fascinating read. ■

STANLEY FINGER

Minds Behind the Brain: A History of the Pioneers and their Discoveries

New York: Oxford University Press, 2000. xii + 364 pp. Illustrated. Cloth $35.00. Paper edition released March 2005. $24.50.

Reviewed by Sam Feldman, New York University

Stanley Finger teaches and does research in neuroscience and its history at Washington University in St. Louis. He was founding president of the International Society for the History of Neuroscience and is editor-in-chief of its journal1. Finger's earlier book, Origins of Neuroscience2, was a major contribution to the history of the field. But like almost all such books it was organized by scientific topic, and the flow of intellectual history was often lost. In the preface to his more recent Minds Behind the Brain, here under review, Finger writes that because his "students wanted to know more about these pioneers as real people" he realized that a biographical approach would benefit not only his students but the field of neuroscience history as well. And I agree. His approach has produced another important book, this time placing major advances in neuroscience within their religious, social, intellectual and political milieux.

The book's introductory chapters on ancient Egyptian and Greek concepts are followed by biographical sketches of seventeen neuroscientists that span about 1800 years -- from Galen (2nd century) and Vesalius (16th); to Descartes and Willis (17th); to giants of the wonderful3 19th century—Galvani, Gall, Broca, Ferrier, Hitzig and Charcot. The 20 th century is represented by Cajal, Sherrington, Adrian, Dale, Loewi, Sperry and Levi-Montalcini, all Nobel laureates. Finger's list is of necessity selective and every scholar will likely have a personal list of egregious omissions. But his selection is well thought through. Alan Hodgkin happens to be my own example of an unfortunate omission because it was Hodgkin's theoretical and experimental work that established contemporary understanding of neuronal biophysics, the basis for all studies of the cellular basis of neural activity.

The biographical sketches bring the science to life: In the chapter on Galvani we learn of his dispute with Volta over animal electricity; the influence of Franklin's experiments with lightning as electricity; and the early medical use of electricity as discussed by Franklin (anti) and, remarkably, John Wesley and Jean-Paul Marat (both pro). The chapter on Ferrier and Hitzig tells of their 1881 dispute over cortical localization and the 24-year old Sherrington's role in its resolution; and the attempt by early antivivisectionists to have Ferrier convicted of a crime because of his surgery on non-human primates. The importance of historical context is also apparent in the story of the Oxford virtuosi during the English Civil War and Commonwealth era, which brought together a core of scientists that had not been seen in Oxford before (or since): Thomas Willis, Christopher Wren, Robert Boyle, Robert Hooke, John Wallis, John Wilkins, Richard Lower, Thomas Sydenham, John Locke (a student of Willis, who practiced medicine well into early middle age). Their interactions and collaborations produced monumental works and eventually led to formation of the Royal Society. They also ushered in the materialistic approach to natural philosophy that realized its full expression in the 19th century, beginning with Gall and coming to full fruition with Darwin and the neuroscientists that he influenced (e.g., Broca). Importantly, Finger devotes full chapters to Thomas Willis and Franz Joseph Gall. Willis' body of published work, especially Cerebri Anatome4 stands as a monumental contribution to our understanding of the organization of the brain. And by devoting another separate chapter to Gall, Finger makes an important statement about Gall's pioneering work in the study of cortical localization of function.

For the past two years I have taught an upper division undergraduate seminar on the history of cortical localization. *Minds Behind the Brain* has served as an important background resource for students and has provided a charming introduction to the topics covered. As my colleagues in the Humanities have long known, historical context really matters—in the ideas

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and discoveries of science as much as in politics, religion and war. Finger has done an admirable job of teaching us about how history shaped these minds behind the brain.

