

HISTORY of Physics

NEWSLETTER

On the History of the Forum

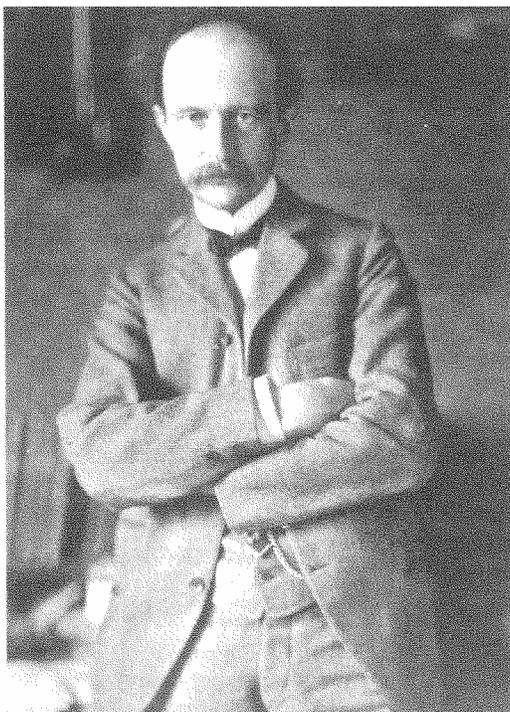
—Laurie M. Brown, Forum Chair

We study the history of science, and of physics in particular, delighting in the world’s magnificent order (and chaos). We take pleasure in viewing the gradually increasing knowledge of nature and in the occasional flight of genius. But there are practical goals as well. We learn from this study, for ourselves and for our students, how physics has really been done, as opposed to the polished (and often sanitized) published versions. History helps us to understand not only the past, but also the present, and we hope it will guide us effectively toward the future. In that spirit, I thought it might be of some value to review the history of the Forum on the History of Physics.

On looking through my files concerning the founding of the Division of History of Physics (its original name), I found Volume I, Number 1 of the *History of Physics Newsletter*, dated August 1982. Produced by Editor Stephen G. Brush and Associate Editors Kathryn Olesko and George A. Snow, it was a remarkably informative first issue, containing a listing of the newly elected first officers and Executive Committee and a report on its first meeting. (The first Chair was Martin J. Klein, who was elected by acclamation.) It gave the program of the Division’s “Inaugural Session” held on 22 April 1981 at the APS Spring Meeting in Baltimore, as well as the programs of three other sessions

organized at the Baltimore and San Francisco national APS meetings that same year. The first Newsletter had editorials, announcements of history conferences, grants, job opportunities, obituaries, queries, reports on recent conferences, seven pages of summaries of recent books and articles, and other news. I was surprised to discover that it also had, beginning on the front page, an article entitled “The History of the Division of History,” written by myself.

The article related how a group of physicists-historians attending the History of Science Society Annual Meeting in Madison, Wisconsin, in October 1978, during a dinner, expressed the frustrations of working on the history of physics within the physics discipline, in their academic departments or at the laboratories where they were employed. They noted that many physicists had a strong interest in the history, but had little contact with each other or with professional historians of science. The time appeared to be ripe for APS to form a new division to represent these interests. A few months later, some of us wrote and began to circulate a petition addressed to the APS Council, stating that (and I quote my 1982 *Newsletter* article here): “important scientific, cultural, and pedagogical values are present in the history of physics, and many members of the APS do



AIP Emilio Segrè Visual Archives, W.F. Meggers Collection

December marks the 100th anniversary of Max Planck’s pioneering paper on radiation quanta which ushered in the age of quantum physics. Planck, shown in this undated photo in “middle age,” was 42 years old when he presented his famous paper to the German Physical Society.

INSIDE

Editor’s Note 2

Reports 3

Forum News 13

APS and AIP News 14

Notes and Announcements 15

Book Reviews 17

- Their Day in the Sun: Women of the Manhattan Project*, Ruth H. Howes and Caroline L. Herzenberg
- The Philosopher’s Tree: A Selection of Michael Faraday’s Writings*, Peter Day
- Heavy Water and the Wartime Race for Nuclear Energy*, Per F. Dahl
- The Science of Energy. A Cultural History of Energy Physics in Victorian Britain*, Crosbie Smith
- American Astronomical Society Centennial Issue of the Astrophysical Journal*, Helmut A. Abt, ed.
- From Galaxies to Turbines: Science, Technology and the Parsons Family*, W. Garrett Scaife
- Black Holes, Wormholes, and Time Machines*, J.S. Al-Khalili

historical research, writing, and lecturing for classes, for APS meetings, and for publication, including articles in *Physics Today*, *Reviews of Modern Physics*, and *American Journal of Physics*.”

Some functions of the new division could be:

- providing communications between members through meetings and mailings
- organizing history sessions at APS meetings
- support for the AIP Center for History of Physics (established in 1965)
- liaison between the APS division and the history of science sections of AAPT, ACS, AAS, AAAS, and the History of Science Society.

[I remark parenthetically that all the stated objectives have been enthusiastically pursued, except the last one. However, the Executive Committee has been actively discussing a recent proposal by Abner Shimony that we request of APS that some form of associate membership in the Forum be offered to interested members of the above societies.]

The petition, with about 200 signatures was presented by Council member Gertrude Scharff-Goldhaber to the APS Council on November 3, 1979,

and APS President Lewis Branscomb appointed an *ad hoc* committee, chaired by Albert Wattenberg, to make a report to the next Council meeting. Among the issues the committee was asked to consider was the question of whether a division or a forum would be more appropriate. The committee favored a division, arguing that this would encourage higher standards of scholarship. The other divisions were polled by the APS Council for their views. Finally, an Organizing Committee, which included the original proposers and the Wattenberg committee, prepared and submitted a set of bylaws for a new division in August 1980. The members were L.M. Brown, S.G. Brush, M. Dresden, W.A. Fowler, G. Holton, G. Scharff-Goldhaber, R. Stuewer, K.C. Wali, and A. Wattenberg. The Inaugural Session of the division in April 1981 provided an opportunity for attendees to enroll as members and was the first of many well-attended sessions of the Division at APS meetings.

In this space it is not possible to do justice to the breadth and excellence of the Division’s sessions, and I will just mention a few illustrative titles: “Mechanics, Relativity, and the Rise of Theoretical

Physics” (1982); “Einstein: From Special Relativity to the Unified Field” (1984); “The Life and Legacy of Robert Oppenheimer” (1986); “History of Semiconductor Physics” (1988). In 1992, the Division became the present Forum on the History of Physics, reflecting the broad interest in history on the part of APS members whose primary attachment is to other specialties. Some recent sessions have been: “Science Advising in the Government” (1994); “Topics in the History of Radioactivity” (1996); “Science and Its Critics” (1998). All readers of this *Newsletter* will know about the fine sessions organized by the Forum at the APS Centennial Meeting in Atlanta last year.

As we move forward in this new century, the Forum will continue to provide enlightenment and pleasure with its history sessions at meetings and its *Newsletter*. The Executive Committee is also working on some other items, such as the possible associate membership referred to above and an award to be given for outstanding lifetime contributions to the history of physics. Please let us know if you have any suggestions for those two initiatives or any others.

EDITOR’S NOTE: *The Impact of History on Teaching and Doing Physics* - I have just finished teaching a summer course in modern physics. History of physics is important in this course for several reasons: it increases student motivation; it brings home the reality of the principles by giving attention to the problems and alternative interpretations encountered in their discovery; it inserts an element of humanity into the course that students can relate to and that helps students see in themselves the possibility of making contributions to science; it gives models for probing the nature of the physical world. In my experience, history of physics, especially when not oversimplified or idealized, enhances teaching of many courses for these and additional reasons. Some of these same issues apply to doing research in physics and to communicating science to the public. I invite readers to share particular experiences you have had in which the history of physics has helped to make a course effective, has given insight into a possible experimental method or research approach, thereby enhancing a particular research effort, or has contributed to communicating science to the public. Please be specific so that these uses of history might be shared with colleagues in this *Newsletter*. (And continue to send brief notes about your current history of physics activities.)

Anniversaries for 2001

400th anniversary of the death of Tycho Brahe (b. 1546). Brahe was succeeded as court astronomer to Emperor Rudolph II by Johannes Kepler.

250th anniversary of Benjamin Franklin’s kite experiment to prove that lightning is electricity.

200th anniversary of Giuseppe Piazzi’s discovery of Ceres, the first known asteroid; Thomas Young’s paper on light as a wave, citing interference and diffraction experiments; Johann Wilhelm Ritter’s discovery of ultraviolet radiation; William Henry’s discovery of Henry’s law, that the mass of gas dissolved in a liquid at equilibrium and constant temperature is proportional to the pressure of the gas.

150th anniversary of Jean-Bernard-Léon Foucault’s pendulum experiment demonstrating the rotation of the earth; George Gabriel Stokes’s introduction of his formula for the motion of a small body through a fluid.

100th anniversary of Annie Jump Cannon’s introduction of spectral subclasses into the Harvard Classification System for stars; Pierre Curie’s measurement of the heat emitted by radium; the founding of the U. S. National Bureau of Standards; Guglielmo Marconi’s broadcast of radio waves from England to Newfoundland.

50th anniversary of Dirk Brouwer’s pioneering use of the computer to calculate planetary orbits; William Wilson Morgan’s demonstration that the Milky Way galaxy has a spiral structure; Harold I. Ewen and Edward M. Purcell’s discovery of radio emissions from hydrogen clouds in interstellar space; John Bardeen’s initial quantum theory of superconductivity; Remington Rand’s introduction of the Univac computer for business use; the first generation of electricity from nuclear power at an experimental reactor in Idaho.

Send me your list of those I have missed here, and I will add to this list in the February *Newsletter*.

(Boxed publication notice)

The *History of Physics Newsletter* is published twice each year by the Forum on History of Physics of the American Physical Society. It is distributed free to all members of the Forum. Others who wish to receive it should make a donation to the Forum of \$5 per year (+\$3 additional for air mail). Each volume consists of six issues. Editor: William E. Evenson, Department of Physics, Brigham Young University, Provo, UT 84602, e-mail: evenson@byu.edu, tel: 801 378-6078.

REPORTS

History of Physics Project Notes

Albert A. Bartlett reports that in 1996 he and **Jack Kraushaar** completed a 400 page history of the Department of Physics of the University of Colorado at Boulder, covering from the opening of classes on September 5, 1877 up to 1996. The Department had about 100 copies printed, and the supply is running low. In anticipation of a new printing, they are updating the volume with events that have taken place since 1996 and minor corrections. They will also update the tables in the appendices that list all of the faculty of the Department, all of the Ph.D.s and the honors and awards that have come to faculty in these four years.

Bert E. Brown, University of Puget Sound, shared two articles from the Washington State University Physics Department's annual newsletter, *Physics Matters*. These articles of reminiscences are by the late Al Butler, regarding war radar work at WSU, and by Ed Donaldson, about Chester Calbick and research on electron diffraction from surfaces at Bell Labs. Brown suggests that retired physicists record their experiences for historians – depositing significant materials at the AIP Niels Bohr Library would make them available.

Louis Brown, Carnegie Institution of Washington, sent information about his research that led to the book, *A Radar History of World War II: Technical and Military Imperatives*. A review of the book will be found in the next issue of *History of Physics Newsletter*.

David Topper, University of Winnipeg, offered the following note about recent research: Newton's diagram of projectiles fired from a mountain has been reproduced in countless textbooks and scientific articles. Surprisingly, however, there has never been a detailed analysis of this famous picture – until now. David Topper and Dwight E. Vincent, "An Analysis of Newton's Projectile Diagram," *European Journal of Physics* **20**:59-66 (1999), presents a mathematical, graphical, and historical study of what Newton was thinking and doing when he drew the first imaginary picture of an artificial satellite. The paper has generated some controversy as reflected in the exchange, Michael Nauenberg, "Comment on, 'An Analysis of Newton's Projectile Diagram'," *European Journal of Physics*, **21**:L5-L6 (2000) and D. Topper & D. E. Vincent, "Reply to Comment on, 'An Analysis of Newton's Projectile Diagram'," *European Journal of Physics*, **21**:L7-L8 (2000). Additional materials, not included in the publications, are available from David Topper, History, University of Winnipeg, Winnipeg, MB, R3B 2E9, Canada (Topper@UWinnipeg.ca).

Topper and Vincent have also studied the historical context of several photos of Einstein in front of a blackboard at the Mt. Wilson Observatory in 1931, as well as the meaning of the equation on the blackboard. This work is reported in David Topper and Dwight Vincent, "Posing Einstein's Question: Questioning Einstein's Pose," *The Physics Teacher*, **38**:278-288 (May,2000), 278-288.

Twenty Years of the Quantum Hall Effect. APS March Meeting, Minneapolis, 22 March 2000. By Richard E. Prange (University of Maryland)

The hall was overflowing for the FHP symposium "Twenty Years of the Quantum Hall Effect" in Minneapolis, and the talks received prolonged applause. The symposium followed the address of Chauncey Starr, who received the George Pake prize. It was a happy choice to combine the symposium with his lecture which was much concerned with the history of nuclear power.

In the quantum Hall effect (QHE) symposium, as might be expected of speakers still very active in research, considerable time was devoted to current and unresolved issues as well as 'history'. No doubt the audience appreciated this, and it contributed to the success of the symposium. The talks, particularly the first three, showed many diagrams and plots of experimental results, as well as photographs of people, equipment, pages of laboratory notebooks, referee reports, and so on. These of course are an important part of the history as well as essential to the understanding of what has become quite a large subject. In this extremely short review, however, the visual material cannot be reproduced. I must even assume that the reader knows, more or less, what the QHE is. At best, I will remind you of some of the terminology. Also, it is impossible to attribute important results completely and accurately, so resort will be made to lists of people mentioned in the talks.

The QHE is the outcome of earlier great discoveries, including, of course, the usual Hall effect, now about 125 years old. In addition, the discovery or invention or development of *two-dimensional* electron gases was essential to the QHE.

The first speaker, **Alan Fowler** of IBM, recalled some of the main parts of this story, in which he was one of the chief players. J. Robert Schrieffer in 1956, as a graduate student before his great successes in superconductivity, first theoretically proposed that at low temperature and not too high gate voltage, the electrons in a sufficiently ideal MOSFET, metal-oxide-semiconductor-field-effect-transistor, could become two dimensional. Field effect transistors and MOSFETs had been around for a while, although they were far from ideal in 1956. Their development into a great technology was on the verge of taking off. A MOSFET is typically single crystal silicon, doped p-type, covered with a thin insulating SiO₂ layer, and a metallic Al layer, the 'gate'. A gate voltage attracts electrons to the Si-SiO₂ interface. Sufficient gate voltage bends the bands so that the usually empty conduction band falls below the Fermi level near the surface, and the electrons are held to the interface by a more or less triangular potential well. The motion of these electrons is quantized perpendicular to the surface, 'in the z direction', but can move along the surface 'in the x, y plane', so the electrons are really two dimensional in that they are quantally frozen in the direction normal to the surface.

Many basic questions needed to be answered to realize the two-dimensional electron gas in a MOSFET and to understand it. Fundamental parameters, e.g. the thickness of the electronic layer (a couple of nm), the typical areal density ($N_s \approx 10^{12} \text{ cm}^{-2}$), were calculated and measured. In 1966, Fowler, Fang, Howard, and Stiles established that motion was indeed two dimensional.

Another major issue was electron scattering. High mobilities were desired and mobilities of order $10^3 \text{ cm}^2/\text{Vs}$ were achieved. At high temperatures above 200 K, phonons, both acoustic and optical, dominated the scattering. At low temperature, in low fields, charges in the oxide were important. In strong fields, interface roughness was interesting. Plasmons were taken into account. Multi-subband effects, nonparabolicity, anisotropy, and the several valley character of the Si band structure was eventually included by the mid-70's.

The electron effective mass was another important issue. It is measured in the temperature dependence of the oscillatory magnetoconductance. In general, the mass due to e-e effects diminished with increasing N_s .

In the late 70's, the issue of Anderson localization due to imperfections in such two-dimensional systems came to the forefront. This was at lower carrier density ($N_s \approx 10^{10-11}$). Activated conductance and variable range hopping were observed. There was much activity on the part of the theorists at this time also. Even now there is considerable interest in issues such as a possible zero temperature phase

transition as a function of N_s .

