History of Physics
NEWSLETTER
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Max Jammer Wins 2007 Pais Prize

By Michael Nauenberg, Chair, Pais Prize Selection Committee

In early October, the American Physical Society and American Institute of Physics announced that Max Jammer has been named to receive the 2007 Pais Prize in History of Physics "for his groundbreaking historical studies of fundamental concepts in physics, including his comprehensive account of the development of quantum mechanics." He joins Martin Klein and John Heilbron, who received this Forum-sponsored Prize in 2005 and 2006. He is the first winner of the Pais Prize — which was specifically intended to be awarded internationally — who is not a US citizen.

Max Jammer

Jammer was born in Berlin, Germany, in 1915. He studied physics and its history first at the University of Vienna and then at the Hebrew University of Jerusalem, where he received his Ph.D. degree in 1942. After active service in the British Army during World War II, he lectured on the history and philosophy of science at the Hebrew University. In the early 1950s, while lecturing at Harvard, he wrote the first of his penetrating studies in the history of physics, *Concepts of Space* (Harvard University Press, 1954), which has a foreword by Albert Einstein. While serving as professor at the University of Oklahoma, he was invited to teach at Bar-Ilan University in Tel-Aviv, where he later served as Rector and President. He also participated in the founding of the Department of History and Philosophy of Science at Tel-Aviv University and served as president of the Israeli Association for the Advancement of Science. He is a member of the Académie Internationale d'Histoire des Sciences and has served on the editorial boards of several scientific journals. For his publications he has received many awards, among them the Israel Prize and the Monograph Prize of the American Academy of Arts and Sciences.

Jammer's main field of research is the history and philosophy of fundamental concepts in physics. In addition to *Concepts of Space*, which appeared in a revised and updated edition and has been translated into several foreign languages, he has written *Concepts of Force* (Harvard, 1957), *Concepts of Mass in Classical Mechanics and Modern Physics* (Harvard, 1964), and *Concepts of Mass in Contemporary Physics and Philosophy* (Princeton University Press, 2001).

Jammer's other books include his pioneering and comprehensive study, *The Conceptual Development of Quantum Mechanics* (McGraw-Hill, 1966), which was republished in a revised edition in 1989 by the American Institute of Physics. He knew many of the main protagonists in his story personally, including Albert Einstein, Paul Dirac and Werner Heisenberg, who read substantial parts of his book and discussed them with him in detail. Jammer also interviewed many other founders of quantum mechanics, including Louis de Broglie, Pascual Jordan and Eugene Wigner. He subsequently published his companion study, *The Philosophy of Quantum Mechanics* (John Wiley, 1974), a historically oriented book that has also become a standard work in the field. His recent book *Einstein and Religion* (Harvard, 1999) has also been well received.

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Inside This Issue

Letter from the Chair:

An FHP To-Do List

By Virginia Trimble, Forum Chair

Come to Our Meetings: 5-9 March 2007 in Denver, Colorado, and 14-17 April 2007 in Jacksonville, Florida. The usual web sites for registration,

http://www.aps.org/meet/MAR07
http://www.aps.org/meet/APR07

are open from mid-August and mid-October respectively. At least portions of the invited session programs are advertised elsewhere in this newsletter in remarks from our program chair, Bill Evenson (p. 3).

You don't like paying the registration fee? There are two possible alternatives. If you are retired, you can pay the emeritus fee — and perhaps donate the difference to APS (a fine idea pioneered in 2006 by Horst Meyer). Or, if you definitely plan to attend *only* the Forum sessions, you should pre-register but need not pay the fee at all. Please tell the chair in advance if you plan to exercise this option.

Give a Talk: Abstracts can be entered at *http://abstracts.aps.org*, with deadlines of 20 November 2006 for March and 12 January 2007 for April. An FHP talk can be given in addition to one you give in your area of physics research (though payment of registration fee is then required). You will need to know your member number, which is included on your *Physics Today* mailing label. Incidentally, the monetary subvention we receive from APS depends on the number of contributed paper sessions we hold. This was zero in March 2006 and only one in April 2006. What to talk about? Well, unless you are much younger than the chair (who is thinking of an April talk on pre-discoveries of the cosmic microwave background), some of what you learned as current events is now history. Or you could pick a brand new topic to explore.

Encourage a Student: We can provide partial travel support, at $600 each, for two John Bardeen students at the March meeting (a donation from his family) and two Rolfe Glover students at the April meeting (a donation from Richard Prange). Students can join APS for free for a year (but must do so before the abstract deadline). What might they talk about? It's easy if you know students working in history of physics. But Ph.D. theses in many areas of physics often begin with a chapter of historical introduction to the problem, and some of these would make good presentations. Please ask the student to send the abstract to the FHP Program Chair (Bill.Evenson@uvsc.edu) at the same time it goes into the APS system, so that we know the person is a Bardeen or Glover candidate.

Host a History- or Einstein/Relativity-Oriented Talk at Your Institution: Last year, FHP co-sponsored a World Year of Physics Speakers' Bureau with the Topical Group on General Relativity. It is now called the Las Cumbres Observatory Speakers' Bureau, thanks to a donation from LCO director Wayne Rosing. Preference goes to four-year colleges, but we try to fill all requests, and there is some money available to cover speaker travel to truly impoverished institutions. To request a speaker, go to:

http://www.phys.utb.edu/WYPspeakers/REQUESTS/howto.html

This site can also be accessed from the LCO site, *http://www.lcogt.net/*, which might interest you in its own right. Or, if you are willing to serve as a speaker, please contact the FHP Chair at vtrimble@astro.umd.edu. Some of you have already given talks, for which many thanks!

Volunteer to Run for Office, or help out with a committee. Committee volunteers please contact Bill Evenson by email at Bill.Evenson@uvsc.edu. Volunteers for FHP elections please contact Virginia Trimble at the email address above.