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1. *Journal of the History of the Neurosciences: Basicand Clinical Perspectives. Philadelphia: Taylor and Francis*
2. *Finger, Stanley. Origins of Neuroscience. New York: Oxford University Press, 1994.*
3. *Wallace, Alfred Russell. The Wonderful Century. New York: Dodd, Mead & Co., 1899.*
4. *Willis, Thomas. Cerebri Anatome. London: Martyn and Allestry, 1664.*

Obsessive Genius: The Inner World of Marie Curie

Noemie Benczer Koller, W.W. Norton & Co., 2004, 320 pp

Barbara Goldsmith

The aura of Marie Slodowska Curie's myth has affected our lives in many sectors, scientific (physics, chemistry, medicine) social and political. Her life story continues to resonate with the aspirations of many young women. She played a particularly important role as a pioneer and as the first woman to achieve major mileposts: her research results were presented under her name, she earned two Nobel Prizes, one shared with her husband Pierre Curie and the other one by herself. However, a Professorship at the Sorbonne eluded her. She was never admitted into the Academy of Sciences and she had to endure feelings of hate and jealousy from her contemporaries.

Barbara Goldsmith wrote a concise but comprehensive overview of Marie Curie's personal and professional life, while maintaining readability and accessibility. In the introduction, she writes about the "disparity between the image and reality" as she sets out to find the "real woman behind this image of towering perfection", a woman "obsessed" by the need to understand nature. I personally do not think the so-called "myth" is so far off from reality. A scientific career, requires, a great deal of focused concentration, dedication, drive if not outright "obsession."

What makes this book different from the many biographies that have already been written: Marie Curie as a scientist, woman, wife, mother, Polish nationalist, or promoter of social causes? The known facts about Marie Curie's childhood and adolescence, difficult start in Paris under modest financial circumstances, social ostracism, intense love for Pierre Curie, the man and the scientist, and, of course, the passionate dedication to her scientific work, are augmented by new data, recently made available to the public, from Marie Curie's personal letters, her diary as well as the book she wrote on Pierre Curie's work after his death.

Goldsmith maintains a reasonable balance in the discussion between the disparate elements of her life: on one hand, depression and illness, professional and social adversity, concerns about her daughters' education, the enormous sadness caused by the untimely death of her husband, the scandal brought on by her relationship with Paul Langevin, social ostracism, and on the other hand, the success in her research efforts, her dealing with fame, the dedication to the WWI effort and humanitarian concerns closely shared with Pierre Curie as well as the recognition that to support her laboratory, even a relatively withdrawn scientist as she was, needs to carry out a certain amount of public relations activity. However, the driving force in Marie Curie's life stems primarily from her deep dedication to science, and therefore the description of her work needs to have priority in any biography, even one focused on personal traits.

While the excitement at the time surrounded applications of Roentgen's discovery of x-rays, for her doctoral research Marie Curie chose to pursue and understand the phenomenon of natural radioactivity discovered by Becquerel. Pierre Curie's brother Jacques had developed a precise piezoelectric quartz balance that Pierre Curie had been using in his own investigations. The application of this very delicate instrument to the quantitative measurement of the ionization produced by the radiations emanating from her samples led to the realization that elements different from the known U and Th, namely Ra and Po, were responsible for the observed radiations. These measurements contrast with the crude separations of microscopic amounts of new elements from tons and tons of pitchblende ore. But both activities define crisply what it takes for success in science. While the ultimate goal of understanding the origin of the emitted energy eluded her, her systematic, quantitative, approach set the standard for research in the field.

Marie Curie's two greatest contributions, the fact that radioactivity could be precisely measured and thus be used as a diagnostic for the discovery of new substances, and that it is a nuclear property, are not given enough emphasis in this book. For a scientist and probably a lay person as well, a more thorough description of the workings of the apparatus should have been given more weight. The technique is nevertheless amenable to a description in lay terms and its discussion would go a long way to give a feeling to the non-scientist of what real life in the laboratory entails. Goldsmith does effectively discuss the aftermath of the discovery, the importance of the need to establish standards of radioactivity, and the interactions with other scientists, in particular Rutherford and Lise Meitner. She is also careful in identifying the work done jointly by Pierre and Marie Curie from the research they caxried out individually. There is also an intriguing reference to Pierre Curie's study of the effect of gravitation on the radiation mechanism.

