History of Physics
NEWSLETTER
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Report From The Chair

Does Physics History Matter?

by Robert H. Romer, Amherst College, Forum Chair

Perhaps the title got your attention, so let me promptly explain what I mean. Is it important that serious history of physics be included in the *professional education of physicists*? I think that for many of us who belong to and support the APS history forum, even — or perhaps especially — for those like me who are not professional historians of physics, it is almost an article of faith that the answer to my question is an unqualified "yes." I said as much myself several years ago, in my election statement when I was a candidate for the forum position I now occupy. But I wonder whether this is really true. My own formal education included many of the usual tidbits of history ("Newton was born in the year that Galileo died." "Einstein was – or was not – influenced by the Michelson-Morley experiment."). But would I have been a better *physicist* if I had had a course or two in the history of physics? Did Feynman's time at MIT and Princeton expose him in a serious way to the fascinating history of our subject? Was he familiar with the "Bohr model", with the trials and tribulations of the "Old Quantum Theory," and if so, did it help (or perhaps hinder) him on the way to his formulation of QED?

Now there is a very large group of people, those who will *not* become professional scientists, who definitely *should* be exposed to enough of the history of science — preferably in their high school or introductory college courses — so that they will understand that neither physics nor any other science is a finished product, that there are numerous false starts, dead-ends, and misleading experimental results. It might just help them to understand that the fact that Darwinian or neo-Darwinian evolution does not explain every last detail of every living organism does not mean that some other idea, untested or untestable, has an equal claim to time in the science classroom, that the fact that none of us were present at the big bang does not mean that the big bang is "just a theory" with no successful explanations to its credit, that someone else's creation myth is just as deserving of our attention. Laboratory experience, too, is important for those who are not en route to scientific careers. In their high school science labs they are not going to "discover the law of conservation of momentum" (what a ridiculous idea!), but they will learn that real experiments deal with real objects in the real world, that many experiments do not work, that equipment is often broken or dropped and that resistors burn out. So-called "simulated experiments" are not only oxymorons but also, as I have written elsewhere, creations of the devil. When I was editor of the *American Journal of Physics*, I once used that term in a letter to a would-be author, rejecting a paper because, I added, his simulation had nothing to do with *physics*. That letter did not make me a new friend. (Few rejection letters do, and making friends is not part of an editor's responsibility anyhow.)

Those who go on to careers in science will learn all too soon that many if not most theoretical adventures are unsuccessful, that

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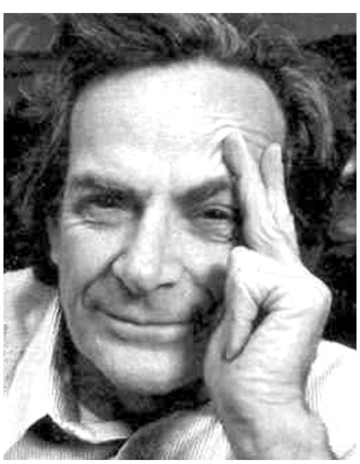
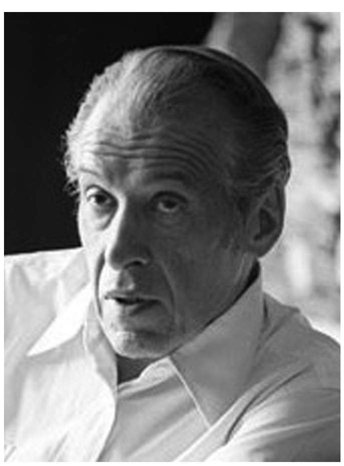

Feynman and Schwinger — See page 19.

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History of Physics
NEWSLETTER

The Forum on History of Physics of the American Physical Society publishes this Newsletter semiannually. Nonmembers who wish to receive the Newsletter should make a donation to the Forum of $5 per year (+ $3 additional for airmail). Each 3-year volume consists of six issues.

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ERRATA

The book review "Obsessive Genius: The Inner World of Marie Curie" which appeared in the last Newsletter inadvertently reversed the author and the reviewer. The author is Barbara Goldsmith. The reviewer is Noemie Benczer Koller. We apologize to both reviewer and author for this error. There was an error in the book review "Ernest Rutherford: Father of Nuclear Science." The reviewer of the book was the son of Catherine Westfall, not the author Naomi Pasachoff.

Report from Associate Editor

For the past three years, Ben Bederson has done an exemplary job as Editor of the *History of Physics Newsletter*, the principal voice of our Forum, after assuming this extremely important post from our long-standing (and long-suffering?) Editor Bill Evenson. All Forum members owe Ben a debt of gratitude for his service — not only in this role but also as Forum Chair and also Chair of the committee that established the Pais Prize. I have been fortunate to work with him as Associate Editor and to serve as a member of that committee.

A few of Ben's fine attributes are his organizational skills, his ability to keep projects or publications moving forward on schedule, and his talent for working well with a wide variety of often headstrong scientists. No doubt he developed some of these attributes while serving as APS Editor in Chief, Editor of *Physical Review A* and as Department Chair and Dean of Graduate Studies at New York University. The Forum has been the immediate beneficiary of his editorial and administrative experience.

I first encountered Ben's editorial side when contributing an article to the volume *More Things on Heaven and Earth: A Celebration of Physics at the Millennium*, a collection of articles on the history of physics that he edited in 1999. Published by the APS and Springer Verlag, it is an impressive book that I was pleased to be a part of. Ben has a soft but firm approach to editing, encouraging his authors along but setting clear deadlines and making sure they respect them.

His administrative skills really came through during the years he chaired the Forum's Award Committee. We had difficult goals to achieve and issues to resolve, and often disagreed on specifics, but Ben's gentle guiding hand on the gavel kept us working together well and focused on our objectives. We almost always reached a consensus that all of us could enthusiastically support. The fact that we surpassed our original goals and were able to establish an APS Prize rather than an Award can be attributed in part to Ben's steadying leadership.

Forum Chair Robert Romer has asked me to step in and try to fill Ben's shoes as Editor for the next three years, and — subject to approval of the Forum Executive Committee when it meets in Dallas — I look forward to this opportunity. As Editor and then Contributing Editor of the SLAC quarterly journal *Beam Line*, I have good experience working with physicist authors and on articles about physics history. After serving an apprenticeship with Ben as the Associate Editor of the *Newsletter*, I feel ready to take on this responsibility. ■

by Michael Riordan

Report from the Chair

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data are almost always contaminated with noise and experimental uncertainties, that experiments fail as often as they succeed. They do not need to take a course to learn those lessons.

Now, of course history of physics is important and the question mark on the title of this brief essay should not be taken as implying anything else. It is an important field of scholarship that should be pursued and supported, like so many others, from medieval history to biology and physics itself. Those of us who are physicists but who make no claim to be historians of physics are in a particularly fortunate position to lend support to the field. And exposure to the subject will enrich anyone's life, as will exposure to Caravaggio and Shakespeare.

But still I wonder whether history of physics is important in the professional education of physicists, *as physicists*. There may well be a literature on this topic that I am unaware of. The few studies I have seen of the professional education of physicists consisted largely of interviews with quite distinguished scientists, generally late in their careers when their memories of the factors that influenced them are often becoming unreliable with the passage of time. It would be more interesting, I think, to look at the educational backgrounds of physicists who are successful but not so much in the limelight and to ascertain not so much the recollections they have in their old age but to talk to them in mid-career or earlier.

Several years ago, I would have thought the question raised in my first paragraph had a simple answer. But the privilege of serving as an officer of this forum led me to wonder about this issue — though in no way has it diminished my view of the importance of this group and of its activities. ■

History Sessions at March & April Meetings

Report by Robert H. Romer

The FHP Program Committee, chaired by Virginia Trimble, has arranged a number of interesting sessions at the March meeting (Baltimore, March 13-17) and the April meeting (Dallas, April 22-25).

At Baltimore, where all FHP sessions will be held on Thursday, March 16, there will be a session of invited papers at 8:00 A.M. on the history of low temperature laboratories, organized by George Zimmerman. Talks by Robert Wheeler, Russell Donnelly, Horst Meyer, and David Lee will be followed by a panel discussion with John Reppy, Robert Romer, Gerhard Salinger, and George Yntema. This will be followed by a series of contributed papers at 11:15 A.M., and then at 2:30 P.M. a series of invited talks on the history of critical phenomena by Michael Fisher, Guenter Ahlers, Leo Kadanoff, Johanna Levelt Sengers, and Alexander Voronel. (Professor Voronel's talk is the "Richard T. Cox Lecture," sponsored by Robert Resnick.)

At Dallas, Saturday, April 22 will feature two sessions on Cosmology: Past Present and Future, cosponsored by the Divsion of Astrophysics. The first session, at 11:45 A.M., will include talks by Dennis Danielson (on prerelativistic cosmology), Elizabeth Barton (on the current status of observational astronomy), and John Carlstrom (on future observations of the cosmic microwave background radiation and other topics in observational cosmology). At 1:30 P.M. on the same day, there will be a second cosmology session, with talks by Helge Kragh (on the establishment of the standard hot big bang paradigm), David Spergel (on the current situation in theoretical cosmology), and Sean Carroll (on the future of theoretical cosmology). Professor Kragh's talk is the "J. Robert Oppenheimer Lecture", sponsored by Philip Morrison and Robert Christy. Then at 3:30 P.M. on Saturday, we will have the first of two sessions on Parity Nonconservation (the fiftieth anniversary of the discovery), cosponsored by the Division of Particles and Fields, with talks by T. D. Lee, R.H. Hudson, and V. Yuan.

On Sunday afternoon, April 23, at 1:15 P.M., there will be a session on Pioneering Women in Astronomy, cosponsored by the Committee on the Status of Women in Physics, with talks by Katherine Haramundanis (on Cecilia Payne, who showed that the stars are made mostly of hydrogen and helium), Jean Turner (on Henrietta Leavitt, who established the period-luminosity relation for Cepheid variables – the "Dorrit Hoffleit Lecture"), and Jill Tarter (Director of the SETI Institute, on "leading teams"). This will be followed at 3:15 by the second session on Parity Nonconservation, cosponsored by the Division of Nuclear Physics, with talks by C. N. Yang, L. Lederman, and J. Conrad.

On Monday afternoon, April 24, at 3:30 P.M., we will have a joint prize session, at which our second Pais Prize winner, John Heilbron, will deliver the Pais Lecture, and where the winner of the Forum on Physics and Society's Szilard Prize will deliver the Szilard Lecture. Following this prize session there will be held the annual business meeting of the history forum, to which all members of the FHP are of course invited.

There will also be one or more sessions of contributed papers during the April meeting. ■

Elections: Candidate Bios and Statements

The FHP Nominating Committee has chosen a slate of candidates for the '06 election. You will be asked to vote for Vice-Chair and three At-Large Members of the Executive Committee. The person chosen to be Vice-Chair becomes Chair-Elect in 2007 and Chair in 2008. If you have an email address registered with APS, you will receive a message inviting you to vote electronically. If not, you should have received a paper ballot by mail. If you want a paper ballot but have not yet received one, please either email your request to josbenlj@corning-cc.edu or contact Larry Josbeno , 539 W. Franklin St., Horseheads NY 14845 (phone 607-739-2292) The closing date of the election for online voting is MARCH 18. The closing date for receipt of paper ballots is MARCH 23.

Biographical information and statements by the candidates appear below. Duplicate copies of this material can be found at http://www.aps.org/units/fhp/elections/candidates05.cfm

VICE-CHAIR

(one to be selected)

Kameshwar C. Wali
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Biographical Information: Kamesh Wali did his undergraduate and graduate work for M.Sc (physics) and MA (mathematics) in India. He received his Ph.D from University of Wisconsin in 1959. After two years of Post-Doctoral work at the Johns Hopkins University, he was at Argonne National Laboratory in the High Energy Physics Division from 1962 to 1969. In 1969 he joined Syracuse University. At Syracuse, he received a Chancellor's Citation for Exceptional Academic Excellence and was named J. Dorman Steele Professor before his retirement in 1998. He is currently a Distinguished Research Professor.