Encourage Your Colleagues to Join the Forum: It is absolutely free for APS members who don't already belong to two (or more) other Forums and only $7 a year for those who do. For a current APS member, joining is as simple as sending an email to membership@aps.org and saying "I want to join FHP!" ■

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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Max Jammer Wins 2007 Pais Prize

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Among his many scholarly articles, Jammer's recent essay, "The Strange Story of the Concept which Inaugurated Modern Theoretical Physics" (*Foundations of Physics*, November 2004), deserves special attention. The enthusiastic reception it received encouraged him to write his latest book, *Concepts of Simultaneity: From Antiquity to Einstein and Beyond*, was published by The Johns Hopkins University Press in 2006. ■

Editors' Corner

This issue begins Volume 10 of History of Physics, the newsletter of the APS Forum on History of Physics. I will serve as Editor of the next six issues, bringing my experience to the job as former Editor and Contributing Editor of *Beam Line*, a quarterly journal of particle physics and cosmology published until 2002 by Stanford Linear Accelerator Center. Robert Romer of Amherst College, who long served as Editor of the prestigious *American Journal of Physics*, has agreed to serve in the crucial role of Associate Editor of this volume — and hopefully will take over from me when I step down three years hence.

I plan to continue the fine traditions established by my predecessor Ben Bederson, and before him by current FHP Chair-Elect Bill Evenson. It is also my fervent hope that Bob and I can expand the newsletter into some interesting new directions, such as publishing brief articles on the history of physics — perhaps based on talks given at the Forum-sponsored sessions of the annual APS meetings. Or we might include some letters and correspondence. Along these lines, let me solicit comments or suggestions from the entire Forum membership regarding any improvements you would like to see in the newsletter. Please direct them to me at my email address: mriordan@ucsc.edu.

Already we have altered the newsletter design to use a serif typeface in the titles; this is generally considered more easily readable. You will see other incremental changes in the next issue. I hope you like what you see and will make good use of this newsletter, which — thanks to the generosity of our current Chair — continues to be available in both paper and electronic versions.

— Michael Riordan, Editor

Forum Sessions at the 2007 APS Meetings

By Bill Evenson, Chair, Forum Program Committee

The FHP Program Committee is planning several interesting sessions at next year's APS March (Denver, CO, 5–9 March 2007) and April (Jacksonville, FL, 14–17 April 2007) meetings. Both of these meetings include opportunities for contributed papers on the history of physics, so we strongly encourage members to submit abstracts by the appropriate deadlines: 20 November 2006 for March and 12 January 2007 for April.

The March meeting will feature a symposium on "Condensed Matter Physics at Synchrotron Facilities: History As Prologue to the Future." This session will have five excellent speakers examining the enormous impact that synchrotron-radiation facilities have had on condensed-matter physics over the last few decades. An evening session is still in the early planning stages for March.

The April meeting will have a two-part symposium on "Nucleosynthesis 50 Years After B2FH," cosponsored with the Division of Astrophysics and tentatively scheduled for Saturday, April 14. We are also cosponsoring a session on the "The Changing Role of Nuclear Weapons in Foreign Policy" with the Forum on Physics and Society (FPS), tentatively scheduled for Sunday, April 15. Our third session in April will examine the "History of Gravitational Physics/General Relativity;" it is jointly sponsored with the Topical Group on Gravitation and also tentatively scheduled for Sunday, April 15. Finally, we have begun organizing what promises to be an insightful session on "Sputnik, 1957: Its Effect on Science in America," tentatively scheduled for Monday, April 16. Max Jammer, winner of the Pais Prize for history of physics, is scheduled to speak at the Joint FHP/FPS Awards Session on Monday, April 16.

We urge FHP members to plan to attend these sessions as part of your participation in the national meetings—as well as to contribute history papers at these meetings. Please note that papers presented in the Forum's contributed sessions can be up to 24 minutes long instead of the usual 12 minutes.

Your suggestions for FHP sessions for future years are always welcome. Please send them to me at my email address, bill.evenson@uvsc.edu.

Call for Fellowship Nominations

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

The FHP unit deadline for the receipt of all materials at APS is May 11, 2007. All nominations submitted to APS will be forwarded to the FHP Fellowship Committee for review. This committee will make its recommendations to the Forum Executive Committee; if approved, the nominations will then go to the APS Council for final approval.

Here are some specific instructions about the nomination process, taken from the APS website. Before submitting your nomination, make sure that the nominee is a member of the Society in good standing. Obtain supporting letters from two sponsors, who do not have to be APS members. Submit a complete original nomination packet (nomination form and supporting letters) and one copy of the entire packet prior to the unit deadline, May 11, 2007. The nomination form may be downloaded from the above website. The nomination materials should be sent to:

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

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

Reports from the 2006 APS Meetings

Low-Temperature Physics: A Historical Perspective

By George Zimmerman

A session on the history of low-temperature physics was held at the March 2006 meeting of the American Physical Society in Baltimore. Taking place on March 16, it included Robert Wheeler of Yale, Russell Donnelly of the University of Oregon, Horst Meyer of Duke, and David Lee of Cornell as invited speakers. Their presentations were followed by a panel discussion in which they were joined by Gerhard Salinger of the NSF and George Yntema. George Zimmerman of Boston University organized the session and chaired it. The Forum on History of Physics sponsored the session and also the taping of the presentations and panel discussion. The tapes were subsequently converted to DVDs with some additional materials added to complement the talks.

The well-attended session was intended to highlight the experimental challenges and achievements of the times when liquid helium was a rarity that was not commercially available. Because of time limitations, the topic was confined to the work that was done mainly in the United States, although there were significant contributions by, and collaborations with, European and Canadian physicists.

Wheeler's presentation, entitled "Low Temperature Physics at Yale in the late 1930s through the early 1950s," concentrated on the legacy of C.T. Lane, who received his doctorate from McGill University in 1929 and built the first Kapitza-type helium liquefier in the United States in 1940. To quote Wheeler's abstract, "In 1933, both C.T. Lane and Lars Onsager were awarded Sterling Fellowships, which initiated a stimulating experimental-theoretical exchange continuing until they both retired." Among the many firsts at that time were the observations of second sound in He-II and the rediscovery of the deHaas-vanAlphen effect in zinc, which, with the insights of Onsager and his students, provided a better understanding of the Fermi surface of metals. Lane was joined by Henry A. Fairbank in 1944, and together they did pioneering work on He-3 impurities in He-4. At the time there was a question about whether He-3 could be liquefied. Their experiments showed that indeed it could be, prior to the availability of He-3 in sufficient quantities. The talk ended with the contrast between the initial, small liquid-helium installation at Yale in 1940, in which ten liters of liquid was deemed a large quantity, and the present Dewars and Collins-type liquefiers capable of producing thousands of liters of liquid helium. Wheeler, Donnelly and Fairbank all received their doctorates under Lane's tutelage.