The final chapters deal with Marie Curie's scientific legacy, the importance of the Institut du Radium in future nuclear physics developments, and of her impact in training young scientists and women in particular. Finally, the influence of Marie Curie on the training of her daughter Irene with whom she had worked while Irene was still a student is stressed. The book draws extensively from the literature, primary sources and interviews with Marie Curie's granddaughter, Helene Langevin. The book has a good bibliography but would have benefitted from a glossary and index. ■

Boltzmann was manic-depressive and also not in good physical health, which probably were much more significant factors in precipitating his suicide in 1906 than the rejection of his atomistic ideas by others. (p. 152) Henri Becquerel, not Marie and Pierre Curie, discovered radioactivity. (pp. 159, 276) Röntgen was born in Lennep, not Düsseldorf, Germany. (p. 182) Bohr believed from 1913 to 1925 that the light emitted when an electron undergoes a transition in an atom consists of electromagnetic waves, not light quanta. (p. 277) They are known universally as the Lorentz, not the Lorentz-FitzGerald transformations. (p. 278).

OTHER ITEMS OF INTEREST

Remembering Julian Schwinger

Edward Gerjuoy, University of Pittsburgh

In about 1999 the APS, in the course of its celebration of the hundredth anniversary of the Physical Review, produced a number of panels providing a time line for the progress of physics during the past century. One of these panels, headed "1945. The Post-War Boom," features a large full length photo of Richard Feynman and includes the following text concerning the problem of making quantum mechanical calculations involving electromagnetic forces: "Together with American colleagues and Japanese physicists who had worked along similar lines while they were out of touch with the West during the war, Feynman solved the problem by creating Quantum Electrodynamics (QED)."

The just quoted text from an APS-prepared panel illustrates my impetus to set down these recollections, namely that in recent years the remarkable researches of Julian Schwinger, who like Feynman unquestionably was one of the most gifted and influential theoretical physicists America has produced, appear to be increasingly overlooked. In fact Schwinger (who died in 1994) and Richard Feynman (who died in 1988) shared (with Shin-Ichiro Tomonaga) the 1965 Nobel Prize in physics for the development of quantum electrodynamics. Moreover Schwinger performed his calculations explaining the two leading QED experiments, namely the Lamb Shift and the value of $(g - 2)$, at least as early as Feynman did. Also, although Schwinger and Feynman were born in 1918 only three months apart, Schwinger began to publish far ahead of Feynman and indeed was much more famous than Feynman up until about 1955.Indeed here is what Feynman himself had to say about about Schwinger in a talk describing the first time he met Schwinger, which was in 1943 when they both were 25 years old: "It was not until I went to Los Alamos that I got a chance to meet Schwinger. He had already a great reputation because he had done so much work...and I was very anxious to see what this man was like. I'd always thought he was older than I was because he had done so much more. At the time I hadn't done anything."

Actually Schwinger had become well versed in many branches of physics even before graduating from his New York City public high school at the age of 16, had published a number of important papers before he was 20, and had earned his Ph. D. degree by age 21. These early accomplishments are all the more remarkable because his knowledge of physics was almost entirely self-taught, via the reading of books in the public library. Schwinger once remarked that he began reading the Encyclopedia Britannica at a very early age, and when he came to the letter P "that was it."

I will devote my remaining space to some personal recollections of mine involving Schwinger. I first met Schwinger in a classical mechanics course at the City College of New York, where we both were undergraduates. It took hardly more than minutes for me and the other students in the class (many of whom were very bright and later became professional physicists) to realize that we were dealing with a higher order talent. By this time, however, Schwinger already had developed his lifelong habit of working at night, with consequent irregular class attendance and instructor displeasure. As a result I earned an "A" in that mechanics course but Schwinger received only a "B", a fact that much amused both Julian and myself in later years. My close interactions with Julian, resulting in a lifelong friendship, began not in City College but in Berkeley, where I was a physics graduate student working for my Ph.D. under J. Robert Oppenheimer when Schwinger showed up in 1939 as Oppenheimer's research associate, in essence his postdoc. Julian and I soon began working on an ultimately published joint paper whose content is of little interest herein beyond the fact that it involved calculating about 200 fairly complicated spin sums, which sums Julian and I performed independently and then compared. To have the privilege of working with Julian meant I had to accommodate myself to his working habits, as follows. Except on days when Julian had to come into the university during conventional hours to confer with Oppenheimer, I would meet him at 11:45 PM in the lobby of his residence, the Berkeley International House. He then would drive us to some Berkeley all night bistro where he had breakfast, after which we drove to Leconte Hall, the Berkeley physics building, where we worked till about 4:00 AM, Julian's lunchtime. After lunch it was back to Leconte Hall until about 8:30 AM, when Julian was ready to think about dinner and poor TA me would meet my 9:00 AM recitation class. Since I had just gotten married, and still was young enough to badly need my sleep, these months of working with Julian were trying, to put it mildly.