Kamesh was a member of United States and Vietnam Research Collaboration and visited Hanoi in 1979 and 1989 to lecture and establish research contacts. He was a Senior Fulbright Scholar in 1995 at the University of Melbourne, Australia. He is a Fellow of the American Physical Society, was one of the founding members of the FHP and has served on its Executive Committee. He is the author of CHANDRA: A Biography of S. Chandrasekhar, editor of A Quest for Perspectives; Selected Works of S. Chandrasekhar and S. Chandrasekhar: the Man Behind the Legend. In this year of physics, celebrating Einstein's "Annus

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Mirabilis," he gave invited talks at the APS meeting in Tampa, Florida and Lincoln, Nebraska on "Bose and Einstein; the Discovery of Bose-Einstein Statistics."

Statement: History of Physics has been an integral part of my research and teaching career. I have done my best to incorporate it in all my undergraduate and graduate courses.. However, this is not generally the case. There is a woeful disregard on the part of students as well as teachers for the historical background of great discoveries and the men and women responsible for them. If elected, besides doing my best to promote the scholarly activities of the forum in APS meetings and special conferences, I would also like to work on how best the forum can help to make history as an important part of physics education (providing encouragement and support for a new genre of books on histories and biographies suitable at high school and undergraduate college levels).

History of physics and physicists' history can provide a strong bridge between the two cultures, culture of humanities and culture of sciences (C.P.Snow). I would like to build on the success of the play like COPENHAGEN to extend the boundaries of physics to humanities by extending the activities of the forum beyond the confines of APS to public at large.

David Cassidy
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Biographical Information: David C. Cassidy is Professor of Natural Sciences at Hofstra University, where he teaches physics for non-science majors, utilizing the historical approach, and writes and researches in the history of modern physics.

Cassidy is an APS Fellow and longtime member of FHP. He served as Secretary-Treasurer of the Forum,1994 -1998, and previously chaired the nominating committee. He has also served as a member and chair of the Section for History and Philosophy of Science of the New York Academy of Sciences, and on prize committees of the History of Science Society.

Cassidy received the BA and MS degrees in physics from Rutgers University, and the PhD in physics from Purdue University in1976 in conjunction with the Dept. of History of Science at University of Wisconsin, Madison. He wrote his dissertation on Heisenberg and quantum mechanics with the advice of Daniel M. Siegel. He then held postdoctoral positions with J. L. Heilbron in the Office for History of Science and Technology at UC Berkeley; and as a Humboldt Fellow with Armin Hermann at the University of Stuttgart, Germany. He was assistant professor in history of science at the University of Regensburg, Germany. During that time, he also served as an editor and consultant for the Bohr Papers in the Bohr Institute in Copenhagen; the Heisenberg Papers in the MPI für Physik in Munich, and the Pauli Letter Collection at CERN, Geneva. Cassidy returned to the US as associate editor of the Collected Papers of Albert Einstein, vols. 1-2, in Princeton and Boston. He has been at Hofstra University since 1990.

Cassidy is the author of Uncertainty: The Life and Science of Werner Heisenberg; Understanding Physics, with Gerald Holton and James Rutherford (an updated sequel to Project Physics for non-science undergraduates and future teachers); Einstein and Our World; and J. Robert Oppenheimer and the American Century ; in addition to numerous research and popular articles in the history of physical science, from meteorology in the 18th century to particle physics, US science policy in Allied occupied Germany, and computing history in the 20th century.

He is the recipient of the AIP Science Writing Award, the History of Science Society's Pfizer Prize, and an honorary doctorate awarded by Purdue University.

Statement:The Forum on History of Physics is a unique organization for bringing together physicists and historians of physics in the common pursuit of scholarly and educational goals, as well as in promoting public awareness and appreciation of physics. I would be delighted to contribute to this effort as Vice-Chair of the Forum.

TWO-YEAR E/C MEMBER-AT-LARGE

(two to be selected)

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Biographical Information: Peter Pesic received a bachelor's degree in physics from Harvard and a doctorate from Stanford, where he worked in the SLAC theory group and his advisor was Sidney Drell. He was a lecturer at Stanford from 1976-80 but has spent most of his career at St. John's College in Santa Fe, NM, where he has devoted much attention to shaping the study of physics from a historical and philosophical point of view within a "great books" curriculum. A concert pianist, he is also the Musician-in-Residence there.

He has edited a series of classic works in physics and mathematics for Dover Publications, providing introductions and detailed notes for reissues of Max Planck's *Lectures in Theoretical Physics*, James Clerk Maxwell's *Theory of Heat and An Elementary Treatise on Electricity*, and Carl Friedrich Gauss's *Investigations on Curved Surfaces*. His work in physics has mainly concerned the significance of indistinguishability in the foundations of quantum theory. He is a member of the History of Science Society and has published two papers in its journal¸ *Isis*.Overall, he has published over forty papers, many of them devoted to issues in the history and philosophy of science. These have led to his own four books, all published by MIT Press: *Labyrinth: A Search for the Hidden Meaning of Science* (2000), *Seeing Double: Shared Identities in Physics, Philosophy, and Literature* (2002, named as one of Choice Magazines Outstanding Academic Books for that year), *Abel's Proof: An Essay on the Sources and Meaning of Mathematical Unsolvability* (2003), and *Sky in a Bottle* (2005). His books have been translated into German, Italian, Japanese, and Norwegian. He is also a contributing editor of *Daedalus*, the journal of the American Academy of Arts and Sciences, for which he has recently written essays concerning the nature of modern science and mathematics.

Statement: My training as a physicist led me to a deep interest in the history and philosophy of physics as I tried to grasp more deeply the essence and implications of its most novel insights. I think that presenting physics in this historical and philosophical light is the royal road to sharing our excitement with the broadest possible public because the study of its history emphasizes the truly striking and surprising aspects of physics as it emerges, tests itself, and takes new forms. I would like to help the executive committee find new ways to reach out to the public in this way. I have had much experience doing this at St. John's College and also in my books, which try to be both serious and engaging, both historical and physical.

I also feel that the study of the history of physics can have a deep interest for physicists themselves doing their own current work. Many physicists are curious about how theories, experiments, or insights really came about, both regarding the human stories and the interplay of fundamental ideas. For instance, the study of the history of quantum theory can lead to many surprising insights into what its founders thought they were doing as well as raising important questions about the fundamental presuppositions of that theory, questions that remain of enduring concern. Most of all, study of history as living reality can help us think more clearly and more penetratingly in our own research as we contemplate those moments when physicists before us struggled with great puzzles. These crucial dilemmas often fascinate students and young physicists in particular, who implicitly hope that they might learn from them something that would help them to make the transition from being textbook problem-solvers to original thinkers capable of finding new and powerful insights. I am very interested in helping the executive committee find new ways of using the history of physics that will engage our colleagues as well as the larger public.

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Biographical Information: Michel Janssen is Associate Professor in the Program in History of Science and Technology and the Department of Physics at the University of Minnesota. He received his "kandidaats" (roughly the equivalent of a B.S.) in both philosophy and physics as well as his "doctoraal" (two a's, the Dutch equivalent of an M.S.) in theoretical physics from the University of Amsterdam, and his Ph.D. from the Department of History and Philosophy of Science of the University of Pittsburgh. He worked for ten years at the Einstein Papers Project, then located at Boston University, and was Assistant Professor in the Department of Philosophy at Boston University before coming to Minnesota in the fall of 2000, trying to fill the large shoes of Roger Stuewer. He is a regular visitor at the Max Planck Institute for History of Science in Berlin. His research so far has focused on the development of the theories of relativity, but he has recently started to branch out to the development of quantum mechanics. He won the 2005 George W. Taylor Career Development Award of the Institute of Technology of the University of Minnesota "for making the history of modern physics accessible and exciting to a broad range of students". For more information, see: www.tc.umn.edu/~janss011/

Statement: I would like to get actively involved in the FHP of the APS because I have found the American physics community to be very hospitable to the kind of research and teaching I do, focusing on the nuts and bolts of the science rather than on its cultural embedding (without unduly neglecting the latter). I see the community of physicists as an important audience for my work in history and philosophy of science and I would be happy to do my bit in promoting and fostering this kind of work within the APS.

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Biographical Information: Ronald E. Mickens is the Distinguished Fuller E. Callaway Professor of Physics at Clark Atlanta University. He received his Ph.D. in theoretical physics from Vanderbilt University and has held postdoctoral positions at the Center for Theoretical Physics-MIT, The Joint Institute for Laboratory Astrophysics, and Vanderbilt University. His current research interests include nonlinear oscillations, difference equations, and numerical integration of differential equations using nonstandard finite difference schemes, mathematical modeling of periodic diseases, and the history/sociology of African Americans in science. He has published more that 250 research papers, authored 240 abstracts, written six books, and edited eight volumes. Professor Mickens serves on the editorial boards of several research journals, including the Journal of Difference Equations and Applications. His professional memberships include AAAS, the American Mathematical Society, the American Physical Society (for which he is an elected Fellow), the Society for Mathematical Biology, and the History of Science Society.

Professor Mickens has organized symposia and special sessions of invited lectures at regional and national meetings of the American Association for the Advancement of Science, the American Physical Society and various other research workshops and conferences in the areas of theoretical physics, history of physics, mathematics applied to vibrational engineering, nonlinear dynamics, and the mathematical biosciences. Mickens' publications in the history of science include biographic essays that have appeared or will appear in

- African American Lives
- American National Biography
- Encyclopedia of African American Culture and History
- New Dictionary of Scientific Biography

His edited volume

- R. E. Mickens (editor), Mathematics and Science (World Scientific, Singapore, 1990); ISBN 981-02-0233-4 (342 pps) explores the many varied relations between mathematics and both the physical and social sciences.

Statement: A deep, fundamental understanding of physics requires an appreciation and knowledge of its history and sociology. The history of physics can be used to find out in particular fields why certain concepts arose, their subsequent evolution, and who were the main players in the development of the important issues related to the field. The teaching of the history of physics is an excellent vehicle to introduce students to science and how its methods and purposes differ from other areas of knowledge. This history can also be used as a mechanism to initiate neophytes into our community by making them aware of its traditions and the realization that they can both belong to and participate in its ongoing processes and institutions. My goals with regard to the Forum on the History of Physics would be to make known to the wider physics community the significant contributions of non-traditional scientists and to create new ideas as to how the history of physics can be effectively used to enhance interest in science at the precollege level.

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Biographical Information: Paul Halpern is a theoretical physicist, specializing in general relativity, complex systems and related areas. He received his B.A. in Physics and Mathematics, with honors, from Temple University in 1982. Working under Max Dresden, he received his Ph. D. from the State University of New York at Stony Brook in 1987. In 1988, he was appointed to an academic position at the University of the Sciences in Philadelphia, where he is currently a Professor of Mathematics and Physics and Fellow in Humanities. He has also held visiting appointments at Hamilton College, Haverford College and Humboldt University of Berlin, as well as an adjunct position at the University of the Arts. For briefer stays, he has been a research visitor at the University of Sussex, the Rudjer Boskovic Institute in Zagreb, and the University of Waterloo. He has published a number of theoretical articles in journals such as Physical Review D and General Relativity and Gravitation, as well as historical articles in Physics in Perspective, and delivered scholarly presentations to meetings as diverse as those of the American Society for Engineering Education, the Society for Literature and Science, the History of Science Society, the New England Complex Systems Institute and the Cornelius Lanczos International Centenary Conference. As a member of the American Physical Society for more than twenty years, he has attended and presented at a number of national and regional meetings. His ten books cover a wide range of topics, from the history of scientific prediction to the development of Kaluza-Klein theory. They have received generally positive reviews and have been translated into a dozen languages. Awards he has received include a Guggenheim Fellowship, a Fulbright Scholar award, a Homiller teaching award, and a literary award from the Athenaeum Society of Philadelphia. His activity in the history of physics is diverse, ranging from a semester-long course on Einstein's legacy, to a commissioned piece for an artistic celebration of the life and work of John Wheeler, and from historical consulting work for the Raab Collection (of manuscripts and letters) to archival research in Copenhagen, Goettingen, Leiden and elsewhere. His current historical interests center on European theoretical physics in the 1920s and 1930s.