Donnelly's talk, entitled "Rotating Superfluids," concentrated on another facet of the studies pioneered at Yale. He complemented Wheeler's biography of C.T. Lane and recounted their early experiments on the hydrodynamics of liquid helium. Donnelly also gave a general overview of this fascinating subject and reviewed the present state of this field of study, which bears many similarities to the recently observed Bose-Einstein Condensates.

David Lee, who received his doctorate under the direction of Henry Fairbank, recounted the research on He-3 in the 1950s, 1960s and 1970s. Lee, Osheroff and Richardson received the 1996 Nobel Prize for the 1972 discovery of superfluidity in liquid He-3. Lee gave a brief review of Fairbank's career and then proceeded to highlight the achievements leading to the development of the technology that led to the discovery. Those were necessary because of the low temperatures, in the milli-degree region, where this transition occurs. Some of those milestones were the exploration of the properties of liquid He-3/He-4 mixtures, which eventually led to the development of the dilution refrigerator. The separation of the liquid mixture into He-4 and He-3 rich phases was discovered at Duke by Henry's brother William Fairbank, who also received his doctorate under C.T. Lane. Henry's students further explored the separation phase diagram. Other groups involved in similar experiments were the Ohio State group led by Daunt and Edwards, and the University of Illinois group under John Wheatley. Those groups, together with researchers at Argonne and Los Alamos, were also instrumental in establishing the Fermi-liquid character of He-3. This led to the observation of the negative slope of the melting curve in He-3, which enabled Pomaranchuk cooling to occur and allowed physicists to reach the milli-degree region long enough to make measurements and discover superfluidity.

After receiving his doctorate at the University of Zurich, Horst Meyer came to Duke University in 1959. That was the year Bill Fairbank left for Stanford; Meyer carried on the Duke low-temperature tradition. His presentation, entitled "Fritz London's Legacy at Duke University," recounted London's interest in He-3 after some of it became available. He was intrigued by the Fermi degeneracy properties of the substance, as opposed to the Bose-Einstein properties of He-4. London and Walter Gordy were instrumental in bringing Bill Fairbank to Duke, where he developed the NMR technique and experimentally demonstrated the He-3 Fermi degeneracy.

In the panel discussion, Gerhard Salinger talked about the research of the Illinois group and the intensity and tone John Wheatley set there. This historical session had representatives mainly associated with the Yale low-temperature group. The audience of about a hundred was composed of both young and somewhat older but still active physicists, who deemed it interesting and instructive to hear about what had come before their time, from physicists who had been there personally.

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Pioneering Women Astronomers

By Virginia Trimble

This session, co-sponsored by the Forum and the Commission on Status of Women in Physics (CSWP), occurred at the April APS meeting in Dallas. Its premise was that the gradual incorporation of women into astronomical research (not yet quite complete) had four stages: (1) a time when women could participate only by assisting a father, brother, or husband (Sofia Brahe, Margaret Hevelius, Caroline Herschel, Maria Mitchell, for instance); (2) the era of the "computer," when women were paid (very little!) to reduce data collected at the telescope, following instructions from the men who had collected it (beginning at Harvard in the 1880s, but the last Mt. Wilson computer retired only about 1970); (3) a moment when women began functioning as full research astronomers, both asking and answering their own questions; and (4) the initial breaking of the glass ceiling, with women serving in leadership roles at observatories, institutes, and universities. The computer era was represented by Jean Turner (UCLA) talking about Henrietta Leavitt, discoverer of the period-luminosity relation for Cepheids. Katherine Gaposchkin Haramundanis (Hewlett-Packard), daughter of Sergei and Cecilia Payne Gaposchkin (the first "true" woman astronomer), spoke about her mother's life and work. She also provided a brief introduction to Dorrit Hoffleit, who bridged phases (2) and (3). And our "ceiling breaker" was Jill Tarter, director of the SETI Institute.

Turner began by explaining that Leavitt discovered the correlation between pulsation period and absolute brightness of Cepheid variable stars. This enables their use as the preferred "standard candles" for measuring galactic and extragalactic distances, with enormous impact on our knowledge of galactic structure, the local universe, and cosmology. Leavitt started out at the turn of the 20th century as a volunteer at Harvard College Observatory and remained there as a "computer" until her death in 1921. In addition to the period-luminosity relation, Leavitt also discovered a large fraction of the variable stars known up to that time and was responsible for one of the first standardized photometric systems. She did not follow up on her P-L discovery, although her 1912 paper clearly outlines the steps needed to establish this method as a reliable means of determining distance. She worked on topics chosen by the observatory director, Pickering, and was not free to pursue this line of research. By all accounts, she seems to have been happy with this research model. According to one colleague, "her sense of duty, justice, and loyalty was strong." There is no indication that she felt the lack of recognition in her lifetime, in contrast to some of the other computers. Leavitt's work was a stepping stone for Harlow Shapley and Edwin Hubble, who used the period-luminosity relation of Cepheid variables to establish the size of our galaxy, the existence of other galaxies, and the expansion of the universe.

Dorrit Hoffleit (b. 12 March 1906) began her astronomical career in 1929 at Harvard College Observatory, where she worked on photographic variables and spectral classification. She took courses part-time to obtain an MA degree in 1932. By 1938, with strong encouragement from Shapley and Bart Bok, she had received her PhD in astronomy from Radcliffe College for a thesis "on the spectroscopic determination of absolute magnitudes" (a subject closely linked to Leavitt's work, to that of Antonia Maury, another of the computers, and to later work by Payne-Gaposchkin). She continued to work at HCO until 1956, when she became director of Maria Mitchell Observatory and a member of the Department of Astronomy at Yale. In her own view, her most original work was an early paper on light curves of meteor trails, but her best-known publications are several editions of the Yale Bright Star Catalogue and several astrometric catalogues. With a strong interest in variable stars, she has worked closely with the serious amateur society, American Association of Variable Star Observers (AAVSO). Her books include histories of astronomy at Yale, women in variable-star astronomy, and the education of women astronomers before 1960. Celebrating her 100th birthday shortly before the Dallas meeting, she was the obvious person for whom to name our first FHP lectureship, co-sponsored with CSWP. For more information, see Dorritt Hoffleit, "Misfortunes as Blessings in Disguise" (AAVSO, 2002).