What made them even more trying is the fact that when Julian and I carefully worked out together the 20 or so spin sums where our independent calculations disagreed, Julian proved to be right every time! I accepted the fact that Julian was a much better theorist than I, but I felt I was at least pretty good, and was infuriated by his apparent constitutional inability to make a single error in 200 complicated spin sum calculations. This inability stood Schwinger well when he embarked on the calculations that earned him the Nobel Prize. As Schweber writes:[1]

The notes of Schwinger's calculations are extant [and] give proof of his awesome computational powers…These traces over photon polarizations and integrations over photon energies…were carried out fearlessly and seemingly effortlessly…Often, involved steps were carried out mentally and the answer was written down. And, most important, the lengthy calculations are error free!

I wish I had the space to recount more of my interactions with Julian. Let me conclude simply by saying that though Julian certainly realized how extraordinarily talented he was, he never gloated

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about his error free calculations or in any other way put me down. I speculate that these generous, gentlemanly attributes of Schwinger's account for the fact that, although his interactions with his students warrant some justified criticisms, he ultimately supervised the successful Ph.D. theses of more than seventy students, including four Nobel Laureates (Ben Mottelson, Shelly Glashow, Walter Gilbert and Walter Kohn) as well as many other distinguished researchers.[2] Even if explicitly mentioning Schwinger's name no longer is fashionable, these Ph.D. students produced by Schwinger, who utterly dwarf Feynman's corresponding production in both number and reputation, continue to instill Schwinger's techniques and his ways of approaching physics problems into the psyches of new generations of physics students.[3] ■

1 *Silvan S. Schweber, "QED and the Men Who Made It: Dyson, Feynman, Schwinger and Tomonaga" (Princeton University Press 1994), p. 300. The talk was given at a celebration of Schwinger's 60th birthday.*

2 *David S. Saxon, "Julian Schwinger (1918-94)", Nature v. 370, p. 600 (August 25, 1994).*

3 *These recollections are discussed in "Climbing the Mountain" (Oxford 2000), the very comprehensive biography of Schwinger by Jagdish Mehra and Kimball Milton, who had interviewed me.*