A good reference for most of this material is the article by Ando, Fowler, and Stern, *Reviews of Modern Physics* **54**(2) 437-671 (1982). Some other researchers mentioned in the talk were Ezawa, Ferry, Fischetti et al., Kawajii, Ning, Sah, Gold, Das Sarma, Prange, Matsumoto, Uemura, Kwok, Goodnick, Hartstein, Laux, Ngai, Economou, Dahl, Sham, Quinn, Lakhani, Smith, Abstreiter et al., Lee et al., Ohkawa, Pan, Tsui, Lozovik, Yuowian, Pollitt, Pepper, Adkins, Licciardello, Thouless, Abrahams et al., Bishop, Dynes, Uren et al., Simmons, Kravchenko, Timp, Butcher, Van Wees, Glazman, Shekter, Englert and von Klitzing.

The second speaker, **Klaus von Klitzing**, discovered the quantum Hall effect in 1980. In his talk, he pointed out that many experiments (some of them recounted by the first speaker) had been done on two dimensional systems in a magnetic field previous to his discovery. A number of experimenters, including himself, had done experiments as early as 1973 which in retrospect indicated a quantized Hall resistance. Ando, Matsumoto and Uemura in a well-known theory paper in 1975 (correctly) predicted that the Hall conductivity, σ_{xy} , will take the value $\sigma_{xy} = (N + 1) e^2 / h$, if the Fermi level is in an energy gap between the Landau level N and $N + 1$. In 1977, von Klitzing's student, Thomas Englert, presented results at the EP2DS-2 in Berchtesgaden (this conference on electronic properties of two-dimensional systems started about 1970 and is still regularly held) which showed that σ_{xy} was within 0.5% of $4 e^2 / h$ for a significant interval of gate voltage. Somehow, it didn't occur to anyone that these results were truly extraordinary. Some distracting factors were that the theory was good, but did not pretend to be exact; localization effects were not well understood; and electron interaction effects were treated in mean field theory. Most interest was focussed on the peaks of σ_{xx} which is obtained from the longitudinal conductance by $\sigma_{xx} = G \times L/W$, where L/W is the length to width ratio of the particular sample. This ratio can be determined to a percent or so at best.

Early in the morning of February 5, 1980, von Klitzing realized that the Hall resistance $R_H = \rho_{xy}$ (which is not afflicted by sample geometry uncertainties) was quantized according to $R_H = h / ie^2$ to very high accuracy, where i is an integer. [Recall a bit some of the details. There are peaks in the longitudinal resistance R when the magnetic field is such that a Landau level is partially filled. When the field is such that the chemical potential lies between Landau levels, R is very small, and the Hall resistance is the inverse of the Hall conductance $\sigma_{xy} = 1 / R_H$. The field is often replaced by the "filling factor" ν , the number of Landau levels filled at the particular field and gate voltage. As ν is varied, there are 'plateaus' or steps at the quantized values of R_H .] At the symposium, von Klitzing showed publicly for the first time pages of his notebook written on that 'birthday of the QHE'. Other records of the next weeks were shown: e.g. pictures of the equipment, not neglecting a view of a baguette and cheese, together with a bottle of Côtés du Rhône, as befits an experiment done in Grenoble at the High Field Laboratory of the Max-Planck Institute. von Klitzing shortly returned to Würzburg, where he improved the experiment in several ways, for example enclosing it in a screened room which he made himself. He obtained a value for e^2 / h to better than a part in a million at that time.

He then submitted a paper, coauthored with M. Pepper and G. Dorda, to *Physical Review Letters*, entitled "Realization of a resistance standard based on fundamental constants". It was rejected. The referee said '... if .. in fact the theory is correct ... that ... the Hall resistance is given by $h / e^2 i$... then their discovery is potentially quite exciting ... they may have really discovered a new way to determine the fine structure constant α to high accuracy.' However, the referee was really quite friendly and in the end, the title of the paper was changed to "... Determination of the Fine-Structure Constant Based on Quantized Hall Resistance".

Even so, it took a few months for the importance of the discovery to sink in. For example, von Klitzing was scheduled to give a talk at the International Conference on the Physics of Semiconductors in Kyoto, September 1980. He requested to change his talk to report his new discovery. However, this was not agreed to by the authorities. The most interesting objection was that the subject was not appropriate for a conference on the 'physics of semiconductors'! [von Klitzing did nevertheless succeed in conveying his results at this conference. At any rate, I learned of it from a colleague who attended the conference.]

After that, many theorists and experimentalists were quickly attracted to the subject. In 1985, von Klitzing was given the Nobel prize for his work.

In his lecture, von Klitzing explained some of the metrology issues. For example, the most accurate realization of the resistance standard before 1990 actually measured the capacitance of a specially designed Thomson-Lampard capacitor where the crucial measurement is of a length. Various national laboratories maintained resistance standards by wire resistors which, however, drifted over time by parts per million.

By 1988, in a committee report adopted internationally in 1990, it was agreed that the QHE is the most stable resistor with a fixed (but unknown) value. It is experimentally reproducible to about 4 parts in 10^9 which is better than the realization of the resistance unit 1 Ohm within the international system of units (SI units). Since then, the realization of the Ohm is based on the Hall resistance in the $i = 1$ plateau with a fixed value $R_{K-90} = 25,812.807 \Omega$ (the conventional value of the *von Klitzing constant*). At the same time a conventional value for the Josephson constant was fixed which allows the realization of a voltage standard with high reproducibility and stability.

The third speaker, **Horst Störmer**, started his lecture by showing a picture of an MBE machine. Such 'molecular beam epitaxy' machines are used, with great care and artistry, to grow crystalline structures layer by layer, a superlattice. The main motivation for the original work at Bell Laboratories was to make optical devices to be used in future computers. The best samples were basically alternate layers of GaAs and AlGaAs of controlled thicknesses.

However, the electronic properties, for a wide GaAs layer, were similar in many ways to the MOSFET. An important advance was made by Störmer, who put the dopant donor atoms in the AlGaAs, well away from the GaAs. The process is called modulation doping. The result is much higher mobilities than in the silicon devices, above $\mu \approx 10^6 \text{ cm}^2/\text{Vs}$ in 1982, above 10^7 today.

After von Klitzing's discovery, Dan Tsui, and Störmer set out to measure the QHE in high mobility samples grown by Art Gossard. By late 1981 they succeeded in finding very low longitudinal resistances when the Hall resistance was quantized. Except for superconductors, these resistances were smaller, by an order of magnitude, than any other resistance measured before that time.

Then in early 1982, they measured, at filling factor $\nu=1/3$, the Hall resistance R_H to be $3 h / e^2$!!! In other words, rather than being inversely proportional to an integer, R_H is inversely and accurately proportional to the fraction $1/3$ when the lowest Landau level is about $1/3$ filled. This was the first measurement of the fractional QHE, while von Klitzing's effect is now known as the integer QHE. They also saw indications of a plateau at $\nu=2/3$.

Unlike the IQHE, there were no theoretical precursors of the FQHE; it came as a complete surprise. In fact, by this time, theorists had given the main ingredients for the understanding of the IQHE, and they predicted integers and not fractions. Bob Laughlin, in particular, gave an elegant argument in which he imagined a quantum Hall annulus threaded by an Aharonov-Bohm flux line. This flux

was increased adiabatically by one flux quantum. He showed that it should be expected that an integer number of charges e are transferred from outside to inside of the annulus by this process, and so obtained the result of the IQHE. This was satisfying as it led to the idea that the QHE was a topological quantum number, and thus independent of many details and perturbative corrections.

Tsui, et al. pointed out in their paper that observation of the fractional effect together with Laughlin's argument then implied that quasiparticles of charge $1/3$ exist. [Störmer reported that as the first experimental traces were being observed, Tsui pointed to the developing plateau and said "charge one-third"!] They initially suggested these quasiparticles could be indicative of a triangular Wigner lattice or charge density wave. This idea is not now believed to be correct, but Wigner lattices and charge density waves are still around in QHE physics. Shortly thereafter, Störmer et al. showed that fractions $3/5$ and $2/5$ appeared as well; then in 1988 came $1/7$, $1/5$, $2/9$, $3/11$, ... a list that now fills an entire transparency.

Relatively quickly, winning a race against intense theoretical competition, Bob Laughlin produced his famous many-electron wavefunction which is the basis for much of the later theory in the subject. Störmer had a wonderful composite cartoon-photo of a very happy Bob Laughlin in a sweatshirt pointing to the formula

$$\Psi_{1/3} = \prod_{i < j}^n (z_i - z_j)^3 \exp \left[-\frac{1}{4} \sum_k^n |z_k|^2 \right]$$

emblazoned on his chest. One of the main features of this theory is that the electron state is 'incompressible'; there is an energy gap that must be overcome to change its density.

Dan Tsui, Horst Störmer and Bob Laughlin received the 1998 Nobel Prize for this discovery. They spoke at the Centennial Meeting of the APS in Atlanta. Unfortunately, for health reasons, Dan Tsui did not attend that meeting or this symposium, although he is nevertheless extremely active in research.

In his lecture, Störmer showed many impressive more recent results of many types, which are difficult to reproduce without benefit of figures. Lacking these visual aids, I will not attempt to recount all he regaled us with.

A list of people credited by Störmer follows: Dan Tsui, Kirk Baldwin, Peter Berglund, Greg Boebinger, Albert Chang, Rui Du, Jim Eisenstein, Hong-Wen Jiang, Woowon Kang, Wei Pan, Bob Willett, Andrew Yeh, Al Cho, John English, Art Gossard, Jim Hwang, Loren Pfeiffer, Mansosur Shayegan, Charles Tu, Gunther Weimann, Ken West, Willy Wiegmann.

von Klitzing and Störmer both succeeded very well in conveying to the audience the rush of excitement and satisfaction that comes when a great scientific discovery is made.

The final talk was by **Bert Halperin**, who set out to give some idea of the range of developments following the QHE discoveries, some idea of the time frames, and the connections between the several ideas, as well as a more detailed look at a few examples.

He gave the following list of some of the main phenomena and issues:

- The FQHE is observed at many fractions with ODD denominators.
- It is also seen at some fractions with EVEN denominators
 - Single layer systems at $\nu = 5/2, 7/2$ (1987)
 - Bilayers with $\nu_{total} = 1/2$
- Unquantized QHE: ("compressible states"). What happens at fractions like $\nu = 1/2$ in single layer systems where quantized Hall plateaus are NOT observed?
 - Surface acoustic wave anomaly was seen at $\nu = 1/2$ (1990)
- Peculiar phases and phase transitions in bilayer systems (1992+)
- Vertical transport in coupled multilayers (1997)
- Applications to organic conductors and other quasi 1 and 2-dimensional compounds
- Phenomena involving the electron spin
 - Transitions between states of different spin polarization (1987+)
 - Spin depolarization near $\nu = 1$ (1995)
 - Coupling to nuclear spins
- Insulating state in very strong B (small ν)
 - (1988+) Wigner crystal (?) in high mobility sample
- Transitions from quantized Hall state to insulator when disorder is important
- Large resistive anisotropy observed in higher Landau levels (1998+) ($\nu = 9/2, 11/2, 13/2, 15/2 \dots$)
 - Charge density wave phase?

He also presented lists of important measurement techniques other than transport and of concepts and calculational techniques. Unfortunately, we do not have room here for these lists, but **the full listing can be found at www.aps.org/FHP/news.html as a web supplement to this Newsletter.**

Halperin then discussed a particular set of ideas in a bit more detail. [He himself contributed greatly to this subject.] These ideas have great theoretical appeal and make extremely surprising predictions, which are largely confirmed by experiment. These ideas have the name composite fermions and bosons and Chern-Simons gauge theories. The origin was a statistical transmutation and fractional statistics in 2D systems, due to Leinaas and Myrheim (1977), Goldin et al. (1980) and Wilczek (1982). The latter introduced the term "anyons". This is a mathematical transformation that can represent 2-D fermions, bosons, or anyons as bosons or fermions (at will) attached to a fictitious "Chern-Simons" gauge field. 'Fractional statistics' was applied by Halperin (1984) as a natural representation for quasiparticles in the FQHE. Girvin and MacDonald (1987) transformed *electrons* in the QHE state in a *strong* magnetic field, to *bosons* in *zero* field, and thus Bose-Einstein superfluidity. This led to a remarkable Ginzburg-Landau theory of the QHE with a 'non-local' order parameter.

In 1989, Jain introduced the composite fermion picture in the form of a trial wave function for fractional quantized Hall states, which very naturally represented the prominent observed plateaus. This was shortly cast in fermions+Chern-Simons form by Moore and Read, Greiter and Wilczek, and Lopez and Fradkin. This in turn was used by Halperin, Lee and Read, and by Kalmeyer and Zhang in 1992 for the *unquantized* (compressible) states at $\nu = 1/2$. Thus the startling prediction was made that these electrons in a very strong field should behave very much like fermions in *zero* magnetic field! Moreover, the *mass* of these fermions has nothing to do with the

electron mass, but is purely electromagnetic in origin.

Störmer had given in his talk a wonderful intuitive picture of the composite particles, but we cannot, unfortunately, reproduce his cartoons. A composite particle is an electron plus m quanta of Chern-Simons flux. If m is odd, the composite particle is a boson, if m is even, it is a fermion. The effective magnetic field seen (on average) by a composite particle is $B_{eff} = B - m\phi_0 N_S$ where ϕ_0 is the flux quantum and $N_S = \nu B/\phi_0$ is the two dimensional density of electrons. (Some examples are given in a **table in the web supplement** to this report.) Thus, at filling factor $1/2$ the composite quasiparticles travel in straight lines, limited by impurity scattering. For ν near $1/2$, the quasiparticles travel in circles of radius $R_c^* = \hbar c k_F / e \Delta B$, where $\Delta B = B - B_{1/2}$, i.e. the cyclotron radius in an effective field ΔB . Several experiments, including surface acoustic wave data taken by Willett et al. confirmed that there was a Fermi surface radius of the correct size. Further, the states at filling $\nu = 1/3, 1/5$, etc. can be regarded as the IQHE based on the filling factor $1/2$ fermions. Störmer showed in his lecture beautiful experimental traces confirming this picture with the basic composite fermion states at $\nu = 1/2, 1/4, 3/4, \dots$

One other class of results was discussed in a bit more detail. This involves the electron polarization in the highest occupied Landau level. This may be maximally or partially polarized, or unpolarized. In GaAs the g -factor is anomalously small, the Zeeman energy is smaller than $\hbar \omega_c$ and also less than the electron interaction energy $e^2 / \epsilon l_0$, so different spin states are possible. Further, the ratio of Zeeman to other energies can be varied experimentally by tilting the magnetic field and/or applying pressure. Predictions of Sondhi, et al. (1993) say that, near $f = 1$ and for small g , the lowest energy charged excitations are “Skyrmions”, i.e. spin textures with many overturned spins and electric charge $\pm e$. Thus, the spin polarization drops rapidly as f moves away from 1 due to formation of Skyrmions. This was observed by Barrett, et al. (using NMR), B. Goldberg et al. (using optical techniques), and Eisenstein et al. (transport data). The Princeton group, (Tsui et al.) observed a *huge* increase in specific heat due to the nuclear spin contribution, since the nuclei can equilibrate only when Skyrmions are present.

Finally, Halperin presented a list of phenomena only partially understood.

- Values of energy gaps and composite fermion effective masses as obtained from transport measurements
- Detailed nature of transitions from quantized Hall states to insulator in very strong fields, and transitions from one quantized Hall plateau to another when impurities are important
- Phenomena when spins are not maximally polarized
- Tunneling into the edge of a quantum Hall system
- Drag measurements in bilayer systems
- Large resistance anisotropy when Fermi level is in the middle of a ‘higher’ Landau level, e.g. ν near $9/2, 11/2, 13/2, 15/2$.

A list given by Störmer, ‘with apologies...’, because he didn’t discuss them, found in the **web supplement** this report, makes an interesting comparison to the above list of unfinished business.

From this report, it can be seen how incredibly seminal von Klitzing’s discovery was, and even more so, the developments beginning with Schrieffer’s idea in 1956, which include much beside the QHE. As far as I can tell, no historian has tried to set down the facts in proper fashion. While it is true that there is a good deal of turmoil in the field now, as is true of any extremely active subject of scientific research, there is much that is very well established, even by historical standards. And, most of the leading participants in these discoveries are still around, and still active. I therefore end this report by encouraging some historian to step forward and take up a very important task, that of recording the history of the quantum Hall effect.