And the obvious person to be our Dorrit Hoffleit Lecturer was Katherine Gaposchkin Haramundanis, who, among her many other accomplishments, co-authored an introductory astronomy text book with her mother, Cecilia Helena Payne Gaposchkin. Cecilia Payne attended girls' schools in England, followed by Newnham College (also women only) at Cambridge University, completing the requirements for a degree in 1923 but not receiving one, since Cambridge began awarding degrees to women only after WWII. Starting out in biology, she soon switched to astronomy, inspired by a lecture by Arthur Eddington. When she asked about the possibility of completing a PhD, he responded that he saw "no insuperable obstacle" provided she moved to the United States. This she did, hitting the ground at HCO running, and by 1925 had completed what Otto Struve called the most important PhD thesis in astronomy up to his time. It was also the first at Harvard with a female author and the first in astronomy. In it, Payne reached two enormously important conclusions: first, that nearly all stars have essentially the same chemical composition and differ primarily in surface temperature (applications of the Boltzmann and Saha equations); and, second, that this composition is strongly dominated by hydrogen and helium. The first conclusion was accepted quickly by her Harvard advisor, Shapley, and by Russell at Princeton. The second was nearly suppressed by them. Russell only gradually came around to her view years later on the basis of his own solar data, but still gave her only grudging credit.

Among Payne's other firsts were the Annie J. Cannon Prize of the American Astronomical Society, the first of its Russell Lectureships awarded to a

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woman, the first woman to attain full professorship in the Harvard School of Arts and Letters, and the first woman chair of the astronomy department there. Much of her later work concerned variable stars, especially novae, a great deal of it in collaboration with her husband, Sergei Gaposchkin. Among the next generation of astronomers on whom she had great influence were Fred Whipple and Jesse Greenstein. Haramundanis drew attention to interesting correlations between Payne-Gaposchkin's rate of publication and "life cycle events" like marriage and the birth of her three children, World War II, and retirement. She also mentioned the reality of the "pink pay check," apparently about 60 percent of what men got for the same work, much as in the days of Leavitt, Hoffleit, and others since. For more information see K. Haramundanis (editor), *Cecilia Payne-Gaposchkin*, 2nd edition, incorporating a new introduction by V. Trimble (Cambridge University Press, 1996).

Jill Tarter, director of the SETI Institute in Mountain View, California, says she learned about teams by leading one. An undergraduate engineering major at Cornell (where her gender kept her from a scholarship reserved for descendants of the founder), she was barred from the informal student study groups held on the other side of campus because the women's dorms were locked at night. Thus she learned more science (having to solve all the problem sets on her own) but fewer interaction skills than the guys. Going on to UC Berkeley, she completed a PhD thesis in infrared astronomy for which she will long be remembered, because it coined the term "brown dwarf" to describe the failed stars that never quite made it to hydrogen fusion.

And then came the unique opportunity presented by living among the first generation of humans who can try to answer the question "Are we alone?" with observations rather than speculation. SETI is the Search for Extra-Terrestrial Intelligence. There has been, from time to time, government support for using the methods of radio astronomy to try to identify signals from other civilizations, but it has been grudging and irregular. Tarter, adopting her normal strategies of working harder than anybody else around and focusing on the goal, not on the methods or on who gets credit, has succeeded in bringing in sufficient private funding to support the Institute and to begin construction of a purpose-built facility, the Allen Telescope Array, which will scan a very large number of promising stars frequently, meanwhile also carrying out important projects in mainstream radio astronomy. During the construction phase, the SETI Institute team also has searches in progress at existing radio observatories, where they operate again in parallel with other programs. She indicates that she will find equally interesting either the end of our cosmic isolation or the acceptance of our singularity. ■

Observational Cosmology: Past, Present and Future

By Virginia Trimble

A pair of sessions on the history of cosmology, co-sponsored at the Dallas APS meeting by the Forum and the Division of Astrophysics, aimed to revise the old cliché to read "he who is willing to study history need repeat only the good parts." Both sessions began with a talk by a credentialed historian, included someone currently active in collecting observations and interpreting them, and ended with a builder of future facilities and future theories—with just one glitch. Among six speakers there ought to be a young person or two and at least one woman. Physics demographics are such that the woman is likely to be young. And young women sometimes decide to have children. Thus, about two weeks before the start of the meeting, Elizabeth Barton ("Observational Cosmology Today") reported that minor complications would prevent her from attending. The session organizer therefore spoke in her place.

A Milton scholar by training, Dennis Danielson (University of British Columbia) opened by asking "why Aristotle took so long to die." He began by pointing out that, for a long time, Aristotle didn't have a lot of competition and that his ideas were a good fit to many everyday experiences. As a result, the earliest thinkers we today classify as scientists (Bacon, Galileo, Newton) cut their teeth trying to show how Aristotle was wrong, but it wasn't easy. His four elements—earth, water, air, and fire—are widely known. Less well known is that earth included all solids, water all liquids (including blood) and air all gases. The idea of "natural place" meant that earth fell downward through water, while water would rise up through earth and fall down through air. Earth falling equally in all directions naturally formed a sphere. Then Aristotle's cosmos was, in a sense, "cosmocentric" rather than "geocentric," though the speaker admitted he does not expect the term to catch on. In the terrestrial "lower story" of the cosmos, there is both natural motion toward natural place and unnatural or imposed motion, which eventually loses, as when you throw a stone upward. The celestial "upper story" had only natural motion, always circular and undying. By temporarily becoming Aristotelians, we can see why Copernicanism took a hundred years to catch on. The heliocentric celestial machine demanded new physics, which nobody (until Newton) had yet provided and for which everyday

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experience gave no evidence, to replace the natural circular motion peculiar to quintessence.