Statement: The overwhelmingly positive reaction to the programs and events of the World Year of Physics signal a growing public interest in the lives and accomplishments of great physicists. I believe that the coming years represent an opportunity to build upon this interest and expand the activities of the FHP. I would work to promote continued attention to the achievements of physicists, as well as the circumstances that affected and influenced their work. Undoubtedly, these stories will serve to draw more young people into the profession, and grant them an appreciation for their predecessors' contributions. My own interest in the history of physics was piqued by my interactions with my graduate advisor, Max Dresden. Every class Max taught contained a wealth of history and humor. Admiring Dresden's dedication, in his later years, to his biography of Kramers, I realized that physicists have much to lend to our understanding of history. Last year, a course I developed on the life and work of Einstein was very well received. Perhaps one potential role of the FHP would be to help share information about courses and programs on the history of physics. I am extremely pleased that the Pais Prize has been so successful, and would work to help promote it. I am very enthusiastic about the growth of the history of physics as a field, and believe that the FHP has an important role to play in its development.

ONE-YEAR REPLACEMENT E/C MEMBER-AT-LARGE

(one to be selected)

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Biographical Information: I received my BA from Bryn Mawr College in Russian Language and Literature in 1950, and my MA from Harvard-Radcliffe in Russian Area Studies in 1953. I worked as translator and foreign editor at the Current Digest of the Soviet Press in New York City from 1953 to 1957. During that period I spent several months (December 1955-April 1956)in Moscow, where I worked as translator for a news digest put out by the English-speaking embassies and also for The New York Times. From 1958-1960 I was Moscow correspondent of the North American Newspaper Alliance, and in1962 I returned to the USSR to write three cover stories for The Reporter magazine on the Stalin question and Soviet intellectuals.

My first book, published by MIT in 1965, was "Khrushchev and the Arts," an account of Khrushchev's effort to hold on to power by reining in the Soviet writers and artists who were demanding greater de-Stalinization. My second book, "Marina and Lee," published by Harper & Row in 1977, was an attempt to explain the death of President Kennedy by laying out the thinking and actions of his assassin. My third book, "The Ruin of J. Robert Oppenheimer and the Birth of the Modern Arms Race," was published by Viking in 2005. This book is an account of President Truman's decision to order speeded-up production of the hydrogen bomb, of opposition to the President's decision by much of the community of physicists in the U.S., and of the Hearings that resulted in withdrawal of Oppenheimer's Q clearance. Working on the book, I spent many hours with the physicists at Los Alamos who developed the bomb and learned some physics myself in order to weigh the claims of rival scientists to the invention of radiation implosion. I came away with great respect for scientific ways of thinking, and for what such thinking can do for our public life. I hope to devote the rest of my career to explaining science, and the values of science, to the larger public.

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Biographical Information: Guy T. Emery is an experimental nuclear physicist with interests in nuclear spectroscopy, nuclear structure, nuclear reactions, and the connections between nuclear properties, atomic properties, and the properties of pions and other particles. He received his PhD from Harvard in 1959. He worked at Brookhaven National Laboratory, and joined the faculty at Indiana University (Bloomington) in 1966. There he became associated with the Indiana University Cyclotron Facility and served as liaison with outside users and for several years as associate director for research. In 1988 he moved to Bowdoin College. He was at various times a visiting faculty member at Stony Brook and visiting researcher at Groningen and Osaka. He has taught a wide variety of courses, from physical science for elementary education majors to graduate quantum mechanics, and has experimented with several courses related to the cultural and historical aspects of physics. He is a fellow of the APS, has done committee service in it and in its Nuclear Physics Division, and has also been active in the AAPT, including its committee on the history of physics. His interests in the history of physics center on the period from the late 1800s to World War II, on spectroscopy as a theme, on the infrastructure of the physics profession, and on the role of physics journals, including The Physical Review.

Statement: Physics history is interesting in itself, and its relations (in both directions) with technological, intellectual and cultural history are of great interest. There are also important connections with general political history, military history, and international relations. Perhaps the most useful role of the Forum on History of Physics is as a unifying force in what often seems a rather fragmented American Physical Society, and it has also done very great service as a collegial meeting ground for professional historians, physicists with an interest in the history of the subject, and the physics profession in general. It might be useful for it to seek ways to get more history of physics into the undergraduate curriculum, both for physics majors and for others.

BYLAWS: Forum on the History of Physics

Below are the proposed bylaws of the Forum, which have been approved by the APS Council. Assuming these cannot be ratified at the April business meeting because of a lack of a quorum (which seems likely) they will be published once again in the next (Fall 2006) Newsletter and a ballot for approval will be distributed to all members. If you wish, you can make comments or suggestions to any or all members of the Executive Committee, whose addresses are listed elsewhere in this Newsletter. In the following text, "Society" shall signify the American Physical Society, "Council" and "Executive Board" shall signify the Council and the Executive Board of the Society, respectively; "Executive Officer" shall signify that Officer of the Society; and "Regular Meeting" shall signify the principal meeting held once a year by the Forum.

ARTICLE I: NAME

This subunit of the American Physical Society, existing in accordance with Article VIII of the Constitution of the Society as revised in 1990, shall be called the Forum on the History of Physics.

ARTICLE II: OBJECTIVE

The objective of the Forum shall be the encouragement of scholarly research in the history of physics and the diffusion of knowledge of this history and its relation to other scholarly disciplines.

ARTICLE III: ENABLING CONSTITUTIONAL PROVISION

Article VIII of the Constitution of the Society, as said Article may be subsequently revised or amended, is hereby incorporated in these Bylaws by reference.

ARTICLE IV: MEMBERSHIP

The members of the Forum shall consist of members of the Society who have been members of the erstwhile Division of History of Physics or who have indicated in accordance with procedures established by Council their desire to join the Forum and who retain membership from year to year by the payment of designated dues or by other method established by Council.

ARTICLE V: EXECUTIVE COMMITTEE

1. Governance. The Forum shall be governed by an Executive Committee, which shall have general charge of the affairs of the Forum.

2. Composition. The Executive Committee shall consist of the Officers of the Forum, the most recent Past Chair, the Forum Councillor, six Members-at-Large elected to staggered three-year terms, and the immediate past Secretary-Treasurer for one year. The newsletter Editor and a representative of the AIP Center for History of Physics shall be ex officio non-voting members of the Executive Committee.

3. Executive Committee Meetings. The Executive Committee shall meet at least once each year. The principal meeting shall be held during the Regular Meeting of the Forum. Any member of the Executive Committee unable to attend a meeting may name a nonvoting alternate to represent him or her, subject to the approval of the Chair. The Chair of the Forum shall preside over the Executive Committee meetings. A majority of the voting members, including at least two Officers, shall constitute a quorum.

ARTICLE VI: OFFICERS AND FORUM COUNCILLOR

1. Officers. The Officers of the Forum shall be a Chair, a Chair-Elect, a Vice-Chair, and a Secretary-Treasurer.

2. Duties of the Chair. The Chair shall preside at all meetings of the Executive Committee and Business Sessions of the Forum at which his or her attendance is possible.

3. Duties of the Chair-Elect. The Chair-Elect shall act in place of the Chair if the latter is unable to perform his or her duties. The Chair-Elect shall perform such other functions as may be explicitly provided in the Bylaws.

4. Duties of the Vice-Chair. The Vice-Chair shall act in place of the Chair-Elect if the latter is unable to perform his or her du-

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FORUM AFFAIRS

CALL FOR NOMINATIONS

Elsewhere in this issue you will find complete information on the forum election that is now underway. In addition, during the coming summer the forum nominating committee will be assembling a slate of nominees for the election to be held in early 2007 of a Vice-Chair (subsequently to become Chair-Elect, Chair, and finally Past-Chair) and two Members-at-Large of the Executive Committee (for two year terms). We welcome suggestions from any FHP members of possible candidates (including self-nominations). Please include, if possible, brief CVs of suggested candidates. (In addition to candidates chosen by the nominating committee, FHP bylaws provide that if as many as 5% of the forum membership suggest the same person for the same office, that person shall be deemed to have been nominated and will appear on the ballot.) Please send suggestions and information to Robert H. Romer (Physics Department, Amherst College, Amherst, Mass. 01002 or rhromer@amherst.edu).

VOLUNTEERS NEEDED

Your Forum always needs people who are willing to help. A bunch of committees will be appointed at or just after the April meeting, including(a) editorial board and publications (duties uncertain), (b) fellowships(evaluates nominations, of which there are very few), (c) nominating (realwork), (d) program for 2007 (real work), and (e) membership (meaning recruitment). If any of these appeal to you, please contact the incoming chair, who officially appoint them, vtrimble@astro.umd.edu. A couple of other committees are perpetuated in other ways, the Pais Award Selection and Historic Sites committees. Please contact their respective chairs, Michael Nauenberg and John Rigden.

GOOD SPEAKERS OFFERED AND SOUGHT

The World Year of Physics is over, but your WYP Speakers' Bureau will live on as the Las Cumbres Speakers' Bureau. Thanks to a grant from Wayne Rosing, founder, director, and expander of Las Cumbres Observatory, there will be money to keep the web site going and to provide some travel support fors peakers going to truly impoverished institutions. Intellectual support is provided by the Topical Group on Gravitation and General Relativity, FHP, and the Division of Astrophysics of APS.

The primary goal is to encourage students graduating from 4-year colleges to stay in the sciences by sharing with them the excitement of physics. We have also been able to fill a number of requests from 2-year colleges, K-12 teachers, community groups, and even a few PhD-granting institutions. About 150 visits have been arranged so far, most of them by our very competent webmaster, Danuta Mogilska of U. Texas, Brownsville. Most of the requests have been for talks about some topic concerning general relativity, relativistic astrophysics (black holes, quasars, cosmology, etc), history of physics, or Einstein.

If you have not already volunteered to be part of the speaker data base and are willing, please send a message to vtrimble@astro.umd.edu with your name, coordinates, topic(s) you might speak about, and the sorts of audiences/institutions that appeal to you.

If, on the other hand, you would like to arrange to have a talk at your institution, club, or whatever, go to http://www.phys.utb/WYPspeakers/REQUESTS/howto.html.
—Richard Price for TGG and Virginia Trimble for FHP

AIP CONGRESSIONAL SCIENCE FELLOWSHIP

AIP will choose one scientist to spend a year working for a congressional office. Qualifications include U.S. citizenship, membership in at least one AIP Member Society at time of application, and a PhD or equivalent in physics or a related field. Please see http://www.aip.org/gov/cf.html for application details; application materials must be postmarked by January 15, 2006 and sent to: APS/AIP Congressional Science Fellowships, c/o Jackie Beamon-Kiene, APS Executive Office, One Physics Ellipse, College Park, MD 20740-3843.

ABRAHAM PAIS AWARD FOR HISTORY OF PHYSICS

Guidelines for Nominations: Anyone (not a member of the Selection Committee) may submit one nomination or seconding letter to the Chair of the Selection Committee in any given year. Nominators and seconders do not have to be members of the APS or of any other scholarly or professional society. Selection Committee members and current APS Forum on the History of Physics (FHP) Executive Committee members are not eligible for nomination for the Award. A nomination should include:

• A letter of not more than 1,000 words evaluating the nominee's qualifications in light of the Selection Criteria and Eligibility for the Award and identifying the scholarly and further professional achievements to be recognized. The Selection Criteria and Eligibility are given in a posted description of the Award and also at the APS Prizes and Awards Web site. There is no nomination form; the nominating letter substitutes for a form.

• A list of the nominee's most important publications. Reprints of up to five of the nominee's articles may be included.