New Perspectives on the Development of Ancient Astronomy. APS April Meeting, Long Beach, 1 May 2000. By Michael Nauenberg (UC-Santa Cruz) and the speakers

This symposium, with three outstanding speakers, was presented to a substantial and appreciative audience at the Long Beach APS meeting. It was organized and chaired by Michael Nauenberg (UC-Santa Cruz). The first speaker, **James Evans** (University of Puget Sound), addressed the topic, “*The First Astrophysical Synthesis: What Ptolemy Did and Why It Mattered*”. The traditional history of Greek astronomy is based on three tenets: (1) It all started in the fourth century BC when Plato gave the astronomers a homework assignment: go save the phenomena in terms of uniform circular motion (philosophy). (2) The goals and methods of Greek astronomy remained the same until Ptolemy’s day (continuity). (3) the Greek astronomers sought only to calculate accurate planetary positions and did not claim that their models represented the real world (instrumentalism).

This interpretation of Greek astronomy was advocated by Pierre Duhem in his influential book, *To Save the Phenomena*. However, a far-reaching revision of the history of Greek astronomy is now under way, and all these three tenets of the traditional account stand in need of correction. One reason that Duhem overestimated the role of Plato is that he relied too much on late philosophical writers, such as Proclus and Simplicius, who reconstructed the history of astronomy out of their heads, in keeping with the neoplatonic philosophy of nature to which they subscribed. And in reading the Greeks as instrumentalists, Duhem made them heroes of positivism, imposing upon them his own nineteenth-century style of physical research. G.E.R. Lloyd has shown that in making this reading Duhem misinterpreted his ancient authorities. Moreover, Wilbur Knorr has shown that the attribution to Plato of a principle of uniform circular motion was a mistake by the late antique writers.

Early Greek astronomy did, of course, have important links to philosophy. Indeed, the primary concern was to understand the world in terms of accepted physical principles. But we should not imagine the planetary theories of Eudoxus and Apollonius as attempts to save the phenomena in a quantitative sense, for there was no tradition of number-crunching among Greek astronomers at this time. Moreover, there were no social institutions for storing observations. We should regard Eudoxus’s system of homocentric spheres as a physical metaphor: the world might work something like this. And, of course, it provided a field of play for a talented geometer.

In contemporary Mesopotamia, the situation was almost exactly the reverse. There a civil service, consisting of the scribes in the temples, was charged with regular observation of the sky and with recording and preserving the results, because celestial events had ominous significance for the kingdom. By 300 BC the scribes developed a planetary theory that permitted prediction of the times and places of important planetary phenomena, such as the onset of retrograde motion. However, these methods were based not on geometrical models, but on arithmetic procedures.

Greek astronomers came into contact with Babylonian astronomy in the third and second centuries BC, and they must have found it astonishing. For the Babylonians could do what no Greek could do. But the lack of a geometrical basis and the absence of any Babylonian equivalent of Aristotle must have been puzzling. In Greek Egypt in the second century AD, two kinds of astronomy still existed side by side. If you were steeped in the physics of Aristotle and the geometry of Euclid, you couldn’t understand how the world

worked unless you thought in terms of deferents and epicycles. But if you were a practicing astrologer, who needed to calculate planetary positions, you had to fall back on arithmetical procedures, since the geometrical planetary theory had no quantitative power. The adept use of Babylonian methods by Greeks in Egypt is well illustrated by the astronomical materials among the *Oxyrhynchus Papyri*, recently published by Alexander Jones. This evidence is especially telling because it comes to us right out of the ground, without having passed through the hands of medieval copyists.

The challenge faced by Claudius Ptolemy in the second century AD was to endow the geometrical planetary theory with quantitative predictive power. Ptolemy introduced an essential new idea into planetary theory – the equant point. This feature of his theory (which corresponds roughly to the empty focus of Keplerian theory) allowed the planets to travel nonuniformly. It made possible, for the first time, the accurate calculation of planet positions from a geometrical theory. The planetary theory of the *Almagest*, including Ptolemy's design of tables, stands out as something quite different from the planetary schemes of his contemporaries. In a sense, his achievement represented a merging of the Greek and Babylonian traditions. It is where our science began.

George Saliba (Columbia University) then spoke on "*Objections to Greek Astronomy in Islamic times and the relationship to the work of Copernicus*". In this talk an attempt was made to identify the main problem with the Greek astronomical legacy as it was perceived by astronomers working within the Islamic civilization and writing in Arabic between the ninth and the sixteenth centuries. Starting with the illustration of the kind of physical spheres the Greek texts of Ptolemy (fl. 150AD) had envisaged, it was pointed out that the mathematical models that were used by Ptolemy to describe the behavior of those spheres was fundamentally flawed in that it implied a contradiction between the physical properties of those spheres and the manner in which their motions were described mathematically. From that perspective, the most outstanding problem that permeated the whole of Ptolemaic astronomy implied the uniform rotation of a sphere around an axis that did not pass through its center. This very problem was later identified by astronomers working in the Islamic civilization as the equant problem and was also identified for the same purposes by Copernicus (d. 1543) as well in the introduction to his *Commentariolus*, which was written around 1510-1515.

Efforts to resolve this problem began in earnest in the Islamic civilization sometime around the middle of the thirteenth century. Astronomer after astronomer attempted to devise non-Ptolemaic mathematical models that would still describe the motions of the celestial spheres in accordance with observations but would at the same time remain consistent with the physical properties of those spheres. In these attempts of model construction two astronomers in particular, Mu'ayyad al-Din al-Urdi (d. 1266) and Nasir al-Din al-Tusi (d. 1274), found themselves obliged to devise two new mathematical theorems, that were not known in the earlier Greek tradition and are now known in the literature as the Tusi Couple and the Urdi Lemma, that would serve this purpose. This tradition continued with astronomers working in later centuries. One in particular, working from the central mosque of Damascus in the fourteenth century, by the name of Ibn al-Shatir (d. 1375), made use of these earlier mathematical theorems and went on to devise a new set of models of his own that would meet the same criteria of consistency between the physical properties of the celestial spheres and the mathematical description of their behavior. As it turned out, research conducted in the history of Arabic astronomy after 1957 has managed to demonstrate that the very same theorems of Tusi and Urdi as well as the model for the moon by Ibn al-Shatir were also used by Copernicus to construct his own alternative astronomy as exposed first in the *Commentariolus* and then in the *De Revolutionibus* which was published in 1543.

Naturally, a question was raised regarding the possibility of transmission of those theorems and models from the Islamic culture to Copernicus and the routes such a transmission could have followed. In the mid seventies, Otto Neugebauer managed to locate a Byzantine Greek manuscript, which was apparently written towards the beginning of the fourteenth century and came to Italy after the fall of Constantinople in 1453 – not yet published and still located at the Vatican Library as Gr. 211 – which contained Greek translations of Arabic and Persian astronomical texts that contained at least one of those theorems, notably the Tusi Couple. With this evidence of a possible direct route of transmission, Neugebauer later concluded in his joint work with Noel Swerdlow, *Mathematical Astronomy in Copernicus's De Revolutionibus*, that those earlier mathematical theorems and models already known in the Islamic world since the thirteenth century must have become widely known in Italy towards the beginning of the sixteenth century when Copernicus was studying for his university degrees in that country.

In an attempt to investigate the situation in Italy during the sixteenth century, and the routes through which such material could have passed from the Islamic world to Italy, this talk documented yet another route of transmission, namely, that through Arabic manuscripts that were sought from the lands of Islam which were heavily studied by contemporaries of Copernicus who were themselves competent in both the linguistic domain as well as the domain of mathematical astronomy. Pages from two such manuscripts were discussed in this talk, where it was demonstrated not only that Renaissance scientists read those Arabic manuscripts that contained advanced theoretical astronomical material, but also that they were heavily annotated on the margin with Latin identifying and explanatory remarks.

The talk concluded with an exposition of further developments in Islamic astronomy that were specifically directed against the Greek astronomical tradition, which went as far as raising the issue of mathematical modeling in describing astronomical phenomena. Furthermore, this new evidence of the continuity between what was taking place in Islamic astronomy and that of Renaissance Italy highlighted the need to re-examine in much greater detail the background of other Renaissance scientists in fields other than astronomy.

Owen Gingerich (Harvard-Smithsonian Center for Astrophysics) gave the concluding talk, on the topic "*Copernicus and the Aesthetic Impulse*". In the post-Newtonian cosmos, with its universal gravitation, the Copernican system seems so inevitably right that it is hard for most modern scientists to comprehend why it took so long for people to accept the obvious. Were the academics so steeped in tradition that they just refused to use their eyes? Were the clerics and universities part of a conspiracy of thought control?

Let me remind you of what Galileo said nearly a century later, when the matter was still far from settled: "I cannot admire enough those who accepted the heliocentric doctrine despite the evidence of their senses." What I am going to argue is that Copernicus relied on aesthetic principles, "ideas pleasing to the mind," and that such concepts are exceedingly powerful but highly treacherous in physical reasoning. Until technology marches on to provide empirical grounding, the aesthetic ideas must be regarded as dangerously seductive, possibly sheer quicksand for the unwary. I'll describe two aesthetic principles that Copernicus endorsed, and I'll show how our modern evaluation essentially turns upside-down the initial reception of Copernicus' *De Revolutionibus*, his life work that was finally published in the year of his death, 1543.

What Copernicus had to offer were two quite independent aesthetic ideas. One was that celestial motions should be described in terms of uniform circular motions, or combinations thereof. The unending, repeating motion in a circle was compellingly suitable for the

heavenly movements, where corruption and decay were never found. There was something almost sacred about this proposal, and it appealed strongly to the sensitivities of the sixteenth century. Unfortunately this beautiful idea was wrong, dead wrong. It was not dumb – it was in fact the most intelligent way to start approximating the motions of the heavens, but in Renaissance celestial mechanics it was destined to be a dead end.

Copernicus' other aesthetic idea, which in *De Revolutionibus* is so intimately tangled up in the first idea, is in fact quite independent of the aesthetic requirement of circular and uniform motion. It is the great idea that makes copies of the first edition of *De Revolutionibus* nowadays estimated at auction at over half a million dollars. This other great aesthetic idea was, of course, the heliocentric arrangement of the planets. But to the sixteenth-century mind, this idea was highly suspect. To begin with, it required new physics. Building a new scaffolding to replace the neatly dove-tailed Aristotelian physics would require more than a generation of inspired work. As Tycho Brahe said, "The Copernican doctrine nowhere offends the principles of mathematics" – that is, aesthetic idea number one is just fine – "but it throws the earth, a lazy, sluggish body unfit for motion into action as swift as the aethereal torches." But it wasn't just new physics that made the new cosmology seem radical and dangerous. Tycho said that Copernicus offended both physics and the Holy Scriptures, always in that order. Biblical passages such as Psalm 104, "The Lord God laid the foundation of the earth, that it not be moved forever," seemed to call for a firmly fixed earth. Copernicus' heliocentric vision was seen as a challenge to the traditional sacred geography, and hence generated the pervasive unease touching even those who would never worry about mere physics.

Because today Copernicus' heliocentrism, his second aesthetic idea, endures, while the first – "celestial motion is uniform and circular or composed of uniform and circular parts" – has faded away into obscurity, it is easy to overlook the appeal of uniform circular motion in the 16th century.

Now aesthetic ideas can be seductively wrong, and in the absence of empirical support it is perhaps best to take a wait-and-see attitude. That's the course the overwhelming majority of 16th-century astronomers adopted. What is unusual about the Copernican revolution is that it took so very long. This leaves the writers of modern secondary sources very uneasy. What was the matter with those people? Were they dumb or something? Or were they just blinded by superstition or religious orthodoxy?

What was lacking was observational evidence to confirm or refute these ideas. Toward the end of the 16th century the idea of an empirical test of the heliocentric idea gradually occurred to a few leading astronomers including the Danish astronomer Tycho Brahe. Tycho attempted to distinguish between the Ptolemaic and Copernican systems by determining the distance to Mars, and he expended a major observational effort on it. He even built a new subterranean observatory to get better stability, and he redesigned the instruments originally built for the windy balconies of his Uraniborg castle to provide greater rigidity and accuracy. Yet in the end he fails to mention his Mars campaign, something that caused his biographers to long overlook this centrally motivating research. Why did Tycho give this major effort the silence treatment? Because, unknown to him, the solar system was 20 times larger than he or anyone else imagined, and his carefully organized research agenda was doomed to failure. Had he been successful, his new technology would have provided the empirical evidence for Copernican astronomy almost three decades earlier than actually happened, and Tycho's reputation as an observer/cosmologist would shine brilliantly in the astronomical firmament. Yet from the ashes of his failed campaign there arose, like a phoenix, the evidence that Copernicus' aesthetic principle number one had to be abandoned. The magnificently precise observations of Mars were the grist for Kepler's mill, who showed that an ellipse worked better and more simply than the circles and epicycles of Copernicus. Furthermore, it offered the prospect of serious new physics, which to Kepler made all the difference. And that physics was a heliocentric physics.

But meanwhile, the acceptance of Copernicus' second aesthetic principle, the heliocentric doctrine, was greatly hastened by an unexpected discovery, one that was critically dependent on a fresh advance of technology. In Galileo's hands, what had been a novel toy was converted into a scientific instrument. When he used the new telescope to examine Venus, he found that the planet exhibited the entire set of phases shown by the moon, guaranteeing that Venus orbited the sun, contrary to the Ptolemaic arrangement. This evidence, in the rhetorical setting of Galileo's *Dialogo*, essentially turned the tide in the favor of the Copernican heliocentric arrangement.

Why had it taken so long? There were comparatively few astronomers in those days, and the pace of invention was not as swift as it is now. Nevertheless, in early modern science we can see in slow motion what can happen in a decade or less today. But it distorts the story to demand that Copernicus's contemporaries should have been able to choose and endorse the great aesthetic idea that we know is right only by 20-20 hindsight. Instead, we should give some sympathy to those who withheld judgement until the evidence was in hand.

Summary and Comments, Michael Nauenberg: Perhaps no other subject in the history of science has had more distortions and misunderstandings than the development of ancient astronomy. For example, one of the most persistent canards in textbooks is that Ptolemy's model of planetary motion required up to 80 epicycles which prompted the search for simpler models. In contrast, the speakers at our session presented the actual historical evidence for some of the observations and problems which led to the development of planetary models. Perhaps least known are the epicycles introduced by Islamic astronomers from the 12-15th century to resolve what was perceived as fundamental problems with Ptolemy's equant. Saliba presented new evidence for the transmission of these ideas during the Renaissance, and their later adoption by Copernicus. We learned from Gingerich that Tycho Brahe's failed attempt to distinguish between the Copernican and Ptolemaic models by measuring the parallax of Mars motivated his continuing improvement of instruments, and led to the crucial data by which Kepler later discovered the true laws of planetary motion. Evans indicated that revisions of the history of Greek astronomy are currently under way, but his statement that before Ptolemy "geometrical planetary theory had no quantitative power" is surprising. In the *Almagest* Ptolemy credits Hipparchus and Apollonius of Perga with developing the basic planetary models which he improved by the introduction of the equant. He claims, however, that Hipparchus "did not even make a beginning in establishing theories for the five planets, not at least in the writings that have come down to us", although he admits that Hipparchus had shown interest in quantitative predictions by "investigating the theories of the sun and the moon". Gingerich attributed to Copernicus the "aesthetic" idea that "celestial motions should be described in terms of uniform circular motion", but as Evans pointed out, this idea was the basis for celestial motions of Greek astronomy. While the idea of compounding uniform circular motion is variously attributed to philosophical and/or aesthetic principles, I would like to suggest that in the absence of a dynamics for celestial motion (which did not appear until later in the 17th century with Kepler, Borelli, Hooke and finally in its correct form with Newton) it is the only simple alternative for periodic motion which is based entirely on geometry.

Historic First FHP Contributed Paper Session: History of 20th Century Physics. APS April Meeting, Long Beach, 1 May 2000. (Report by Bill Evenson and the speakers)

This first FHP contributed paper session consisted of four papers: “*The APS in Public Affairs*” by **Harry Lustig** (U. of New Mexico), “*The History of Nuclear India*” by **Ram Chaturvedi** (SUNY College at Cortland), “*Imaginary Forces*” by **Nathaniel P. Longley** (Macalester), and “*Long Complicated Derivations in Mathematical Physics: The Two-Dimensional Ising Model, and The Stability of Matter*” by **Martin Krieger** (USC). Summaries of the first and final papers of the session follow, as provided by the authors.

“*The Aps in Public Affairs*”, by *Harry Lustig* (University of New Mexico)

(This contribution is an abbreviated version of the talk at the FHP session in Long Beach. The talk itself consisted of edited excerpts from the author’s article, “*To Advance and diffuse the knowledge of physics: an account of the one-hundred year history of the American Physical Society*”, in the July 2000 issue of the *American Journal of Physics*.)