Danielson credited Tycho's limit on the geocentric parallax of the nova stella of 1572 (placing it in trans-lunar space) as the critical event. He attributed the discovery to Wolfgang Schuler of Wittenberg (although a case has also been made for Francesco Maurulyco). Examining Aristotle's long monopoly on physics—based on what had grown to look like common sense—can also help us to gain perspective on present habits of thought, and perhaps even to find that those habits, as exemplified by modern astrophysics, still conceal unpurged remnants of Aristotelianism.

Inevitably, Virginia Trimble (UC Irvine and Las Cumbres Observatory) took over the microphone and gave the most Aristotelian-as-possible overview of current observational cosmology. She pointed out that cosmology is a "hot topic," making up only about 5 percent of recent published papers, but attracting 10 percent of the citations. Fifty years ago, cosmology meant (according to Allan Sandage) "a search for two numbers"—the Hubble constant and the deceleration parameter. It progressed 25 years ago to "large scale structure and evolution of the universe," and now includes the desire to have precision values for many more numbers (Sandage's two, the age of the universe, spatial curvature, the cosmological constant, normalization of the fluctuation spectrum that evolved to galaxies, temperatures of photons and neutrinos, the current amplitude of density inhomogeneities, and the optical depth back to the time of reionization). Also being sought is a complete inventory of matter and energy in all forms, the initial conditions coming out of inflation that lead to all these numbers, and how we got from those initial conditions to everything we can see today. The "how we got here" phase includes formation and evolution of galaxies and clusters, first lights and reionization, and the growth of perturbations and mergers.

The Aristotelian part is the claim that quite of lot of this information is actually in good shape, especially the parameters and inventory. Indeed, some of it could have been said a long while ago, for instance the case for dark matter and its contribution to total mass density, using only observations published before World War II. Among the residual problems she remarked on are some that probably belong to theory (details of small-scale structure; the nature of the dark matter and dark energy), but observations are particularly relevant to the "how we got here" phase.

Barton's research focuses on finding and characterizing the first proto-galaxies and evaluating how they changed with time, by observing objects at different redshifts. She is currently pursuing a search for very early star-forming regions at a redshift $z \sim 8$ (the most distant known galaxies are at $z = 6.5$). She is looking through a window in atmospheric opacity at $z = 8.3$ where the Lyman alpha line is red-shifted to 1.2 micrometers. Young stars ionize hydrogen, which emits this radiation as it recombines. And the emissions can actually reach us if there are small star-forming gas clouds inside regions already ionized by the first large star-forming regions. Her current upper limits already rule out some of the most optimistic estimates of early star-formation rates.

John Carlstrom (University of Chicago) looked from present observations toward future ones. Observations of the cosmic microwave background (CMB) radiation with the WMAP satellite have provided better values of almost all of the dozen numbers Trimble mentioned: 4 percent baryons, 23 percent dark matter, and 73 percent dark energy, in a universe 13.7 billion years old, and so forth. The March 2006 release of three-year results from WMAP has particularly improved on the slope of the spectrum of density fluctuations to values ranging from 0.93 to 0.95 (versus a naive value of 1.00) and on the amount of electron scattering of the CMB radiation since recombination.

Scattering, Carlstrom reminded us, polarizes radiation, and the WMAP observations of polarization now indicate that reionization began near $z = 12\text{-}15$, rather than earlier (as seemed to be the case from the first-year WMAP data). He described upcoming observational facilities: the European Planck space mission planned for launch early in the next decade, and the ground-based Atacama Large Millimeter Array (ALMA) under construction in the Chilean desert), among others. These measurements will focus on characterizing the CMB temperature anisotropy on finer angular scales and the polarization anisotropy on all angular scales. They have the potential to tell us (a) the mass of the heaviest neutrino (through its influence on small-scale structure), (b) the equation of state of the dark energy and whether its pressure/density ratio changes with time, (c) much about the structures in our own Milky Way and in intervening space and time that are the foregrounds to the CMB radiation but are important in themselves, and, most exciting of all, (d) the energy scale at which inflation occurred. If this is near 10^{16} GeV, then there should be structure in the polarization maps at about 1 percent of the scattering-induced part, which Planck should be able to observe. If the energy were much larger than that, we would already know about it from WMAP. ■

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My regret was that we were not able to feature other groups, although some of their achievements came out in the presentations. The DVDs contain comments of one of the first salesmen for the Collins Helium Liquefier sold by A. D. Little and a brief summary of the NBS Low Temperature group by R. Hudson. I hope to gather some more reviews of other groups' achievements and edit a CD to supplement the DVDs. I already have a summary of the Los Alamos work from William Keller. I also hope that in the near future we will be able to organize historical sessions in similar subjects so that younger physicists can learn more about their past. ■

Theoretical Cosmology: Past, Present and Future

By Virginia Trimble

The second of two sessions on the history of cosmology held at the Dallas APS meeting focused on theory. Helge Kragh (the J. Robert Oppenheimer lecturer, sponsored by Robert Christy and the late Philip Morrison) opened the session by pointing out that the Big Bang was proposed three different times over 30 years, almost independently, and yet came to dominate cosmological thinking only slowly before 1965. Although Alexander Friedmann referred to "the creation of the world" in his 1922 paper, it was only in a mathematical sense. In spite of the great importance of Friedmann's paper, it did not lay the foundation of modern Big-Bang cosmology. A much better candidate is Georges Lemaitre, who in 1931 presented the first example of a universe with an initial-state, primeval-atom model, which he likened to an exploding super-radioactive atom. He suggested that the presently observed cosmic rays were the fossil remnants of the explosion.

Big-Bang type models remained controversial during the 1930s and were sometimes associated with Edward Milne's cosmology rather than with general relativity. Basically, what was lacking was a connection between nuclear physics and the early universe. This was precisely what George Gamow provided with his renewal of big-bang cosmology in the late 1940s. His research program with Ralph Alpher and Robert Herman was in many ways progressive, but in the end no more successful than Lemaitre's. It led to detailed calculations of the helium abundance in the universe and predicted the existence of microwave background radiation. Yet the theory was almost entirely ignored, with only a single paper on the subject appearing between Gamow's last in 1953 and the revival by Yakov Zeldovich, Robert Dicke, and others beginning in 1963–64. Why? Generally speaking, there were sociological as well as scientific factors, among them the existence of a strong rival theory, the steady-state universe. It is interesting that the Big Bang, which eventually triumphed, had stronger competition than Aristotle, who eventually lost! (See Observational Cosmology, p. 6.) For more on this subject, see H. Kragh, *Cosmology and Controversy* (Princeton University Press, 1996).