• At least two seconding letters, but no more than four.

• A biographical sketch would be helpful but is not required.

Five copies of the complete nomination package should be mailed to the Chair of the Selection Committee, whose name is posted on the APS Prizes and Awards Web site and is published in APS News.

The name and address of the Chair of the Selection Committee can also be obtained from: APS Honors Program, One Physics Ellipse, College Park, MD 20740-3844, Tel: (301) 209-3268, Fax: (301) 209-0865, E-mail: honors@aps.org. For general information about APS prizes and awards policies see www.aps.org/praw/history/index.cfm.

LATE BREAKING NEWS

The Canadian Association of Physicists (CAP) has just started a new Division on the History of Physics. We are happy to welcome this new example of the present vitality of the field, and look forward to future interactions with it. The mandate for the Division is given on the CAP site http://www.cap.ca/prof/divisions/dhp.html. The interim Chair is Allan Griffin, Professor Emeritus, University of Toronto, FRSC, Toronto, Ontario, M5S1A7, Canada, tel. 416 978 5199/7135, griffin@physics.utoronto.ca. Further details are given on http://cap06.brocku.ca/english/.

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ties. The Vice-Chair shall perform such other functions as may be explicitly provided in the Bylaws.

5. Duties of the Secretary-Treasurer. The Secretary-Treasurer shall maintain the records of the Forum including minutes of Executive Committee meetings and Business Sessions, Forum activities, and membership lists. The Secretary-Treasurer shall notify the Executive Committee of matters requiring the decision of said Committee and shall prepare the agenda of Executive Committee meetings and Business Sessions. The Secretary-Treasurer shall prepare minutes of Executive Committee meetings and Business Sessions and shall submit these minutes to each member of the Executive Committee and to the Executive Officer within four weeks after each meeting. Following elections, such minutes are to include the results of the election and a roster of the current Executive Committee membership.

The Secretary-Treasurer shall keep the Council and Executive Officer of the Society informed of the activities and needs of the Forum. The Secretary-Treasurer shall have responsibility for all funds in the custody of or placed at the disposal of the Forum and shall authorize disbursements from such funds for expenses in a manner that is consistent with the general policies of the Society and the Forum. Financial records shall be kept on an annual basis consistent with the fiscal policies of the Society. The Secretary-Treasurer shall present a financial report at the principal meeting of the Executive Committee and at the annual Business Session of the Forum.

6. Duties of the Forum Councillor. The Forum Councillor shall serve as liaison between the Council of the Society and the Executive Committee of the Forum.

Following each Council meeting, the Forum Councillor shall report to the Chair and the Secretary-Treasurer regarding Council actions that affect the status and operations of the Forum. Reports shall be made to the entire Executive Committee during their regularly scheduled meetings.

ARTICLE VII: ELECTION AND TENURE OF THE OFFICERS, EXECUTIVE COMMITTEE MEMBERS, AND FORUM COUNCILLOR

1. Qualifications. Officers, Forum Councillor, and Members-at-Large of the Executive Committee must be current members of the Forum for at least two years prior to at the time of nomination.

2. Ballot. The Vice-Chair, Secretary-Treasurer, Forum Councillor, and Members-at- Large of the Executive Committee shall be elected by mail and/or electronic ballot as hereinafter provided.

3. Nomination and Election of the Vice-Chair, Secretary-Treasurer, and Executive Committee Members. The Secretary-Treasurer shall invite Forum members to suggest candidates for the various offices and Executive Committee positions and convey these suggestions to the Nominating Committee. If as many as five percent of the total Forum membership determined on 30 June of the year preceding the election suggest the same person for the same office, that person shall be deemed to have been nominated. Each year the Nominating Committee, taking account of suggestions received from the membership shall nominate at least two candidates for the office of Vice-Chair, for Secretary-Treasurer during the final year of the term of the current Secretary-Treasurer, and for open positions of Members-at-Large of the Executive Committee. The Nominating Committee shall nominate at least two candidates for the office of Secretary-Treasurer during the final year of that officer's term, unless the current Secretary-Treasurer is re-nominated, in which case a second candidate may or may not be nominated. The Nominating Committee shall notify the Secretary-Treasurer of the results not later than sixteen weeks in sufficient time for the election to occur before the Regular Meeting. The Secretary-Treasurer shall inform the Forum members of the nominations made and shall invite these members to suggest candidates for the various offices and Executive Committee positions. If as many as five percent of the total Forum membership determined on 30 June of the year preceding the election suggest the same person for the same office, that person shall be deemed to have been nominated.

The Secretary-Treasurer shall poll the Forum membership by mail and/or electronic ballot, stating a closing date at least three weeks prior to the Regular Meeting. Ballots shall be returned to and counted by the Secretary-Treasurer or his or her designate.

Election shall be decided by a plurality of those voting. If there is a tie, the Executive Committee shall decide the election, with the Chair voting only in the case of a tie among the other Executive Committee members. The Secretary-Treasurer shall communicate the results of the election to the Chair and to the Executive Officer at least two weeks prior to the Regular Meeting and shall publish the results in a manner designated for official announcements.

4. Nomination and Election of the Forum Councillor. The Executive Committee shall nominate at least two candidates for the position of Forum Councillor. The Secretary-Treasurer shall invite inform the Forum members of the nominations made and shall invite these members to suggest additional candidates for the position of Forum Councillor during the last year of the current Councillor's term. If as many as five percent of the total Forum membership determined on 30 June of the year preceding the election suggests the same person, that person shall be deemed to have been nominated. The Executive Committee, taking account of suggestions from the membership, shall nominate at least two candidates for the position of Forum Councillor, unless the current Forum Councillor is re-nominated, in which case a second candidate may or may not be nominated. The Secretary-Treasurer shall poll the Forum by mail and/or electronic ballot, stating a closing date at least three weeks before 1 September. Ballots shall be returned to and counted by the Secretary-Treasurer or his or her designate. Election shall be by plurality of those voting. If there is a tie, the Executive Committee shall decide the election, with the Chair voting only in the case of a tie among the other Executive Committee members. The Secretary-Treasurer shall communicate the results of the election to the Chair and to the Executive Officer before 1 September of the year prior to that in which the new Councillor assumes office and shall publish the results in a manner designated for official announcements.

5. Official Year. The official year shall extend from the close of one Regular Meeting to the close of the next Regular Meeting.

6. Vice-Chair, Chair-Elect, and Chair. The member elected as Vice-Chair shall serve in that office for one year, then for one year as Chair-Elect, and then for one year as Chair. The Chair shall not be eligible for the office of Vice-Chair in the year fol-

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lowing his or her term of office.

7. Terms of Office. The terms of office of the Officers and Members-at-Large of the Executive Committee shall begin at the close of the Regular Meeting of the Forum following their election. The Secretary-Treasurer shall serve for a term of three years and may not serve more than two consecutive terms. The tenure of a Member-at-Large of the Executive Committee shall terminate in the event of his or her assumption of a post as an elected Officer of the Forum, and the unexpired portion of his or her term shall be filled as hereinafter provided for a vacancy.

The term of office of the Forum Councillor shall begin at the beginning of the calendar year following his or her election. Forum Councillors shall serve for a term of four years and may not serve more than two consecutive terms unless otherwise specified by Council.

8. Vacancies in the Offices. If a vacancy occurs in the office of Chair, the Chair-Elect shall succeed and complete the term and shall serve as Chair also in the following year.

The Vice-Chair shall serve simultaneously as Chair-Elect during the remainder of the term and shall continue to serve as Chair-Elect in the following term.

If a vacancy occurs in the office of Chair-Elect otherwise than through advancement to Chair, the Vice-Chair shall become Chair-Elect. In this case, and also if the office of Vice-Chair becomes vacant for other reasons, the office of Vice-Chair shall remain vacant for the remainder of the term. In the next scheduled election, candidates for both Chair-Elect and Vice-Chair shall be nominated.

If vacancies occur in the offices of both the Chair and the Chair-Elect, the Vice-Chair shall become Chair and shall complete the term. In this case a special election shall be held to fill the offices of Chair-Elect and Vice-Chair. The members so elected shall continue to serve as officers in the normal succession order.

Vacancies in any other elected office shall be filled (or left unfilled) by the Executive Committee until such time as the vacancy can be filled by regular election procedures.

ARTICLE VIII: APPOINTED COMMITTEES

1. Nominating Committee. The Nominating Committee shall consist of four members appointed by the Chair to staggered two-year terms and one member appointed by the Council for a one year terms. The Chair shall ascertain through the Executive Officer the identity of this member. The Nominating Committee shall prepare a slate of candidates for the positions of Vice-Chair, Secretary-Treasurer, and Members-at-Large of the Executive Committee according to Article VII.3 of these Bylaws. The Nominating Committee shall advise the Chair on suitable candidates for Society committees, including relevant Society Prize and Award committees, and on candidates for Society offices. The Nominating Committee shall perform such other duties as described in the Bylaws.

2. Program Committee. The Program Committee shall consist of the Chair, the Chair-Elect, the Vice-Chair, the Secretary-Treasurer and three other members appointed by the Chair, upon recommendation of the Chair-Elect. The Chair-Elect shall serve as Chair of the Program Committee. The Program Committee shall have the responsibility of assisting the Executive Officer, or his or her designate, in arranging the meetings of the Society. For meetings of the Forum, including the Regular Meeting, the Program Committee shall be responsible for the solicitation and selecti n of invited and review papers and for the arrangement of the programs of such meetings.

3. Fellowship Committee. The Fellowship Committee shall consist of the Vice-Chair and four other members appointed by the Executive Committee, upon the recommendation of the Chair to a one-year term. The Vice-Chair shall serve as Chair of the Fellowship Committee. The Fellowship Committee shall promote the nomination of candidates for Fellowship, shall review the qualifications of such candidates, and shall report its recommendations to the Executive Committee for approval before submission is made to the Executive Officer of the Society.

4. Publications Committee. The Publications Committee shall consist of three members appointed by the Chair to staggered three-year terms. The Chair shall appoint the Chair of the Publications Committee from among the members. The Publications Committee shall solicit articles for Physics News, and shall serve as the Forum interface with editors and publications for the popular press.

5. Editorial Board. The Editorial Board shall consist of three members appointed by the Chair to staggered three-year terms. The Editorial Board shall assist the Editor in policy decisions regarding the Forum newsletter, History of Physics Newsletter, and any other publication sponsored by the Forum.

6. Award Committee. The Award Committee shall consist of at least three members appointed by the Chair. The Award Committee shall consider policies and procedures for any Forum award and make recommendations regarding this award to the Executive Committee. The Award Committee shall be responsible for fund raising for any Forum award that is approved by APS Council.

7. Terms of Office of Appointed Committee Members. The terms of committee members appointed or recommended by an incoming Chair shall commence at the beginning of the year in which he or she assumes office.

8. Ad Hoc Committees. The Chair shall appoint other ad hoc committees as necessary, which shall serve only during his or her term as Chair.

ARTICLE IX: MEETINGS

1. Regular Meeting. One meeting of the Forum, to be known as the Regular Meeting, shall be held annually at such time and place as shall be ordered by the Executive Committee, subject to approval by the Executive Board. Whenever it shall be feasible and not to the disadvantage of the members of the Forum, the Executive Committee may order this or any other meeting to be held conjointly with a Meeting of the Society or of another society, conference, or group, so long as such joint meeting does not conflict importantly with the schedule of Meetings of the Society as determined by the Executive BoardOfficer. The registration fee for the Regular Meeting, when not held jointly with a Meeting of the Society, shall be fixed after consultation with the Executive Officer. Non-members of the Society shall pay a surcharge to be set each year by the Executive Board as specified in the Bylaws of the Society.

2. Annual Business Session. Each year the Forum shall hold a Business Session which shall be a session of the Regular Meeting. This Business Session shall be devoted exclusively to the reports of officers and committees, election results, and the transaction of business affairs. A majority vote of those Forum members present shall be sufficient for approval of actions, provided that those present number at least five percent of the membership of the Forum. No scientific program of the Division Forum shall be presented simultaneously with the Business Session.