When Arthur Gordon Webster issued his call in 1899 for the formation of an American Physical Society, what was uppermost in his mind was to create a forum for the “interchange of ideas among American physicists and for learning of one another’s work. But, significantly for the future, the call added that although for “such a society there is small need to speak”, nevertheless “an organization like the one proposed could not fail to have an important influence in all matters affecting the interest of physicists, whether in connection with work done under Government auspices or otherwise”.

The future was not long in coming. At the meeting of February 24, 1900, the Council created a committee to “...draw up a memorial to Congress... favoring the establishment of a Bureau of Weights and Measures...”. This agency, the Bureau of Standards, was established, with the support of other scientific societies, in 1901, a speedier and more triumphant return on lobbying than the APS has enjoyed since. (The word “lobbying” was, to be sure, not admitted to the APS’s vocabulary until the mid-1990’s.) The next initiative, in 1906, urging passage of a bill “which provides for use of the Metric System in all government departments” is still pending.

To be sure, during the first fifty years of its existence APS spoke out more sparingly than since. And, significantly, for most of its existence, the Society has been leery to promote the *economic* welfare of physics, much less that of physicists. As late as 1977 the historian Daniel J. Kevles recognized and explained this reluctance in his important book *The Physicists – The History of a Scientific Community in Modern America*: “Although physicists, like other Americans, have embraced political engagement in arenas of technological policy such as arms control, they have tended to resist it on behalf of their science, fearing that it would undercut their social authority, not to mention their self-image, if they behaved like just another interest group in American society”.

The freedom of physicists to pursue their work without political constraints has however long been a concern of the APS. An important case occurred in 1953, when the Secretary of Commerce in the Eisenhower Administration tried to fire the head of the Bureau of Standards, the physicist Allen V. Astin, because the Bureau had determined that a battery additive produced by a California firm added nothing to the life of batteries. In successfully calling for Astin’s reinstatement, the Council proclaimed that “it is the duty of a scientist to investigate scientific and technical problems by openly-stated objective methods without shading its conclusions under political or other pressures... We never doubted that the work of the Bureau of Standards has been conducted in this spirit...”

In what was to be the first of a number of assertions that physics and physicists knew few national boundaries, the Council, on November 10, 1945, less than three months after V-J day, decided to treat Germans and Japanese in the same way as other foreign members whose participation had been interrupted by the war. However, with the initiation and escalation of the Cold War, not only did relations with *Soviet* scientists become all but impossible, but many in the US who were suspected, rightly or wrongly, of harboring sympathies for communism, or of being “security risks”, had their loyalties questioned and their ability to practice their professions circumscribed.

The first prominent physicist to be attacked was the eminent and outspoken Edward U. Condon. In 1948 – two years after he had served as APS President – the House Un-American Activities Committee pronounced Condon “one of the weakest links in our atomic security”. Protests erupted from the scientific community which notably included what appears to have been the first public defense by the APS of one of its members. The New York Times (March 5, 1948) stated with only slight hyperbole, that the American Physical Society, in a move “unprecedented for an organization devoted exclusively to the affairs of pure science, entered the field of politics yesterday with a letter vigorously assailing the actions of the House Un-American Activities Committee in reference to Dr. Edward U. Condon... The distinction between this message and those from other organizations lies in the fact that the American Physical Society prides itself on its aloofness from all matters except the intricacies of pure physics.” The Atomic Energy Commission cleared Condon, but he was, for the rest of his life, never able completely to overcome the effects of the accusations.

Major change came to the APS with the rise in social consciousness and radicalization in the universities in the 1960’s, in particular as a consequence of opposition to the Vietnam War and the sponsorship of physics research by the military. Although a number of those in leadership positions shared the sympathies and convictions of the activists, the Council as a whole had at first to be pushed into action by these grassroots. And there was hesitation to proceed without approval by the membership as a whole. In the wake of the violent suppression of antiwar protests at the Democratic national convention in Chicago in 1968, many members petitioned APS not to hold its scheduled 1970 meeting in that city. The majority of the Council was against cancellation, but the Council eventually ordered a special poll of the membership by mail ballot; it then affirmed the vote of the majority of the voting members against canceling the selection of Chicago.

Almost two decades earlier, during the cold war inspired restrictions on who could speak at universities, the APS had to face a decision to move a meeting for reasons of principle. The Ohio State University, on whose campus the March 1952 meeting was to be held, had instituted a requirement of screening those who would be allowed to speak on its campus. No consultation of the membership was necessary or contemplated on that occasion. Acting on a firmly established commitment to scientific freedom, and after a brief discussion by the Council, APS past president F. Wheeler Loomis wrote to the Ohio State Physics Department “that the Physical Society could under no circumstances tolerate any screening by any outside agency, of its speakers or its program”. The president of Ohio State University backed down.

In February 1969, a group of activist physicists, led by Martin Perl – who was to become a founding member of the important Forum on Physics and Society and, while serving as its second chair in 1973-74, made the Nobel prize winning discovery of the tau meson – and by Charles Schwartz, a physics professor and antiwar activist at Berkeley, urged APS to conduct sessions at its meetings on politically charged defense issues. Two months later, at the Society’s meeting in Washington, the first such session was held, before an

audience of 2000, at which Hans Bethe, Donald Brennan, George Rathjens, and Eugene Wigner debated the Nixon administration's proposed antiballistic missile program. The discussion had been billed as limited to "technical aspects of the ABM", but in fact touched on many nontechnical aspects as well. The following day, some 250 physicists held an orderly march to the White House to protest the ABM and then went on to call on members of Congress.

The sea change of attitudes at the grass roots level toward involvement with social, economic, and political issues resulted in the creation of parallel structures to pursue initiatives in these areas at the official level. A number of "public affairs and outreach" committees were activated by the Council. The earliest and, at least initially, most important and powerful of these was POPA, the Panel on Public Affairs, which was established in 1975. One of POPA's main achievements has been its studies of issues at the intersection of physics and society and the subsequent preparation of policy statements that were eventually adopted by the Council. An even more important contribution has been the recommendation and initiation of larger studies, by panels of outside experts and with external financing, of such public interest issues as the technical aspects of the more efficient use of energy, the safety of nuclear reactors, the potential of photovoltaics, and the prospects for directed energy weapons.

In the eighties APS greatly expanded its outreach programs. In education, high school teachers days were added to general and divisional meetings and a "scientist-teacher alliance" contributed to the reform of teaching and learning in a number of school districts. In the international arena, the APS, from the mid-eighties to the early nineties, conducted a highly successful "China Program" for the training, as postdocs in US universities, of many of the present leaders of Chinese physics. After the collapse of the Soviet Union, APS, led by its 1992 President, Ernest Henley, helped to select (through the work of scores of volunteers) and to fund (first through contributions from its members and NSF and later with moneys from George Soros) hundreds of Russian and other former Soviet Union physicists to enable them to survive and continue their research.

The new structures, the raised consciousness, and a number of activist presidents, led, during the past two decades, to statements on public policy matters that would not have been issued in earlier times. On November 18, 1978, the Council, by a vote of thirteen to ten with two abstentions, came out in support of the Equal Rights Amendment (for women) and, more significantly and controversially, resolved not to hold APS meetings in states that have not ratified the Amendment. An unprecedented statement in favor of nuclear arms control, drafted by Hans Bethe, Sidney Drell, Marvin Goldberger, Wolfgang Panofsky, and Herbert York, was issued on January 23, 1983 under the leadership of Robert E. Marshak, arguably APS's most activist president. It evoked an extraordinary negative response from George A. Keyworth III, President Reagan's science advisor. Other more recent public interest pronouncements have included statements against charlatanism in science, such as the claim that ambient electromagnetic fields have caused cancers, and in support of maintaining the national helium reserve.

While these statements and initiatives were, in the old tradition of the APS, disinterested and even altruistic, the last three decades of the 20th century have seen an increasing preoccupation with the economic advancement of the physics profession and the welfare of its practitioners. With the adequate funding for physics research in considerable doubt, the Society, in 1989, created the Physics Planning Committee (PPC), to be composed of the most recognized available leaders and practitioners of research. While its original assignment, the preparation of a balanced plan for (the funding of) physics research, has proved to have been unrealistic, PPC has been instrumental in helping to organize and carry out what the Society now finally recognizes and supports as "lobbying" for physics. In recognition of this reality, the committee was renamed the Physics Policy Committee in 1997. The (tongue only slightly in cheek) answer to those members who have questioned the difference between POPA and PPC, has been that POPA concerns itself with what physics can do for the country, while PPC worries about what the country can do for physics.

The generally welcomed, aggressive pursuit by the Society of the economic welfare of the discipline – the lobbying for funds – which is now often indistinguishable from the pursuit of jobs for physicists – carries some dangers. Not that in a culture which, if one did not know better, one would think consists *only* of self-serving interest groups, the physics community can afford to be disinterested and disengaged. Even if it wanted to, its friends in government – and by no means only in the federal agencies with whom there is a congruence of economic and professional interests – but also in the Congress, practically beseech the APS to lobby them for funds.

The first danger may arise when the APS speaks out, not on economic, but on political matters, as it did through much of its history, in defense of the Astins and the Condons, or on controversial public issues such as arms control and ballistic missiles, or against "junk science" and pseudoscience. The public may well discount the findings and statements as those of just another self-serving special interest group. A second and related danger is that the Society will become reluctant to engage these issues out of fear that to speak out will offend politicians and members of the public whose support and good will is needed in the battles of the budget. This has not happened yet, but some leaders have used this very argument in urging against statements that they feared would engender a negative reaction.

The third danger is perhaps the most vexing. It is the temptation, in the pursuit of support for science in general and one's research in particular, to say things that one knows are not accurate or germane, because one believes that the public and the politicians want to hear them. In justification it is said – often accurately – that this approach works, at least for a time. Thus for more than four decades physics research (and teaching), including the most basic research with no discernable or even conceivable applications, was handsomely supported by the uncontested popular belief that it all was necessary to fight the cold war. With the cold war over and physics budgets threatened, some, including the late George Brown, science's greatest friend in Congress until the arrival of Vernon Ehlers, have come to believe that this was a mistake. In one of the last interviews before his death, he told *The New York Times*: "... to build the funding of science for the next generation on the basis of the cold war was not well advised. That implied that science was not important enough to survive without a cold war."

Now the emphasis in lobbying for better support for physics is on its essential contributions to technology and to medicine. Since antiquity science and scientists have always been supported, at least in part, for what they could do for their king and country. But for the scientists themselves and ultimately for all of civilization, purely curiosity driven research, undertaken for no reason except to want to know, is just as important and worthy of support. If that is the case, science and particularly physics should be presented to the public on that basis. As the 1999 President, Jerome Friedman, expressed it on the occasion of APS' Centennial, "...the physics community must continue to reach out to the public and the government with the message about the intellectual *and* practical benefits of science." Insuring a *modus vivendi* for an elitist profession in an egalitarian society will challenge the American Physical Society in its second century.

“Long Complicated Derivations in Mathematical Physics: The Two-Dimensional Ising Model and The Stability of Matter”, by Martin H. Krieger (University of Southern California)

I reread several germinal papers in mathematical physics, having long complicated derivations, in light of subsequent work. My motive to was prove out an intuition that the formalism and the mathematics is “physical”. (Much of this is drawn from Martin Krieger, *Constitutions of Matter: Mathematically Modeling the Most Everyday of Physical Phenomena* (Chicago, 1996) and a book in draft, “Mathematical Practice”.) In general, my impression is that people do not read these papers any longer, or even read them much longer after they were published, once a “clearer” derivation appeared. They were “superceded”. They are hard to read: You can follow each step, but . . .

The lessons from this exercise are: (1) The technical details of the mathematical physics help reveal the essential physics of these systems. The formalism and the rigor are not just “for show”. (2) The original papers repay reading by contemporary research physicists and mathematicians, even if their interest is not historical.

The Problems:

The first problem is the thermodynamics of the two-dimensional Ising lattice, as a model of ferromagnetism. It was solved by Onsager in 1944 (*Physical Review*, **65**:117-149). When we read it, we might wonder why Onsager ended up with a Clifford algebra and elliptic functions? Eventually, from subsequent papers, we might ask how is the combinatorics (it is a partition function!) embodied in the algebra and the analysis? Eventually, we find out that elliptic functions reflect two symmetries or periodicities of the lattice: lattice translation and an analog of Lorentz invariance. As for Clifford algebra, the quasi-particles are fermions (but McCoy has argued for the spins or bosons).

The second problem is the stability of matter, that the energy is bounded from below by a constant times the number of particles. Dyson and Lenard worked out the solution in 1967 (*Journal of Mathematical Physics*, **8**:423-434; **9**:698-711). The needed physics is Heisenberg, Pauli, and screening—but how to build it in? And, why is there such a jungle of formulas, hacking through a forest of inequalities? Answer: You have to chop up space into cubes. Answer: Build the chopping-up into the physics (Thomas-Fermi atoms do not bind).

In each case there is predecessor work that sets up these endeavors, and successor work that makes it transparent. All agree that it matters to know that the problem can be solved somehow—it encourages successors to do a better job. I should note that I am not settling questions of attribution and credit. That depends not only on priority, but on a theory of what is the crucial advance.

Usually the advances demand a better understanding of the physics; sometimes better mathematics. Still, the variety of “old” papers may provide valuable perspectives. In fact, their variety may be meaningful. For example, all the Ising model work over the years would appear to fit systematically under the rubrics of the Langlands Program of analytic number theory and representation theory, a modern version of the 19th century analogy of (Riemann’s) geometric theory of functions, algebraic functions (Dedekind-Weber), and algebraic numbers.

Some Observations:

Mathematical physics is like analytic philosophy—showing what we mean, what is necessary, what is contingent. Rigor allows one to isolate what is crucial from what is ancillary.

1. It would appear that scientists know what they are doing, even if they are not sure of its full meaning. They are not sleepwalking. They often realize many of the subtleties revealed in much later work, although they may not be able to prove those subtleties.

2. Mathematics may lead the way, rather than physics. Formalisms and techniques may provide the path, rather than physical intuition.

3. There’s physics in the mathematics. In the details lies the physics. That may only be revealed years later, but in general every little movement has a meaning of its own. “Peculiar” objects (spinors, elliptic functions, Painlevé transcendents) point to something physically interesting.

4. Sometimes, there is what might be called a conservation of effort. Subsequent proofs often are rather more perspicuous and shorter. More is already built into apparatus and machinery that has been invented or is being invented for this purpose. But there is also efficiency, when new ideas are introduced and they cut lots of the effort.

The Seven Pines Symposium by Roger H. Stuewer (University of Minnesota)

The Seven Pines Symposium is dedicated to bringing historians, philosophers, and physicists together for several days in a collaborative effort to probe and clarify significant foundational issues in physics, as they have arisen in the past and continue to challenge our understanding today. The symposium takes its name from Seven Pines Lodge, located near Lewis, Wisconsin, which was built in 1903 as a trout-fishing camp and since 1978 has been on the National Register of Historic Sites. In the past, President Calvin Coolidge and other notables vacationed there. Today, its idyllic setting and superb cuisine make it an ideal location for small informal meetings.

The fourth annual Seven Pines Symposium was held from May 10-14, 2000, on the subject, “Issues in Modern Cosmology.” Twenty-three historians, philosophers, and physicists were invited to participate in it. Unlike the typical conference, twice as much time is devoted to discussions following the talks than to the talks themselves, and long mid-day breaks permit small groups to assemble at will. As preparation for the talks and discussions, the speakers prepare summarizing statements and select appropriate background reading materials, which are distributed in advance to all of the participants.

Each day the speakers set the stage for the discussions by addressing major historical, philosophical, and current issues in cosmology. In the morning of Thursday, May 11, John D. North (Groningen) and Helge Kragh (Aarhus) spoke on “The Emergence of Cosmology as a Science,” the former treating the period to the end of the nineteenth century, the latter the twentieth century to the discovery of the microwave background radiation. In the afternoon, P. James E. Peebles (Princeton) spoke on “Late Twentieth-Century Cosmology after the Discovery of the Microwave Background Radiation.” In the morning of Friday, May 12, John Earman (Pittsburgh) and Michael S. Turner (Chicago) spoke on “Inflation.” In the afternoon, Ernan McMullin (Notre Dame) and Neil G. Turok (Cambridge) spoke on “The Anthropic Principle.” In the morning of Saturday, May 13, Alexander Vilenkin (Tufts) and William G. Unruh (British Columbia) spoke on “Quantum Cosmology.” In the afternoon, James H. Hartle (UC Santa Barbara) and Yuri Balashov (Georgia) spoke on “Laws and Initial Conditions in Cosmology.” A closing roundtable discussion on Sunday morning, May 14, was chaired by Roger H.