David Spergel (Princeton) carried the story forward, pointing out that cosmology now has a standard model that describes the large-scale distribution of galaxies, detailed observations of the microwave background, observations of supernovae, and the abundances of the light elements, as well as a host of other astronomical observations. In this model, the universe is spatially flat, homogeneous and isotropic on large scales. It is composed of ordinary matter, dark matter, and radiation and has a cosmological constant.

Spergel highlighted two results from the recently released WMAP three-year data. First, the spectrum of primordial fluctuations is not quite scale-invariant (i.e., Harrison-Zeldovich). It is tilted a bit in the direction of smaller-scale structures, which agrees with some inflationary predictions and may help with some details in modeling structure formation. Second, the optical depth to electron scattering of the CMB radiation since reionization began seems to be a bit less than in the earlier data, meaning that "first lights" and the beginning of this phase become a bit easier to model. While this simple model has had many successes, we still want to know more about what happened during the first moments (nanoseconds or less) of the Big Bang, more about the dark energy (and dark matter), and about how star-formation began. Future observations and modeling of the large-scale distribution of galaxies, properties of distant supernovae, and the CMB radiation and its polarization at finer scales can help to address these questions. For more information, see D. Spergel et al., *Astrophysical Journal*, in press, www.arxiv.org/astro-ph/0603449

Sean Carroll (University of Chicago) then leapfrogged into the future, asking what the universe might look like to astronomers 100 years hence, when advances in technology should have answered the questions Spergel had left hanging. He pointed out that there are many well-motivated candidates for dark matter, some of which could be produced in laboratory experiments and others observed astrophysically. On the other hand, the only well-motivated candidate for dark energy is vacuum energy, which leads to an energy density larger than what is observed by a factor of 10^{120}, or 10^{120} times the observed density of 10^{-8} erg per cubic centimeter. It is not clear that any laboratory experiments presently contemplated can shed light on dark energy, but there are possible astronomical footprints. Inflation, which plays an important role in our understanding of the origin of the universe, is also being experimentally tested. However, there are important conceptual questions remaining about the onset of inflation and its connections to particle physics. Better understanding of the underlying physics of inflation has the potential to put this idea on a firmer footing and may lead to a concrete picture of the early universe beyond what we can observe.

The evolution of the universe from primordial fluctuations to current structures requires numerical simulations. Here is where we can confidently predict revolutionary changes. Computers will be immensely more powerful in 100 years, perhaps with the aid of quantum technology. And the type of computation will be qualitatively different. Computers will not simply be running simulations; they will be functioning as theorists themselves. We can only hope there will still be some place for human cosmologists 100 years from now!

There is also, Carroll noted near the end of his talk, an important philosophical issue. Any theory is likely to make some untestable predictions, but we judge it by its testable ones. The multiverse concept is not a theory, he said, but rather a prediction of some as-yet unborn theory! For more information, see the series of "Early Universe" papers in *Nature*, 27 April 2006, especially S. M. Carroll, "Is Our Universe Natural?" pp. 1132–36. ■

FHP Session on Contributed Papers at Dallas

By Robert H. Romer

A well-attended session (chaired by Robert H. Romer) of five 24-minute contributed papers occurred on Monday afternoon, 24 April 2005, during the Dallas APS meeting. Harry Lustig of CCNY led off with an interesting talk titled "Was Nazi Germany on the Road to an Atomic Bomb after All?" Historians generally agree that the Germans were not close to a bomb by 1945. But why did they not succeed? Some have argued that Heisenberg did not understand the physics sufficiently well, others that the German scientists deliberately failed in order to save the world from Hitler having this awesome weapon, and others have claimed that the Germans suffered from poor organization and lack of facilities. In a provocative new book from Germany, *Hitlers Bombe*, Rainer Karlsch asserts that in 1945 the Germans actually did make and test a primitive fission and fusion bomb! But Lustig pointed out that they surely had very little U-235 and probably no plutonium at all. Nothing in the "Farm Hall transcripts" supports the claim that they made and tested a bomb or were anywhere close to it. Karlsch argues that Kurt Diebner and Walther Gerlach, rather than Werner Heisenberg, were the leaders of the German bomb project, at least in its later phases. In spite of these dubious claims about a successful bomb, this information about the German project make Karlsch's book worthy of attention.

Paul Halpern (University of the Sciences in Philadelphia) gave a talk entitled, "Einstein and Oskar Klein: The Fifth Dimension as a Bridge across Quantum Chasms," in which he explored conceptual, philosophical and historical connections between Einstein's and Klein's attempts to unify gravitation and electromagnetism in a single five-dimensional set of equations. Halpern began by outlining the early attempts by Gunnar Nordström and Theodor Kaluza to accommodate Maxwell's equations within unified five-dimensional models. He then analyzed the similarities and differences between Einstein's and Klein's theories, examined the correspondence between the two theorists, and delved into the reasons each came to embrace and abandon the idea of the fifth dimension. He discussed the advice and support offered by Paul Ehrenfest, George Uhlenbeck, and Heinrich Mandel and showed how Pauli played a major role in the decision of both Einstein and Klein to give up on five-dimensional unification. Finally, he commented on the reasons why Einstein pressed forward with deterministic unification models as a substitute for probabilistic quantum mechanics long after Klein had embraced the Copenhagen school of thought on this issue.