3. Other Meetings. Meetings of the Forum, other than the Regular Meeting, may be initiated by the Executive Committee or by petition of twenty percent of the members of the Forum, subject to approval by the Executive BoardOfficer. Special conferences may be sponsored in whole or in part by the Forum, subject to the rules and regulations specified in the Society Constitution and Bylaws.

4. Papers at Meetings. Programs of meetings of the Forum may provide for the inclusion of both invited and contributed papers. When a meeting of the Forum is held in conjunction with a meeting of the Society, the rules of the Society shall apply to submitted papers. When a meeting of the Forum is not held in conjunction with a meeting of the Society, the Executive Committee shall prescribe the subject and character of the meeting, which may include limitations on the subject matter of submitted papers. The Secretary-Treasurer shall fix the deadline date for receipt of titles and abstracts in consultation with the Executive Officer and shall designate the place to which they should be sent. The amount of time to be allowed for the presentation of a paper at the Regular Meeting shall be determined by the Program Committee, except as otherwise directed by the Executive Committee. These allotments of time shall be consistent with the Constitution and Bylaws of the Society and with regulations of Council.

ARTICLE X: DUES

Dues for maintenance of membership in the Forum shall be established by Council.

ARTICLE XI: NEWSLETTER

The newsletter of the Forum, *History of Physics Newsletter*, shall be managed and edited by an Editor, who shall be elected by the Executive Committee for a term of three years. The Editor shall oversee the preparation and distribution of the History of Physics Newsletter. There shall be at least one issue per year; the frequency and timing of these issues shall be determined by the Editor in consultation with the Secretary-Treasurer, subject to approval of the Executive Committee or its delegate. The Executive Committee may direct the Secretary-Treasurer to distribute complimentary copies of the newsletter to specified non-members of the Forum. The Editor shall be assisted in policy decisions by an Editorial Board.

ARTICLE XII: OFFICIAL ANNOUNCEMENTS

Official announcements shall be made in the History of Physics Newsletter, and in such other publications as the Executive Committee may direct.

ARTICLE XIII: PROCEDURE OF AMENDMENT OF BYLAWS

Proposal of an Amendment to these Bylaws may be made by the Council, by the Executive Committee, or by a petition to the Chair signed by not fewer than ten percent of the members of the Forum. If the proposed amendment originates within the Forum, it must be approved by Council before further action can be taken.

Following Council approval, the Secretary-Treasurer shall distribute copies of the proposed Amendment to all members of the Forum, usually in the winter issue of the newsletter or electronically, but not less than three weeks before the Regular Meeting.

Opportunity shall be given for discussion during the Business Session. With the unanimous consent of those members present and voting, the voting on the proposed Amendment may be carried out at the Business Session, provided that those present number at least five percent of the membership of the Forum. Without that consent, the voting on the proposed Amendment shall be as follows. Not later than the next issue of the newsletter after said Regular Meeting. During the next regularly scheduled election, the Secretary-Treasurer shall again distribute copies of the proposed Amendment and include a vote on the amendment on the ballot, accompanied by ballot forms, by mail and/or electronically. Adoption of the Amendment shall require a two-thirds favorable vote by those voting. ■

BOOK REVIEWS

Michael Frayn's Copenhagen in Debate—Historical Essays and Documents on the 1942 Meeting Between Niels Bohr and Werner Heisenberg

Edited by Matthias Dörries, Office for History of Science and Technology, University of California, Berkeley, 2005

Reviewed by Benjamin Bederson

The now famous Frayn play has stirred an old hornet's nest—the meeting between Bohr and Heisenberg in Copenhagen, September 15-21 1941. The play itself was a huge success in London, where it originated, New York where its premier was accompanied by a powerful symposium, organized by Brian Schwarz, that explored its meaning, and elsewhere. Since that time it has generated other symposia and conferences; it has resulted in the reissuing of the Frayn play along with several postscripts by Frayn and it has likewise generated numerous critical commentary that among other things probed its historical accuracy.

If one were to try to get at the core of the conflict of opinions, probably oversimplifying it, it would be that in some fashion or other the play attempted to place the moral and ethical behavior of these two physics giants on a relatively equal footing, somehow equivalent to their standings as physicists. Frayn himself has stated that he was inspired to write the play after reading "Heisenberg's War" by Thomas Powers. In that book Powers had expressed the opinion that Heisenberg discouraged atom bomb research in Nazi Germany, at least partly out of ethical motives

In this volume the Editor, Matthias Dorroes. Professor for History of Science at the University of Strasbourg, invited a number of science historians to offer their opinions on the play. To make it even more interesting, it happened that Gerald Holton released

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a shocker at the New York symposium. He revealed that the Bohr family had in their possession a number of draft letters, never sent, written by Bohr, addressed to Heisenberg. The family had decided not to release these letters until 2012. However, as a result of the notoriety resulting from the Frayn play, with the existence of the letters now publicly revealed, they decided to release them now, and in fact posted them on the Bohr website, along with translations, for all to see. They are at http://www.nbi.dk/NBA. The present volume also reproduces images of all of them, along with typed versions in German and in English translation. The Editor also asked the writers to supply postscripts to their articles, after they had seen the Bohr letters.

Among the commentators were Finn Aaserud, Director of the Niels Bohr Archive in Copenhagen, David Cassidy, Dieter Hoffmann, Thomas Powers, Paul Rose , Mark Walker, and Gerald Holton. Each of the eleven had useful things to say, though I have to admit, some more than others. What I found particularly interesting was the fact that in virtually every case their original takes on the meetings were reinforced in their postscripts after having read the Bohr draft letters. What does this say about our abilities to have our opinions altered after they are once formulated?

At least two themes are explored in this volume. The first represents the opinions of the contributors concerning the historical validity of the Frayn play, that is, did Frayn portray accurately the meetings between Heisenberg and Bohr in Copenhagen in 1941? The play, which consists basically of dialogues between these two, with Bohr's wife Margarethe acting as a sort of intermediary, is almost entirely talk, and Frayn is the first to admit that the play's conversations were invented, not recorded. The dramatist's license thus taken is certainly legitimate, assuming that the sense of the conversations are accurate reflections of what actually occurred. The second theme is the even dicier question: did Heisenberg deliberately try to discourage (at the least) or actually sabotage (at the most) the Nazi bomb efforts? The two most extreme positions on the latter question were taken by Rose and Powers, both adhering to their long-held opinions, with Rose claiming that Heisenberg, while not a Nazi, was a strongly patriotic supporter of the German side, and would and did work enthusiastically in search of a fission bomb, only failing because of his ineptitude in the quest, with Powers sticking to his claim that Heisenberg did what he could to hamper its development and ensure that the Nazis would not succeed in building such a bomb before the end of the war. The Bohr letters did nothing to change either of their minds. Other writers took more nuanced positions, although most (including Cassidy, Holton and Walker) were decidedly skeptical about Heisenberg's unwillingness to cooperate with the Nazis. While Bohr's unsent letters to Heisenberg made no mention of bombs or fission, he was quite firm in his denial of the somewhat self-serving version of the meetings described by Heisenberg to Robert Jungk in *Brighter than a Thousand Suns*.

One opinion, frankly expressed by Finn Aaserud, Director of the Niels Bohr Archive in Copenhagen, captured my attention. He states..."increased public interest in our field created by Copenhagen may add to the readership of our publications…and make funding agencies more positively inclined toward history of science scholarship." Something like what happened to the support of physics in the US after Sputnik.

My personal inclination lies more towards that of Rose than of Powers. Once you have started on the road to compromise and accommodation, as Heisenberg certainly did in deciding to work within the Nazi regime, where does it end? Bohr understood this, to his everlasting credit, while Heisenberg did not.

In the October 2005 issue of Physics Today, David Mermin offers a remarkable quote by Einstein, made in reference to those who testified during the McCarthy hearings: "Every intellectual who is called before one of the committees ought to refuse to testify, i.e. he must be prepared for jail and economic ruin…If enough people are ready to take this grave step they will be successful. If not, then the intellectuals of this country deserve nothing better than the slavery which is intended for them." Einstein practiced what he preached—he was an outspoken pacifist living in Germany throughout the first world war.

The volume contributes significantly to the extensive literature on that famous encounter. Yet, I still recommend actually reading the play. I recently did just that to refresh my memory, having attended the New York premier. Even on paper, without that splendid production, it remains a powerful work of art that places those distant eras of revolutionary physics of the 1920s and that almost equally distant era of the horror of Nazi Germany into the realm of historic legend and consuming literature. ■

IWAN RHYS MORUS

When Physics Became King

The University of Chicago Press, Chicago and London, 2005). xii + 303 pages. $25.00 (paper).

Reviewed by Roger H. Stuewer, Professor Emeritus of the History ofScience and Technology, University of Minnesota

Iwan Rhys Morus, Lecturer in the Department of History and Welsh History at the University of Wales, Aberystwyth, has written what he calls an "unashamedly cultural history" of 19th-century physics, seeing its development as a "collective enterprise," the "product of the mass mobilization of material and social resources on an unprecedented scale" in which physicists carved out "a cultural niche for themselves and their new discipline." (pp. 4-5) His is not a comprehensive history, following a simple chronological time line from beginning to end; instead, he devotes each chapter, a synthesis of recent scholarship, to a particular aspect of 19th-century physics, with several overarching themes running through them—that, for example, physicists in their investigation of nature and discovery of universal physical laws were "crucially dependent on a range of cultural and material resources," (p.18) that institution building was vital in establishing physics as a discipline, and that physicists "had to find ways of defining themselves and what they did in relation to their audiences in such a way as to convince them that they could indeed be trusted to speak for nature." (p. 21). He begins each chapter with an introduction and closes it with a conclusion. The result is an understandable, widely accessible, beautifully written, fascinating, and insightful portrait of 19th-century physics. Morus sets the stage by discussing, first, the enormously influential scientific and educational institutions that were born at the end of the18th century in revolutionary France, among them the Institute of France and the École Polytechnique; second, the subsequent attempts to reform the teaching of mathematics and the Mathematical Tripos examination at the University of Cambridge; and third, the origin of a research ethos and extraordinary professorships in theoretical physics in German universities in the middle decades of the century, treating, in each of these national contexts, the significant physical and mathematical contributions of prominent figures such as Pierre-Simon Laplace, Charles Babbage, and Herman Helmholtz. Morus then turns to attempts to uncover the unity of nature, from the search for a transcendental unity in nature by proponents of the Romantic Naturphilosophie in Germany at the beginning of the century, to the later recognition by Michael Faraday and others that the "forces" of nature, such as electricity and magnetism, could be converted one into the other, and even, as James Prescott Joule found later, totally conserved. Indeed, to William Thomson and Peter Guthrie Tait, as well as to James Clerk Maxwell, energy and its conservation became new unifying physical principles and ones that underlay the quest to understand the mechanics of the ether. Further, the conservation of energy became the "ideal tool for creating and holding together" the new discipline of physics, one that "crossed the boundaries between factories, laboratories, and university studies and lecture halls." (p.86) Electricity, as Morus shows next, not only became of fundamental scientific interest as a consequence of the work of Alessandro Volta, Hans Christian Oersted, and Faraday, it also was turned into a "technology of display" by William Sturgeon's invention of the electromagnet in 1824 and Joseph Henry's subsequent construction of enormous ones, supporting weights of up to 1600 pounds. Moreover, through the development of telegraphy in the late 1830s and early 1840s,especially by Charles Wheatstone in Britain and Samuel F.B. Morse inAmerica, electricity became a saleable commodity, with electrical devices being featured prominently in the Great Exhibition of 1851 in London. At the end of the century, Nikola Tesla's enormous high-frequency, high-potential induction coils fascinated American audiences with their huge and noisy discharges. Sadi Carnot, also motivated by economic concerns, published his profound analysis of the motive power of heat in 1824 and thereby brought "abstruse natural philosophy to bear on a very practical question," (p.132) namely, "how to make steam engines work better," that is, of maximum efficiency. His work was clarified a decade later by another French engineer, Émile Clapeyron, through which William Thomson learned about Carnot's work in 1845. Thomson became professor of natural philosophy at the University of Glasgow in 1846, learned about Joule's experiments in 1847, and conceived the second law of thermodynamics in1851. In that he was anticipated in 1850 by Rudolf Clausius at the