Stuewer (Minnesota).

Lee Gohlike, the founder of the Seven Pines Symposium, has had a life-long interest in the history and philosophy of physics, which he has furthered through graduate studies at the Universities of Minnesota and Chicago. To plan the symposia, which will be held annually, he established an advisory board consisting of Roger H. Stuewer (Minnesota), Chair, Jed Z. Buchwald (MIT), John Earman (Pittsburgh), Geoffrey Hellman (Minnesota), Erwin N. Hiebert (Harvard), Don Howard (Notre Dame), and Alan E. Shapiro (Minnesota). Also participating in the fourth annual Seven Pines Symposium were Michael J. Crowe (Notre Dame), Alan H. Guth (MIT), Michel Janssen (Boston), Jesús Mosterin (Madrid), John S. Rigden (American Institute of Physics), Christopher J. Smeenk (Pittsburgh), and Robert M. Wald (Chicago).

The fifth annual Seven Pines Symposium will be held from May 30-June 3, 2001, on the subject, "The Quantum Nature of Gravitation, Space, and Time."

FORUM NEWS

Forum Officers

Laurie M. Brown, Department of Physics and Astronomy (emeritus), Northwestern University (brown@nuhep.phys.nwu.edu), became Chair in April 2000 at the end of Allan Franklin's term. **Benjamin Bederson**, Department of Physics (emeritus), New York University (ben.bederson@nyu.edu), was elected Chair-elect and will succeed to Chair in April 2001. **Hans Frauenfelder**, Los Alamos National Laboratory (frauenfelder@lanl.gov), was elected Vice-Chair and will succeed to Chair-Elect in April 2001.

Elizabeth Urey Baranger, University of Pittsburgh (eub@pitt.edu) and **Michael E. Fisher**, University of Maryland at College Park, were elected to three-year terms on the Executive Committee. The remaining members of the Executive Committee are **Alanna Connors** (aconnors@frances.wellesley.edu) and **Martin C. Gutzwiller**, IBM Yorktown Heights (retired) (MoonGutz@aol.com), whose terms expire April 2001; and **A.P. French**, MIT (apfrench@mit.edu), and **Michael Riordan**, SLAC (michael@slac.stanford.edu), whose terms expire April 2002.

Bill Evenson, Brigham Young University (evenson@byu.edu), continues as Secretary-Treasurer until 2001; **Gloria Lubkin**, *Physics Today* (gbl2@aip.org), continues as Forum Councillor until 2002, and **Spencer R. Weart**, Director of the AIP Center for History of Physics (sweart@aip.org), serves as *ex officio* member of the Executive Committee.

Many thanks to **Allan D. Franklin**, Department of Physics, University of Colorado (Allan.Franklin@colorado.edu), for his good work as Chair during 1999-2000, and **Roger H. Stuewer**, School of Physics and Astronomy, University of Minnesota (rstuewer@physics.spa.umn.edu), for his continued help as Past Chair during 1999-2000. Thanks also to **Lillian Hoddeson**, Department of History, University of Illinois (hoddeson@uiuc.edu) for her service as Vice-Chair during 1999-2000 and to **Dudley Herschbach**, Department of Chemistry, Harvard University (herschbach@chemistry.harvard.edu), and **Abner E. Shimony**, Department of Physics, Boston University (helencwalk@aol.com), for their work on the Executive Committee during the last three years.

Executive Committee

The annual meeting of the Executive Committee was held on April 30, 2000, at the Long Beach APS Meeting. It was chaired by Laurie Brown, since Allan Franklin was out of the country. Brown thanked the many Forum members who helped with FHP projects this year, especially the Program Committee, the Award Committee, the Nominating Committee, and Abner Shimony for his proposal for associate membership. A proposal for a Forum Award was discussed and approved to be sent to the APS Council for their consideration this fall. A proposal for associate membership was discussed. This proposal requires a new category of APS membership, which would be difficult to arrange. The Committee agreed to approach this issue initially by offering the *Newsletter* to members of related organizations and by seeking to have reciprocal access to their newsletters (see below). Membership increased once again during the past year (by 139 members).

Forum Committees

For 2000-01, the Standing Committees of the Forum are:

Program Committee: **Ben Bederson** (chair), Alanna Connors, Michael Nauenberg, Michael Riordan

Nominating Committee: **Elizabeth Urey Baranger** (chair), Gloria Lubkin, Allan Franklin, Robert S. Cohen

Fellowship Committee: **Hans Frauenfelder** (chair), Martin Gutzwiller, Laurie Brown, Michael Fisher

Membership Committee: **Bill Evenson** (chair), A. P. French, Abner Shimony

Award Committee: **Martin Gutzwiller** (chair), Laurie Brown, Michael Riordan

Publications Committee and Editorial Board: **Bill Evenson** (chair), Michael Riordan, Spencer Weart

APS Fellow Nominations

Hans Frauenfelder is chair of the Forum's Fellowship Committee for 2000-01. Any Forum members who wish to nominate a candidate for Fellow in APS are invited to send him their suggestion(s), along with a c.v. and letter describing the candidate's achievements in history of physics. Mail suggestions to Dr. Hans Frauenfelder, CNLS, Los Alamos National Laboratory, MS B258, Los Alamos, NM 87545, or email frauenfelder@lanl.gov.

Meeting on "The Foundations of Quantum Physics Before 1935" Cosponsored by FHP

The American Physical Society has endorsed the meeting, "The Foundations of Quantum Physics Before 1935" to take place December 14-16, 2000 in Berlin. The APS Forum on the History of Physics is a cosponsor of this meeting with the Division of the History of

Physics of the German Physical Society, the Max Planck Institute for the History of Science, the Commission on the History of Modern Physics of the IUHPS Division of History of Science, and the Interdivisional Group on History of Physics of the European Physical Society.

Invited speakers at the Berlin Conference include Gerald Holton (Harvard), John Heilbron (Berkeley), Dudley Herschbach (Harvard), Roger Stuewer (Minnesota), Helge Kragh (Aarhus), James Cushing (Notre Dame), Michael Eckert (Munich), Olivier Darrigol (Paris), and Juergen Renn (Berlin). In addition, Martin Klein (Yale) will be a member of a Roundtable Discussion Panel. There also will be contributed papers that will be selected by the Program Committee whose members are Dieter Hoffmann (Berlin), Fabio Bevilacqua (Pavia), Helge Kragh (Aarhus), Juergen Renn (Berlin), and Roger Stuewer (Minnesota).

Contributed papers for the April 2001 meeting

Following the success of the Forum's first contributed paper session this spring, the Executive Committee has obtained approval for a similar contributed paper session on the history of physics at the April 2001 APS meeting in Washington, DC. Historical papers will again be allowed 20-minute contributed papers. The deadline for submitting abstracts for our contributed paper session at the April meeting is January 12, 2001. *Members are strongly encouraged to consider submitting abstracts on their current work.*

Newsletter Sharing with Related Organizations

We have made contact with the History of Science Society (HSS), the Philosophy of Science Association (PSA), and the History of Philosophy of Science Working Group (HOPOS) to offer our *Newsletter* to their members and to inquire about interested FHP members getting access to their newsletters. These contacts have been received enthusiastically.

We are working out arrangements with HSS and will have more information in the next *Newsletter*.

The *PSA Newsletter* is only published electronically, and our members can subscribe by sending email to elam@philosophy.umkc.edu and asking to be put on the PSA e-list. Note that this was authorized by George Gale.

The *HOPOS Newsletter* is, for the most part, an electronic publication (in pdf format), so you can go to the website, at scistud.umkc.edu/hopos/ and then go to the *Newsletter* section.

This is an experiment right now, but we hope to continue it indefinitely. We hope this effort can bring various scholars interested in history and philosophy of physics into closer contact.

Sally Hacker Prize awarded to two FHP members

The first Sally Hacker Prize was awarded by the Society for the History of Technology (SHOT) to Michael Riordan and Lillian H. Hoddeson last October. This prize is for the best book of the previous three years on the history of technology aimed at general audiences. They received it for *Crystal Fire: The Birth of the Information Age* (W. W. Norton, 1997), on the invention and development of the transistor. Riordan, a member of the FHP Executive Committee, was a 1999-2000 Guggenheim fellow and is a physicist at the Stanford Linear Accelerator Center and adjunct professor of physics at the University of California, Santa Cruz. Hoddeson, former FHP Vice Chair, is a 2000-2001 Guggenheim fellow (see following) and is professor of history and senior researcher at the University of Illinois at Urbana-Champaign, as well as the Fermilab historian.

Guggenheim in Physics History

Lillian H. Hoddeson, last year's FHP Vice Chair, has received a year 2000 John Simon Guggenheim Memorial Foundation Fellowship to complete her study of the "Life and Science of John Bardeen."

APS AND AIP NEWS

"This Month in Physics History"

APS News has started a new feature, "This Month in Physics History." Alan Chodos, editor of *APS News*, has requested suggestions for this column (chodos@aps.org).

APS Centennial Online

Centennial Symposia and Plenary Talks from the APS Centennial Meeting in Atlanta (March 1999) are available online at apscenttalks.org. A Century of Physics Time Line Wall Chart is also available at timeline.aps.org.

2000-2001 APS/AIP Congressional Science Fellowship Programs

The American Institute of Physics and the American Physical Society are accepting applications for their 2001-2002 Congressional Science Fellowship Programs. Fellows serve one year on the staff of a Member of Congress or congressional committee, learning the legislative process while they lend scientific expertise to public policy issues. Qualifications include a PhD or equivalent research experience in physics or a closely related field. Fellows are required to be U.S. citizens and, for the AIP Fellowship, members of one or more of the AIP Member Societies. A stipend of up to \$49,000 is offered, in addition to allowances for relocation, in-service travel, and health insurance premiums. Applications should consist of a letter of intent, a 2-page resume, and 3 letters of recommendation. Please see the websites: www.aip.org/pubinfo or www.aps.org/public_affairs/fellow.html, for detailed information on applying. If qualified, applicants will be considered for both programs. All application materials must be postmarked by January 15, 2001 and sent to APS/AIP Congressional Science Fellowship Programs, One Physics Ellipse, College Park, MD 20740-3843.

AIP Center for History of Physics

New web exhibit on Marie Curie

A major new exhibit has been mounted on the web to explain the life and work of Marie Curie. "Marie Curie and the Science of Radioactivity" is offered by the Center for History of Physics at the American Institute of Physics, already well known for its award-winning exhibits on Albert Einstein, Werner Heisenberg and other scientists. The new exhibit was written by Naomi Pasachoff, author of a book on Madame Curie aimed at high-school students, and it is expected that the largest number of viewers will be young women

and girls with an interest in science.

The exhibit covers every aspect of Marie Curie's career, including her turbulent youth, her entry into science and the discoveries that won her two Nobel prizes, her marriage and complex emotional life, her creation of medical services at the Front during the First World War, her foundation of the Radium Institute as a world scientific center, and her legacy, including her daughter Irène, another Nobel-winning scientist.

The exhibit is augmented by 90 striking illustrations and English translations of articles by Marie Curie, plus supplementary pages explaining the science of radioactivity in simple language. The entire exhibit has been checked and corrected by leading historians of science, with the cooperation of the French Association Curie et Joliot-Curie and the Museum and Archives of the Radium Institute, Paris. It may be seen at www.aip.org/history/curie. For information contact sweart@aip.org.

Syllabi and bibliographies for teaching history of physics

A newly updated and enlarged collection of syllabi for courses on the history of science, primarily physical sciences, can be seen at the Web site of the Center for History of Physics, American Institute of Physics: www.aip.org/history/syllabi. They would appreciate receiving more syllabi from colleagues. Please send inquiries or information to Alexei Kojevnikov (akojevni@aip.org).

Grants-in-Aid for History of Modern Physics and Allied Fields (Astronomy, Geophysics, etc.)

The AIP Center for History of Physics has a program of grants-in-aid for research in the history of modern physics and allied sciences (such as astronomy, geophysics, and optics) and their social interactions. Grants can be up to \$2500 each. They can be used only to reimburse direct expenses connected with the work. Preference will be given to those who need funds for travel and subsistence to use the resources of the Center's Niels Bohr Library (near Washington, DC), or to microfilm papers or to tape-record oral history interviews with a copy deposited in the Library. Applicants should name the persons they would interview or papers they would microfilm, or the collections at the Library they need to see; you can consult the online catalog at our web site, www.aip.org/history, and please feel free to make inquiries about the Library's holdings.

Applicants should either be working toward a graduate degree in the history of science (in which case they should include a letter of reference from their thesis adviser), or show a record of publication in the field. To apply, send a vitae, a letter of no more than two pages describing your research project, and a brief budget showing the expenses for which support is requested to: Spencer Weart, Center for History of Physics, American Institute of Physics, One Physics Ellipse, College Park, MD 20740; phone: 301-209-3174, Fax: 301-209-0882 e-mail: sweart@aip.org. Deadlines for receipt of applications are June 30 and December 31 of each year.

NOTES AND ANNOUNCEMENTS

Herbert C. Pollock Award.

Dudley Observatory has made a \$5000 award, in honor of Herbert C. Pollock, long-time Board member and past President of the Board of Trustees of the Observatory, to **Prof. Alice Walters**, Department of History, University of Massachusetts, Lowell, MA in support of her project "Objectifying Nature: Culture, Commerce and Science in Britain and America, 1750-1850."

Job Notices.

From time to time we receive notices of position openings in history of physics. Since these often come out of sequence with the *Newsletter* publishing schedule, the notices are put on the FHP web site. If you are looking for a position, please check the web site regularly: www.aps.org/FHP/index.html, then follow the announcements link.

Fellowships at the National Air and Space Museum, Washington, D.C.

The National Air and Space Museum, Smithsonian Institution, Washington, DC, has announced three programs of fellowship opportunities for the academic year 2001-2002. Application deadline is January 15, 2001, and successful applicants will be notified by April. These fellowships are for research in aviation and space history and include the Guggenheim Fellowship, the A. Verville Fellowship, and the Ramsey Fellowship in Naval Aviation History. Information and fellowship application packages can be obtained from Ms. Collette Williams, Fellowship Coordinator, Rm. 3313, National Air and Space Museum, Smithsonian Institution, Washington, D.C. 20560-0312 (collette.williams@nasm.si.edu). Applications packages will be mailed about November 15 and will soon be made available on the Museum website at www.nasm.edu/nasm/joinnasm/fellow/fellow.htm. Potential applicants are also encouraged to investigate the Smithsonian Institution's Office of Fellowships and Grants program. Information can be found at www.si.edu/ofg/fell.htm.

In addition to these fellowships, the Museum offers the **Charles A. Lindbergh Chair in Aerospace History**. Senior scholars with distinguished records of publication who are working on, or anticipate working on, books in aerospace history, are invited to write letters of interest for the academic year 2002-2003 or later. The Lindbergh Chair is a one-year appointed position; support is available for replacement of salary and benefits up to a maximum of \$100,000 a year. Please contact: for topics in aviation, Dr. Peter L. Jakab, Aeronautics Division, National Air and Space Museum, Smithsonian Institution, Washington, DC 20560-0312 (peter.jakab@nasm.si.edu); for space history topics, Dr. Michael J. Neufeld, Space History Division; National Air and Space Museum, Smithsonian Institution, Washington, DC 20560-0311 (mike.neufeld@nasm.si.edu).

Career Opportunities with the Air Force History and Museums Program.

The Air Force is looking for two Museum Curators for assignment to the USAF Museum at Wright Patterson AFB, Dayton, OH, and one Historian who will train at Randolph AFB, San Antonio, TX, and then be reassigned somewhere else in the continental United States after a training period. All three jobs are permanent civil service positions starting at the GS-7 level and promote to GS-11 in three years. John Kuborn is the point of contact for this personnel action: email John.Kuborn@afpc.randolph.af.mil. All of the information required in applying for these positions may be found at www.afpc.randolph.af.mil/cp/recruit. Once you reach the Air Force recruitment site, click on the button for the PALACE Acquire Intern Program and then move on to the career field button on that page. Listed there are the three positions available.

A New Centennial of Flight Commission Website

This new website has information for aviation enthusiasts, educators, students, and all those who may be planning projects and activities to help the country celebrate the Wright Brothers' first powered flight centennial on and around December 17, 2003. See centennialofflight.gov.

Copenhagen.