More About Einstein—Einstein Revisited

Robert A. Naumann, Dartmouth College and H. Henry Stroke, New York University

It has been fifty years since the two of us spent an hour with Albert Einstein talking about atomic clocks and the possibility of observing the gravitational red shift in the hyperfine transition in cesium. This was just over two weeks before he died. Ten years ago we decided to recount this visit, the story of the experiment proposed by I.I. Rabi, a little of the Princeton scene and Einstein's non-physics acquaintances who led to the meeting. Earlier in this Newsletter Robert H. Romer describes his excitement as a graduate student at the opportunity to meet Einstein. We, though presumably more seasoned, (RAN as a young Princeton University faculty member, HHS a fresh post-doc), were no less excited, and when presented with the invitation at the very beginning of April our reaction was "April Fools' joke" This stuck in our minds, and when we wrote about our Sunday morning visit, a keen reader picked up on the fact that our visit was not 1, but 3 April 1955! Anyway, we prepared a rather extensive manuscript on the meeting and sent it to Physics Today. The reply was: interested in atomic clock story, not so much about Einstein. We then thought of a different publisher: Physics World. They answered: interested in Einstein story for their last page "Lateral Thoughts", not so much in the clock story. So we ended with two stories (plus an addendum)[1, 2, 3]. We would have left it at that, except for the fact that on the eve of the Einstein year we were contacted by Physics World to get possibly some more information on him, toward the end of his life, from a diminishing number of people who had contact with him. RAN sent on to Physics World a letter describing once again sketches of Einstein in Princeton, as an attendee at Palmer Laboratory (Princeton Physics Department) colloquia, and as an everyday inhabitant of Princeton. We quote from this letter: "On the memorable Sunday morning in April 1955, Einstein's friend Paul Oppenheim-Errera drove us to the white wooden residence on Mercer Street. After entering the home we climbed the stairway leading to the upper enclosed sun porch where Einstein stood up to meet us. After introductions, Einstein asked us to sit and immediately came to the point: "Please inform me 'what is an atomic clock?' We began by reminding our host that atoms possessed discrete energy levels 'Ja, Ja, I understand that;'then reminding him that atomic transitions between levels had definite frequencies. After some further discussion, we finally realized that the key technological advance that Einstein was not informed about was that electronic scaling circuits had been developed so that time intervals could be digitized and measured to high precision by gating a very stable (atomic) oscillator source. With the help of a sketch of the registers of a cascade binary counting circuit we illustrated the successive registrations of the peaks from a sinusoidal signal. As we already wrote earlier[1], at that point Einstein, having understood the principle of electronic counting, exclaimed 'this is wonderful, this is just like our decimal system!' Following some further discussion, Einstein graciously accompanied us downstairs, warmly thanked us for our visit and saw us out." The remainder of recollections were peripheral. Einstein was known to be an avid, but not necessarily the most accomplished musician. One of us (HHS) had occasion to acquire a lovely Pleyel grand piano from Einstein's next-door neighbour, Mrs. Tin Harper. She had had Einstein over a good number of times for musicales. Raphael Hillyer, a founding member of the Juilliard String Quartet, told us that on some such occasions when Einstein joined in, he would ask them to slow down the tempo, and even then... But this is not why the world remembers him! ■

1 *R.A. Naumann and H.H. Stroke, Physics World 9, 76, April 1996.*

2 *Robert A. Naumann, H. Henry Stroke, Physics Today 49, 89-90, May 1996.*

3 *Robert A. Naumann, H. Henry Stroke, Physics Today 51, 15, 97 February 1998. 2*

Born Again—An Interchange

Did Heisenberg Spit at Max Born? Two Recent References and a Non-Reference

Paul Lawrence Rose, Mitrani Professor of European History, The Pennsylvania State University, University Park, PA 16802 (PLR2@psu.edu)

Two recent references to the episode when Heisenberg spat at the feet of Max Born during an antisemitic tirade have indicated skepticism about the event having occurred as it was recounted in my book Heisenberg and the Nazi Atomic Bomb Project. A Study in German Culture, University of California Press, 1998, pp. 314-7. In the abstract of his paper read at the American Physical Society's Tampa Meeting in April 2005, Harry Lustig asserts that "it is highly unlikely that Heisenberg spit at Born or on the floor on which they stood". The basis of Dr. Lustig's conclusion seems to be that "none of the historians of science, German and American, whom I have consulted credit it". It seems to me unlikely that such a statement can have any evidentiary or probative value for either physicists or historians. Of similar character is another argument put forward: "No known biography of Heisenberg mentions the alleged episode, and none of his obituaries alludes to it". The second recent discussion by Nancy Thorndike Greenspan in her new biography of Max Born, The End of the Certain World, Basic Books, New York, 2005, is also somewhat relaxed in its critical understanding of evidence. In a Special Endnote at p. 309, Dr Greenspan observes that a spitting incident is reported to have taken place, as she vaguely says, "sometime in the early 1950's", but that she has found no written evidence of it, although "I have heard the story from the person who heard of it from [his wife] Hedi". But then Dr Greenspan proceeds to throw doubt on it: "I have no doubt that she and Hedi had a conversation about an unpleasant incident. A possible explanation is that in the telling the story became garbled". There seems to be some circularity of argument here; Dr Greenspan assumes the story is unfounded and then concludes the account is garbled. Dr. Greenspan also denies any written source for the incident. As we shall see, there is indeed a letter describing it which has been printed in part in two books which she is familiar with, one by Arnold Kramish and the other my own. Dr Greenspan is fully aware of the evidence in my book since I discussed it attended a Washington meeting in which I referred to it.