University of Halle, Germany, who also had learned about Carnot's work through Clapeyron's and about conservation of "force" through the earlier and independent publications on it of his countrymen, Julius Robert Mayer and Herman Helmholtz, both of whom were motivated to investigate the science of work by physiological considerations. Clausius extended his analysis and introduced the term "entropy" in1865, for which Ludwig Boltzmann at the University of Graz, Austria, who along with Maxwell founded the kinetic theory of gases, gave a statistical interpretation in 1877. Morus pointedly concludes that, "Making common ground among physicists from different cultures and backgrounds as to what the science of work really was itself required work." (p. 155)

In his next chapter, Morus returns to developments in electricity, now in the second half of the 19th century, and discusses the new phenomenon of its discharge between two oppositely charged poles in evacuated glass tubes, as studied experimentally in the 1850s by William R. Grove and his friend John Peter Gassiot in England, and by Julius Plücker and his student Wilhelm Hittorf in Germany. The London chemist William Crookes, however, became the most prolific researcher in this area in the 1870s, inventing the radiometer and arguing that these cathode rays constituted a "fourth state of matter," one rarer than gases, and one tied to the physics of the ether, which to him and a substantial number of other prominent British scientists, all believers in spiritualism, soon "offered a way of communicating with the dead." (p. 178) Earlier, however, in 1873, Maxwell had published his great Treatise on Electricity and Magnetism, on the basis of which Oliver Heaviside and Oliver Lodge concluded theoretically, before Heinrich Hertz in Karlsruhe, Germany, conclusively confirmed Maxwell's prediction of electromagnetic waves in the ether in 1888, that the primary limiting factor in the transmission of electricity in a cable was its self-induction, not its resistance, as William Henry Preece, the practically-minded head of the British telegraph network, vigorously mantained. This rancorous dispute between the "theoreticals" and "practicals," Morus observes, "underlines the point that, as in many other cases, what was at stake here was authority. Physics and physicists had to find a cultural role for themselves if the new discipline was to be ultimately successful." (p. 174) The "cultural authority" of physics--"its claims to provide a better way of looking at and understanding the world--did not burst full-grown from Jupiter's head. It had to be argued for." Wilhelm Conrad Röntgen's discovery of Xrays and Henri Becquerel's discovery of radioactivity at the end of the century "were an important part of this process.... They provided hard evidence ... that physics really could deliver the goods." (p. 191) Turning to astronomy, Morus notes that George Bidell Airy, after being appointed as Astronomer Royal in 1835, "imposed a 'factory mentality' on the Royal Observatory" in Greenwich. He organized work according to a "strict hierarchy," with Airy at the top, his managers below him, and the "obedient drudges," the human computers and observers, at the bottom--a system that Morus labels "industrial astronomy." One consequence was that Airy committed the Royal Observatory to undertake only systematic daily observations, and he therefore refused to search the sky to try to find the planet Neptune that John Couch Adams had predicted in 1845. On the positive side, Airy soon thereafter developed a grand plan for an international network of observatories with Greenwich as its nodal point. Concurrently, Lord Rosse was constructing his giant reflecting telescope, the Leviathan, at Parsonstown, an enormous undertaking that also was very much a "product of industrial culture," (p. 209) using it to search the heavens for support for William Herschel's nebular hypothesis and life on other worlds, a controversial supposition then as now. The middle decades of the century also saw both photography and spectroscopy introduced into astronomical practice and developed into crucial laboratory research tools. The rise of laboratory science and precision measurements, in fact, became hallmarks of experimental physics in the 19th century,

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beginning with the establishment of teaching laboratories in German universities in the second quarter the century and culminating in the construction of the Cavendish Laboratory in Cambridge beginning in 1871 with Maxwell as Cavendish Professor of Experimental Physics, and that of the Physikalisch-Technische Reichsanstalt in Berlin beginning in 1887 with Helmholtz as President. Both became seats of precision measurements, for example of the standard unit of electrical resistance, the ohm, which was essential for the commercial exploitation of electricity in telegraphy and other areas. "Precision mattered," Morus notes, because it was "a way of inculcating new disciplinary regimes as much as anything else. It was a crucial element in the fashioning of physicists as much as of physics." (p. 260)In his final chapter, Morus surveys fin-de-siècle physics and its institutions. William Thomson, now Lord Kelvin, in Britain and the now-ennobled Hermann von Helmholtz in Germany "had come to stand for a particular kind of imperial physics," a "cocksure new science, its spokesmen confident and self-assured, convinced that they held the keys to unlocking nature's secrets." (p. 262) Its buzzword was progress, and physics "lay at the very heart" of culture. Still, novelists were less sanguine. Mary Shelley's Frankenstein, published early in the century, expressed the fears of what natural philosophy might become; Bram Stoker's Dracula, published late in the century, raised the specter of the limitations of physics; and H.G. Wells's Time Machine of 1895 envisioned a bleak future for humanity, and his War of the Worlds of 1898 presented the nightmare of annihilation by alien species who were scientifically and technologically superior to our own. But the Great War of 1914-1918 proved that we did not have to look to other worlds for means of destruction: The National Physical Laboratory in Teddington under Richard T. Glazebrook became a center for war work, just as the Physikalish-Technische Reichsanstalt in Berlin under Emil Warburg did. In sum, while I caught a few slips, Morus has written a book that is filled with insights into the cultural history of 19th-century physics, and one that historians, physicists, and their students can read with great profit. It also would serve well as a textbook or for supplementary reading in history courses, or even as recommended reading in physics courses. Morus's final words are worth pondering by both historians and physicists: Science is not a given. It is a cultural achievement of immense and unprecedented significance.... Since physics is a product of culture, as the nineteenth century recognized, it is also part of a common culture. The shape of modern scientific institutions, the status of scientific experts, their relationship to government and to industry are not engraved in stone. It is up to citizens of the twenty-first century to decide whether and how they value physics and physicists — what role they will play in this century's culture. (pp. 284-285) ■

DAVID KAISER

Drawing Theories Apart: The Dispersion of Feynman Diagrams in Postwar Physics

University of Chicago Press, 2005, 316 p.

Reviewed by Laurie M. Brown for History of Physics Newsletter

The title of this book (according to the author) is an "inverted analogy" to that of the article "Drawing Things Together" by the French science studies guru Bruno Latour. David Kaiser's book draws on "Latourian themes: the building of networks, the work of translation and enrollment, and so on." (p. 7) However the science studies discussions are confined mostly to the first and last chapters (as in Andy Pickering's "Constructing Quarks") and to some footnotes. That said, I will couch the rest of this review in language more accessible and perhaps more congenial to most readers of this newsletter.

Kaiser considers the Feynman diagrams to be a theorist's tools, analogous to the instruments that experimentalists use to investigate nature. He describes their dissemination (or, as he prefers "dispersal") from their origin with Feynman and Dyson at Cornell to physicists at other centers of theoretical physics, acquiring local characteristics and new applications, other than their original use as mnemonics and aids to "visualizing" physical processes in quantum electrodynamics (QED).

The second chapter of the book includes a brief review of the QED problems raised by the spectacular Columbia University experimental results (Lamb Shift, electron anomalous moment, hyperfine structure) presented at the Shelter Island Conference of 1946. Part of the intended audience for this book (including historians, sociologists, and philosophers) will unfortunately have to look elsewhere to find an adequate account of what the original meaning and purpose of the diagrams. At the Pocono Conference of 1947, where Feynman gave his first public demonstration of the diagrams, some of the older masters present refused to accept them, in part because they seemed to be "visualizations."

At the Pocono Conference, Schwinger spent most of one day expounding his theory, while Feynman had little time for his presentation. They were both a bit slow to publish their theories, and Freeman Dyson's papers "The radiation theories of Tomonaga, Schwinger, and Feynman" (Feb.1, 1949) and "The S matrix in quantum electrodynamics" became the first publications that described the diagrammatic method. For a while the procedure was attributed to Dyson, and for a long time theorists referred to Feynman-Dyson diagrams. Robert Oppenheimer, at the Institute for Advanced Study, had shown Dyson papers that Sin-itiro Tomonaga had sent to him. These were written in Japan toward the end of the war and afterwards (under terrible circumstances), and Dyson was the first to point out the valuable contributions of Tomonaga and his school.

In the third chapter (Dyson and the Postdoc Cascade), Kaiser describes how Dyson became the "diagrammatic ambassador". His lectures, preprints, and personal contacts helped to spread to Great Britain and to places like Princeton, Columbia, and Harvard (where due to Schwinger's presence they were used sub rosa). Internationally the news spread mainly by papers and lecture notes, especially to Japan, which produced 97 diagrammatic articles from 1949 to 1954, compared to 139 in America and 23 in Great Britain. (Kaiser might have noted that the Bethe-Salpeter two–body relativistic equation (Dec. 1951), that he devotes a section to, was written down in a paper in Progress of Theoretical Physics by Yoichiro Nambu in August, 1950). The Soviet theorists, who later made important contributions to renormalized QED, were in this earlier period inhibited in publication by the cold war and their involvement in the Soviet H-bomb.

Kaiser traces "family resemblances"

in particular schools, in part by their particular ways of drawing the diagrams. He discovers (or rather, I think, invents) what he calls the Feynman Dyson split. He claims that Dyson viewed the diagrams as devices for "merely" writing compactly the quantum field theoretical amplitudes, while thought of them as an alternative to quantum field theory and, in some sense a visual representation of a physical process. In fact, no one was more aware than Feynman that he was doing perturbation theory and that QED is a low-energy effective field theory. He had originally hoped to eliminate the fields, but since fitting the Lamb shift experiment requires vacuum polarization, he became committed to quantum fields.

When particle theorists turned their attention to the meson theory of strong interactions, using the successful renormalized QED as a paradigm, most of them realized that the strong (or rather, intermediate) coupling would defeat any perturbation theory. Thus they tried to abstract other "good" features of QED, using modified Feynman diagrams. They replaced three-particle vertices by "blobs", used thick lines for renormalized propagators, etc., and the modified diagrams were employed in dispersion relations, single and double, and in the "analytic S matrix". The last idea, first advanced by Heisenberg who later dropped it, was to construct the scattering amplitudes from its analytic properties, poles and cuts in the complex momentum plane, which were supposed to contain the "real physics".

David Kaiser's book will prove absorbing and rewarding, especially to those who have worked in the fields of physics that he discusses. Others interested in science history and sociology will also profit, but should be aware that his conclusions are not without controversy. That is bound top be a consequence of any original approach. ■

OTHER CONTRIBUTIONS

The Light Quantum: Celebrating Einstein's Paper of June 1905

Eugen Merzbacher, University of North Carolina at Chapel Hill

I speak as one of the rapidly dwindling number of physicists who saw Albert Einstein (1879-1955) in person. In 1950-51, as a postdoc member of the Institute for Advanced Study in Princeton, I often saw Einstein walking around ten in the morning from his house on Mercer Street to his office in the Institute and occasionally at afternoon tea. My lasting memory is the surprise engendered by his commanding tall stature, contradicting the popular image of him as hunched in an easy chair. Not wishing to waste his time, few of us junior scientists ever spoke to "the professor," but I recall an anecdote worth recounting.[1]

Giulio Racah was a visiting professor at the Institute during the same year. Famous for applying group theory to atomic spectroscopy, he was an Italian theorist who had joined the faculty of the Hebrew University. When he arrived at Princeton, he followed European etiquette and announced that he would pay his respects to Professor Einstein. He found Einstein in his office at work, presumably on unified field theory, with his assistant (Bruria Kaufman, I believe). They chatted for a few minutes and Einstein asked Racah the usual question: "What are you working on these days?" Racah explained that he was applying quantum mechanics to atoms and nuclei. Einstein apparently expressed some of his misgivings about quantum mechanics. At the end of the academic year, before returning to Israel, Racah again knocked on Einstein's office door to say a formal goodbye. Einstein called "come in," and upon seeing Racah at the door asked him: "Are you *still* working on quantum mechanics?"