A website on the Symposium, "Creating *Copenhagen*," inspired by Michael Frayn's play, can be found at web.gsuc.cuny.edu/ashp/nml/copenhagen.

The Sommerfeld Project

The project has made all of Arnold Sommerfeld's scientific correspondence available electronically. See www.lrz-muenchen.de/~Sommerfeld/WWW/AS_Suche.html.

Physics in Perspective.

Most journals are targeted to a small group of scholars. That is not the case for the journal *Physics in Perspective*, which has now been published since early 1999 for a wide audience of historians, philosophers, physicists, and the interested public. The editors believe that scholarly papers written by historians of physics, philosophers of physics, and physicists themselves can be an effective means for bringing the ideas, the substance, and the methods of physics to non-specialists, provided jargon is avoided and care is taken in the writing.

Physics in Perspective is published quarterly. Besides articles and book reviews, the journal has two regular features: first, "The Physical Tourist," identifies sites for the traveler whose interests include artifacts from the history of physics, laboratories with historical significance, birthplaces of well-known physicists, and the like; second, "In Appreciation" is written about a physicist by a student, first-hand acquaintance, or colleague. *Physics in Perspective* is available to members of the American Physical Society at the special subscription rate of \$35 per year plus \$10 shipping and handling. Additional information can be found at the Birkhäuser Verlag web site, www.birkhauser.ch/journals/1600/1600_tit.htm.

First-hand accounts of participants in interesting and important research projects – experimental, theoretical, or computational – often become documents of historical import. The editors of *Physics in Perspective* welcome such first-hand accounts and hereby extend an invitation to physicists, and particularly to members of the Forum on History of Physics, to submit manuscripts for publication. (John S. Rigden, American Institute of Physics, One Physics Ellipse, College Park, MD 20740, jsr@aip.org and Roger H. Stuewer, Tate Laboratory of Physics, University of Minnesota, 116 Church Street SE, Minneapolis, MN 55455, rstuewer@physics.spa.umn.edu).

Robert H. Goddard Historical Essay Award.

The National Space Club is soliciting entries for the Robert H. Goddard Historical Essay Award. Essays may explore any significant aspect of the historical development of rocketry and astronautics, and will be judged on their originality and scholarship. They cannot be more than 5,000 words long, fully referenced, and must be submitted by December 1, 2000. The prize is a plaque and \$1,000 award. For further information contact the Goddard Historical Essay Contest, c/o National Space Club, 2000 L Street NW, Suite 710, Washington, DC 20036.

National Endowment for the Humanities Programs:

NEH OUTLOOK, an email newsletter of the National Endowment for the Humanities (www.neh.gov) can be obtained by sending an email to newsletter@neh.gov; type the word "subscribe" in the body of the message. NEH offers summer programs for professors and school teachers and supports Chautauquas around the country in addition to summer stipends for research and other programs.

NASA History: News and Notes

This is published quarterly by the NASA History Division, Office of Policy and Plans, Code ZH, NASA Headquarters, Washington, DC 20546. You can receive *NASA History: News and Notes* via email. To subscribe, send a message to domo@hq.nasa.gov. Leave the subject line blank. In the text portion simply type "subscribe history" without the quotation marks. You will receive confirmation that your account has been added to the list for the newsletter and to receive other announcements that may interest you. The latest issue of this newsletter is also available on the web at www.hq.nasa.gov/office/pao/History/nltrc.html.

Reconsidering Sputnik: Forty Years Since the Soviet Satellite

This book will be published soon (Harwood Academic, 2000). This work will contain papers from an October 1997 conference on Sputnik cosponsored by NASA, the Smithsonian, and George Washington University. Edited by Roger D. Launius, John M. Logsdon, and Robert W. Smith, the collection includes key essays by Walter McDougall, Sergey Khrushchev, James Harford, Peter Gorin, and Asif Siddiqi. To view an electronic version of the text on the History site see www.hq.nasa.gov/office/pao/History/sputcon2.htm.

Meetings

The **14th Annual Convention of the Society for Literature and Science** will be held in Atlanta, Georgia, on 5-8 October 2000. The theme of the 2000 conference is "**Media: Old and New**". Anticipated topics include explorations of the social and cultural implications of the emergence of new forms of media, as well as papers and panels in the type of science and technology studies traditionally presented at the SLS such as medical humanities, gender in science and technology, and the history and philosophy of science and technology. For more information, consult sls2000.lcc.gatech.edu or email Hugh Crawford (Hugh.Crawford@lcc.gatech.edu).

On 12-15 October 2000 St. Louis University is sponsoring, **Writing the Past, Claiming the Future: Women and Gender in Science, Medicine, and Technology**. Contact Charlotte G. Borst, Department of History, St. Louis University, 3800 Lindell Blvd., P.O. Box 56907, St. Louis, MO 63156.

History of Science Society (HSS) will meet in Vancouver, BC, 2-5 November, 2000. See depts.washington.edu/hsexec/.

A **Symposium on 100 Years of Quantum Theory – History, Physics and Philosophy**, will be held at the Universidad Complutense Madrid, Madrid, Spain, 22-25 November, 2000. See fs-morente.filos.ucm.es/centenario.

A meeting on “**The Foundations of Quantum Physics Before 1935**,” cosponsored by FHP, will be held December 14-16, 2000 in Berlin. See details above, under Forum News.

An **International Conference on Galileo**, sponsored by the Canary Orotava Foundation for the History of Science, will be held 19-23 February 2001 at Tenerife, Canary Islands. Details from www.iac.es/project/galileo/galileo.html or s_ortava@redestb.es.

On 26-29 April 2001 the **Organization of American Historians** will hold its annual meeting at the Westin Bonaventure Hotel in Los Angeles, CA, with the theme, “Connections: Rethinking our Audiences.” Contact the 2001 Program Committee, Organization of American Historians, 112 North Bryan Avenue, Bloomington, IN 47408-4199. For further information on the conference visit the OAH web page: www.indiana.edu/~oah/meetings/2001_program/call.html.

The XX1st International Congress of History of Science will be held in Mexico City from 8-14 July 2001. Prof. Juan José Saldaña, conference chair, has provided a new congress website and email address: www.smhct.org, email: xxiichs@servidor.unam.mx.

Dibner Institute for the History of Science and Technology: Fellows Programs 2001-2002

The Dibner Institute for the History of Science and Technology invites applications to its two fellowship programs for the academic year 2000-2001: the Senior Fellows program and the Postdoctoral Fellows Program. There will be some twenty Fellows at the Institute each term. The Dibner Institute is an international center for advanced research in the history of science and technology, established in 1992. It draws on the resources of the Burndy Library, a major collection of both primary and secondary material in the history of science and technology, and enjoys the participation in its programs of faculty members and students from the universities that make up the Dibner Institute’s consortium: The Massachusetts Institute of Technology, the host institution; Boston University; Brandeis University; and Harvard University. The Institute’s primary mission is to support advanced research in the history of science and technology, across a wide variety of areas and a broad spectrum of topics and methodologies. The Institute favors projects that address events dating back thirty years or more.

The deadline for receipt of applications for 2001-2002 is December 31, 2000. Fellowship recipients will be announced in March 2001. Please send requests for further information and for application forms directly to: Trudy Kontoff, Program Coordinator, Dibner Institute for the History of Science and Technology, Dibner Building, MIT E56-100, 38 Memorial Drive, Cambridge, Massachusetts 02139. Phone: 617-253-6989, Fax: 617-253-9858, email: dibner@mit.edu, website: dibinst.mit.edu.

Dibner Institute Names Fellows for 2000-2001

The Dibner Institute for the History of Science and Technology has appointed fourteen Senior, eight Postdoctoral, and four Graduate Student Fellows and reappointed six Postdoctoral Fellows. They come from several nations and pursue many different aspects of the history of science and technology. Those working in or close to history of physics are listed below. *A full listing of all Dibner Institute Fellows with information about their backgrounds and projects can be found at www.aps.org/FHP/news.html as a web supplement to this Newsletter.*

Olivier Darrigol is Directeur de recherche at CNRS, (REHSEIS) Paris VII. He will continue with his study, “Aspects of Nineteenth Century Hydrodynamics.”

Karin Figala is University Professor for the History of Sciences, Deutsches Museum, Munich. She will continue her research on Isaac Newton’s ‘exact’ alchemy.

Mary Jo Nye is Thomas Hart and Mary Jones Horning Professor of the Humanities and Professor of History, Oregon State University. She is currently working on “Scientific Practice and Scientific Politics in Modern Britain: Essays on P.M.S. Blackett (1897-1974).”

Robert W. Seidel, Professor of the History of Science and Technology, University of Minnesota, will continue his work on a manuscript tentatively titled “From Lavoisier to Lewis: Chemical Engineering in the West, 1775-1955.”

Theodore Arabatzis is a Lecturer in the Department of History and Philosophy of Science at the University of Athens, Greece. He has two interrelated projects: the first, the debate over the “discovery” of the electron and the existence of atoms and the effect of skepticism on the practices of anti-atomists; and the second, the bases for believing in unobservable entities.

Elizabeth Paris, Lecturer in the History of Science Department, Harvard University, will continue her research on the development of colliding beam storage rings.

Christophe Lecuyer, a recipient of the Ph.D. from Stanford University, will complete a book manuscript on the history of Silicon Valley.

BOOK REVIEWS

Ruth H. Howes and Caroline L. Herzenberg, *Their Day in the Sun: Women of the Manhattan Project* (Temple University Press, 1999). 264 pages, \$34.50.

Reviewed by Frieda A. Stahl, California State University, Los Angeles.

Numerous historical reviews have been written about the Manhattan Project, which was organized early in World War II to determine the feasibility of a chain reaction in nuclear fission and to build the colossal bomb that such a reaction would enable. The heroes in those sagas are the Great Men of Nuclear Physics: Fermi, Oppenheimer, et al. However, the Manhattan Project was not a stag party. In *Their Day in the Sun*, Ruth Howes and Caroline Herzenberg detail the participation and contributions of the many women recruited for that effort. At various levels of education and experience, they were physicists, chemists, mathematicians, biomedical scientists, and non-scientists trained on the job they were assigned. Military women as well as civilians are included in the book.

For their research the authors conducted a chain reaction of inquiries. As they write in the Prologue, at the March 1990 APS meeting they met and consulted Melba Phillips. Phillips had not worked on the Project, but she referred them to Naomi French, also present at that meeting, who had. French then spent hours with them recounting her experiences as a mathematician at Los Alamos, and gave them names and addresses of former colleagues with whom she was still in contact. They followed up with phone interviews, in the

course of which they were led to more Project alumnae. They later circulated a questionnaire, and thereby gathered data as well as anecdotes.

They also sleuthed through the historical and biographical literature on the Manhattan Project, and ultimately were able to account for more than 300 women who had been employed at its various facilities. In the Epilogue as well as the Prologue they emphasize their conviction that they could not have found every woman at every Project site.

Howes and Herzenberg begin the main text of the book with two historical summaries, the first on the Manhattan Project's development and the second on the beginnings of 20th century nuclear physics, particularly its "Founding Mothers": Marie Curie, Mileva Maric, Irene Joliot-Curie, Ida Noddack, and Lise Meitner. They also write of other women scientists of that era who made significant discoveries in radioactivity.

The chapters that follow group the women by disciplinary category. For each, the authors relate the experiences of the most traceable women who worked on the research, design, construction, and testing that were carried out at the separate sites serving the Project. A few of these women eventually became eminent in their post-war careers, notably Chien-Shiung Wu, Maria Goeppert-Mayer, Leona Woods Marshall Libby, Isabella Lugoski Karle, Mina Rees, and Edith Quimby. But the women, like most of the men, were not stars. They made material contributions in theoretical as well as experimental sections, and did so with enthusiasm as well as skill. For the most part the women were dispersed and worked in isolation from one another, so that there was no collective impression formed of women scientists involved in the Project. The long-range impression became one of no women, as related in the Prologue.

Howes and Herzenberg weave national history, women's history, and basic scientific concepts into a seamless narrative that frames the context in which the women they describe lived and worked. With relatively few exceptions, the women and men employed by the Project were young. A number of the women had been part of the first wave of females grudgingly accepted into "mainline" American universities for graduate study in science. There some of them married men graduate students and were hired along with their husbands, but assigned to different groups. Other young couples met and married at the labs. Some women were traditional wives who accompanied their husbands to their new jobs; many of them found employment in technical as well as clerical duties. This was especially the case at Los Alamos, where working wives in that close and regulated town were given priorities for household help and nursery school enrollment for their children. Many of them worked on technical problems, pounding mechanical desk calculators in that pre-computer age.

That age was also one of traditional double standards. Salaries and job titles for women were lower than for men. At one facility, women with degrees were hourly employees and had to punch a time-clock, but men with degrees were on salary. In some instances women trained men who out-ranked and out-earned them. Yet their prevailing spirit was one of pleasure at being able to use their hard-earned educations.

In their final chapter the authors describe both the professional careers and the public-issue activities of some of the Project's women after their war work ended. In politics, much of the movement for control of atomic energy was supported by women who had collaborated in its generation. In science, some resumed and completed their interrupted educations. Some returned to previous positions as instructors or research associates and had to restart the struggle for professional parity. Some found no employment and dropped out of science. In the authors' words, "With the end of the fighting, all the barriers to women that the war had lowered went back up." In the Epilogue they add, "[I]t wasn't until the feminist movement of the 1970s that policy makers relearned the lesson of the Manhattan Project: that women represented a valuable but untapped resource of scientific talent."

The book has several interesting and useful features in addition to its narrative: another voice, in the Foreword by Ellen C. Weaver; 16 pages of vintage photographs; a roster of all the women the authors found, traced through their inevitable name changes; a 50-year chronology of discoveries and events related to radioactivity, nuclear fission, and the development of the bomb; and a 15-page bibliography.

In all research there is the chance of error; two have crept into this otherwise meticulous work. On page 32 there is a statement about Otto Hahn and Fritz Strassmann sharing the Nobel Prize for the discovery of nuclear fission. However, only Hahn was awarded the Prize, in Chemistry for 1944. The reference cited for that datum does not contain this error. On page 103 Occidental College is identified as "now UCLA," which it is not. Occidental is an independent institution about 25 miles east of UCLA. Members are well advised to overlook these slips and to use and enjoy the book for all its valid information and insight.

This book is a strong addition to the succession of historical and biographical studies on women scientists which appeared almost annually during the 1990s. Beyond its value as a resource for women's studies, it provides essential corrections for the historical study of nuclear energy development, where serious errors of omission can now be reversed.

Peter Day, *The Philosopher's Tree: A Selection of Michael Faraday's Writings* (Institute of Physics Publishing, Bristol, 1999). 211 pp., illus. \$29 (£16), paper.

Reviewed by Ryan D. Tweney, Department of Psychology, Bowling Green State University.

In the Introduction to this pleasant book, Peter Day, a noted physical chemist who served as Director of the Royal Institution from 1991 to 1998, notes that Michael Faraday (1791-1867) is one of the few truly eminent historical figures in science whose writing, both published and unpublished, shows a vivid and readable style, accessible to general readers across the centuries. In the book, Day has gathered some striking exemplars of the claim, tied together with his own astute (and appropriately well-written!) commentary. The result can be enjoyed by readers at many levels of expertise, from the rawest novice in physics to the expert in field theory and its history.

Day has capitalized upon the many published transcriptions of Faraday's own extensive diaries and journals, supplemented by writings and correspondence that have not yet been published but are held by the Royal Institution and (across town, in London) by the Institution of Electrical Engineers. Faraday was an indefatigable note taker and, as a result, may well be one of the best-documented scientists ever to have lived. In addition to his monumental *Diary*, published in an excellent 8 volume set in the 1930s, there have been recent volumes based on his smaller notebooks. Most notably in recent years, the *Correspondence of Michael Faraday*, edited by Frank A. J. L. James, the first four volumes of which are now available, is making available all of the surviving letters to and from Faraday.

In fact, so much material is available by and on Faraday, that a small volume of "gleanings" is a welcome addition to the literature. By steering away from an attempt at another scholarly collection, Day has rendered a service to those with little or no knowledge of this inspiring figure. Day's "mix" of material is nicely balanced between the personal writings and the scientific. The range is wide, from

love letters of Faraday and his future wife to diary accounts of the discovery of electromagnetic induction, from travel notes in Italy to comments on the political doings of the Royal Society, from reflections on the nature of science and religion to Faraday's justly renowned lectures before juvenile audiences. Day keeps his balance in presenting such a diverse selection. Faraday the person emerges from the result, as well as a good deal of Faraday the scientific genius.