What then is the evidence for the spitting event having actually taken place? It is a letter written by Max Born's long-time Scottish secretary, Mrs. Ray W. Chester, at the University of Edinburgh, to the American physicist Arnold Kramish while he was preparing his book The Griffin, Boston 1986, a biography of the Allied spy in Berlin, Paul Rosbaud. Rosbaud transmitted a great deal of crucial information on the German atomic bomb project to London during the war and was well acquainted with Heisenberg and other leading figures in German science. At p. 44 of his book, Dr Kramish reports this spitting incident and quoted a few sentences from the letter. However, editorial intervention stemming from the book's legal difficulties resulted in the episode being placed in a context that suggested it had occurred in the 1930's (according to p. 118, it was in 1933) rather than in the post-war period as in fact it did. In my book I dated the incident to Born's visit to Goettingen in 1953, but there is a possibility it may have occurred either during Born's first return to Germany in 1948, or in the course of a later visit of April 1954 when he is known definitely to have had a meeting with Heisenberg. Subsequently Dr Kramish was generous enough to supply me with a photocopy of the letter for use in my own book, where it is reprinted in extenso at pp. 315-6. I give below an abridged version for readers' convenience:

"[I told Paul Rosbaud who had a hostile opinion of Heisenberg that] I had been repelled by .his behavior towards Max Born years after the war on the occasion of the Borns returning to Goettingen to arrange restitution of his pension rights... Heisenberg was by then professor in Goettingen and when the Borns went to visit him at that time, they were met with anti-Jewish sneers and obscenities and in the end Heisenberg spat on the floor at Max Born's feet....When they returned and that first morning when I warmly enquired about their trip and how they had fared, very reluctantly Max Born confided in me about this great shock and painful encounter...Later Mrs Born gave me her version and ended with a statement which I never forgot and said simply at the end, `And my poor Max, he wept'...

This was one of the stories Nick Kemmer wanted me to repeat to him before he wrote his review...and after I had repeated it, he then said what puzzled him was that years later at some international meeting or other, he had seen Max and Heisenberg apparently amicably talking to each other. I did not doubt that statement...Indeed, when Max Born first confided this story after his return, he begged me to keep it confidential since this Heisenberg had already gained so much hostility and suspicion that must irk him - and would be punished enough without either he or Hedi circulating that ugly experience amongst other colleagues and friends...Max had patted my hand and said simply that he and Hedi had had so many more years of learning to forgive."

This sincere and highly circumstantial letter is powerful evidence for the incident and it seems strange to me that any open-minded person could read it and doubt its veracity. In any case, in the face of such assertive and positive testimony, the onus of disproving its validity rests upon those who would ignore it or reject it. It is particularly noteworthy that the letter states that Max's account was corroborated by Hedi, thus reducing the possibility that the secretary may have misunderstood Max's own telling of it. Moreover, there is an allusive confirmation of the letter's content and the writer's reliability in a review of Heisenberg's Collected Works that Born's Edinburgh successor, Nicholas Kemmer, wrote for Nature, 313, 28 February 1985, 826: "I am reminded of the way a formerly close colleague of Heisenberg's returned from a postwar reunion in deep distress—a gulf remained unabridged."

Incidentally, the provocation by Heisenberg on this occasion must have been extreme. Born was usually able to regard Heisenberg's pro-Nazi attitudes with some degree of ironic amusement —or resignation. But he was quite clear-eyed about how ingrained these attitudes were. After the war, Born commented in a letter to

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Einstein regarding a meeting with Heisenberg in December 1947 that his colleague was "as pleasant and intelligent as ever, but noticeably `Nazified,' "no doubt an allusion to Heisenberg's irresistible urge to continue to rationalize the Nazi regime which so appalled and bewildered his old German-Jewish friends as well as other British and American physicists. The letter of 31 March 1948 is printed in The Born-Einstein Letters, new edition, with preface by D. Buchwald and K. Thorne, Macmillan, New York, 2005, p. 163, there is no mention of the incident itself.

If Professor Lustig wants to argue that the incident never took place, he must refute the positive evidence of this letter by demonstrating that the writer was delusional or lying outright. From the high regard in which she was held by her circle at Edinburgh, including the Borns and Kemmer, both these characteristics seem impossible to attribute to her. The veracity of the spitting incident, therefore, holds. Its description does not need to be proven. Rather, it is those skeptics who persist in denial of the historical truth, who need to disprove the letter's recounting of the affair.