From 1935 to 1947, before coming to America, I lived in Ankara, Turkey, with my family. On weekend excursions we would often talk with peasants and shepherds in the villages on the Anatolian steppes. Their knowledge of the outside world was sketchy, but invariably they had heard of three important men: Adolf Hitler, Charlie Chaplin (referred to by his French nickname, Charlot), and Albert Einstein. Of the three, Einstein, born in 1879, was the oldest. Oddly, ten years later in 1889, Chaplin and Hitler were born within four days of each other. Einstein and Chaplin knew each other in the early 1930's in California. They shared political tendencies. Hitler cast his dark shadow over both of them and was caricatured in Chaplin's film, *The Great Dictator*.

On 17 March 1905, when Einstein finished the first of his astonishing series of papers in his *annus mirabilis*, "Concerning an heuristic[2] point of view toward the emission and transformation of light,"[3] he had just turned twenty-six. He was employed as technical expert third class at the Patent Office in the capital of Switzerland, Bern, and had recently become a father. This paper, submitted even before his Ph.D. dissertation, was his first incursion into the quantum theory, which – for better or worse – held his interest to the end of his life (18 April 1955). A fine English translation by A. B. Arons and M. B. Peppard was published in the *American Journal of Physics* **33**, 367 (1965), with an acknowledgment of help from Martin Klein, the first 2005 winner of the APS/AAIP Abraham Pais Award for the History of Physics.

For perspective it helps to note what else happened in 1905, Alfred Binet invented the IQ test. The Aliens Act came into force in Great Britain. The Tsar aggravated the political turmoil in Russia on "Bloody Sunday," when over a hundred striking workers were killed by the Cossacks, starting the Revolution of 1905. Germany, wishing to challenge British dominance of the seas, began building the Dreadnought battleship. Picasso launched his *Pink Period* in Paris. Debussy composed *La Mer* and Richard Strauss Salome. Sir Arthur Conan Doyle published *The Return of Sherlock Holmes* and Edith

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Wharton *The House of Mirth*. The XX chromosome was identified as female and the XY chromosome as male determinant. The use of tree rings to establish dates was introduced as dendrochronology. In physics, R. W. Wood observed resonance radiation from mercury vapor at low pressure and Langevin worked out the theory of magnetism.

It is interesting to speculate what physics and indeed the world would be like if Albert Einstein had not survived into adulthood. Such rhetorical questions are usually answered by asserting that the science has a life of its own and that, sooner rather than later, the same discoveries would have been made and the same theoretical concepts developed. This is surely true for most advances, which are "in the air," and, like everyone else, Einstein stood "upon the shoulders of Giants" (Newton to Hooke in 1675/6, but traceable to an 11th century Italian Talmudist). By the time he was twenty-six he had acquired an impressive amount of knowledge about contemporary physics, both theoretical and experimental. Painstaking archival research by excellent historians of science has laid bare the sources of his inspirations and has even shed light on his human frailties. When all is said, however, we remain in awe of Einstein's astonishing originality. Without him and his publications, some of the sharp turns in direction that his insights ultimately gave to physics in the first half of the twentieth century would have reached our consciousness and our textbooks, if at all, only years later than they actually did.

Einstein's light quantum hypothesis, the birth of what (since 1926, when the name was introduced by G. N. Lewis for something quite different [Pais p.407][4]) we came to call the *photon*, was the first of these pivotal turning points to which his unerring intuition led him. Seventeen years later the circumspect Swedish Academy finally awarded him the Nobel Prize for 1921 "for his services to theoretical physics and especially for his discovery of the law of the photoelectric effect." (Pais, p.510) In 1913, halfway between 1905 and the Nobel Prize, Planck and some of his distinguished colleagues recommended Einstein for membership in the Prussian Academy, writing this: "In sum, one can say that there is hardly one among the great problems in which modern physics is so rich to which Einstein has not made a remarkable contribution. That he may sometimes have missed the target in his speculations, as, for example, in his hypothesis of light quanta, cannot really be held too much against him, for it is not possible to introduce really new ideas even in the most exact sciences without sometimes taking a risk." (Pais, p.382)

By 1922, however, the Nobel committee was evidently happy to bestow its award to Einstein for his discovery of the law of the photoelectric effect, which is based on the light quantum hypothesis, avoiding any explicit mention of Special and General Relativity. Presumably, the committee felt more comfortable citing the photoelectric effect, because in 1916 Robert Millikan's extensive and careful measurements had confirmed Einstein's predictions and convinced most physicists of the correctness of his theory.

In view of all this it is not surprising that many physicists think that the March 1905 paper on the light quantum is primarily about the photoelectric effect. Actually, the theory of the photoelectric effect appears only at the end of the paper as an application of 4 the light quantum hypothesis, which is put forward in the bulk of the paper as a novel means of interpreting Planck's 1899 formula for the spectral distribution of blackbody or cavity radiation.

I cannot possibly improve on Einstein's own words expressing the basic idea near the beginning of his paper:

"It seems to me that the observations about 'schwarze Strahlung' (blackbody radiation), fluorescence ('Photolumineszenz'), the production of cathode rays by ultraviolet light and other related phenomena connected with the emission or transformation of light are more readily understood if one assumed that the energy of light is discontinuously distributed in space. In accordance with the assumption considered here, the energy of a light ray spreading out from a point source is not continuously distributed over an increasing space but consists of a finite number of energy quanta ('Energiequanten') which are localized at points in space, which move without dividing, and which can only be produced and absorbed as complete units."

Having stated his bold program, Einstein retreats to familiar ground and derives what we now call the Rayleigh-Jeans Law from the energy equipartition law of classical kinetic theory. The resulting formula for the energy density of blackbody radiation,

$$\rho_\nu = \frac{R}{N}\frac{8\pi\nu^2}{c^3}T$$

fits the experimental data at low radiation frequencies ν (long wavelengths) and high temperatures T, but not at high frequencies and low temperatures. If extrapolated to frequencies beyond its range of applicability, it predicts the total radiation energy in a finite volume to be infinite. Five years earlier, Planck had proposed his famous, experimentally confirmed, expression for the spectral distribution of radiation in a cavity, which Einstein writes as

$$\rho_\nu = \frac{\alpha\nu^3}{e^{\frac{\beta\nu}{T}} - 1}$$

Here α and β are empirical constants, determined by fitting the existing data over the entire frequency and temperature range. (See the Figure depicting the Cosmic 5 Microwave Background spectrum, which follows Planck's distribution accurately for T=2.73°K.) For low temperatures and short wavelengths, the Planck formula agrees with Wien's radiation formula, proposed in 1896,

$$\rho_\nu = \alpha\nu^3 e^{-\frac{\beta\nu}{T}}$$

In modern notation, we write

$$\alpha = \frac{8\pi h}{c^3} \text{ and } \beta = \frac{h}{k}$$

where k is Boltzmann's constant, a term introduced earlier by Planck, and h is Planck's constant. In passing, Einstein rederives from the empirical values of α and β a value for Avogadro's number that is accurate to within two percent.

In the next sections of his paper Einstein uses thermodynamics to obtain an expression for the entropy of a particular frequency component of the radiation as a function of the volume, and in the limit of high frequency and low radiation density (or low temperature) he finds a logarithmic dependence on the volume, just like the case of an ideal gas or a dilute solution. This is his clue for his interpretation of radiation as composed of discrete quanta of energy. He connects the entropy to Boltzmann's probability and infers that, if the frequency is high, "monochromatic radiation of low density…behaves thermodynamically as though it consisted of a number of independent energy quanta of magnitude ***Rβν/N*** ."

Einstein's astonishingly sophisticated arguments leading to his hypothesis of the discrete nature of electromagnetic radiation were based on an assumption that we now know not to be tenable: That radiation can be considered as composed of lumps of energy, or quanta, localized in space. If he had made his calculation in momentum space instead of coordinate space, he would have reached the same conclusion by legitimate means. Ironically, the principles of (special) relativity prevent us from thinking of light quanta, as these constituents of radiation soon came to be called, as localizable in ordinary space.

At this point Einstein turns to applications of his "heuristic" viewpoint to processes involving the production and transformation of light. He points out that in 6 ordinary fluorescence conservation of energy implies that the emitted light must have a smaller frequency than the absorbed light – as observed and known as Stokes's Rule. All along, Einstein is careful to emphasize that his speculation applies only to radiation of high frequency and may not be appropriate in the low frequency regime.

Finally, in Section 8 of the paper, we reach the photoelectric effect, "the production of cathode rays [i.e., photoelectrons] by illuminating solid bodies." Suggesting that in the process of photoemission the energy Rβν N of a light quantum is transferred to an electron, he proposes that the maximum kinetic energy of the photoelectrons is

$$\Pi\varepsilon = \frac{R}{N}\beta\nu - P$$

or in modern notation,

$$E_{max} = h\nu - \varphi$$

Here Π is the retarding potential required to stop the escape of electrons from the body, ε is the magnitude of the electron charge, and P stands for the minimum energy necessary to extract an electron from the surface of the solid body, now called the *work function* φ, characteristic of the nature of the solid. The theory thus predicts that the maximum kinetic energy of the electrons E_{max} is a linear function of the frequency, with a universal slope. In the last section of the paper Einstein predicts that in the process of photoionization of a gas, the absorbed light energy L will produce L Rβν moles of ions in the gas.

The paper was submitted on 17 March 1905 and published on 9 June. It took many years for the physics community to accept light quanta (or photons) as a legitimate species of animals in the particle zoo, with mass zero. In 1905 Einstein did not think of them as particles, and late in his life, in 1950, he wrote to his friend Michele Besso: "All these fifty years of pondering have not brought me any closer to answering the question, What are light quanta?[5]" Over time, Einstein himself and many others showed that he had been too cautious in stipulating that his heuristic hypothesis referred only to light quanta of high frequency. The strange concept of a *wave-particle duality* became a new paradigm in physics and generated many debates and much confusion.[6] Finally, in 1923, Arthur Compton's experimental results on scattering of x rays from electrons provided the evidence that convinced even the last skeptics, like Niels Bohr, of the physical reality of light quanta. Einstein's contributions to our understanding of the physics of light quanta spanned a period of twenty years, until the advent of quantum mechanics in the 1924-1927 period. He introduced the A and B coefficients for emission and absorption, as well as induced or stimulated emission, of radiation by atoms. Together with Satyandra Nath Bose, he introduced the new non-classical statistics, now known as Bose-Einstein statistics and generalized to all particle species with integral spin. And he extended the theory to the calculation of the specific heat of solids at low temperatures.

To appreciate the importance of Einstein's contributions to present-day quantum physics we only need to think of modern quantum optics and the new experiments with Bose-Einstein condensates.

The discovery of quantum mechanics in the third decade of the twentieth century lifted the fog for many physicists. As illustrated by the anecdote recounted at the beginning of this paper, it made Einstein more uncomfortable, because of the intrinsically probabilistic nature of quantum mechanics and its implications of a "spooky" action-at-a distance and what he saw as the demise of objective physical reality. The much-cited 1935 (EPR) paper by Einstein, Boris Podolsky, and Nathan Rosen, entitled "Can quantum-mechanical description of physical reality be considered complete?" expressed Einstein's abiding discomfort with quantum mechanics.[7] It is ironic that Einstein's light quanta have been central in the crucial experiments (on entangled states) that establish conclusively that the nonlocality, which Einstein so abhorred, is here to stay.