The ordering of the material is roughly chronological, beginning with two chapters on Faraday's earliest years (up to roughly 1812), a long chapter on his tour of France and Italy as Davy's assistant in 1813-15, then a chapter each on his methods of work and on his colleagues and friends. Three chapters follow which center on the science; one on Faraday's careful consideration of scientific terminology, one on his laboratory work, and one that excerpts key portions of his diary, especially the entries following his 1831 discovery of electromagnetic induction. The last third of the book turns again toward the personal; Faraday's lectures for young people and adults, his humble attitude toward scientific honors and awards, his role as a public advocate, and his final days, including, as a closer, a last touching letter to his long-time friend Carl Schoenbein, written shortly before his death: "I will not write any more. My love to you, ever affectionately yours, M. Faraday" (p. 204). There is a brief and useful bibliography and an index, and the book is nicely illustrated with a mix of sketches from the diaries, contemporary images of Faraday's lab, and a few scientific diagrams.

Faraday's life has long been a fascinating one to popularizers of science. The self-taught bookbinder who gets "discovered" by the famous Humphry Davy and whose devotion to knowledge was so strict that he relentlessly worked to pull one elegant discovery after another out of his long and active career in science; this is the stuff of romantic fiction. It would have been easy to idealize the man, or to simply repeat the clichés of others, but Day's footing is too assured. We do get the now-standard portrait, but we also see something of the obsessiveness, and a touch of Faraday's tendencies toward self-righteousness.

Scholars might cavil with Day's judgment here and there, but I found nothing egregious enough to merit mention in this review, given the book's aim. To nitpick dates, or spellings, or the precise emphasis of an interpretation would miss the point. This is biography "in the small," written partly as appreciation and partly to tease the reader into learning more. As a result, its audience is potentially a very broad one. I could recommend reading it to a high school student but also to a particle physicist. Even Faraday scholars might profit from the balance manifest here, perhaps as antidote after a hard day untangling the confusing detail of 19th century chemical and physical terminology found in some of the more serious works about Faraday. For classes in the history of science, the book could serve as supplementary reading, or perhaps as the starting point for students to dig further into the history of specific aspects of Faraday's life, his times, his social milieu.

Peter Day has done Faraday's memory a fine service in bringing his words, thoughts, and feelings to a wide audience. Michael Faraday believed that he had a moral responsibility to share the results of his science with the public. This is what led him to prepare lectures for young people, to serve without pay on government commissions, and to advocate, as a matter of public policy, widespread education in science for all citizens. In all, a fitting tribute, one that will help Faraday to extend his goals.

Per F. Dahl, *Heavy Water and the Wartime Race for Nuclear Energy* (Institute of Physics Publishing, Bristol and Philadelphia, 1999). 399 pages. \$60 (£35)

Reviewed by A. P. French, Professor Emeritus of Physics, Massachusetts Institute of Technology.

The historical scope of this book is much greater than the title would imply. The author (after giving us a preview of a central event that occurred in Norway in March, 1940) takes us back briefly to the earliest years of radioactivity in the late 1890s. He continues the prologue by introducing us to Irène Curie and Frédéric Joliot (the latter playing a significant part in the subsequent narrative) and then to Chadwick and the discovery of the neutron in 1932. At last the curtain really rises, since this was also the year in which Harold Urey announced his discovery of deuterium (named as such by him after some vigorous arguments).

Surprisingly quickly the production of heavy water, rather than heavy hydrogen itself, became a focus of interest. Its extraction from natural water through intense and prolonged electrolysis became the accepted technique. Initially the main concern was with its chemical and biological properties. Of course, no thought of nuclear energy production arose at the time, although the use of deuterons as bombarding particles in nuclear experiments had been quickly taken up. The resources needed for separation of D₂O on a large scale already existed in Norway, thanks to the country's massive investment in hydroelectric power, generated from waterfalls, over the previous 30 years. This development had been started by a partnership between the physicist Kristian Birkeland and a civil engineer, Samuel Eyde, with the object of producing nitrates for fertilizer from electric discharges in air. The company, Norsk Hydro, formed for this purpose had its chief plant at Rjukan, about 130 km west of Oslo; in 1929 it turned to making fertilizer by a different method, by producing electrolytic hydrogen from water and then combining this with nitrogen to make ammonia. It was a chemist, Leif Tronstad (one of the central characters in this book) who in 1933, in association with Jomar Brun, the head of Norsk Hydro's hydrogen electrolysis plant, proposed that the water left over from hydrogen production, already somewhat enriched in D₂O, be further processed to make almost pure heavy water. It was a cascade process, coupled with recycling, culminating in more than 99% pure D₂O. By January 1935 the material was becoming available in amounts of more than 100 g; Urey himself received 500 g. During 1938 about 80 kg was produced.

The discovery of fission at the end of 1938, together with the realization that Germany and the Western allies would probably soon be at war, created a totally new situation. Attention on both sides turned to uranium and the possibilities of nuclear chain reactions and bombs. The major work at this time was being done at Columbia University, where Fermi was located, and at the Radium Institute in Paris, where Joliot led the experiments, aided by Hans Halban and Lew Kowarski. In March 1939 Germany completed its occupation of Czechoslovakia. This prompted efforts by Leo Szilard, Victor Weisskopf and others, aimed particularly at Joliot, to bring about a moratorium on publications about fission. At the time, however, Joliot declined to cooperate – perhaps, according to Dahl, because he was skeptical about the possibility of making a bomb, or because of his basic opposition to censorship even if self-imposed. Joliot did, however, ask Francis Perrin to make calculations on the critical size of a mass of uranium or uranium oxide, and the Paris group ended up by writing three secret patents for chain-reacting systems (one of them for an explosive device). The French had easy access to the uranium resources controlled by Belgium and originating in the Belgian Congo, and in May 1939 received 5 tons of uranium oxide for pilot experiments.

In both New York and Paris, research efforts were directed toward making a self-sustaining reactor from uranium or uranium oxide embedded in a moderating material. Ordinary water was initially considered in both places. Fermi soon turned to the possibility of graphite, which was also considered by the Paris group, but the latter's studies in this regard were not encouraging, and soon after the

outbreak of war in September 1939 the group turned its attention to heavy water, with which Halban had done some experiments in Copenhagen in 1937. In December 1939, Norsk Hydro learned that Germany, too, was interested in heavy water, for unspecified reasons. In March 1940 a French emissary met with Norsk Hydro's General Director (Axel Aubert) to discuss a request, initiated by Joliot, for a loan of the entire existing stock of heavy water. Aubert told the French of Germany's interest and made clear his sympathy with France. The result was the event with which Dahl's book opens. On March 12 an aircraft carrying half of the heavy water took off for Scotland while another aircraft, believed by the Germans to be carrying it, headed for Amsterdam and was intercepted. The rest of the heavy water was successfully smuggled out the next day. From Edinburgh the water was transported by train to London and then to Paris, where it was stored in the cellars of the Collège de France. Only a few weeks after this exploit (on April 8, 1940) the Germans invaded Norway, which like the other Scandinavian countries had declared neutrality. The Hydro plant at Rjukan was an important objective, and was soon taken over by the Germans.

The safe keeping of the heavy water in Paris was soon threatened. On May 10, 1940 the massive German attack through the Low Countries began; a week later the water was transported to Clermont-Ferrand, 200 miles south of Paris. Joliot and his team gathered there, but the rapid collapse of France forced further travels. Halvan and Kowarski took the water to Bordeaux and thence by ship to England, where they arrived on June 21. They soon joined the British atomic bomb effort and set up a reactor research group at the Cavendish Laboratory, using the heavy water as moderator. Joliot remained in France and returned to Paris, where he was closely questioned by the Germans about the whereabouts of his uranium and the heavy water; he (perhaps correctly) declared ignorance of both.

Meanwhile, in Germany, Hitler's "Uranium Club" had been getting under way. Paul Harteck, who as a research student under Rutherford in the early 1930s had made his own separations of small amounts of heavy water, recognized the potential of this material as a moderator, and had Heisenberg's backing for following this route toward a reactor. Heavy pressure was applied to Norsk Hydro to step up its production; by the end of 1941, the output was about 100 kg per month more. A new catalytic process devised by Harteck was expected to lead to further improvements. Jomar Brun, still in charge of the electrolysis plant, kept Leif Tronstad informed of these developments. Shortly afterward, Tronstad disappeared from Norway and joined the Special Operations Executive (SOE) in Britain, where he helped organize sabotage activities in Norway. The Allies could not know how poorly the German nuclear effort was going, and Norsk Hydro became a prime target. The first priority was to destroy the production plant. A disastrous first attempt, with tragic loss of life, was made in November 1942, but it was followed in February 1943 with a brilliantly executed operation by a Norwegian team that destroyed all of the final-stage concentration cells, along with the heavy water contained in them. Production was, however, resumed in May 1943. In November the plant was badly damaged in an American air raid, and in December, by German orders, the production of heavy water there was discontinued, with a view to setting up a replacement operation in Germany itself. However, there remained stocks of material near Rjukan awaiting transport to Germany. In February 1944 the ferry that was to transport it on the first stage of that journey was sunk in another daring sabotage operation, which ended this particular phase of the war.

Despite their major commitment to graphite, the Americans did not ignore heavy water. In December 1940 they learned of an encouraging report from Halban and Kowarski in England "that the D_2O -U slow neutron reaction will go." Urey was naturally interested, and a visit by Halban in early 1942 gave further support. Thus, alongside the firm commitment to use graphite for the production reactors, there developed a back-up project to produce heavy water in quantity. The place initially chosen was an existing electrolytic-hydrogen plant for production of ammonia at Trail in British Columbia, just across the border from Washington state, but things went slowly there and the main job was handed over to several plants operated by Du Pont. By late 1945 they were producing more than 1000 kg of 99.8% D_2O per month. This did not contribute to the war effort, but led to the world's first heavy-water reactor, at Argonne, Illinois.

In Germany, on the other hand, the supply of heavy water was never sufficient. In 1942 Heisenberg in Berlin had most of it – about 1000 kg. But two different groups were, in effect, in competition for it (the other one was headed by Kurt Diebner, a nuclear physicist who worked for Army Ordnance, also in Berlin). The Allied bombing forced the relocation of both groups. During 1944 Heisenberg moved his headquarters to Hechingen in southern Germany, although work on one uranium assembly continued in Berlin until January 1945. Harteck made a visit to Norsk Hydro in a final attempt to obtain some more heavy water. The last home of the German criticality experiments was in a cave at Haigerloch, near Hechingen. In April 1945, Haigerloch was reached by American troops, followed by the famous *Alsos* team with Samuel Goudsmit. But the uranium, the heavy water and the scientists themselves were missing. All were tracked down shortly thereafter. As is well known, none of the German assemblies reached criticality.

In Norway, meanwhile, the future of Norsk Hydro remained in the balance. Leif Tronstad, who had been centrally involved in organizing the sabotage operation in 1943, was now with the Norwegian High Command in London. Looking toward the end of the war, he was concerned with safeguarding the Norsk Hydro installations (and others) from possible scorched-earth operations by the occupying forces. He arranged to be parachuted into the country to organize the Norwegian resistance forces to counter this. In October 1944, a party led by him landed on the Hardanger Plateau. But in March 1945 he encountered a local sheriff who was in collaboration with the Gestapo, and this ended in his being shot to death by the sheriff's brother. He had been a central player in the heavy-water saga ever since his own researches with deuterium and his proposal for massive heavy-water production by Norsk Hydro in 1933. In the event, the German forces surrendered without incident in May 1945.

It is clear that Per Dahl has chosen to tell a story full of action and high drama, with Norway at center stage. I assume that his own heritage provided at least some of the motivation for this. But, as mentioned at the beginning of this review, he has embraced a wider range of events. This includes two complete chapters covering artificial radioactivity, especially the work of Irène and Frédéric Joliot-Curie, and the discovery of nuclear fission, neither having anything to do with heavy water as such. In this respect it has something in common with his earlier book, *Flash of the Cathode Rays: A History of J. J. Thomson's Electron* (1997), reviewed by Joseph F. Mulligan in the Fall 1997 issue of the *FHP Newsletter*; the contents of the book go well beyond what one would expect from the title. I do not regard this as a defect. Like Mulligan, however, I must criticize IOP Publishing for various errors and infelicities in English language that seem to have escaped the copy-editor's eye (I should be glad to supply details). But these are minor complaints; Dahl has written a fascinating tale of scientific, technical and military history, very well researched and documented.

Crosbie Smith, *The Science of Energy. A Cultural History of Energy Physics in Victorian Britain* (University of Chicago Press, 1998). 404 pages. \$25

Reviewed by Joseph F. Mulligan, Professor Emeritus of Physics, University of Maryland Baltimore County.

As this book convincingly demonstrates, the most rapid progress in our knowledge of energy physics took place during the nineteenth century in the Scottish or "North British" universities, especially Glasgow and Edinburgh, rather than at Oxford and Cambridge in the south. James Prescott Joule (1818-1889), William Thomson (1824-1907), James Clerk Maxwell (1831-1879), and their associates in the process, in particular Peter Guthrie Tait (1831-1901) and W.J. Macquorn Rankine (1820-1872), were the main contributors. These scientists and engineers worked to construct, piece by piece, an acceptable science of energy for the nineteenth and subsequent centuries. They are the key actors in the drama of energy recounted by Crosbie Smith in this stimulating book.

After a brief introduction, in his second chapter the author emphasizes the role of religious conviction (in the form of Scottish Presbyterianism) as the inspiration for the development of a science of energy that was both theoretically beautiful and eminently practical. This theme resounds throughout Smith's book:

Just as ministers of the reformed Kirk [the Scottish Presbyterian Church] presented themselves not as guardians of esoteric knowledge but as teachers of the people (often by analogy or parable), so the scientists of energy advocated a physics with strong visualizable foundations accessible to everyone, in contrast to the far more elite symbolic formulations of their continental competitors (p.241).

One of the best of the fourteen chapters that make up *The Science of Energy* is the fourth, entitled "Mr. Joule of Manchester," this being the industrial city roughly halfway between London and Glasgow. In the years 1838 to 1864 James Joule published 21 papers on experiments related to heat, work and energy, all in highly-regarded British journals. These established Joule as a leading contributor of trustworthy data to the science of energy, despite his lack of any advanced formal training in physics.

In chapters seven and eight the author presents an account of the work of Hermann von Helmholtz (1821-1894) and Rudolf Clausius (1822-1888) in Germany on the physics of energy and discusses their interactions with the North British group working along similar lines. The author presents a very clear exposition of Helmholtz's "Erhaltung der Kraft," written in 1847, but rejected for publication by Poggenorff's *Annalen der Physik* as too theoretical for readers of that journal. Fortunately Helmholtz had his 71-page article printed privately by a local publisher, and its illustration of the usefulness of the conservation-of-energy principle in mechanics, electromagnetism, chemistry and physiology gradually led to the acceptance of this pivotal principle by scientists not merely in Germany but throughout the world.

After a brief discussion in chapter nine of territorial disputes over the development of the science of energy, Smith moves to a discussion of Thomson and Tait's 1867 *Treatise on Natural Philosophy*. He discusses the first (and only published) volume of this projected three- or four-volume treatise on mechanics and electromagnetism to be unified around the concept of energy.

Chapter eleven is devoted to James Clerk Maxwell, whose ideas went a long way to fill in the structure of mechanics that Thomson and Tait (T and T') had planned for subsequent volumes of their *Treatise*, but which never appeared in print. Maxwell's growing commitment after 1850 to a science of energy led to his break with Wilhelm Weber's action-at-a-distance electromagnetics. Here Smith clearly demonstrates how close were the interactions between T and T', and how Maxwell in his 1873 *Treatise on Electricity and Magnetism* had adopted an approach to electromagnetism similar to that used by T and T' in their 1867 *Treatise*.

Maxwell, though never a great experimentalist, was firmly convinced of the need for exact standards. In the Preface to the first edition (1873) of his *Treatise on Electricity and Magnetism*, he had written:

The most important aspect of any phenomenon from a mathematical point of view is that of a measurable quantity. I shall therefore consider electrical phenomena chiefly with a view to their measurement, describing the methods of measurement, and defining the standards on which they depend.

In the latter half of the nineteenth century, accurate measurement became the hallmark of physics in the British Empire, in Germany and even in France, where Henri Regnault (1810-1878) carried out precise and highly accurate measurements of the properties of gases. William Thomson spent the spring semester of 1845 in Regnault's laboratory in Paris, and later acknowledged his debt to Regnault for teaching him "a faultless technique, a love of precision in all things, and the highest virtue of the experimenter – patience." (*Science of Energy*, p. 273).