Benjamin Bederson (NYU) gave an interesting talk about his WWII experiences at Los Alamos. As a young draftee with two years of undergraduate physics, he was contacted by a recruiter for the Manhattan Project. As a New Yorker, Ben jumped at the chance. "Manhattan, that's for me!" he said to himself, "It has to be better than a foxhole." And so he soon found himself in New Mexico, still in the US Army but now a member of the "Special Engineering Department" (SED), along with a number of other young soldiers with some technical background. Fellow members of the SED included such future scientists as Val Fitch, Murray Peshkin, Peter Lax, John Kemeny, and Richard Davisson. Once they had obtained security clearance, SED soldiers were informed about the nature of the project, worked side-by-side with senior scientists, and attended all the technical lectures. Thus the SED became, as Ben put it, a "breeding ground" for future scientists.

Donald Salisbury of Austin College presented a talk on "Rosenfeld, Bergmann, and the Invention of Constrained Hamiltonian Dynamics," a subject that, he said, constitutes the theoretical foundation for the canonical quantization of all modern gauge theories, including general relativity, quantum chromodynamics, and brane theory. According to conventional wisdom, this approach was developed independently by Paul Dirac and Peter Bergmann, beginning in 1949. Both authors apparently became aware of the pioneering 1930 work of Leon Rosenfeld only after they had made substantial progress in deriving and interpreting phase-space constraints. Rosenfeld had indeed written down an expression for the Hamiltonian for general relativity in terms of tetrad fields. He could easily have displayed this object in terms of tetrad fields and conjugate momenta, anticipating by almost 30 years the gravitational Hamiltonian invented by Dirac in 1958. Rosenfeld also sought and obtained a correct phase-space generator for infinitesimal general coordinate transformations. He may have realized that commutators of these phase- space generators did not reproduce the correct Lie algebra of infinitesimal transformations. One indication that he may have grasped the problem is his decision to confine his group-theoretical analysis to a realizable subgroup of the full group of general coordinate transformations. Bergmann and Arthur Komar took the first steps in 1972 by recognizing the true nature of the canonically realizable general coordinate symmetry group. Analyses of both Rosenfeld's and Bergmann's early work, including a translation of Rosenfeld's 1930 paper, will soon be available as online preprints of the Max Planck Institute for the History of Science in Berlin.

For the finale, Virginia Trimble (UC, Irvine) gave a talk titled "Historic Patterns in Astronomical Incomprehension." To those of us who lived through them, she began, the 23 years from the 1973 discovery of gamma-ray bursters (GRBs) to their definitive 1996 identification with the formation of massive, rapidly rotating black holes (so rare that a typical galaxy has one about every million years but so powerful that we can see them throughout the visible universe) seemed like a very long time. But 23 years is far from a record for the time between an observed phenomena being regarded as puzzling and its final, consensus explication. While the in-

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terval was, at most, a few months for pulsars and a few years for quasars, it was 259 years for pulsating stars like Cepheid variables and also many decades for the source of stellar energy, the nature of the emission lines from the solar corona, and the precession of the perihelion of Mercury. In two of these cases (the GRBs and the Cepheid variables), the community was for many years in almost universal agreement about a model that later proved to be totally wrong—eclipsing binary stars for the variables and star quakes on old, nearby neutron stars for the GRBs. Another parallel set of patterns in astronomical incomprehension can be seen when someone has taken a theory (or model or scenario) seriously enough to work out some predictions, and efforts to verify or falsify the predictions have gone forward. Again there have been short intervals (21 cm radiation from hydrogen) and long ones (fluctuations in the temperature of the cosmic microwave background on the sky), with the record surely held by heliocentric parallax, predicted by assorted Greeks more than 2000 years before its nearly simultaneous, independent measurement by three nineteenth-century astronomers. There are, of course, incomplete stories in both classes (spiral arms of galaxies among many puzzling phenomena and gravitational radiation among the predictions). Trimble had time for details of only the Cepheid-variable and GRB sagas but ended with a long list of current astronomical research topics that audience members were invited to sort into the two classes. ■

The Seven Pines Symposium

By Roger H. Stuewer

The tenth annual Seven Pines Symposium was held from May 3rd to 7th, 2006, on the subject of "Probability and Improbability in Science." This meeting brings leading historians, philosophers, and scientists together for several days to probe and clarify significant foundational issues in science, as they have arisen in the past and continue to challenge our understanding. It was held in the Outing Lodge at Pine Point near Stillwater, Minnesota, a beautiful facility surrounded by spacious grounds with many trails for hiking and birdwatching. Its idyllic setting and superb cuisine make it an ideal location for small meetings like this.

Unlike in typical conferences, talks are limited to 30 minutes, with 45 minutes devoted to the subsequent discussions, and long midday breaks permit small groups to assemble at will. As preparation for their talks, the speakers prepare summarizing statements and background reading materials that are distributed to participants in advance. Twenty-one prominent historians, philosophers, and scientists participated in this year's symposium. They had been selected by an advisory board consisting of Roger H. Stuewer (Minnesota), Chair; Michel Janssen (Minnesota), Vice Chair; John Earman (Pittsburgh); Geoffrey Hellman (Minnesota); Don Howard (Notre Dame); and Robert M. Wald (Chicago). Symposium founder Lee Gohlike, owner of Outing Lodge, outlined its goals in his opening remarks.

Each day the speakers set the stage for the discussions by addressing the major historical, philosophical, and scientific issues pertaining to the subject of the Symposium. Thus the morning of Thursday, May 4, was devoted to the general topic of "History of Probability and Statistical Inference," with Jos Uffink (Utrecht) speaking on "History of Probability" and Stephen M. Stigler (Chicago) on "History of Statistical Inference." That afternoon the topic was "Problems in Statistical Inference," with Allan Franklin (Colorado) talking about "Bayesian Methods" and Teddy Seidenfeld (Carnegie Mellon) about "Alternatives to Bayesian Methods." The Friday, May 5, morning session addressed the topic "Interpretations of Probability in Quantum Mechanics," with Itamar Pitowsky (Hebrew University) speaking on "Subjective Probabilities" and Geoffrey Hellman (Minnesota) on "Objective Probabilities." That afternoon the topic was "Probability and Improbability in Cosmology," with Alan H. Guth (MIT) speaking on "How Probable is Our Universe?" and Robert M. Wald (Chicago) on "Use of Probability Arguments in Cosmology." The morning of Saturday, May 6, was devoted to the topic of "Probability and Improbability in Statistical Physics," with David Ruelle (IHES, Bures-sur-Yvette) speaking on "New Concepts" and Daniel L. Stein (NYU) on "Probability in Disordered Systems." That afternoon the topic was "Probability and Improbability in the Evolution of Life," with Michael Travisano (Houston) speaking on "The Importance of Rare Events." Lee Gohlike concluded the afternoon session with a fascinating talk, "Barney Oldfield and Ralph de Palma: America's Most Famous Racers." The closing discussion on Sunday morning, May 7, was chaired by Roger H. Stuewer.