Response to Professor Rose

Harry Lustig

One thing I never encountered before is a 1300 word critical response to a 200 word abstract for a 20 minute paper which the critic has not heard or read. (Professor Rose devotes a few lines to attacking Nancy Thorndike Greenspan for stating in her biography of Max Born that the spitting incident did not take place, but most of his letter is devoted to attempting to refute what I wrote in the abstract. I should add that when I wrote it, Ms. Greenspan's book had not yet come out, but when it did, it helped me to strengthen the case that Heisenberg almost certainly did not spit at Born.) My response to Paul Lawrence Rose will be briefer than his letter; for corroborative details and documentation he will have to await the publication of the paper.

Professor Rose makes two dialectical points: 1) the account by Mrs. Chester that the spitting incident took place is unimpeachable and 2) all the accumulated circumstantial evidence that it did not is worthless.

Rachel (Ray) Chester is the only source for the allegation. Nicholas Kemmer's veiled allusion, Arnold Kramer's account in The Griffin, and Paul Rose's expanded claims in his book all come from Chester and they did not get her story independently. Rather, Kramish heard from Kemmer, got Chester to tell her story and passed the story on to Rose. I should add that Ms. Chester's account, as reproduced by Rose in his letter from pp 314 - 317 of his book is fully rendered in my paper. Ms. Chester told her story in 1985, thirty-two years after she allegedly heard it from Mrs. Born. To argue, as Professor Rose does, that the only way to be allowed to discount Ray Chester's account is to demonstrate that she "was delusional or lying outright" is absurd. Historians know that such long delayed dredging up from memory is notoriously unreliable. Nancy Greenspan is of the opinion that Ray Chester may have confused the Heisenberg non-happening with a humiliating incident that Max Born was subjected to earlier at the Indian Institute of Science in Bangalore.

Nowhere else is in the extensive Born literature is there the slightest mention of or allusion to the event recounted by Rose and Kramish, Not in Kemmer's long obituary for Born (perhaps because Kemmer had realized by then that, contrary to what he had written in the Nature review, to the effect that the gulf between Heisenberg and the unnamed colleague – presumably Born – remained unbridged, he knew that their relationship had remained cordial till the end of Born's life). Not in any of the numerous letters by Born (including those to Einstein) or to Born that Nancy Greenspan found in the archives in Britain, in Germany, and in the possession of the Born family. Not in Hedwig (Hedi) Born's diary, in which she recorded the most intimate details of her and Max's life. Not in any of the places where one would reasonably expect to find mention of an incident which, according to Chester, moved Born and his wife to tears.

Can I prove conclusively that the spitting incident related by Rose did not take place? To do so one would have to prove that Heisenberg was not in Goettingen while Born was there to receive the freedom of the City in 1953. Although there is evidence to that effect, it is not foolproof. Besides Professor Rose has already taken out insurance against such an eventuality, by mentioning, in his letter, two other dates when the event could have taken place, although in his book he states unequivocally that it occurred in July 1953.

As for "those sceptics and Heisenberg apologists who persist in their denial of the historical truth," there is in the first place no question of anyone having persisted, because to the best of my knowledge, no one before has taken the trouble to look into the Rose–Kramish allegation that Heisenberg spit at Born. As to being a sceptic of poorly documented claims, I plead guilty. The insinuation that I am a Heisenberg apologist is not worthy of a refutation but will nevertheless receive one in the published paper. ■

Editor's Note:

The reader will of course have to judge the merit of these two opposing opinions. Should anyone care to offer additional hard information concerning this matter we will be happy to consider same for publication in the Newsletter. I must however point out one journalistic test of reliable information, which is especially important these days because of some recent transgressions of the criterion: It is generally accepted that two separate, independent sources are required for reliable information.

SIDNEY COLEMAN—QFT and QCD: Past, Present and Future

Harvard University, March 17-18, 2005

Nina Byers, Kameshwar C. Wali

A stellar array of speakers, former students and associates participated in a historic symposium (Sidneyfest 2005) honoring Sidney Coleman, teacher and seminal contributor to quantum field theory (QFT) and quantum chromodynamics (QCD). For the proceedings see http://physics.harvard.edu?QFT/sydneyfest.htm.