In his 1908 obituary of W. Thomson (after 1892, Lord Kelvin), Joseph Larmor (1857-1942) stressed Kelvin's contributions to the science and the spirit of precise measurement, which was especially important because of Kelvin's major role in both theoretical and commercial science:

If one had to specify a single department of activity to justify Lord Kelvin's fame, it would probably be his work in connection with the establishment of the Science of Energy, in the widest sense in which it is the most far-reaching construction of the last century in physical science (quoted in *Science of Energy*, p. 287).

In his last chapter: "Sequel: Transforming Energy in the Late Nineteenth Century," Smith considers the major changes in the science of energy following on the death of Maxwell in 1879. Included are comments on the British love of mechanical models for the aether, a brief review of Hertz's work (both experimental and theoretical) on electromagnetism, and the debate between German 'Energeticists' like Georg Helm and Wilhelm Ostwald and more conventional thinkers on mechanics like Ludwig Boltzmann on whether the concept of energy should replace the laws of mechanics as the basis of physical science. Smith's brief discussion of these topics is based on secondary sources, but mainly on extraordinarily good ones like Bruce Hunt's *The Maxwellians*, Jungnickel and McCormmach's *Intellectual Mastery of Nature*, and the 1989 definitive biography of Lord Kelvin by Crosbie Smith and M. Norton Wise.

After reading *The Science of Energy*, one wonders if some parts of lesser importance might have been omitted or reduced in size to make room for a fuller and more satisfactory discussion of the impact of the science of energy on more applied fields like the telegraph industry, steam engines, and the energy-devouring marine industry.

The Science of Energy is a splendidly clear book of solid scholarship that includes much good physics history by a historian who really understands physics. It contains 40 pages of endnotes, derived mostly from primary sources, a 30-page bibliography, to which the endnotes refer, and a carefully prepared index that readers will find both complete and easy to use. Smith's book could have been improved by a better cover drawing (or photo) with closer ties to the energy theme of his book, and by a few good photographs of the scientists who contributed so much to its development.

Helmut A. Abt, ed., *American Astronomical Society Centennial Issue of the Astrophysical Journal* (University of Chicago Press, 1999). 1200 pages, \$50

Reviewed by Virginia Trimble, Department of Physics and Astronomy, University of California-Irvine and Department of Astronomy, University of Maryland.

What is an astrophysicist? Possible answers include (a) an astronomer who is tired of being asked to cast horoscopes, (b) a physicist who is trying to solve the quasar problem without knowing what an A star is, or (c) someone who publishes in the *Astrophysical Journal*. This centennial volume of ApJ (which itself dates back to 1895) was one of several celebrations of the 100th anniversary of the 1899 founding of the American Astronomical Society, initially under the title Astronomical and Astrophysical Society of America. It consists of reprints of 53 papers, originally published in ApJ or the *Astronomical Journal* (AJ) and identified as being of great importance by 50 living (but mostly quite senior) astronomers.

There are many potential ways of reacting to such a volume, not all of them complimentary. James Glanz of the *NY Times* described the flavor as “almost Victorian.” My response, when asked to look at it from the perspective of a member of FHP, was to try to see what it tells us about what we mean by astrophysics, how it is done and by whom, and how these have changed through the century.

First, a set of limitations. The papers had to be published in ApJ or AJ, and many outstanding astronomical discoveries, including most work on the solar system, were not. The restriction also means that about 3/4 of the authors were American-born. Most selectors picked something very close to their own interests (indeed, a few picked their own papers), and we were not a random sample of current astronomers. Many selectors also seem to share my prejudice that serious science began just about the time that each of us entered graduate school, producing a pile-up of 21 papers published between 1950 and 1960. The median date was 1956, and the quartiles 1945 (that is, 1/4 before the end of WWII) and 1965 (which was when I started graduate school; not a causal connection).

Next comes a confession. Some of the “well-known, obvious trends” that I had intended to quantify turned out not to be true. Thus I expected a gradual shift away from stars to more distant objects. In fact, there were 11 star papers each in the first and second chronological halves of the selections, though studies of our Milky Way as a whole became less common and those of the universe as a whole (cosmology) more common. What about balance between theory and observation? Again, not much change: 9 purely theoretical and 10 purely observational papers in the first half and 8 theoretical and 10 observational in the second. The fraction of authors born outside the USA was also roughly constant. Cross disciplinary contributions are another continuing factor. Each quartile includes a handful of authors with backgrounds in mathematics, physics, or engineering.

What has changed? Everything has become more and bigger. The first instrument-oriented paper was the stellar interferometer of Albert Michelson and F.G. Pease (1921) which cost perhaps \$1000; the last was a catalog for the Hubble Space Telescope (cost not given, owing to shortage of 0's in Irvine). Papers have grown longer (and today's ApJ Letters at 4000+ words are as long as the average paper of the 1920s). Collaborations have expanded. In the first quartile, 11 of 13 papers had just one author, compared with 10 of the middle 26 and only 3 of the last 14. The first three-author paper came precisely in the middle, in 1956, when Milton Lassell Humason, Nicholas U. Mayall, and Allan Rex Sandage reported extensive data on brightnesses and redshifts of galaxies, which, collectively, reduced the value of the Hubble constant from about 250 to 180 km/sec/Mpc. The last paper had seven authors.

Women started creeping into the field, with the first female author (Nancy Grace Roman on correlations between the motions of stars and the strengths of the absorption lines due to metals in their spectra) in 1950, and three more since (two of whom, Henrietta Swope, an assistant to Water Baade, and Beatrice Tinsley, the first person to evolve a galaxy, are no longer with us, though Nancy looked great when I saw her last fall).

More subtle shifts have taken place in just what we mean by the words “stellar” astrophysics, theory, and observation. The first couple of stellar papers pertained exclusively to the outer layers, Arthur Schuster on transfer of radiation through atmospheres and George Ellery Hale on the discovery of magnetic fields in sunspots (1908). Later, the focus is on internal structure and evolution, particularly nuclear reactions and how they build up heavy elements from hydrogen and helium. This general territory is called nucleosynthesis, and the eight papers pertaining to it cluster between 1950 (Roman, just mentioned) and 1969 (Donald Clayton, Stirling Colgate, and Gerald Fishman on emission of gamma rays as a signature of nucleosynthesis in supernovae, a phenomenon finally seen in 1987). Many details, of course, remain to be worked out, but in large measure nucleosynthesis, along with stellar structure and evolution in general is a solved problem, and there will be no more seminal papers on it in the foreseeable future.

Even the lines between theory and observation have gradually shifted. The word “astrophysics” was initially restricted largely to analysis of spectra of stars, etc. to learn their temperatures, gas densities, and chemical compositions. Schuster's 1903 paper on radiative transfer was part of that process and would have been described by him and his contemporaries unambiguously as theory. In 1951, when Joseph Chamberlain and Lawrence Aller (who, being a modest fellow, selected a paper by Donald Menzel, late director of Harvard) were demonstrating that certain classes of stars were deficient in heavy elements relative to the sun, the work counted as a mix of theory and observation. By the end of that decade, when Larry Helfer, George Wallerstein (who selected the Chamberlain and Aller paper), and Jesse Greenstein (who selected one of his own) were doing the same sort of thing, it was equally unambiguously observational astronomy.

Which paper did I select and why? Ah, for that and much else you will have to make contact with your own copy of the Centennial Issue. ApJ subscribers (including libraries) and selectors received them automatically. And the publisher will be happy to sell you one.

W. Garrett Scaife, *From Galaxies to Turbines: Science, Technology and the Parsons Family* (Institute of Physics Publishing, Bristol and Philadelphia, 2000). xvi +579 pp., illus. \$45 (£35)

Reviewed by Deborah Jean Warner, Curator, Physical Sciences Collection, National Museum of American History, Smithsonian Institution, Washington, D.C. 20560-0636.

The generally acknowledged truth, that an engineer with an interest in history is in need of a good editor, is well borne out in this instance. W. Garrett Scaife certainly tells us a great deal about the lives and works of William Parsons (1800-1867) and his youngest son, Charles A. Parsons (1854-1931), and many of their relatives, but he does so in a manner that is redundant and poorly organized, and that will probably fail to interest all but the most devoted enthusiasts. And this is too bad, because William and Charles made important contributions to astronomy and engineering, because Scaife has examined everything written by and about the Parsons, including a fine cache of family correspondence, and because Scaife has a secure command of the science and technology of which he writes.

The Parsons forebears had participated in the Tudor settlement of Ireland in the late 16th century, enriched themselves with lands taken from an Irish family, changed the name of their village from Birr to Parsonstown (though their home is still known as Birr Castle), obtained titles (the head of the family being the Earl of Rosse, and his eldest son being Lord Oxmantown), and, time and again, married English women of means. Unlike most members of the Anglo-Irish gentry, however, the Parsons seem to have been fairly decent to their tenants, and interested in the life of the mind.

William Parsons graduated from Magdalen College, Oxford in 1821, and joined the recently established Royal Astronomical Society, in London, soon thereafter. By the end of the decade he had gathered a team of assistants and embarked on the daunting and expensive experiments that would lead to the construction of a 36-inch reflecting telescope. When tested in 1839, this instrument was judged to be the most powerful telescope ever built. In the early 1840s, William became the Third Earl of Rosse, received an honorary LLD from Cambridge University, presided over an annual meeting of the British Association for the Advancement of Science, and built a telescope boasting a 72-inch metal mirror. This amazingly powerful instrument, known as the Leviathan of Parsonstown, revealed numerous details of spiral galaxies that had not even been imagined. In short order, Parsons was elected President of the Royal Society of London and an honorary member of the Institution of Civil Engineers, and he became one of the first recipients of the Royal Society's Gold Medal. Unfortunately, as might have been expected, Parsons' great telescopes did not fulfill their promise, as the mirrors tarnished, and the seeing conditions in Ireland were far from optimal.

Charles Parsons was one of the most innovative industrialist/engineers of his generation and, like his father, much of his success came from sophisticated experimentation. Parsons prepared for his career by reading mathematics at St John's College, Cambridge and serving an apprenticeship in the Elswick Works at Newcastle-on-Tyne. He obtained the first of his many patents in 1877, this for a four-cylinder rotary steam engine in which the cylinders rotated as well as the shaft. He made his mark, however, with steam turbines that generated electricity for domestic and industrial uses. Convinced that steam turbines might also be applied to marine propulsion, Parsons designed and built the *Turbinia*, a small ship that, in 1897, obtained a record speed of 34½ knots. By the end of his life Parsons had received numerous honors, including honorary degrees, the Rumford and Copley Medals of the Royal Society, and other medals from Institution of Civil Engineers, and the Institution of Electrical Engineers.

J.S. Al-Khalili, *Black Holes, Wormholes, and Time Machines* (Institute of Physics Publishing, Bristol and Philadelphia, 1999). 256 pp., illus. \$16.50 (£9.99), paper.

Reviewed by B. Kent Harrison, Professor of Physics, Brigham Young University.

This book, for the lay reader, is well reasoned and is written in a straightforward, rather lighthearted style. The author treats several current matters in physics and astronomy and generally succeeds in making them interesting and somewhat comprehensible for the nonphysicist, avoiding technical language.

The book's background theme is time travel, motivated by the author's boyhood interest in it. The main text begins with a chapter on the fourth dimension, using the familiar simplified model of a two-dimensional world imbedded in three dimensions. Newtonian gravitation and Einstein's general relativity are treated in the next chapter. While this material is fairly good, some of it is imprecise, which may leave the reader confused. For example, p. 25 states that an amusement park ride capsule feels like it is really accelerating (of course, it is!) and says the "principle used in these rides is known as Einstein's principle of equivalence and is so simple it can be stated in one word: g-force." This sounds mysterious rather than simple. Another confusing remark is "the force you feel when accelerating ... and the force of gravity are equivalent to each other." However, a later paragraph about a rocket trip gives a good picture of equivalence. Weightlessness is treated reasonably well, although (28) the author states that in free fall one is experiencing zero gravity – then says, "more correctly", that gravity has been cancelled out by one's acceleration. The author's treatment of a horizontally moving ball or light ray is accurate, as is his discussion of Einstein's concept of gravity as an effect of curved space and his treatment of stellar evolution and element generation.

Chapter 3, on cosmology and the Big Bang, presents a nice summary of the basic points of the history of this subject. An unusual tidbit of information is the fact that Edwin Hubble almost became a heavyweight boxer (50) (what a loss that would have been!) The author's sense of humor is evident when he says that the cosmological constant has made more comebacks than the Rolling Stones (55). To the author's credit, he revised his text before publication to include the recent indications from supernovae data that the Universe expansion may be speeding up (71). There is a good discussion of Olbers' paradox. Inflation is discussed briefly.

The chapter on black holes gives a good history of our understanding of light, through Newton, Young, Maxwell, waves and photons. Although Einstein is correctly credited with the explanation of the photoelectric effect, he is also given credit for helping develop quantum mechanics in the 1920s (81)! The black hole discussion gives proper early credit to Michell and Laplace. However, the text states (88) that, for a star in which the escape velocity equals the speed of light, general relativity requires that the surface gravitational force required to keep it from collapsing further on its surface would be infinite. This is not true; the infinite value is just a coordinate effect, and an infalling observer experiences finite gravitational force. The infinite force needed to prevent collapse would be *nongravitational*. However, infall, tidal forces, and gravitational redshift are properly discussed later. X-ray binaries, Hawking radiation, and white holes are treated briefly.

The next chapter treats the Big Bang and the beginning of time, both classically and quantum mechanically. This leads to a discussion of entropy and the second law of thermodynamics (which the author calls the most important law of physics (123), a debatable assertion.) The treatment of arrows of time is good but complex; in connection with a discussion of an opinion of Hawking, the author thinks that the time of maximum expansion in a closed universe would mark the end of time.

I found the author's treatment of special relativity to be somewhat unsatisfactory. Believing spacetime diagrams to be "impenetrable", he avoids their use. I think this is a mistake; I find them useful, even in elementary discussions. The Michelson-Morley experiment is treated only briefly because it is "tricky". The old analogy of a boat going up and down the river or across the river would have been helpful.

Some rather negative statements by the author may prejudice the reader to think that it really is impossible to understand special relativity and that there are real paradoxes in it. An example is that if the reader accepts Einstein's postulates that he will be selling his soul to the devil (147). Then (148) he implies that common sense opposes relativistic velocity addition; a better term would be "common experience".

The discussion of time dilation (148-152) unfortunately perpetuates the sense of paradox. There is no attempt to explain time

dilation by comparing *one* moving clock with *two* stationary clocks, which would remove the paradox. The author does use the usual “light clock”, with a diagram, to justify time dilation, but shows only one stationary observer, thus missing an essential point. The need for two observers in observing length contraction is also not mentioned. A single observer could be invoked if the relativistic rotation discovered by Terrell were described, but it is not. However, the discussion of the twin paradox seems generally correct.

Minkowski spacetime is diagrammed as a “block” universe (later used in discussing the matter of whether the future “already” exists). While I understand the author’s use of this diagram, I would have preferred use of a light cone. The author makes the distinction between time and space depend only upon time’s having a direction, whereas space directions do not. The discussion of simultaneity is garbled; while the author correctly notes that causality must apply, he states (169) that there is a problem in disagreement in time ordering only for events very close in time, suggesting a quantitative difference instead of a qualitative one (and one might ask in which frame the separation is “small”.)

The chapter on time travel paradoxes is interesting, entertaining, and treats the matter more thoroughly than I have seen elsewhere. It includes both classical matters and quantum matters, leading into parallel universes.

The treatment of wormholes is good. Much of the discussion is based on Wheeler’s original idea of wormholes, but also on later investigations by Thorne and his students, on Thorne’s popular book, and on Visser’s book on wormholes. The book under review may be compared with the latter two. Thorne’s book is more carefully done, although its intent was somewhat historical and autobiographical as well as expository; Visser’s book is of course too technical for the lay reader.

The chapter on time machines is very detailed, speculative of course. Various time machines – Tipler, cosmic string, and wormhole – are considered. Much of this also is based on Thorne’s previous work. The last chapter, on ultimate theories, is also good. The bibliography has many nice listings, both technical and nontechnical.

There are a few other quibbles. Eddington’s 1919 eclipse expedition (32) actually went to the island of Principe, while another group went to Brazil. Tachyons were not so much “predicted” by special relativity (44) as found consistent with it. Niels, of Niels Bohr, is misspelled twice (248).

Overall, I would recommend this book for the lay reader; there is much in it to catch the interest.