The eleventh annual Seven Pines Symposium will occur May 2–6, 2007, on the subject "Emergence: From Physics to Biology." ■

We Hear That . . .

Chair Emeritus Nina Byers has alerted us about a book of essays on the much-ignored (until recently) physics contributions of women, which she edited with Gary Williams. Titled *Out of the Shadows: Contributions of 20th Century Women to Physics*, it contains a foreword by Freeman Dyson and was just published by Cambridge University Press. "This wonderful book beautifully illustrates that scientific talent has absolutely nothing to do with gender," says Nobel laureate Jerome Friedman of MIT. We hope to review this book in a future edition of this newsletter. ■

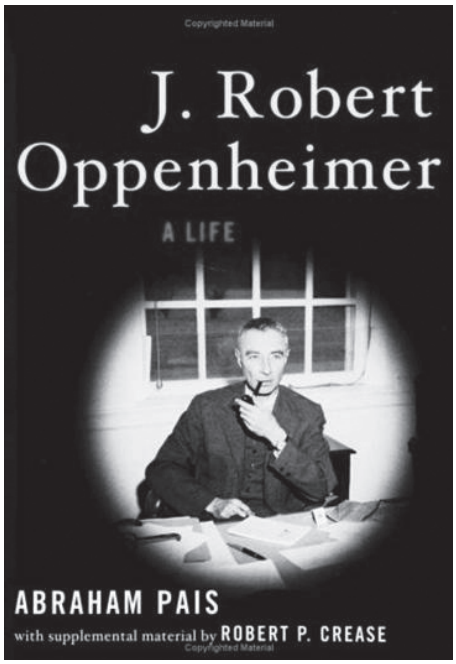
New Books Of Note

J. Robert Oppenheimer: A Life

by Abraham Pais, with supplemental material by Robert P. Crease

Oxford University Press, 2006, 353 pages.

Reviewed by Benjamin Bederson



This is the long-awaited book that Pais had been working on before his death, which prevented him from completing it. It has been eagerly anticipated, not only because Pais was a masterful writer but also because he was a friend of Oppenheimer, notably during the latter's years as director of the Institute for Advanced Study at Princeton. We had reason to expect an illuminating, personal portrait that would provide new material about his thoroughly explored, though still not fully revealed, character. We are not disappointed. The book is both personal and revealing. Pais makes no attempt to hold back his own opinions and feelings, and as a result the book paints a new portrait of Oppenheimer that, despite the voluminous literature already existing on the subject, adds new insight into this "enigmatic" physicist.

The strictly biographical aspects of this book reveal little that has not already been exhaustively covered, perhaps even more thoroughly, in previous books, though with a few anecdotal surprises. No principal of the scientific-political scene of the past half century has endured more probing and poking than has J. Robert "Oppie" Oppenheimer. In the past several years alone, a slew of fine books about him has appeared, including the 700-page biography by Kai Bird and Martin J. Sherwin, *American Prometheus*; the David J. Cassidy volume, *J. Robert Oppenheimer and the American Century*; the short but insightful study by Jeremy Bernstein, *Oppenheimer: Portrait of an Enigma*; and an interesting synthesis of a recent conference about him, *Reappraising Oppenheimer—Centennial Studies and Reflections*, with contributions by Oppenheimer scholars and colleagues. Notwithstanding all this recent literature, Pais brings to the subject his own immense knowledge and wisdom, offering honest appraisals of Oppie's strengths and (quite substantial) flaws that present yet another perspective on this iconic figure.

Much of the material in this volume covers well-plowed ground. I include here Oppie's postdoctoral experiences in the great European centers of physics where quantum mechanics was born, followed by his enormously productive years at Berkeley and Cal Tech—where he not only did his best work, but also became the young but effective father-figure for an impressive group of graduate students—plus the Los Alamos days and his service as Institute director. Pais delivers some pungent personal opinions on all this, including valuable material on the science that Oppenheimer produced during the early days. But I experienced a feeling of great frustration because Pais's contribution to this volume ends on the fateful day that the AEC security hearings began. Robert P. Crease takes over for the final 82 pages and thoroughly describes these infamous hearings, finishing up the narrative on Oppenheimer's life afterwards. Crease's description of the trial—in which the denial of Oppenheimer's security clearance was a forgone conclusion—once again gives readers an opportunity to fume over Oppie's treatment at the hands of the "prosecution" and over the behind-the-scenes actions of his nemesis, Lewis L. Strauss. Here readers will find no surprises, for this hearing has been examined previously in great detail.

Pais was most familiar with Oppenheimer during the latter's tenure at the Institute for Advanced Study, and it shows. We encounter many inside stories and much gossip about the Institute, a subject that both Pais and Crease cover in delicious detail. Those readers with academic backgrounds will be surprised—and amused—to discover that the halls of the lofty Institute house the same kinds of academic struggles, trivial quarrels, and deep animosities that we know all too well from personal experience.

I was greatly taken by Pais' informal writing style. Had he lived, I think, he would have edited and polished the book, thereby removing much of its informal charm. As one example, the title of the chapter describing the beginnings of Oppenheimer's security problems is "In which the excrement hits the ventilator." One juicy tidbit that was new to me was Pais's statement, gleaned from a conversation with Francis Ferguson, one of his subject's early friends, that Oppy visited prostitutes in London.

In summary, Pais's portrait (which he actually calls an essay) presents a balanced view of this iconic physicist. He clearly exhibits ambivalence regarding his own feelings about Oppenheimer. Every expression of approval or admiration about his science, charisma, and leadership qualities is tempered by a darker appraisal of his character. After reading this volume, I emerged with the final impression that Oppenheimer was a deeply flawed near-genius whose impact upon society was truly important, but not in a completely positive way. ■

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