Suffice it to say here that in addition to his fundamental contributions to theories of strong interaction symmetries, of spontaneous symmetry breakdown, of duality in relativistic quantum field theories, of the consmological constant and of quantum effects in black hole dynamics, we owe to Sidney our characterization of the property of QCD that explains strong interaction physics' asymptotic freedom and infrared slavery. ■

Seven Pines Symposium

Report by Roger Stuewer

The Seven Pines Symposium is dedicated to bringing leading historians, philosophers, and physicists together for several days in a collaborative effort to probe and clarify significant foundational issues in physics, as they have arisen in the past and continue to challenge our understanding today.

The ninth annual Seven Pines Symposium was held from May 4-8, 2005, on the subject, "The Classical-Quantum Borderlands." It was held in the Outing Lodge at Pine Point near Stillwater, Minnesota, a beautiful facility surrounded by spacious grounds with many trails for hiking and bird-watching. Its idyllic setting and superb cuisine make it an ideal location for small meetings. Its owner, Lee Gohlike, is the founder of the Seven Pines Symposium; he outlined its goals in his opening remarks.

Unlike the typical conference, the talks are limited to 30 minutes, twice as much time is devoted to discussions following the talks, and long midday breaks 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 of the participants. Twenty-two prominent historians, philosophers, and physicists were invited to participate in this year's symposium.

Each day the speakers set the stage for the discussions by addressing major historical, philosophical, and physical issues pertaining to the subject of the symposium. Thus, the morning of Thursday, May 5, was devoted to the general topic of "Classical and Quantum Worlds," with Gerard't Hooft (Utrecht) speaking on "Is there Classical beneath Quantum Physics?" and Klaas Landsman (Nijmegen) speaking on "Between Classical and Quantum." That afternoon the general topic was "Classical and Quantum Chaos," with Martin C. Gutzwiller (IBM) speaking on "History and Problems" and Eric Heller (Harvard) speaking on "Quantum Chaos." The morning of Friday, May 6, was devoted to the general topic of "Reductionism and Emergence," with Don Howard (Notre Dame) speaking on "Philosophical Perspectives" and Philip Stamp (British Columbia) speaking on "Emergence Behavior in Complex Systems." That afternoon the general topic was "Decoherence and Quantum Measurement," with Wojciech H. Zurek (Los Alamos) speaking on "Decoherence and the Interpretation of Quantum Mechanics" and Anthony J. Leggett (Illinois) speaking on "Does Decoherence Solve the Quantum Measurement Problem?" The morning of Saturday, May 7, was devoted to the general topic of "Decoherence and Gravity," with James B. Hartle (UC Santa Barbara) speaking on "Classical Predictability in a Quantum Universe" and Roger Penrose (Oxford) speaking on "Gravitationally Induced State Reduction." That afternoon Michel Devoret (Yale) spoke on "Macroscopic Quantum Physics in Laboratory Experiments" and Michel Janssen (Minnesota), to recognize the World Year of Physics, spoke on "Einstein: The Sage of Princeton versus the Scientist as a Young Man." The closing discussion on Sunday morning, May 8, was chaired by Roger H. Stuewer (Minnesota).

Lee Gohlike, the founder of the Seven Pines Symposium, has had a lifelong interest in the history and philosophy of physics, which he has furthered through graduate studies at the Universities of Minnesota and Chicago. To plan the annual symposia, he established an advisory board consisting of Roger H. Stuewer (Minnesota), Chair, Jed Z. Buchwald (Caltech), John Earman (Pittsburgh), Geoffrey Hellman (Minnesota), Don Howard (Notre Dame), Alan E. Shapiro (Minnesota), and Robert M. Wald (Chicago). Also participating in the nineth annual Seven Pines Symposium were Robert Batterman (Ohio State), Richard Healey (Arizona), Antigone Nounou (Minnesota), Serge Rudaz (Minnesota, and William Unruh (British Columbia). ■

History *of* Physics
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