This ends my brief account of Einstein's first salvo in 1905. More was to come shortly: The theory of Brownian motion, Special Relativity, and E = mc2 .I thank Martin J. Klein for showing me, by personal example as well as through his important articles about Einstein[8], how one should approach the history of physics. Although I have benefited from many books and articles, Max Jammer's *The Conceptual Development of Quantum Mechanics*[9] remains my major reference for the early history of quantum physics. I owe thanks to Jeremy Bernstein and Marc Lange for helping me to improve this paper, which originated as a colloquium talk on the occasion of the 2005 Einstein Centennial in my home department at Chapel Hill. ■

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1 A personal note: When I arrived in the U.S. in 1947, friends told me that, without my knowledge, Einstein had placed my name on a list of needy immigrants whom he recommended to one of the Jewish aid organizations for financial support in the aftermath of World War II. Einstein had a reputation for signing such petitions liberally, without much investigation. He had little patience with bureaucracy and probably helped scores of people start a new life behind the scenes. I thought it best to ignore what I was told and extend my thanks only to the agency from which I received the help that was essential for coming to graduate school in America. Undoubtedly, Einstein's secretary, Helen Dukas, was instrumental in bringing cases like mine to Einstein's attention.

2 Webster defines heuristic as "valuable for stimulating or conducting empirical research but unproved or incapable of proof."

3 "Über einen die Erzeugung und Verwandlung des Lichtes betreffenden heuristischen Gesichtspunkt," Annalen der Physik 17, 132 (1905). 10

4 References to Pais are to the excellent biography: Abraham Pais, "Subtle is the Lord…," Oxford University Press, New York, 1982.

5 "Die ganzen 50 Jahre bewusster Grübelei haben mich der Antwort der Frage 'Was sind Lichtquanten' nicht näher gebracht." (Pais, p.382)

6 The President of the American Physical Society, Marvin Cohen, was recently interviewed on a radio show and asked, "What do you answer a student when he/she asks if light is a particle or a wave?" He said, "I answer 'yes'."

7 Nathan Rosen was Einstein's assistant from 1934 to 1936. He was on the faculty of the University of North Carolina at Chapel Hill from 1941 to 1952, when he moved to Israel and I was appointed to the resulting vacancy.

8 Martin J. Klein, "Einstein's First Paper on Quanta"in The Natural Philosopher, vol. II, p.59, Blaisdell Publishing Co., 1963, and "Einstein and the Wave-Particle Duality" in The Natural Philosopher, vol III, p.3, Blaisdell Publishing Co., 1964.

9 Max Jammer, The Conceptual Development of Quantum Mechanics, McGraw-Hill Book Company, New York, 1966.

How Einstein Discovered the Photon

Michael Nauenberg, Physics Department, University of California, Santa Cruz

During May 1905, Einstein wrote a letter to his old school friend Conrad Habicht promising to send him four papers, one of which dealt with the nature of light, and which Einstein regarded as "very revolutionary." In this paper, entitled *On a heuristic point of view concerning the production and transformation of light*, he compared the entropy of low density monochromatic radiation in thermal equilibrium with the entropy of an ideal gas, and concluded that this radiation behaved *as if* it consisted of a gas of "energy quanta" which now we call photons. This contradicted the generally accepted view that radiation was a wave phenomena, as demonstrated by interference experiments, and for a long time Einstein's insight was rejected even by Planck who often is given credit for it.

Physics students know that Einstein energy quanta explains the photo electric effect (for which he received the Nobel Prize in 1921), which could not be understood by classical theory. But as late as 1915, Millikan, who carefully established the experimental validity of this phenomenon, wrote in the introduction to his paper that "Einstein's photoelectric equation ... cannot in my judgment be looked upon at present as resting upon any sort of satisfactory theoretical foundation," because as Wien had remarked in his 1911 Nobel prize acceptance speech, "it is a priori impossible to introduce a dualistic approach into optics, e.g. to assume simultaneously Huygens' wave theory and Newton's emanation theory."

When Planck and some of his colleagues proposed Einstein for membership to the Prussian Academy of Sciences in 1913, they concluded "that although he may sometimes have missed the target in his speculations, as, for example, in his hypothesis of light quanta, this cannot really be held against him, for it is not possible to introduce really new ideas even in the most exact sciences without sometimes taing a risk." Even Bohr quipped that "if Einstein sends me a telegram that he has found proof for light quanta I will hold it as contrary evidence, because his telegram arrived by an electromagnetic wave."

In his paper Einstein made a very careful analysis of Planck's original derivation of his black-body formula, and revealed some fundamental inconsistencies in its derivation. Planck had applied classical mechanics and Maxwell's electrodynamics to derive an equation for an ensemble of charged oscillators in thermal equilibrium with electromagnetic radiation inside a cavity. But then he took over Boltzmann's statistical counting method for a fictious molecular gas having discrete energy levels, without requiring that this level spacing ϵ vanish in the continuum limit corresponding to classical physics. Instead, for his oscillators he set $\epsilon=h\nu$, where ν is the radiation frequency and h is a constant, now known as Planck's constant. Einstein criticized Planck's arguments, and proceeded to apply his profound understanding of statistical mechanics (by 1902 he had developed the canonical ensemble and thermal fluctuation theory independently of Gibbs) to obtain a relation for the entropy of *monochromatic* radiation at low densities. For this purpose, Einstein applied the Wien distribution which also is the low density limit of the Planck distribution. Martin Klein commented that "Einstein based his calculation on this Wien distribution, perhaps because of its greater simplicity... ," but there is a profound reason why Einstein considered this limiting distribution. In making his analogy, Einstein must have realized that the ideal gas model would be applicable to a real gas only in the limit of low densities, where interactions can presumably be neglected, and therefore, if thermal radiation was due to energy quanta which also might have interactions, he had to consider the same limit. This important point has escaped the attention of commentators of Einstein's paper.

Einstein's derivation of a relation for the radiation entropy followed along lines similar to those developed earlier by Planck, who had applied the thermodynamics relation $dS/dE=1/T$ to his ensemble of charged oscillators with mean energy E and entropy S in thermal equilibrium at temperature T. When the temperature dependence of E is known, this relation can then be solved for S as a function of E.

For monocromatic radiation in thermal equilibrium, S is the entropy density per unit frequency and E is the energy density per unit frequency which Einstein took to be the Wien distribution. However, in this case Einstein had to justify the application of this thermodynamic relation, and for this purpose he introduced a novel variational method. It turns out that from the viewpoint of radiation as a gas of photons, Einstein's variational method is justified because photon number is not a conserved quantity. This was a piece of good luck, because in retrospect a similar derivation for massive bosons would not have been possible.

In the Wien limit, Einstein found that the total entropy of monochromatic radiation in thermal equilibrium depends logarithmically on the volume V, as is the case for the entropy of a molecular gas in the low density limit. Einstein also showed that the number density N of the radiation quanta is determined by the relation $N=E/h\nu$ where ν is the frequency of the radiation and h corresponds to Planck's constant (he represented h in a somewhat different form). Finally, he concluded that

> Monochromatic radiation at low density [i.e., within the domain of validity of the Wien radiation formula] behaves thermodynamically as if it consists of mutually independent energy quanta of magnitude $h\nu$

However, there is a gap in Einstein's argument. In a footnote in his paper, Einstein noted that the pressure p of a molecular gas is given by $p=TdS/dV$, but he did not apply this relation to obtain the radiation pressure. Since S depends logarithmically on V, one finds that $p \propto NT/V$, but as originally pointed out by Maxwell, $p=(1/3)E/V$, from which Boltzmann derived the T^4 law for the temperature dependence of thermal radiation. But neither Einstein, nor his contemporaries commented on this paradox which has been neglected also by modern commentators of Einstein seminal paper. The solution of this paradox is that the entropy depends also on the frequency ν of the monochromatic radiation, and this frequencey does not remain *constant* when the volume of the cavity is changed adiabatically, but varies as $V^{(-1/3)}$. Taking this volume dependence of the frequency into account leads to

Maxwell's correct relation for the pressure of isotropic radiation.

It is plausible that Einstein also considered the consequences of his analysis in the case that the energy density of radiation is given by the Planck black-body formula, but he did not mention this in his paper. In this case the volume dependence of the entropy is rather complicated, which would have been a puzzle. Its solution became evident nineteen years later when Bose showed that photons do not obey Boltzmann statistics. The statistics of photons introduced by Bose implies that photons are correlated, which accounts for the more complicated volume dependence of the entropy obtained from Planck's formula. Hence, already in 1905 Einstein could have discovered the quantum statistics of photons, had he followed more fully the implications of Planck's rather than Wien's distribution. ■

Feynman and Schwinger

Letter to Edward Gerjuoy, by Hans Christian von Baiyer

Readers can refer to the original Gerjuoy article on Julian Schwinger in the Fall 2005 FHP Newsletter. It can be accessed at the FHP website.

Dear Prof. Gerjuoy,

As admirer of Schwinger, and author of the words quoted in the opening paragraph of your fine essay in the History of Physics Newsletter (Vo.IX, No.5, Fall 2005), allow me to mount a gentle defense. The essays accompanying the large portraits on the A CENTURY OF PHYSICS timeline are not meant to tell the story of physics in the twentieth century. Instead, they are illustrations, vignettes, if you will, and their function is to draw in the audience. The essays are colored by my opinions, and hence subjective. The actual history is told in the dated boxes below them, which try to be more objective. The essays frequently refer to events that are included further down the page in their historical context. This is the case here, too.

A foot below the words you cite, in a prominent place near the middle of the panel, there is a box with the following text:

"1948 THE MODERN THEORY OF LIGHT AND ELECTRONS IS FORMULATED. The American physicists Richard Feynman* & Julian Schwinger*, and the Japanese physicist Sin-Itiro Tomonaga* develop quantum electrodynamics (QED), the first complete theory of the interaction of photons and electrons." [The asterisk stands for a little Nobel medal.]

I wanted to point this out in light of your remark that "explicitly mentioning Schwinger's name no longer is fashionable."

Respectfully,
Hans Christian von Baeyer
Chancellor/Professor of Physics
College of William and Mary
Williamsburg, VA

Reply by Edward Gerjuoy

Dr. von Baeyer's letter is dignified and without animus; I hope to reply in kind. This said, I cannot agree that my article's reference (in its first paragraph) to the panel which is the subject of his letter was inapposite. Running across the full length of the top of the panel are a 10"x6" almost full length photograph of Feynman, accompanied by 7"x13" of text extolling his brilliance and (as quoted in my article) stating, without any mention of Schwinger, that Feynman created QED. There is no photo of Schwinger on the panel. The only explicit mention of Schwinger on the panel occurs in a box accurately quoted by von Baeyer. This box, whose size is 1.75"x4.5", is surrounded by a collection of 23 other boxes, most of which are larger than the one mentioning Schwinger. Moreover quite a few of those other boxes are accompanied by photos of the physicists they list. Thus even accepting, as I do accept, that the motivation for the panel's design was as von Baeyer recounts, I stand by my assertion (in the second paragraph of my article) that the panel, prepared only about five years after Schwinger's death, illustrates the fact "that in recent years the remarkable researches of Julian Schwinger…appear to be increasingly overlooked." Finally, to close my response to von Baeyer's letter, I do not see how the aforementioned panel box can be regarded as evidence against my assertion (in the last paragraph of my article) that "explicitly mentioning Schwinger's name no longer is fashionable," an assertion I continue to endorse.

Before signing off I take the opportunity to acknowledge an error in my article, originally pointed out to me by Michael Fisher. Walter Gilbert did not, as I wrote, obtain his Ph.D. under Schwinger's direction; rather he was a postdoc of Schwinger's. My error stems from my reliance on the text of the obituary of Schwinger published in Nature (reference 2 of my article), whose accuracy I should have independently checked. My article's assertion that Schwinger's Ph.D. students include four Nobel Laureates fortuitously remains correct, however, by virtue of the 2005 physics Nobel Prize awarded to Roy Glauber.

E. Gerjuoy
University of Pittsburgh

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